

# Is there a Psychological Poverty Trap?\*

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## Abstract

The question whether poverty perpetuates itself through psychological mechanisms has recently received renewed interest. For example, poverty may have affective consequences, such as increasing stress and depression, which in turn exacerbate poverty because they impair decision-making or productivity. Whether this feedback loop constitutes a poverty trap in the strict sense, i.e. a situation from which people cannot free themselves without significant intervention, depends on whether a specific technical condition is fulfilled: the product of the elasticity of psychological well-being with respect to income, and the elasticity of income with respect to psychological well-being, has to be larger than one over some range. Here I test whether the elasticity of psychological well-being with respect to income is large enough for this condition to be plausibly fulfilled. I re-analyze data from a large randomized experiment in rural Kenya in which 503 poor families received one-time unconditional cash transfers of USD 404 or USD 1525. Comparing these families to 432 randomly chosen control households and using detailed questionnaire data on both income and various measures of psychological well-being allows me to estimate the elasticity of psychological well-being with respect to income. I find large elasticities, with several estimates around unity, especially for the larger of the two transfer amounts. Together with existing evidence showing a large elasticity of productivity with respect to psychological well-being, this result suggests that a poverty trap which operates through psychological well-being is plausible.

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# 1. Introduction

In recent years, the question whether poverty may perpetuate itself through psychological mechanisms has received renewed interest from psychologists and economists. The basic formulation of the hypothesis is that poverty may have particular psychological consequences, which in turn reinforce poverty (Mani, Mullainathan, Shafir, & Zhao, 2013; Shah, Mullainathan, & Shafir, 2012; Mullainathan & Shafir, 2013). For example, poverty might lead to depression, and depression might decrease labor supply and productivity, leading to more poverty (Haushofer & Fehr, 2014). Together, these relationships could constitute a vicious cycle in which people fall ever deeper into poverty.

It is tempting to describe this mechanism as a “psychological poverty trap”, as has been done on occasion in the popular press (e.g. Schechter, 2012). However, the feedback loop described above does not necessarily trap people in poverty: an improvement in either poverty or its psychological consequences could easily turn the vicious cycle into a virtuous one. In contrast, the word “trap” implies that people cannot easily escape. Indeed, in economic theory, the term “poverty trap” is reserved for states of poverty which an individual cannot escape unless an extremely large change takes place, such as a dramatic improvement in their economic situation. A precise technical definition, which will be detailed below, formalizes this requirement. Whether or not individuals are truly trapped in the technical sense has serious policy implications: in a trap, only very large interventions are effective, and piecemeal improvements are a waste of money. This idea motivated “big push” arguments to reduce poverty in Sub-Saharan Africa (e.g. Sachs, 2006).

The purpose of the present paper is to examine whether psychological poverty traps in the strict sense might exist. What would it take for this to be the case? The technical requirement for a poverty trap in economic theory is perhaps best explained graphically. Consider the stylized relationship between income today,  $y_t$ , and income tomorrow,  $y_{t+1}$ , shown in Figure 1. The red line in panel (a) shows a relationship between income today and tomorrow which represents a poverty trap: an individual whose income is to the left of

$y_M$  will inevitably end up with income  $y_L$  over time. The dotted lines represent their gradual descent into poverty: a starting income of  $y_0$  turns into  $y_1$  tomorrow. When tomorrow rolls around,  $y_1$  turns into  $y_2$  the day after;  $y_3$  the day after that, etc., until the individual eventually ends up at  $y_L$ . Conversely, an individual with a starting income to the right of  $y_M$  inevitably converges to an income of  $y_H$  over time. This is not true for individuals whose evolution of income follows the blue function in panel (b); they will inevitably converge to  $y_M$ , regardless of their starting conditions.

This graphical representation illustrates the technical requirement for the existence of a poverty trap: the function mapping income today into income tomorrow must cross the 45-degree line from below. With two variables that mutually affect each other, such as income and psychological well-being, this condition can be expressed as follows: the product of the elasticities of income and psychological well-being with respect to each other must be greater than one over some range.

In the following, we briefly derive this condition. Denote the relationship between income today and psychological well-being by  $\psi = g(y_t)$ , and that between psychological well-being and income tomorrow as  $y_{t+1} = f(\psi)$ . For the relationship between income today and tomorrow to cross the 45 degree line from below at a point  $y^*$ , the slope of the function  $y_{t+1} = f(g(y_t))$  must be greater than 1 at that point:

$$\frac{\partial y_{t+1}}{\partial y_t} > 1$$

Differentiation yields the following expression:

$$\begin{aligned} \frac{\partial y_{t+1}}{\partial y_t} &= \frac{\partial y_{t+1}}{\partial \psi_t} \frac{\partial \psi_t}{\partial y_t} \\ &= \frac{\partial y^*}{\partial \psi_t} \frac{\partial \psi_t}{\partial y^*} \end{aligned}$$

The last equality uses the fact that  $y_t = y_{t+1}$  at  $y_t^*$ . Notice that this expression is the product of two elasticities: the elasticity of income with respect to psychological well-being is  $\frac{\partial y^*}{\partial \psi_t} \frac{\psi_t}{y^*}$ , and the elasticity of psychological well-being

with respect to income is  $\frac{\partial \psi_t y^*}{\partial y^* \psi_t}$ . The terms  $\frac{\psi_t}{y^*}$  and  $\frac{y^*}{\psi_t}$  cancel in the multiplication. Thus, for a psychological poverty trap to exist, the product of these elasticities has to be greater than unity over some range. For example, if a 0.4 standard deviation (SD) increase in income increases psychological well-being by 0.2 SD, the elasticity of psychological well-being with respect to income is  $\frac{0.2}{0.4} = 0.5$ . Suppose further that a 0.2 SD increase in psychological well-being in turn increases income by 0.5 SD, the elasticity of income with respect to psychological well-being is  $\frac{0.5}{0.2} = 2.5$ . The product of the two elasticities is therefore  $0.5 \times 2.5 = 1.25$ . Under these circumstances, there is thus a psychological poverty trap: a one-unit increase in income today leads to an increase in income tomorrow that is greater than one unit, and the converse is true for a one-unit decrease in income.

How might we assess the evidence for such a psychological poverty trap? The old and large literature on nutrition-based poverty traps in economics offers a clue. In a nutrition-based poverty trap, poverty perpetuates itself because the poor cannot consume enough calories to be productive, which makes them even poorer, and so on. Whether such a trap exists has traditionally been studied by considering the ingredient elasticities separately, and asking if they are “large enough” over some range to make a trap plausible. For instance, if the elasticity of calorie consumption with respect to income is so low that the other elasticity—that of income with respect to calorie consumption—has to be very high for a trap to occur, then a trap is unlikely. Thus, studying one of the two elasticities can provide a first indication as to whether a trap might exist. A rule of thumb is that an elasticity close to one makes a trap plausible. The literature on nutrition-based traps has shown that the effect of income on calorie consumption is so small that it would take an implausibly large effect of calorie consumption on income (through productivity) for a nutrition-based poverty trap to exist (Subramanian & Deaton, 1996; Almås, Haushofer, & Shapiro, 2019). As a result, nutrition-based poverty traps are now regarded as unlikely (Banerjee & Duflo, 2011).

In this paper, we apply this approach to psychological poverty traps. We consider the elasticity of psychological well-being with respect to income,  $\frac{\partial \psi_t}{\partial y^*}$ :

Is this elasticity sufficiently large over some range to make a psychological poverty trap plausible?

A problem that arises in estimating the elasticity of income with respect to psychological well-being is that these variables may mutually affect each other – in fact, this is precisely the premise of a psychological poverty trap. Thus, we cannot easily read off the causal effect of income on well-being from the cross-sectional relationship of these variables. We solve this problem by re-analyzing the data of a randomized controlled trial which experimentally manipulated income through randomly assigned unconditional cash transfers (Haushofer & Shapiro, 2016). Between 2011 and 2013, a random sample of 503 poor households in 60 randomly chosen villages in rural Kenya received unconditional cash transfers of either USD 404 (366 households) or USD 1525 (137 households) from the NGO GiveDirectly.<sup>1</sup> These transfers correspond to about 1 year and 4 years of per capita income, respectively. The transfers were unanticipated, one-time gifts that did not have to be re-paid and could be used however households wished. A sample of 432 households in 60 randomly chosen control villages served as the control group. (A third group of households were non-recipient households in treatment villages; comparison of this group to the control group identifies spillover effects. This group is not part of the analysis in this paper.)

About one year after transfers, we surveyed both the treatment and the control group on their income and psychological well-being. The survey included several measures of subjective well-being, including the happiness and life satisfaction questions from the World Values Survey; the Perceived Stress Scale (Cohen, Kamarck, & Mermelstein, 1983); a custom scale about the frequency of everyday worries; and the CESD depression scale (Radloff, 1977). We analyze these measures separately, and also combine them into a psychological well-being index by computing their standardized average (Anderson,

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<sup>1</sup>All USD values are calculated at purchasing power parity, using the World Bank PPP conversion factor for private consumption for KES/USD in 2012, 62.44. The price level ratio of PPP conversion factor (GDP) to KES market exchange rate for 2012 was 0.5. These figures were retroactively changed by the World Bank after 2013; we use those that were current at the time the study was conducted.

2008). In addition, the survey included a detailed expenditure module, which we use as our measure of income, as is standard in developing country surveys. The reason to not ask for income directly is that people in developing countries do not have regular streams of income, and this income often does not reflect real living standards as measured by what people actually consume. Expenditure – i.e. what people spend on goods and services – is thought to be a better measure (e.g. Deaton, 2006).

Using the experimental variation, we can estimate the effect of transfers on both income and psychological well-being, as previously done in Haushofer and Shapiro (2016). In contrast to the previous work, however, we use instrumental variable (IV) regression to estimate the elasticity of psychological well-being with respect to income. This technique proceeds in two steps: first, income is regressed on treatment assignment. Second, psychological well-being is regressed on the predicted values of income from the first regression. In practice, the two steps are combined to obtain correct estimates of standard errors. Instrumental variable regression is useful in this context for two reasons. First, it isolates the variation in income that is caused by the randomly assigned cash transfers, and thus reveals the causal effect of income on psychological well-being. Second, when the variables are defined appropriately, the IV estimator is the elasticity of income with respect to psychological well-being. This is immediately obvious from the fact that the IV estimator in the bivariate case is the “reduced form” divided by the “first stage”. In the concrete case, the reduced form is the effect of cash transfers on psychological well-being, and the first stage is the effect of cash transfers on income. As illustrated in the example above, this is precisely the elasticity of psychological well-being with respect to income.

## 2. Intervention, experimental design, and econometric approach

The intervention, experimental design, and econometric approach used in this study have previously been described elsewhere (Haushofer & Shapiro, 2016; Haushofer, Reisinger, & Shapiro, 2019), and are briefly summarized here. We refer the reader to the companion paper for details. The study was approved by the IRB of Innovations for Poverty Action Kenya. Data and code are available at [princeton.edu/haushofer](http://princeton.edu/haushofer), and the pre-analysis plan for the original analysis is available at <https://www.socialscisceregistry.org/trials/19>.

### 2.1 Intervention

*GiveDirectly, Inc.* (*GD*; [www.givedirectly.org](http://www.givedirectly.org)) is an international NGO founded in 2009 whose mission is to make unconditional cash transfers to poor households in developing countries. At the time of the study, eligibility was determined by living in a house with a thatched (rather than metal) roof. Recipients were informed that they would receive a transfer, and that this transfer was unconditional and one-time. Recipients were provided with a *Safaricom* SIM card and had to register it for the mobile money service *M-Pesa* in the name of the name of the designated transfer recipient. Transfers were delivered via *M-Pesa* at pre-specified dates, as detailed below. The “small” transfer of USD 404 PPP corresponded to about 1 year of per capita income, and the large transfer of USD 1525 PPP to about 4 years; thus, these transfers were relatively sizable.

### 2.2 Design and timing

An overview of the design and timeline is shown in Figure 2. Among the 120 villages with the highest proportion of thatched roofs in Rarieda district, Kenya, 60 were randomly chosen to be treatment villages, while the other 60 were control villages. Within the treatment villages, half of all eligible households were randomly chosen to be treatment households, while

the other half were “spillover” households. The latter group are omitted from the analysis in this paper. A household was eligible if it had a thatched roof, which is a poverty proxy. This process resulted in 503 treatment households at baseline. Villages had an average of 100 households, of which an average of 19 percent were surveyed, and an average of 9 percent received transfers. The transfers amounted to an average of 10 percent of aggregate baseline village wealth (excluding land).

Among treatment households, we further randomized whether the transfer went to the husband or the wife (in dual-headed households). In addition, 137 households in the treatment group were randomly chosen to receive “large” transfers of KES 95,200 (USD 1,525 PPP, USD 1,000 nominal) per household, while the remaining 366 treatment households received “small” transfers of KES 25,200 (USD 404 PPP, USD 300 nominal) per household. Finally, we randomly assigned the transfer to be delivered either as a lump-sum transfer, or as a series of nine monthly installments. The delivery date of the lump-sum transfer was randomized across households, so that the *average* transfer date of the lump-sum and monthly transfers is the same across the two groups. The focus of the present paper is on the differential effects of large and small transfers; we do not conduct separate analyses for gender and timing effects, which have been covered elsewhere (?).

We conducted a baseline survey with all treatment households before they received the first transfer, and an endline after the end of transfers. Households received the first transfer an average of 9.3 months before endline, the last transfer an average of 4.4 months before endline, and the mean transfer an average of 6.9 months before endline.<sup>2</sup> The order in which villages were surveyed at baseline was randomized, and at endline it followed the same order. In a small number of households, the endline survey was administered before the final transfer was received. These households are nevertheless included in the analysis to be conservative (intent-to-treat).

Control villages were surveyed only at endline; in these villages, we sampled

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<sup>2</sup>The mean transfer date is defined as the date at which half of the total transfer amount to a given household has been sent.



432 “pure control” households from among eligible households. Because these pure control households were selected into the sample just before the endline, the thatched-roof criterion was applied to them about one year later than to households in treatment villages. This fact potentially introduces bias into the comparison of households in treatment and control villages; however, we showed in our previous paper (Haushofer & Shapiro, 2016) that this bias was negligible, amounting to 5 households, or 1.1 percent of the sample.

In each household, we surveyed both primary members; usually a husband and a wife. At endline, we observe 1410 individuals in 471 treatment and 432 pure control households, implying that 507 of these 903 households have both a husband and a wife, and the remainder have a single household head.

### **2.3 Data and variables**

In each surveyed household, we collected two survey modules: a household module, which collected information about assets, consumption, income, wealth, food security, health, and education; and an individual module, which collected information about psychological wellbeing, intra-household bargaining and domestic violence, and economic preferences. The two surveys were administered on different (usually consecutive) days. The household survey was administered to any household member who could give information about the outcomes in question for the entire household; this was usually one of the primary members. The individual survey was administered to both primary members of the household, that is, husband and wife, for double-headed households; and to the single household head otherwise. During individual surveys, particular care was taken to ensure privacy; respondents were interviewed by themselves, without the interference of other household members, especially the spouse.

In this study, we focus on the expenditure module collected during the household survey, and the psychological well-being modules collected during the individual survey. Impacts on other outcome categories have been reported in our previous paper (Haushofer & Shapiro, 2016). Appendix A reproduces

the exact survey modules used in this paper in English.

The expenditure module surveyed respondents about spending on 132 food items, such as cereals, meat, and fruits, and 67 non-food items, such as mobile phone airtime, firewood, and travel expenses. For each item, we ask whether money was spent on it by the household during a particular recall period, and how much was spent. The recall period was one week for food items, one month for frequently purchased non-food items such as airtime and transport, and one year for less frequently purchased non-food items, such as home repairs or weddings. All values were converted to purchasing-power parity adjusted USD values and scaled to monthly spending. This variable was then z-scored, i.e. converted into standard deviation units, using the mean and standard deviation of the control group. Our income variable is therefore the z-scored total monthly expenditures across all categories at the household level.

To measure psychological well-being, we administered several questionnaires. All questionnaires were extensively piloted and translated into Swahili. The translations were verified by back-translation into English, followed by reconciliation of any differences.

The World Values Survey happiness question was: “Taking all things together, would you say you are “very happy” (1), “quite happy” (2), “not very happy” (3), or “not at all happy” (4)?” The World Values Survey Life Satisfaction Question was: “All things considered, how satisfied are you with your life as a whole these days on a scale of 1 to 10? (1= very dissatisfied...10= very satisfied)”. We used the 4-item version of the Perceived Stress Scale (Cohen et al., 1983), which asks about the frequency of feelings and thoughts during the past month on a scale from 1 (never) to 5 (very often), including questions like “How often have you felt that you were unable to control the important things in your life?” and “How often have you felt difficulties were piling up so high that you could not overcome them?”. The Worries questionnaire was a scale we developed from scratch to capture worries in this particular context. It asked respondents how worried they are about each of 13 different areas of their life, on a scale from 1 (not at all worried) to 4 (very worried). Areas covered were “health problems and illness”, “problems at home and with rel-

atives”, “accidents and disasters”, “not enough money for basic needs”, “death of a family member”, etc. Finally, the CESD depression scale (Radloff, 1977) asks respondents 20 questions about how often they have felt a particular way during the past week, on a scale from 1 (rarely or none of the time) to 4 (all of the time). Items include “I was bothered by things that usually don’t bother me”, “I felt depressed”, “My sleep was restless”, etc.

Responses to the happiness and life satisfaction questions were z-scored directly. Responses to the Perceived Stress Scale, the custom Worries scale, and the CESD depression questionnaire were summed to a total score for each scale, reverse-coding items appropriately in the process such that higher total scores corresponded to higher levels of stress, worries, and depression. The total scores were then z-scored.

Finally, all five measured were combined into a single psychological well-being index by computing their standardized weighted average at the respondent level. “Weighted” refers to the fact that we weighted each index component by the sum of the rows of the inverted covariance matrix, following Anderson (2008). In practical terms, this procedure means that variables which are highly correlated with others in the index are down-weighted, while those which are less highly correlated with others, and thus putatively add more “new information”, receive relatively more weight.<sup>3</sup> “Standardized” refers to the fact that this index was then z-scored.

## 2.4 Integrity of experiment

We had low levels of attrition; 471 of the 503 treatment households (93.6 percent) were surveyed at endline. We have shown previously that our results are unlikely to be affected by this attrition (Haushofer & Shapiro, 2016). Because we have no baseline data from pure control villages, we cannot run the standard baseline balance checks for these comparisons. However, in our previous within-village analysis, we found that our study had good baseline balance on our outcomes of interest within villages (Haushofer & Shapiro, 2016).

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<sup>3</sup>This specific way of aggregating the individual scales into the index was pre-specified before analysis. In practice it makes very little difference relative to standard z-scoring.

## 2.5 Statistical approach

For the IV regression of psychological well-being on income to be valid, a strong “first stage” is required: treatment assignment has to strongly influence income. We therefore first regress endline income on treatment assignment, using the following OLS model:

$$y_{hv} = \beta_0 + \beta_1 T_{hv} + \varepsilon_{hv} \quad (1)$$

Here,  $y_{hv}$  is endline income of household  $h$  in village  $v$ , denoted originally in USD PPP but then z-scored;  $T$  is assignment to transfers; and  $\varepsilon_{hv}$  is an idiosyncratic error term. We estimate this regression separately for small and large transfers, by restricting the treatment group accordingly. Standard errors are clustered at the village level, which is the level of randomization.

We then estimate the elasticity of psychological well-being with respect to income. As described above, this is achieved through the IV regression of psychological well-being on income, where income is instrumented by treatment assignment. Thus, we estimate the following model:

$$\psi_{ihv} = \beta_0 + \beta_1 y_{hv} + \varepsilon_{ihv} \quad (2)$$

Here,  $\psi_{ihv}$  is the z-scored measure of psychological well-being observed in individual  $i$  in household  $h$  in village  $v$  at endline. Crucially,  $y_{hv}$  is instrumented with treatment assignment;  $\beta_1$  is thus the IV estimate of the elasticity of psychological well-being with respect to income. We estimate three versions of this model. First, we restrict the sample to small transfer recipients and control households; this allows us to estimate the elasticity of psychological well-being over the range from the average income of the control group to that of the treatment group receiving small transfers. Second, we restrict the sample to large transfer recipients and the control group; this analysis estimates the elasticity over the range from the average income of the control group to that of the entire treatment group. Finally, we restrict the sample to transfer recipients only, and use as the instrument assignment to the large transfer.

Because randomization into large vs. small transfers was done within villages, we can include village-level fixed effects in this specification to increase precision. The coefficient on  $\beta_1$  in this model therefore estimates the elasticity over the range from the average income of the small transfer recipients to that of the large transfer recipients. In the first two specifications, standard errors are clustered at the village level; in the last, they are clustered at the household level, again reflecting the relevant level of randomization.

### 3. Results

The first row of Table 1 presents the first stage results, i.e. the impact of treatment with small (column (1)) and large (column (3)) transfers on income. Because the income variable is z-scored, the coefficients are in standard deviation units. Small transfers of USD 404 PPP increase income by 0.25 SD, with a 95 percent confidence interval that excludes zero. Large transfers of USD 1525 PPP increase income by 0.44 SD, with a 99 percent confidence interval that excludes zero. As an aside, we note that we observe some evidence of decreasing returns in the response of income to cash transfers: large transfers are 3.77 times the size of small transfers, but lead to an increase in income that is “only” 1.76 times as large.

We next turn to the reduced-form impact of cash transfers on psychological well-being, shown in columns (1) and (3) of Table 1. Note that these reduced-form effects were previously reported in Haushofer and Shapiro (2016). We find positive impacts of both small and large cash transfers on our measures of happiness and life satisfaction, and negative impacts of both transfer magnitudes on stress, depression, and worries. The effects of transfers on psychological well-being are less concave than those on income: for example, the treatment effect of large transfers on the psychological well-being index is almost exactly twice as large as that of small transfers. Many of the individual coefficient estimates are statistically significant at conventional levels.

The core question of this paper, however, is about the magnitude of the elasticity of psychological well-being with respect to income. Figure 3 plots co-

efficient estimates of the reduced-form regressions of psychological well-being on transfers against those of income on transfers. The elasticity of a given variable is the slope of the lines: each line represents the change in psychological well-being as a function of a change in income, i.e.  $\frac{\partial \psi}{\partial y}$ . The dotted lines represent 45-degree lines, and can be used to visually assess whether the slope of the well-being–income relationship is smaller or larger than one. Numerical estimates are shown in columns (2), (4), and (5). Column (2) shows the elasticity for small transfers; column (4) that for large transfers relative to the control group; and column (5) that for large transfers relative to small transfers. Thus, column (2) corresponds to the slope of the line between the origin and the point estimate for small transfers in the figure; column (5) corresponds to the slope of the line between the point estimates for small and large transfers; and column (4) corresponds to the slope of the (not plotted) line between the origin and the point estimate for large transfers.

The figure and the numerical estimates in the table show elasticities with absolute values between 0.16 and 1.26. The “worries” variable has the shallowest relationship with income, while the stress variable and the index variable show the strongest relationship. Several of the point estimates are close to one, or even exceed one. For example, the point estimate for the psychological well-being index variable is 0.96 for small transfers, 1.09 for all transfers, and 1.26 for large transfers relative to small transfers. All three estimates are statistically distinguishable from zero, and none of them is significantly different from one. For stress, the point estimate for small transfers is  $-0.4$ , for all transfers  $-0.82$ , and for large transfers relative to small transfers,  $-1.25$ . For all but two variables (happiness and worries), the effect of going from small to large transfers is larger than that of going from the control group to small transfers. This is evident in the convexity of the lines in Figure 3 for these variables. Thus, large transfers are more likely than small transfers to lead to increases in psychological well-being that imply an elasticity around one.

## 4. Discussion

The goal of this paper was to assess whether the causal effect of income on psychological well-being is sufficiently strong to make a psychological poverty trap plausible. Following the literature on poverty traps in economics, we estimate the elasticity of psychological well-being with respect to income. We find relatively large estimates: our psychological well-being shows an elasticity greater than one over a particular range of incomes. Thus, a one-unit change in income leads to a change in psychological well-being greater than one unit in this range. This result implies that if the elasticity of income with respect to psychological well-being is moderately large, a psychological poverty trap exists. Specifically, note that if the first elasticity is e.g. 1.26, it is sufficient for the other elasticity to be larger than its reciprocal for their product to exceed one. In other words, if the elasticity of income with respect to psychological well-being is larger than 0.79, a psychological trap exists.

Of course, understanding whether this is the case would require a causal estimate for the impact of psychological well-being on productivity. To get a sense of what the answer to this question might be, we turn to a study by Oswald, Proto, and SgROI (2015), in which the happiness of participants was experimentally raised through a video clip, and productivity in a piece-rate task was subsequently measured. The instrumental variable estimate of happiness on productivity, with happiness instrumented by treatment assignment, is 8.92 points for a one-point change in happiness (their Experiment 2, Table 2). To express this value in standard deviations of both variables, we proceed as follows. First, the standard deviation of productivity in the placebo group is 6.76 (Oswald et al. Appendix Table B4). Thus, the instrumental variable implies an  $8.92 / 6.76 = 1.32$  standard deviation effect on productivity for a one-point change in happiness. Second, the standard deviation of happiness in the placebo group before the clip is 0.86, and after the clip, 0.95. The right-hand side variable in their instrumental variables regression is the difference between these measures. Because we add variances when we add or subtract random variables, the standard deviation of the difference

is  $\sqrt{SD_{after}^2 + SD_{before}^2} = 1.28$ . A one-point change in happiness therefore corresponds to  $1/1.28 = 0.78$  standard deviations of happiness. Putting this estimate together with the one derived first, a one SD change in happiness would be predicted to generate a  $1.32/.78 = 1.69$  SD increase in productivity. Taken at face value, this estimate is very large, and would generate a poverty trap in combination with several of our estimates. This is the case, for example, for all three estimates of our happiness elasticity, which range from 0.63 to 0.93. Even the smallest would (just barely) generate a trap in combination with this substantial elasticity of productivity with respect to happiness.

Thus, our estimates of the elasticity of psychological well-being with respect to income are large enough that a psychological poverty trap is at least plausible. An important caveat is that, for this conclusion to be valid, we have to believe that what is *reported* on our measures of well-being actually reflects *experienced* well-being, rather than a possibly non-linear transformation of it. This point is general and applies to many studies of subjective well-being, but, as pointed out by Oswald (2008), it is particularly salient if we aim to make statements about the *shape* of the function that maps income into well-being. Oswald points out that if well-being is experienced and reported in a similar fashion as e.g. perceptual stimuli, it could be that the “reporting function” of well-being is concave, similar to the power law in psychophysics, while “actual” well-being has a different shape. A silver lining is that this argument is most damaging when reported happiness is concave in income, whereas our finding here is precisely that it is linear or even convex. However, the point remains that we have to believe that our self-report measures capture well-being veridically.

This result contributes to a growing literature in psychology and economics which investigates the psychological consequences of poverty (Shah et al., 2012; Mani et al., 2013; Mullainathan & Shafir, 2013; Haushofer & Fehr, 2014). A theme in this literature has been that poverty might be self-perpetuating through its effect on psychological outcomes and decision-making. However, whether this mechanism constitutes a poverty trap in the strict sense of the word as it is used in economic theory is incompletely understood, and is the



question posed in this paper for the special case of psychological well-being. Of course, other variables might plausibly be affected by poverty, and might in turn affect poverty. Perhaps the most prominent candidates are cognitive processes such as attention, which are emphasized in the work of Shah et al. (2012), Mani et al. (2013), and Mullainathan and Shafir (2013). This mechanism could operate independently of, or in concert with, the one considered here. For instance, effects of poverty on psychological well-being might exacerbate its effects on decision-making.

In addition, this study contributes to a large literature that estimates the relationship between income and psychological well-being. Whether or not there is a positive correlation has been the subject of substantial debate ever since the seminal contributions of Easterlin in the 1970s (e.g. Easterlin, 1974). Most recently, this literature has developed in at least three directions. First, it was shown that income is correlated with several measures of happiness both within and across countries, and over time, despite early protestations to the contrary (Stevenson & Wolfers, 2008; Sacks, Stevenson, & Wolfers, 2012). Second, researchers have begun to distinguish between different psychological variables, such as happiness or hedonic well-being on the one hand, and life satisfaction or evaluative well-being on the other (Kahneman & Deaton, 2010). Finally, an increasing number of studies is in a position to estimate causal, rather than correlational effects (e.g. Gardner & Oswald, 2007; Lindqvist, Östling, & Cesarini, 2018). The present study continues these developments in that it shows a positive causal impact of income on various measures of well-being. In addition, however, it takes seriously the magnitude of the estimated relationships, and asks what their theoretical implications are. Future work might attempt to combine interventions that allow estimation of the elasticity of well-being with respect to income with other interventions which allow estimating the elasticity of income with respect to well-being. The study by Oswald et al. (2015) provides a working example; other possibilities are studies that improve psychological well-being through psychotherapy interventions or mindfulness training.

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Figure 1: Illustration of a Poverty Trap

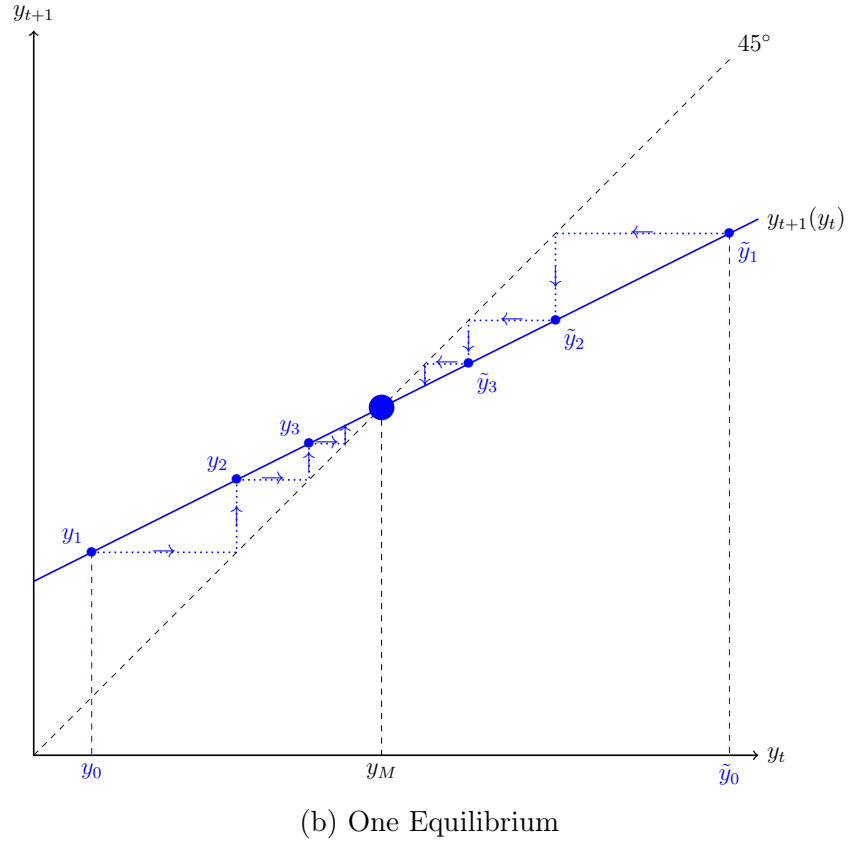
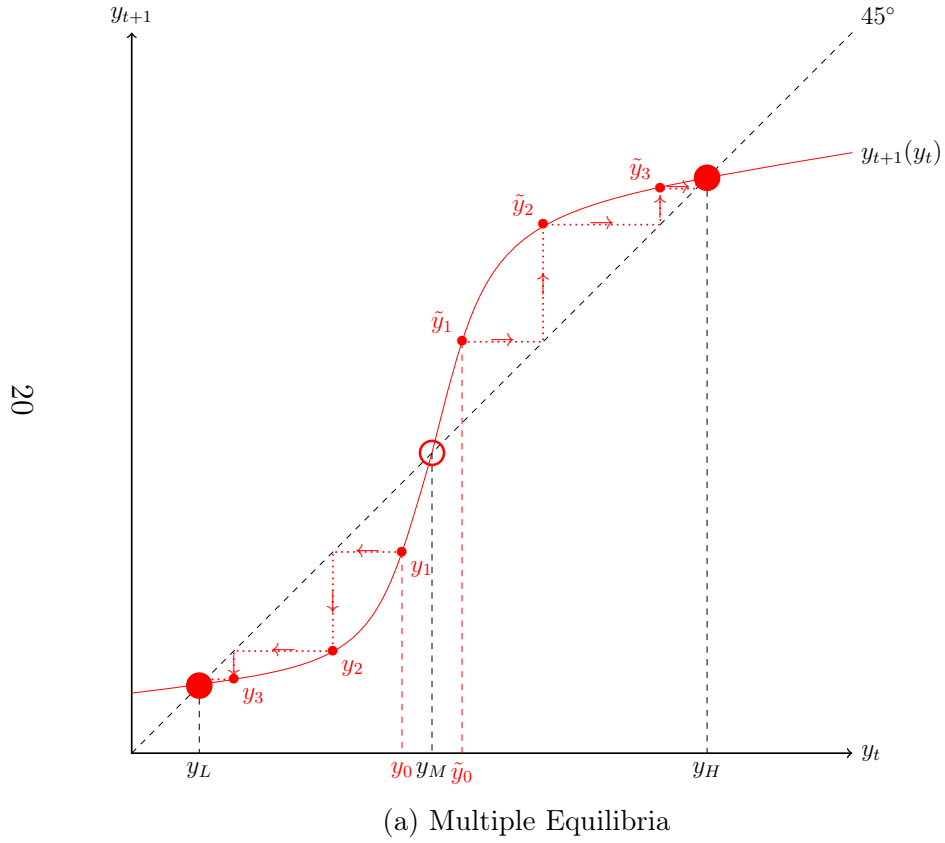


Figure 2: Timeline of study

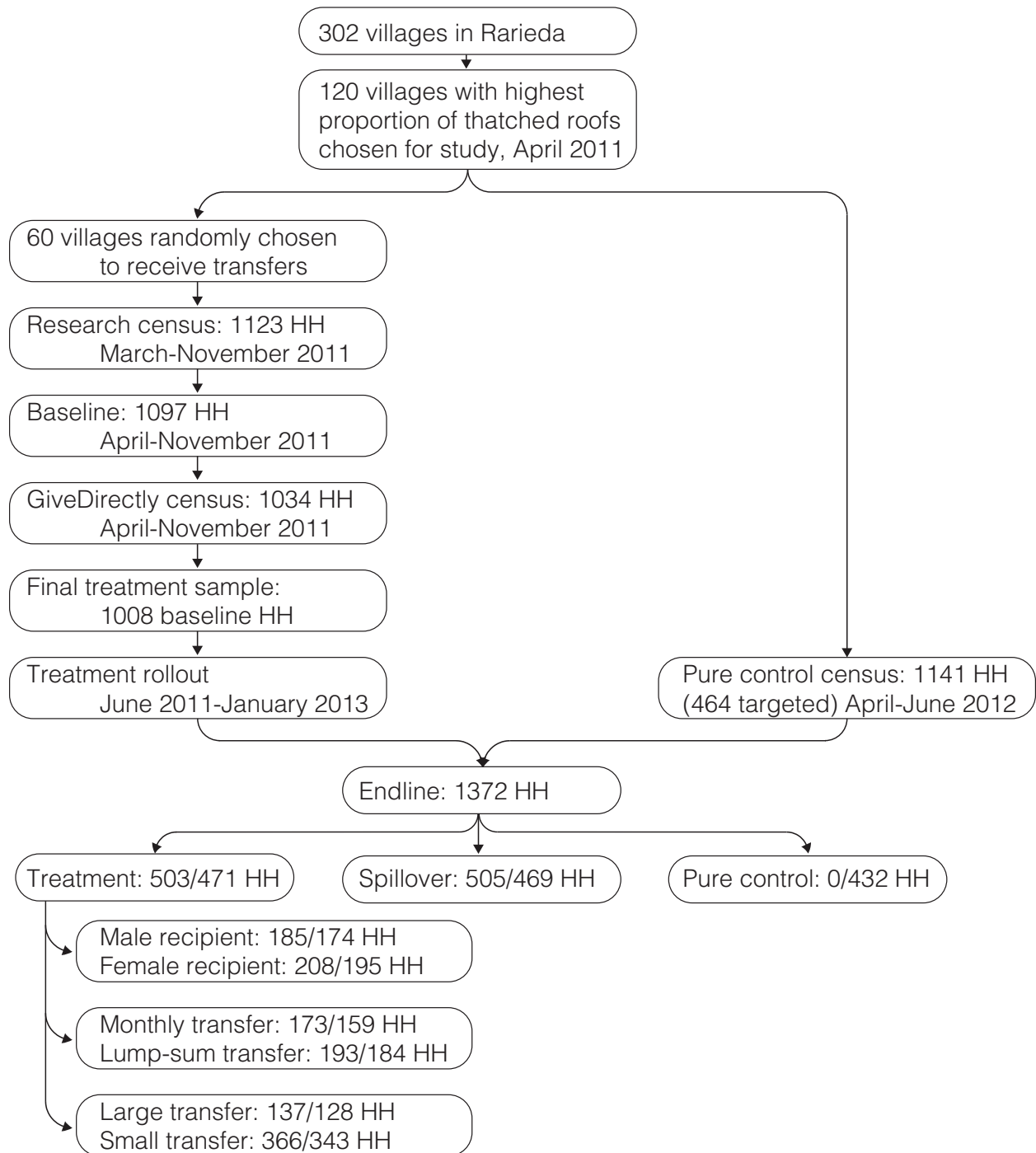
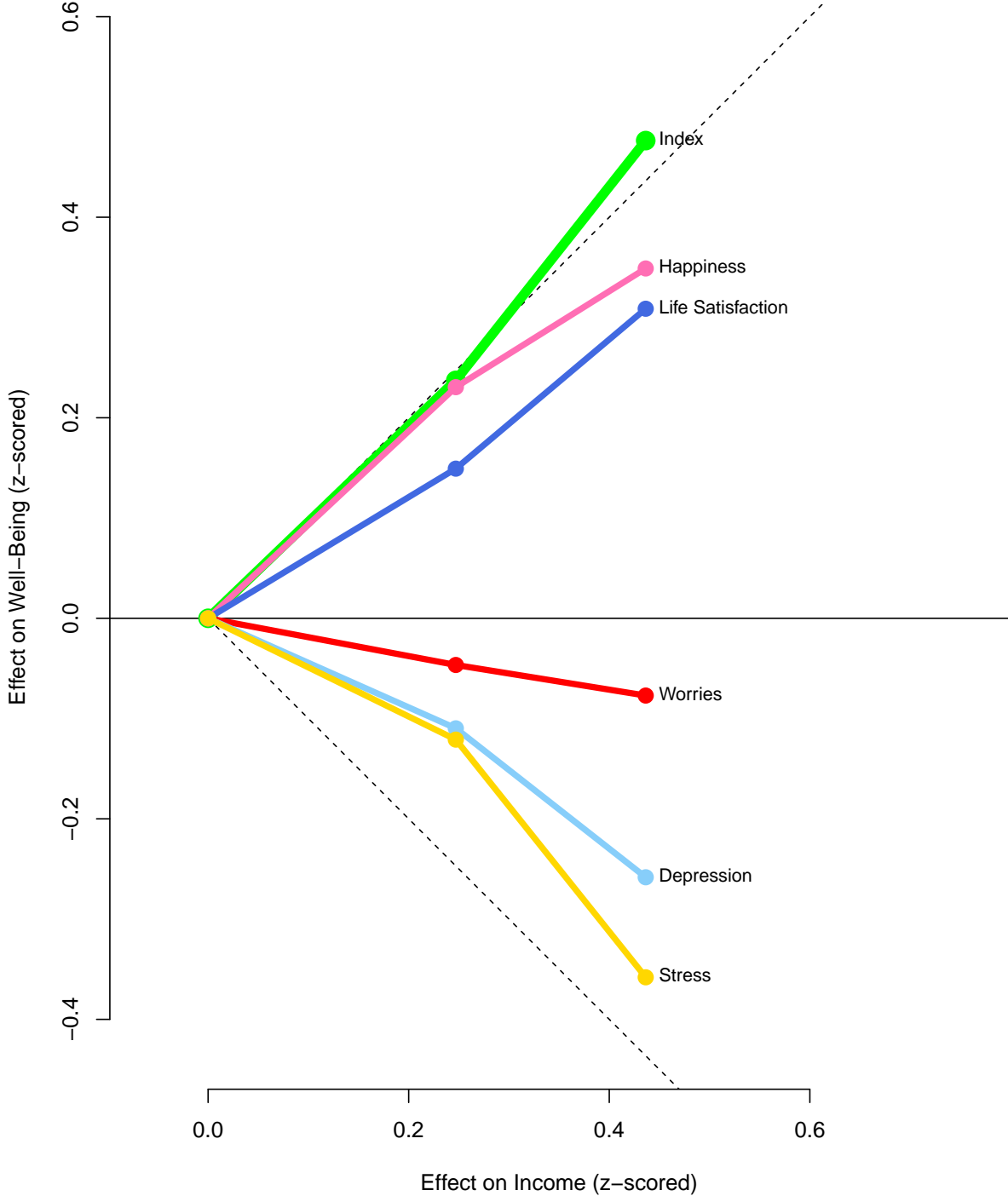


Figure 3: Consumption and well-being responses to small and large transfers



Notes: Regression estimates for the effect of cash transfers on income, and cash transfers on psychological well-being. Each dot represents the impact of randomly allocated small (USD 404 PPP) or large (USD 1525 PPP) cash transfers on income, measured through a comprehensive consumption questionnaire, and plotted on the x-axis; and various measures of well-being, measured through self-report questionnaires and plotted on the y-axis. All units are in standard deviations. The elasticity of psychological well-being with respect to income corresponds to the slopes of the lines.

Table 1: Income and Well-being Effect

	Small Transfers		Large Transfers		
	(1) Treatment Effect	(2) Income Effect (IV)	(3) Treatment Effect	(4) Income Effect relative to control (IV)	(5) Income Effect relative to small transfer (IV)
<i>First Stage</i>					
Income (z-scored)	0.25 (0.021)**		0.44 (0.000)***		
<i>Reduced Form and IV</i>					
Psychological Well-Being Index (z-scored)	0.24 (0.002)***	0.96 (0.048)**	0.48 (0.000)***	1.09 (0.002)***	1.26 (0.091)*
Happiness (z-scored)	0.23 (0.000)***	0.93 (0.071)*	0.35 (0.000)***	0.80 (0.005)***	0.63 (0.250)
Life Satisfaction (z-scored)	0.15 (0.054)*	0.60 (0.102)	0.31 (0.001)***	0.71 (0.008)***	0.84 (0.145)
Stress (z-scored)	-0.12 (0.122)	-0.49 (0.141)	-0.36 (0.001)***	-0.82 (0.004)***	-1.25 (0.085)*
Worries (z-scored)	-0.05 (0.514)	-0.19 (0.521)	-0.08 (0.412)	-0.18 (0.404)	-0.16 (0.710)
Depression (z-scored)	-0.11 (0.108)	-0.44 (0.180)	-0.26 (0.008)***	-0.59 (0.042)**	-0.78 (0.166)

Notes: OLS estimates for the effect of cash transfers on income and psychological well-being, and IV estimates for the effect of income on psychological well-being, with income instrumented by cash transfers.  $p$ -values are shown in parentheses. \*\*\* denotes significance at the 1 percent level, \*\* 5 percent, and \* 10 percent level. Column (1) shows the reduced-form effect of randomly allocated small (USD 404 PPP) cash transfers on monthly household income, measured by a comprehensive consumption questionnaire (first row), and coded originally in USD PPP, but then z-scored; and various measures of psychological well-being, also z-scored. Column (3) shows the same effects for large (USD 1525 PPP) cash transfers. Columns (2), (4), and (5) show the instrumental variable estimates for the effect of income on the various measures of psychological well-being, where income is instrumented with the random assignment to a cash transfer. In Column (2), we restrict the treatment group to small transfer recipients, and the listed instrumental variable estimates thus correspond to the elasticity of psychological well-being with respect to income over the range of income from the average control group income to the average income of the small transfer recipients. Similarly, in Column (4), we restrict the treatment group to the large transfer recipients, and the IV estimates therefore correspond to the elasticity over the range from the average control group mean to that of the large transfer recipient group. Finally, Column (5) shows the elasticity of well-being with respect to income when income moves from the average of the small transfer group to that of the large transfer group. Standard errors are clustered at the village level in columns except column (5), where they are clustered at the household level. Column 5 additionally includes village-level fixed effects.

## A. Survey modules

### A.1 Expenditure module

Now I would like to ask you questions about how you spent money in the last few months. Do not include business expenses here.

1. Food (for each item read list and calculate total)

Did your HH consume this item in the past one week? (1 = Yes 2 = No) What was the value of the amount consumed in total (KES)?

(a) Cereals

- i. Bread
- ii. Rice
- iii. Cakes
- iv. Maize grain or flour
- v. Biscuits
- vi. Green maize
- vii. Wheat grain or flour
- viii. Millet grain or flour
- ix. Sorghum grain or flour
- x. Barley and other cereals
- xi. Breakfast cereals/oats
- xii. Pasta (spaghetti, macaroni)
- xiii. Other

(b) Roots and tubers

- i. Potatoes
- ii. Sweet potatoes
- iii. Arrow root
- iv. Cassava and/or cassava flour
- v. Yams
- vi. Crisps
- vii. Other

(c) Pulses



- i. Beans
- ii. Grams
- iii. Black grams
- iv. Peas
- v. Groundnut
- vi. Cowpea
- vii. Other

(d) Vegetables

- i. Onions/leeks
- ii. Cabbages
- iii. Carrots
- iv. Tomatoes
- v. Spinach
- vi. Kale
- vii. Pilipili hoho
- viii. Cucumber
- ix. French beans
- x. Celery
- xi. Mushrooms
- xii. Cauliflower
- xiii. Eggplant
- xiv. Pumpkins
- xv. Okra
- xvi. Dania
- xvii. Other

(e) Meat

- i. Beef
- ii. Pork
- iii. Bacon
- iv. Mutton/goat
- v. Chicken
- vi. Camel

- vii. Other
- (f) Fish
  - i. Fresh
  - ii. Dried
  - iii. Frozen fillet
  - iv. Smoked
  - v. Prawns
  - vi. Other
- (g) Dairy products and eggs
  - i. Milk
  - ii. Condensed milk
  - iii. Baby milk
  - iv. Sour milk (Mala)
  - v. Yoghurt
  - vi. Fresh cream
  - vii. Cheese
  - viii. Eggs
  - ix. Other
- (h) Other animal products
  - i. Offals/Matumbo (kidney, liver, etc.)
  - ii. Sausages
  - iii. Ham/Salami
  - iv. Corned beef
  - v. Other
- (i) Oils and fats
  - i. Butter
  - ii. Ghee
  - iii. Margarine
  - iv. Cooking fat
  - v. Cooking oil
  - vi. Lard (Animal fat)

- vii. Peanut butter
- viii. Other
- (j) Fruits
  - i. Ripe banana
  - ii. Cooking banana
  - iii. Oranges
  - iv. Pawpaws
  - v. Avocado
  - vi. Mangos
  - vii. Pineapples
  - viii. Passion fruit
  - ix. Pears
  - x. Plums
  - xi. Apples
  - xii. Lemons
  - xiii. Grapefruit
  - xiv. Strawberries
  - xv. Other berries
  - xvi. Melons
  - xvii. Grapes
  - xviii. Coconuts
  - xix. Other
- (k) Sugars
  - i. Sugar
  - ii. Sugar cane
  - iii. Jaggery
  - iv. Icing sugar
  - v. Other
- (l) Jam, honey, sweets, candies
  - i. Jam
  - ii. Marmalade

- iii. Honey
  - iv. Chocolate
  - v. Sweets
  - vi. Chewing gum
  - vii. Other
- (m) Non-alcoholic drinks
- i. Preserved fruit juice
  - ii. Tea
  - iii. Coffee
  - iv. Soda
  - v. Soya drink
  - vi. Health drink
  - vii. Drinking chocolate
  - viii. Mineral water
  - ix. Other
- (n) Alcoholic drinks
- i. Spirits
  - ii. Wine
  - iii. Beer
  - iv. Brews (Buzaa, etc.)
  - v. Cider
  - vi. Other
- (o) Tobacco
- i. Cigarettes
  - ii. Cigars
  - iii. Tobacco
  - iv. Snuff
  - v. Khatt or Miraa
  - vi. Other
- (p) Spices and condiments
- i. Salt

- ii. Tomato sauce
- iii. Chili sauce or powder
- iv. Baking powder
- v. Yeast
- vi. Mustard
- vii. Vinegar
- viii. Pickles
- ix. Pepper
- x. Other

(q) Prepared foods

- i. Vendor food
- ii. Cafes/Takeaways
- iii. Kiosks
- iv. Restaurants/hotels
- v. Other

(r) Other foods

- i. Tinned beans or pulses
- ii. Soups
- iii. Tinned fish
- iv. Baby food
- v. Other

(s) Please specify what other food items you have spent money on

2. Did you spend money (or goods) on this in the past one month? (1 = Yes 2 = No)  
How much did you spend in total (KES)?

(a) Airtime, internet, other phone expenses

(b) Travel, transport, hotels (NOT including medical reasons)

- i. Petrol
- ii. Road tolls
- iii. Ferry tolls
- iv. Taxi
- v. Parking charges

- vi. City bus
- vii. Matatu
- viii. Country bus
- ix. Boda/Piki
- x. Train
- xi. Local flight
- xii. Hotel stays
- xiii. Other
- (c) Lottery tickets/gambling
- (d) Clothing and shoes
- (e) Recreation/entertainment (read list below, calculate total)
  - i. Cinema tickets
  - ii. Videos
  - iii. Music/CDs
  - iv. Books/Magazines
  - v. Stadium tickets
  - vi. Concert tickets
  - vii. Park/Safari tickets
  - viii. Other
- (f) Personal items
  - i. Haircut
  - ii. Aftershave, Body lotion, Toothpaste, razors, sanitary pads
  - iii. Hair oil, Perfume, Petroleum jelly, Baby oil/powder, Cotton wool, Handkerchiefs
  - iv. Cosmetics: lipstick, nail polish, eye-liner, etc.
- (g) Household items (read list below, calculate total)
  - i. Soap
  - ii. Combs
  - iii. Toilet paper/tissue, etc.
  - iv. Detergent
  - v. Dish washing paste

- vi. Broom/mop/duster
  - vii. Disinfectant
  - viii. Air freshener
  - ix. Floor polish
  - x. Insecticide
  - xi. Shoe polish
  - xii. Match box
  - xiii. Candles
  - xiv. Laundry
  - xv. Other
- (h) Firewood, kerosene, charcoal
  - (i) Electricity
  - (j) Water
3. Did you spend money (or trade goods) on this in the past 12 months? (1 = Yes 2 = No) How much did you spend in total (KES)?
- (a) House rent/mortgage
  - (b) Fixing home damage or improving or expanding the home
  - (c) Religious expenses or other ceremonies (excluding weddings and funerals)
  - (d) Charitable donations
  - (e) Weddings
  - (f) Funerals (including outside their household if contributed to their costs)
  - (g) School/college fees, uniforms, books, or other supplies
  - (h) Medical expenses, (including consultation fees, medicines, hospital costs, lab test costs, ambulance costs, and related transport)
  - (i) Household durables
    - i. Cutlery
    - ii. Pots, pans
    - iii. Blender
    - iv. Light bulb
    - v. Cups/glasses/mugs

- vi. Curtains
- vii. Carpets
- viii. Lamps
- ix. Fans
- x. Plates
- xi. Vases
- xii. Mirrors
- xiii. Chairs
- xiv. Tables
- xv. Other furniture
- xvi. Other durables
- (j) Dowry/bride price
- (k) Fees paid to the village elder or chiefs

## **A.2 Psychological well-being modules**

### **A.2.1 Happiness and Life Satisfaction (World Values Survey)**

Now I will ask some more questions about your feelings and opinions.

1. Taking all things together, would you say you are “very happy” (1), “quite happy” (2), “not very happy” (3), or “not at all happy” (4)?
2. All things considered, how satisfied are you with your life as a whole these days on a scale of 1 to 10? (1= very dissatisfied...10= very satisfied)

### **A.2.2 Perceived Stress Scale (Cohen, 1983)**

The questions in this scale ask you about your feelings and thoughts during the last month. In each case, you will be asked to indicate your response by telling me how often you felt or thought a certain way. Although some of the questions are similar, there are slight differences between them and you should treat each one as a separate question. The best approach is to answer quickly. That is, don't try to count up the number of times you felt a particular way but rather indicate the choice that seems like a good estimate.

Response codes: 1 = Never, 2 = Almost never, 3 = Sometimes, 4 = Fairly often, 5 = Very often



1. How often have you felt that you were unable to control the important things in your life?
2. How often have you felt confident about your ability to handle your personal problems?
3. How often have you felt that things were going your way?
4. How often have you felt difficulties were piling up so high that you could not overcome them?

### **A.2.3 Risks and Worries**

Now I would like to ask you questions about which risks you face and what your worries are. How worried are you about the following areas of your life?

Response codes: 1 = not at all worried, 2 = not very worried, 3 = somewhat worried, 4 = very worried

In the past one year, did you experience this? (1=Yes, 2=No)

1. Health problems, illness
2. Problems at home and with relatives
3. Accidents and disasters
4. Problems with people in other ethnic groups
5. Not enough money for basic needs (such as food and clothing)
6. Not being able to educate all children
7. Not enough money for other living expenses
8. Not enough money for medicines and medical treatment
9. Difficulty finding work
10. Idleness of children or spouse
11. Alcohol consumption of children or spouse
12. Death of a family member
13. Debts owed to others
14. Other

#### A.2.4 CESD Depression Scale (Radloff, 1977)

I will read out a list of some of the ways you may feel or behave. Please indicate how often you have felt this way during the past week, using the following scale.

Response codes: 1 = Rarely or none of the time, 2 = Some or a little of the time, 3 = Occasionally or a moderate amount of time, 4 = All of the time

1. I was bothered by things that usually don't bother me
2. I did not feel like eating; my appetite was poor
3. I felt that I could not shake off the blues even with help from my family
4. I felt that I was just as good as other people
5. I had trouble keeping my mind on what I was doing
6. I felt depressed
7. I felt that everything I did was an effort
8. I felt hopeful about the future
9. I thought my life had been a failure
10. I felt fearful
11. My sleep was restless
12. I was happy
13. I talked less than usual
14. I felt lonely
15. People were unfriendly
16. I enjoyed life
17. I had crying spells
18. I felt sad
19. I felt that people disliked me
20. I could not "get going"