The Impact of a Large Scale National Cash Transfer Program on Household Time Preference

This Version: November 2012
Sudhanshu Handa, David Seidenfeld
with
Gelson Tembo[1]

Summary: Over three dozen countries in the developing world now implement large scale cash transfer programs to alleviate poverty. A steady and predictable source of income such as what these programs provide can alter individual discount rates by making recipients less myopic and more forward looking, and thus more willing to delay current consumption for future investment. In addition, participation in a cash transfer program may alter a household's expectations about the future quality of life and thus make it more interested and oriented toward investing in the future. Similarly, participation in a cash transfer program may make people feel happier which itself may influence current decision-making. We use quasi-experimental methods to test whether the Government of Zambia's cash transfer program has affected time preference, and whether the effect works through affect or future expectations. We find that the program impacts discount rates both directly and through increased happiness and positive expectations about the future. This is the first study to investigate changes to discount rates through social protection cash transfer programs.

1. Introduction

How important are preferences in determining economic success? The economic concept of time preference or 'patience'—the importance an individual places on immediate versus delayed consumption—plays a key role in economic models of savings, growth and economic development (Rae, 1905; Samuelson 1937; Smith 1776). Indeed most development economists view this parameter as a major determinant of household poverty, citing 'myopia' as the reason why households do not invest in the means of production (human, physical or financial) which could significantly alter their future standard of living (Bardhan and Udry, 1999). This line of reasoning suggests that individuals who have low discount rates, that is, who are more patient, will accumulate more wealth. The relationship between wealth and low discount rates has been reported for developed countries in Hausman (1979) and Harrison, Lau & Williams (2002) and for developed countries by Pender (1996), Yesuf (2004) and Tanaka et al (2010).

[1] Sudhanshu Handa, Department of Public Policy, University of North Carolina at Chapel Hill (shanda@email.unc.edu); David Seidenfeld, American Institutes for Research (AIR), Washington DC, (dseidenfeld@air.org); Gelson Tembo, Department of Agricultural Economics, University of Zambia (tembogel@gmail.com). We are grateful to Carolyn Huang and Leah Prencipe for excellent research assistance, to UNICEF-Zambia and DFID-Zambia for financial support, and to Manzunzu Zulu and Stanfield Michelo for useful discussions.

Beyond economics, time preference has also been posited to affect self-control in general, and believed to influence other important dimensions of welfare such as addiction, obesity, overconsumption and reduced human capital accumulation (Bernheim & Rangel 2004; Frederick, Lowenstein & O'Donoghue 2002; Kirby et al. 1999). One influential study has shown that children who are more able to delay gratification have higher SAT scores later in life (Mischel et al. 1989). Time preference thus appears to be a key preference parameter with the scope for determining many outcomes related to monetary poverty and human welfare.

A key challenge in the empirical estimation of the relationship between a preference parameter such as the discount rate and wealth accumulation or other decisions is disentangling the direction of causality—do preferences cause economic or social conditions or do such conditions cause preferences (Bowles 1998)? Large scale random assignment of individuals to economic conditions is clearly not feasible, necessitating the use of econometric methods such as instrumental variables to control for the endogeneity of income or wealth or other social outcomes. In this paper we provide new and more robust evidence on the relationship between economic circumstances and time preference by evaluating the impact of a large scale cash transfer program in Zambia on discount rates. The key feature of our design is that potential beneficiaries of the program were randomly assigned to intervention and control status, thus providing an exogenous source of variation that can be used to identify the relationship between time preference and household economic security.

This article makes important contributions to several distinct but related literatures. We contribute to the small but growing literature on the economic impacts of unconditional cash transfer programs in Africa. Cash transfer programs are now ubiquitous across the developing world with at least 16 government programs now in existence in Africa alone (Davis, Gaarder, Handa, Yablonski 2012) and a further two dozen across other parts of the developing world (World bank 2008). But unlike the familiar Latin America and Caribbean programs, those in Africa tend to be unconditional and provide benefits to families that are significantly poorer and with less access to infrastructure. And while most impact evaluations of cash transfer programs focus on social outcomes (Kenya CT-OVC Evaluation Team 2012; Schultz 2002) this is the first that investigates a key behavioral parameter such as the discount rate. We also contribute to the literature on the determinants of time preference by providing new evidence on the relationship between household economic circumstances and time preference based on a stronger identification strategy than most population-based studies—the random assignment of households to economic circumstances (cash transfer receipt). Third, we collected a wide range of information on future expectations, self-assessments of well-being and affect, and so also make a contribution to the literature on the relationship between emotional status or affect and time preference (Ifcher & Zarghamee 2011). Finally, our study is based on a large scale field survey (rather than a small laboratory experiment) and comes from a cash transfer program that is very similar to ones in operation in other African countries, which greatly enhances the external validity of our results to other poor countries and other African cash transfer programs.

2. Program description

In 2007 Zambia's Ministry of Community Development and Social Services started the rollout of a cash transfer program in the Monze district. Zambia had been implementing cash transfer

programs since 2004 in three districts, trying different targeting models in each district. The government decided to scale up the Kalomo model to new districts including Monze. This model targets labor constrained and destitute households deemed by the community to be the 10 percent most poor. Labor constrained and destitute households include elderly headed households, households with disabled people, households caring for chronically ill, households with a high dependency ratio, and households that experience persistent hunger. Beneficiary households receive 40,000 or 50,000 kwacha a month (equivalent to $8 or $10 respectively) depending if the household has children, in which case they receive the higher amount. Payments are made every other month and there are no conditions to receive the money. The purpose of this program is to supplement the income for poor households to ensure that they can eat at least one meal a day, improve diet diversity, and help them access government services such as schools and health clinics.

Cash transfer programs are defined by their targeting mechanism. Monze implements a community-based targeting method to identify beneficiary households. Community Welfare Assistance Committees (CWAC) first convene to nominate households in their community that they believe meet the labor constrained or destitute criteria defined in the operations manual. Next, CWAC members collect data on the nominated households and the data are confirmed by the village headman as valid. At a second meeting, all of the nominated households are ranked by their level of destitution and a cutoff line is drawn to identify the most destitute 10 percent of the community. At a third meeting the entire list with the cutoff score is presented to the community for transparency and open debate about the household scores. After the community agrees on the list of identified beneficiary households, the CWAC members submit the list to the District Social Welfare Office (DSWO) where the list is further scrutinized by district officers in the presence of CWAC members. The DSWO makes the final decision about household eligibility and determines who will become beneficiaries. An explanation is provided for each beneficiary and rejected household. Finally, households are then notified of their final eligibility status.

3. Identification Strategy

The Monze impact study was originally designed as a randomized controlled trial with random assignment at the CWAC level. However, baseline data collection happened after the initial community level process was completed and potential beneficiaries were identified, but before the final beneficiaries list was identified. The evaluation sample was drawn from the initial (potential) eligibility list and not from the actual beneficiary list. Consequently the study design for the impact evaluation changed from an RCT to a design with random assignment at the CWAC level and selection at the household level within CWACs, requiring a matched comparison group. On average, 3,000 households in the 90 randomly assigned treatment and control CWACs looked similar at baseline, indicating that randomization worked to create equivalent groups. Although treatment and control groups were selected to provide equivalence, the baseline data collection occurred before final selection of beneficiaries within CWACs, making the actual treatment group that resulted after baseline look dissimilar to the overall control group (which consisted of all potential beneficiaries) as well as the original treatment group. This additional round of administrative selection introduced differences between the two groups and threw off the balance that randomization had achieved. The selected households in the treatment communities are different from the full list of households. They are smaller

households, with more elderly, and with heads that have less education and are more likely to be female. The true treatment households therefore also have different characteristics from the full sample of control households. Table 3 lists the means for household characteristics of the original treatment, original control, and true treatment groups. There is only one statistically significant difference between the original treatment and control groups. However the true treatment group differs from the original control group on almost every characteristic due to the prioritization process of households by the central administration.

Although the balance that resulted from randomization was lost with the selection of beneficiaries after baseline, we are able to statistically restore balance by creating a comparison group from the control households that resembles the true treatment group. We selected households from the control group that appear to be most similar to the selected treatment group by using propensity score matching (PSM) – (Heckman, Ichimura, and Todd 1998). The PSM method estimates the likelihood (propensity) a household is selected for the program based on the characteristics of households that actually were selected to receive the program. Households from the control group are matched to households from the true treatment group by their likelihood to receive the program, creating a comparison group from the control group that best matches the beneficiaries in the treatment CWACs.

The PSM technique proceeds in two stages. First, for each household in the truly treated and control samples, a propensity or balancing score is constructed which indicates the likelihood of receiving treatment (the higher the score, the greater the likelihood). This score is predicted from the coefficient estimates of a multivariate regression that contains variables that are thought to be associated with selection into treatment. Table 1 shows results of this estimation, and indicates that age of head (positive), household wealth score (negative), orphans (positive)and distance to clinic (farther away, more likely of selection) are the most important characteristics associated with selection into treatment status. The overall fit of the regression is low due to the fact that these households are generally quite similar, thus making it difficult to precisely predict selection.

Table 2 shows the distribution of values of the generated log of the odds ratios derived from the logistic regression (the propensity or balancing scores). As we would expect, the median value is higher for the truly treated group (-0.499) as is the lower bound and the 25^{th} and 75^{th} percentile values, though the upper values are roughly the same for both samples. There is sufficient overlap (or common support) between the distributions to identify good matches—households in the control group with identical or similar scores to those in the truly treated group. Indeed there are only 8 truly treated households with scores that are ‘off the common support’—that is, with scores that are higher than the highest score of any control households. In our application of PSM we impose one-to-one nearest neighbor matching without replacement. That is, for each truly treated household, we choose the control household with the closest propensity score as its ‘match’ and we allow control households to only be matched once (they cannot be replaced in the pool of potential matches). This technique allows us to generate a matched comparison group of equal size as the truly treated group. We then use this matched comparison group in our analysis of treatment effects.

Table 3 contains the mean differences between the true treatment and PSM constructed comparison group. The number of differences and the magnitude of difference in household characteristics between the true treatment and comparison groups is greatly reduced after implementing PSM. There are now only two statistically significantly different variables, no toilet and no access to a protected water source, instead of the 11 that resulted from comparing the true treatment group with the control group (results not shown). Additionally, the magnitude of the differences between the true treatment and comparison groups are not substantively meaningful because they are 0.12 standard deviations difference on these variables. Thus, the PSM method successfully created a comparison group that looks very similar to the true treatment group and removed observed differences that resulted from the selection process in the treatment CWACs that never occurred in the control CWACs. Additionally, we are back to having a balanced sample size between the treatment and comparison groups, with 516 and 508 households respectively. This sample size is less than half of the original sample due to the greatly reduced number of households that were selected to receive the program, but meets requirements that the two groups have similar characteristics. Note that the Monze SCT is a supply driven program with selection purely driven by observables, making PSM an appropriate method in this context. Other features of the design also strengthen the case for PSM: data from the two samples were collected at the exact same time using the exact same field work team and identical survey instruments (Diaz & Handa 2006; Handa & Maluccio 2010).

The sample used in this study contains 1,024 households with 516 in the treatment group and 508 in the matched comparison group. The treatment group represents households that the community believes are the most vulnerable and labor constrained in the area and the matched comparison group looks very similar to them. Table 3 contains the means for demographic and poverty information about the true treatment sample (beneficiaries of the program). The average head of household is 64.4 years old, has less than three years of education, and is female 68 percent of the time. The average household has 4.9 people, with 2.7 children and 2 orphans. Over 68 percent of the households do not have access to their own toilet (including pit latrine) or a protected water source and live over six kilometers from the nearest clinic. These are rural households that rely on subsistence farming as a livelihood, however 95 percent of the households did not purchase fertilizer in the farming season prior to the baseline data collection.

4. Empirical Strategy and Hypotheses

Empirical Strategy: Our special module on time preference, expectations and self-assessed quality of life were only administered at the follow-up survey in 2010 so our core analytical strategy is a comparison of these outcomes three years after program initiation. As described earlier, the original evaluation sample is drawn from the initial eligibility list drawn up by the community with some communities assigned to control status randomly. From this sample we identify those who are actually in the program ('truly treated') and use these households to select a comparison group from the control sample with PSM using the key baseline characteristics used by program managers to prioritize households (age of head and the dependency ratio). We use the resulting matched sample of 508 treated households and 516 matched households as our final analytical sample, and perform multivariate analysis of the form:

$$(1) \quad Y_i = \alpha + \beta_1 \cdot T_i + \beta_2 \cdot X_i + \varepsilon_i$$

where Y is the outcome of interest (usually a measure of time preference). T is an indicator that the household is in the program, and X is a vector of control variables that include sex, age, age-squared and years of schooling of the respondent, log of household size and log time it takes to walk to the nearest main road. Table 4 and Figure 2 illustrate that intervention households are much more likely to wait for all values above ZMK 200,000 with mean differences increasing up to about ZMK 500,000 and then declining very slightly at ZMK 600,000. We use an indicator of whether the individual is willing to wait for ZMK 300,000 as our key dependent variable to measure time preference, since this is the smallest delayed amount at which we see a significant difference between the two groups.

Hypotheses: A steady and predictable source of income can alter individual discount rates, making recipients less myopic and more forward looking, and thus more willing to delay current consumption for future investment such as schooling of children, health, and economic activities which provide benefits in the future rather than the present. Indeed several studies have demonstrated a causal relationship between receipt of unconditional cash transfers and investment in schooling (Kenya CT-OVC Evaluation Team; Samson et al 2011). In addition, participation in a cash transfer program may alter a household's expectations about the future quality of life and thus make it more interested and oriented toward investing in the future. Finally, to the extent that the Monze SCT successfully mitigates the negative consequences of food insecurity and abject poverty, and thus makes people feel better about their current circumstances, this itself may influence current decision-making. This is consistent with psychological theories of decision-making which suggest that beyond cognitive activity, decisions are influenced by emotions and feelings since individuals often use their current emotional state as cues for making choices (Lowenstein et al. 2001). Evidence on the effect of positive affect (happiness) on time preference has recently been reported in Ifcher & Zarghame (2011). At the same time, there is a concern that measures of time preference in large-scale field surveys may simply reflect short-term circumstances, particularly exogenous shocks such as crop failure, drought, or the death of a breadwinner or a shock to one's own health.

With these potential factors in mind, our test of a causal relationship between receipt of the cash transfer and time preference proceeds in three stages. First we establish the mean difference in the proportion willing to wait for ZMK 300,000 between the intervention and comparison group, as well as the impact of the program on a set of other outcomes that might themselves potentially affect time preference. These include a five item quality of life score (measured in logs to account for skewness), two sub-items from this scale, one of whether the respondent feels generally happy and whether the respondent is satisfied with her health, and three variables indicating whether the individual feels that the future will be better. As noted above, the program may directly affect each of these self-assessments of well-being which in turn may affect time preference. In stage two, we add these latter variables on self-assessed welfare and affect to regressions determining time preference to see if their inclusion attenuates the treatment effect. We then assess the validity of our time preference measure as an indicator of long term preferences by controlling for current adverse 'shocks' to see if they help explain the decision to take money earlier rather than later.

5. Variables and Descriptive Statistics

This study investigates the impact of a cash transfer program on a person's preference to delay gratification. In order to assess one's willingness to wait, we asked the head of household in the study a series of hypothetical questions about whether they prefer to have a certain amount of money now or a different amount of money in the future.[2] The immediate option remains the same for each question while the delayed option continually increases. The specific questions are:

> Suppose that you suddenly find out that a relative has left you ZMK200,000.
> You can choose to receive:
>
> A. 1. ZMK200,000 today or 2. ZMK200,000 in one month. Which do you choose?
> B. 1. ZMK200,000 today or 2. ZMK300,000 in one month. Which do you choose?
> C. 1. ZMK200,000 today or 2. ZMK400,000 in one month. Which do you choose?
> D. 1. ZMK200,000 today or 2. ZMK500,000 in one month. Which do you choose?
> E. 1. ZMK200,000 today or 2. ZMK600,000 in one month. Which do you choose?
> F. Now suppose you can choose between receiving:
> 1. ZMK220,000 today or 2. ZMK200,000 in one month. Which do you choose?

To put these amounts into context, ZMK 200,000 is about 40 US dollars which represents a large amount to households in the study given that the average household's monthly consumption is about 30 US dollars at baseline. The last question (F) tests whether the respondent understands the scenario because there is no clear benefit to choosing the second option.[3] About 10 percent of the sample indicates they will wait for less money and further analysis shows that these individuals always chose the option of waiting, so we exclude these observations from the results reported here on the grounds that these individuals likely did not understand the question.

As expected, the proportion of people willing to delay gratification increases as the value of the delayed option increases. Only 33 percent of the entire sample (treatment and control) are willing to wait for ZMK 300,000, 47 percent will wait for ZMK 400,000, 55 percent will wait for ZMK 500,000, and 56 percent will wait for ZMK 600,000. There is also strong internal consistency among these measures with a Cronbach's Alpha of 0.84 for items A. through E. A greater proportion of the treatment group than the control group is willing to delay for each of the delayed option values. Figure 2 depicts the percentage of households by study condition willing to delay gratification for each delayed value. Table 4 contains the means and standard deviations for each group and for each delayed value.

We also investigate potential mediating mechanisms that might explain the effect of the cash transfer program on time preference. The potential mediating variables we investigate fall into two general categories: 1. Psychological status, self-assessed health and expectations about the

[2] Hypothetical questions such as these have been used with success by development economists in field studies in similar study settings (Cardenas & Carpenter 2008; Anglewicz et al. 2009).
[3] During field work we came across some cases where respondents clearly seemed to understand the question yet chose to wait for less money. Upon probing, we were told that they had no safe place to keep the money, or that the one month time period allowed them to plan how they would use the money effectively whereas if they received the money immediately they would probably 'waste' it.

future and; 2. Wealth and shocks to farming. With respect to the first category of health and happiness, three years into the program the treated households have a slightly higher quality of life, are more satisfied with their health (80 percent versus 75 percent), and report being generally happier (84 versus 78 percent). We also asked them about their expectations of their future; if their life will be better in one, two, and five years from now. The treated consistently reported being more optimistic about the future with 31 percent claiming life will be better one year from now, 30 percent two years from now, and 24 percent 5 years from now (versus 21 percent, 20 percent, and 18 percent respectively from the comparison group). With respect to the second category of variables we investigate as potential mediators, we find that the treatment group has fewer households in the lowest wealth decile (23 percent versus 28 percent). Otherwise the two groups experienced roughly the same number of deaths in a household (7 percent), bad rains in the previous season (77 percent), persistent crop failure (15 percent), and crop failure last season (44 percent). We expect these last variables to be similar between treatment and control households since communities were randomly assigned, thus weather conditions that affect crop production should balance between groups. As such we view these variables as a way of checking the validity of our time preference measure as a true indicator of long term preferences, rather than a short term measure that is influenced by short term economic or social circumstances. Table 4 contains the means and standard deviations for all of the potential mediating variables that we investigate.

6. Results

Program impacts on time preference, self-assessed welfare and expectations: Table 5 presents regression results on the determinants of time preference as well as six other variables reflecting quality of life, affect and expectations of the future. All models are linear probability except for column 3 which is OLS. Column 1 indicates an 8.7 percentage point (pp) mean difference in the proportion of individuals in the intervention group that are willing to wait one month for ZMK300,000. Column 2 adds a vector of control variables to the model and this does not change the size of the treatment effect despite the fact that at least one of these variables has a direct association with time preference.

Columns 3-6 show the impact of the SCT on the quality of life score (measured in logs) and two sub-items in the score, whether or not the respondent is satisfied with her health and whether she is generally happy. Program participants on average have higher scores on the quality of life composite measure, and are 4.5 and 6.1 pp more likely to feel satisfied with their health or generally happy, with the latter statistically significant at 5 percent. Finally, columns 6-8 show impacts on expectations about the future and here we again see strong program impacts in the range of 9 percentage points for1-2 years in the future and 6 points five years in the future. This diminishing impact on an individual's positive expectations for periods further in the future are intuitive since there is more uncertainly over the longer time period, including with respect to continuation in the program itself. Table 5 as a whole establishes that the SCT has had an important impact on several dimensions of preferences and psychological status—time preference, quality of life, affect, and future expectations. But since the latter variables could themselves impact time preference we now turn to the question of whether they indeed offer an explanation of why the cash transfer program reduces the rate of time preference.

Tests for mediation: Table 6 presents a series of regressions determining time preference where the possible mediators are added one at a time. For ease of comparison column 2 repeats the basic impact of the SCT on time preference reported in column 2 of Table 5. There are two main results of interest in this table. First, each one of the potential mediators has a direct and statistically significant effect on time preference in the expected direction and these effects are quite large. For example, those who feel generally happy are 20 points more likely to be willing to wait for future money; similarly those who believe their life will be better in one year are also 20 points more likely to wait for future money. The mean of the dependent variable over the full sample is 28 percent so these effects are about 71 percent of the mean. The second main result is that while the treatment effect is somewhat attenuated with the inclusion of these variables, none of these factors can fully explain the strong relationship between treatment status and time preference. The strongest attenuation occurs in the models with future expectations (columns 5-7), where the treatment effect is reduced from 8.7 to 6.8 once we allow for the effect of the program on future expectations. One further result of interest is the coefficient of gender, which increases in columns 2-4, indicating that females report lower quality of life and affect relative to males.

Controls for short term circumstances: Does our measure of time preference really reflect a structural preference parameter or does it simply reflect short-term circumstances? We investigate this question in Table 7 where we add a series of variables to our basic model that reflect short term adverse circumstances which might make a person 'desperate' for money. Once again for ease of comparison we include our basic impact estimate from table 5 in the first column. Looking across the first row of this table we see that inclusion of these additional variables do not alter our basic result of an 8.7 percentage point mean difference in the likelihood of being willing to wait for future money. However this in part reflects the fact that our core treatment and control groups are randomly assigned and so we expect that these factors will be balanced across the two arms. On the other hand, the intervention might have influenced the propensity to suffer from crop failure (by allowing the purchase of pesticides) or to avert a death in the household (by enabling health care access). Note that only one of these shock variables has a statistically significant relationship with the dependent variable—crop failure in the last season—and the direction of its effect is counter-intuitive.

The shock variables in Table 7 are measured at the household level but we also have several measures of the actual health status of the respondent which we add to the regressions in Table 8. The first is an indicator of whether the individual felt her health had improved over the last year. The second is a score based on responses on five activities of daily living[4] (higher scores indicate better physical health) and the third is an indicator if the individual scored above the 20th percentile of the activities of daily living score. These regressions also include all the individual and household covariates reported earlier. The first row of Table 8 shows that none of these measures of individual health status attenuate the treatment effect on time preference and indeed, none of the health measures have any direct effect on the time preference measures themselves, providing additional support for the proposition that our time preference measure represents more than simply short-term circumstances.

[4] The five questions are: 1) Can you stand from a sitting position; 2) Can you draw a pail of water from a well; 3) Can you carry a heavy load such as a pail of water for 20 meters; 4) can you walk 5 kilometers; 5) Can you bend, squat or kneel? The responses are 'cannot', 'with difficulty' and 'easily'.

A further test of causality: Our case for a causal effect of the SCT on time preference is weakened by the fact that we do not have baseline measurements of time preference and related psychological and expectation measures. Our study design allows us to perform a falsification test that can provide additional support for a causal interpretation of our results. Recall that because our sample was drawn from the initial pool of eligible households and not the final prioritized list of actual beneficiaries, we have a group of households in the treatment arm that did not actually receive cash transfers. The 'unmatched' households in the control arm, that is, those households that were not selected via PSM to be part of the comparison group, would naturally be the comparison group for these 'selected but not prioritized' households. Since neither group received the cash transfer, we would not expect to see differences in time preference and other affect and expectation measures between the two groups if indeed the only reason these differences exist in our main sample is due to the cash transfer.

Table 9 repeats the analysis in Table 5 on these 'untreated' and 'unmatched' households and as before we exclude individuals that reported they would wait to receive less money. The first row of Table 9 reports the coefficient of the dummy variable indicating that the respondent is from a household in the randomized in but untreated sample and 0 indicates the individual is from a household in the control sample but not selected to be in the matched comparison group. The results in the first row show that in no case is the mean difference between these two groups statistically significant though in one case (column 6) the t-statistic is 1.60 and the mean difference is 4.0 percentage points, still less than half the actual treatment effect of 9.7 reported in column 6 of Table 5. Note also that many of the other coefficients have the same pattern of direct effects on time preference. For example, in columns 6-8 schooling is statistically significantly and positively associated with future expectations, and distance to road negatively so, the same pattern that exists in Table 5 for these outcomes. Similarly in columns 3-5 being female is negatively associated with positive affect, health or overall quality of life. These results provide further support for the idea that our measure of time preference reflects a true structural parameter and that our estimates in Tables 5-6 represent a causal effect of the SCT on time preference.

7. Discussion and conclusion

This study investigates the impact of receiving cash through an SCT on people's time preference to delay gratification for a reward. Using a randomized experiment of the Monze SCT in Zambia, we find that cash transfers make people less myopic and more willing to invest by affecting their discount rate. This effect occurs both directly from the program and indirectly through mediators of the SCT. Literature suggests that decisions are influenced by emotions and feelings. Our findings are consistent with the literature because self assessed indicators such as general affect, perceived quality of life and health, and expectations of the future all impact time preference. However, we still find a strong impact of the cash transfer program after controlling for these emotions and feelings. The internal validity of our results is strengthened by a falsification test, where we compare differences in time preference and the emotions- and feelings-related measures between two untreated groups, one of which was originally assigned to intervention and the other to control status, but neither of whom eventually entered into the program. We find no differences in these outcomes between the two groups, suggesting that our

results are not merely reflective of pre-program differences among the two study arms. Therefore, we believe we have isolated the program's direct impact on discount rates.

We also find an indirect effect of the program through mediating variables. The SCT has a positive impact on emotions and feelings with program recipients being happier, claiming a better quality of life, healthier, and more positive about their future than their matched comparison. In turn, these emotions and feelings have a positive impact on time preference. Thus the SCT impacts an individual's discount rate directly and also through mediators.

The Monze SCT targets labor constrained and destitute households in subsistence farming communities that are sensitive to immediate threats to their livelihood such as poor rains, deaths in the family, and reduced health. These threats might make a household desperate for money in the short run and affect how they respond to the time preference questions. However, we find that controlling for these threats does not change the impact estimates of the program on time preference. Therefore, the time preference measure used in this study is not a reflection of short term circumstances but rather a person's general discount rate.

This is the first study to investigate impacts on time preference and discount rates. It demonstrates that SCTs have psychological impacts that positively influence investment and decision making. SCTs have been implemented in developing countries for almost 20 years, with the first programs in Mexico, Brazil, Columbia, Honduras, Turkey, Cambodia, and Nicaragua. These programs were proven highly successful through rigorous impact evaluations.[5] Children's health, nutrition, and education were markedly improved. Beneficial outcomes included increased height, reduced stunting, increased school enrollment, decreased school dropout, and increased use of health services.[6] We believe this study is useful for policymakers with limited resources who often need to choose between social protection programs and those that will increase productivity. This study provides evidence that SCTs can have productive impacts for individuals in addition to impacts on social protection outcomes like education and health.

[5] http://conferences.ifpri.org/2020Chinaconference/pdf/beijingbrief_adato.pdf
[6] http://www.iadb.org/intal/intalcdi/PE/2009/03011.pdf

REFERENCES

Anglewicz, P., Adams, J., Onyango, F., Watkins, S., Kohler, H-P. (2009). The Malawi Diffusion and Ideational Change Project 2004-06: Data Collection, Data Quality, and Analysis of Attrition. *Demographic Research*, 20(21):503-540.

Bardhan, P. K., & Udry, C. (1999). *Development Microeconomics*. Oxford: Oxford University Press.

Barnett, T. 7 Weston, M. (2008). Wealth, Health, HIV and the Economics of Hope. *AIDS*, 22(Suppl 2): S27-S34.

Bernheim, Douglas, and Antonio Rangel. "Addiction and Cue-Triggered Decision Processes". NBER July 2004. http://www-psych.stanford.edu/~knutson/bad/bernheim04.pdf

Bowles, Samuel. Endogenous Preferences: The Cultural Consequences of Markets and Other Economic Institutions. Journal of Economic Literature, Vol. 36, No. 1.pp 75-111.

Cardenas, J. C., & Carpenter, J. (2008). Behavioural development economics: Lessons from field labs in the developing world. *Journal of Development Studies, 44*(3), 311-338.

Davis, Benjamin, Marie Gaarder, Sudhanshu Handa and Jenn Yablonski. Evaluating the impact of cash transfer programmes in sub-Saharan Africa: an introduction to the special issue. The Journal of Development Effectiveness. 2012, vol. 4, issue 1, pages 1-8

Diaz, Juan Jose & Sudhanshu Handa, "An Assessment of Propensity Score Matching as a Non Experimental Impact Estimator: Evidence from Mexico's Progresa Program," Journal of Human Resources Vol. 41(2): 319-345, 2006.

Frederick, Shane, George Lowenstein, and Ted O'Donoghue. "Time discounting and time preference: a critical review." Journal of Economic Literature. Vol. XL (June 2002), pp 351-401.

Handa, Sudhanshu, Benjamin Davis, Marco Stampini & Paul Winters, "Heterogeneous Treatment Effects in Conditional Cash Transfer Programs: Assessing the impact of Progresa on Agricultural Households," Journal of Development Effectiveness, Vol.2(3): 320-335.

Harrison, Glenn, Morten Igel Lau, E. Elisabet Rutström and Melonie B. Williams (2003). "Eliciting Risk and Time Preferences Using Field Experiments: Some Methodological Issues". http://karlan.yale.edu/fieldexperiments/pdf/Harrison,%20Lau,%20Rutstrom%20and%20Williams_Eliciting%20Risk%20and%20Time%20Preferences%20Using%20Field%20Experiments_Some%20Methodological%20Issues.pdf

Hausman, Jerry (1979). "Individual discount rates and the purchase and utilization of energy using durables." The Bell Journal of Economics. Vol.10 (1): 33-54.

Heckman, James, Hidehiko Ichimura, and Petra Todd (1997) "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Program,." Review of Economic Studies. 64: 605-654.

Heckman, James, Hidehiko Ichimura, and Petra Todd (1998) "Matching as an Econometric Evaluation Estimator." Review of Economic Studies. 65: 261-294.

Ifcher, John & Homa Zarghamee, "Happiness and Time Preference: The Effect of Positive Affect in a Random-Assignment Experiment," American Economic Review Vol. 101(7): 3109-3129, 2011.

Kenta CT-OVC Evaluation Team, "The impact of Kenya's Cash Transfer for Orphans and Vulnerable Children on human capital," Journal of Development Effectiveness, Vol.4(1): 38-49, 2012.

Kirby, Douglas (1999) "Reducing Adolescent Pregnancy: Approaches that Work," Contemporary Pediatrics, 16(1), pp. 83-94.

Miller, C., Tsoka, M. G., & Reichert, K. (2011). Impacts on children of cash transfers in Malawi. In S. Handa, S. Devereux & D. Webb (Eds.), Social Protection for Africa's Children. New York: Routledge.

Mischel, Walter, Yuichi Shoda, Monica Rodriguez. Delay of Gratification in Children. Science, May 26, 1989.

Pender, John (1996). "Discount rates and credit markets: Theory and evidence from rural India". Journal of Development Economics. Vol.50 257-296.

Rae, J. M. C. W. (1905). The Sociological Theory of Capital. New York: The Macmillan Co.

Sadoulet, Elizabeth & Alain de Janvry (1995) Quantitative Development Policy Analysis, Baltimore: Johns Hopkins University Press.

Samson, M., Heinrich, C., Kaniki, S., Regalia, F., Mac Quene, K., Muzondo, T., et al. (2011). Impacts of South Africa's Child Support Grant. In S. Handa, S. Devereux & D. Webb (Eds.), Social Protection for Africa's Children. New York: Routledge.

Samuelson, P. A. (1937). A note on measurement of utility. The Review of Economic Studies, 4(2), 155-161.

Smith, A. (1776). An Inquiry into the Nature and Causes of the Wealth of Nations. London: W. Strahan and T. Cadell.

Tanaka, Tomomi, Colin Camerer & Quang Nguyen, “Risk and Time Preference: Linking Experimental and Household Survey Data from Vietnam,” American Economic Review Vol 100(1): 557-571, 2010.

UNICEF (2008) Social Protection in Eastern and Southern Africa: A Framework and Strategy for UNICEF, Nairobi: UNICEF ESARO.

Table 1: Logit Estimates of Odds of Receiving Treatment

```
Logistic regression                               Number of obs   =       1544
                                                  LR chi2(10)     =      91.09
                                                  Prob > chi2     =     0.0000
Log likelihood = -932.54756                       Pseudo R2       =     0.0466

------------------------------------------------------------------------------
        true |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
Wealth       |  -.4142414   .0961207    -4.31   0.000    -.6026345   -.2258483
HH Size (ln )|   .0136932   .1448765     0.09   0.925    -.2702595    .2976458
Food
  Consumption|  -.0934523   .0717227    -1.30   0.193    -.2340262    .0471215
Dependency
  Ratio      |  -.0536663   .0432201    -1.24   0.214    -.1383762    .0310437
Female headed|   -.073021    .139713    -0.52   0.601    -.3468536    .2008115
Age of head  |   .0138464   .0044974     3.08   0.002     .0050316    .0226612
Orphans in HH|    .091899   .0478315     1.92   0.055    -.0018489    .1856469
Education    |  -.0245427   .0220237    -1.11   0.265    -.0677083    .0186229
Distance to
  school     |  -.0259855   .0528666    -0.49   0.623    -.1296021    .0776312
Distance to
  Clinic     |   .1941962   .0654338     2.97   0.003     .0659484    .3224441
Constant     |  -1.323803   .9042987    -1.46   0.143    -3.096196    .4485901
------------------------------------------------------------------------------
```

Table 2: Values for log of the odds ratios by sample

	minimum	25th percentile	median	75th percentile	max
Comparison	-3.61	-1.220	-.778	-.421	.561
Treated	-2.65	-.835	-.499	-.257	.588

Figure 1: distribution of balancing score by household type

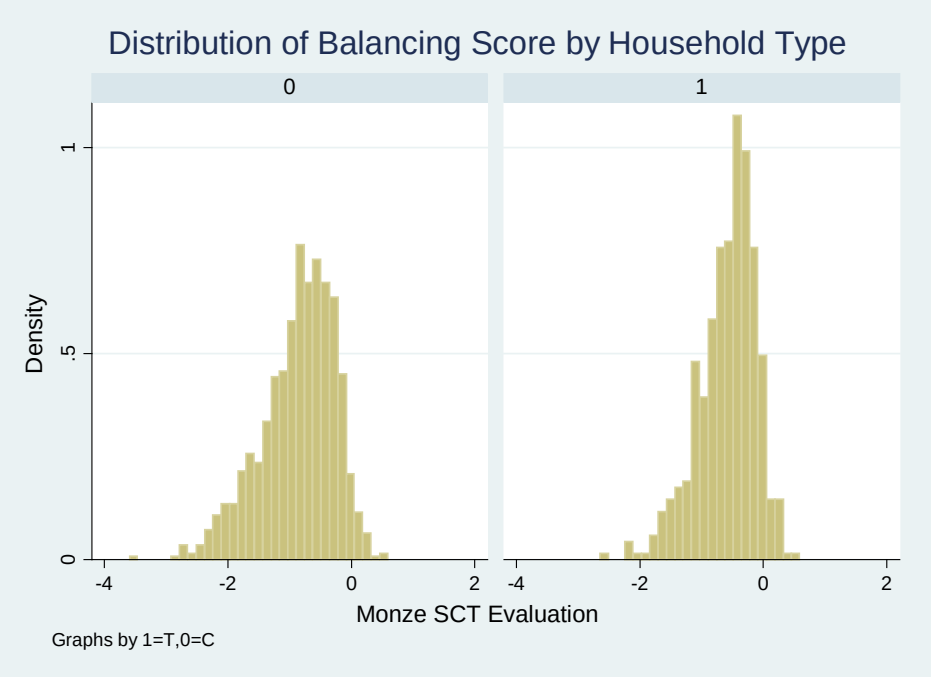

Table 3: Mean differences between treatment and control, original and matched samples.

	Original Controls	Original Treatment	Matched Comparisons	Actual Treatme
	Mean	Mean	Mean	Mean
Total food consumption per month per capita (Kw)	21910.02	20974.29	23126.74	21947.3
Bought fertilizer (1=Yes 2=No)	**1.9**	**1.92**	1.93	1.95
DHS style wealth index	0.09	0.01	-0.31	-0.27
Head's age (years)	59.4	59.31	64.65	64.43
Head's education (years)	3.87	3.85	2.73	2.81
Female Headed Household	0.59	0.59	0.69	0.68
Household size	5.44	5.58	4.69	4.89
Dependency ratio	2.43	2.42	2.43	2.43
# of orphans	2.03	2.07	2	2.09
# of children (0-18)	3.2	3.23	2.63	2.71
# of adults (19-64)	1.66	1.74	1.4	1.42
# of seniors (65+)	0.56	0.6	0.69	0.75
Distance to nearest secondary school	27.67	22.41	33.19	23.74
Distance to nearest clinic	5.84	6.23	6.27	6.34
No toilet	0.57	0.6	**0.61**	**0.68**
Unprotected water source	0.57	0.6	**0.61**	**0.68**
Observations	1072	1145	508	516

Bold indicates significance at 5 percent or less for t-test for statistical difference in means between treatment and comparison group.

Table 4: Means of key outcome and mediator variables

		Treated			Comparison group	
Variable	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Will wait one month for less money	514	0.123	0.328	501	0.076	0.265
Will wait one month for:*						
ZMK 200,000	452	0.024	0.154	468	0.026	0.158
ZMK 300,000	452	0.323	0.468	467	0.236	0.425
ZMK 400,000	452	0.511	0.500	467	0.398	0.490
ZMK 500,000	452	0.586	0.493	467	0.480	0.500
ZMK 600,000	452	0.593	0.492	463	0.492	0.500
Quality of life score (log)	515	2.739	0.404	505	2.687	0.416
Satisfied with health	515	0.798	0.402	505	0.752	0.432
Generally happy	515	0.841	0.366	506	0.781	0.414
Expect life will be better in:						
1 year	515	0.307	0.462	506	0.211	0.409
2 years	514	0.294	0.456	506	0.204	0.403
3 years	515	0.239	0.427	506	0.182	0.386
Lowest wealth decile	515	0.229	0.421	506	0.277	0.448
Bad rains last season	513	0.756	0.430	506	0.783	0.413
Persistent crop failure	390	0.151	0.359	383	0.141	0.348
Crop failure last season	391	0.435	0.496	388	0.456	0.499
Death in household	515	0.083	0.277	506	0.069	0.254

* Only calculated for the 88 percent of sample who would not wait for less money.

Figure 2: Propensity to Delay Gratification

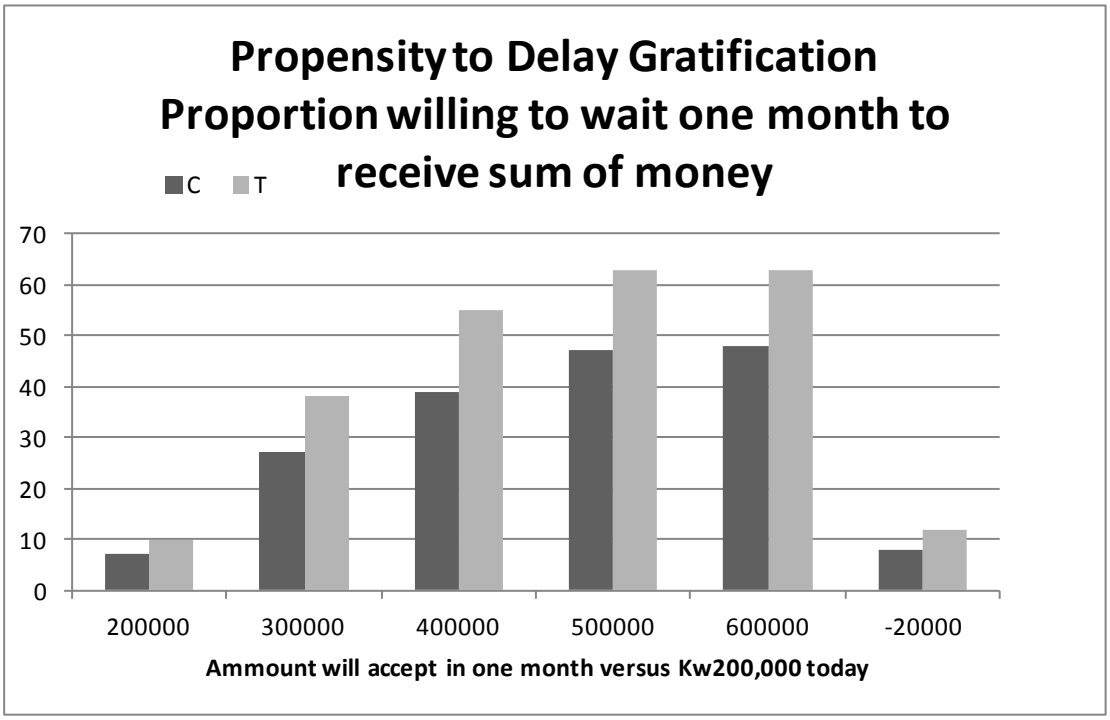

Table 5: Impact of SCT on time preference, subjective self-assessment and future expectations

Dependent variable:	Will wait one month for ZK 300,000		Quality of life index	Satisfied with health?	Generally happy?	Do you expect your life to be better in ….? 1 year	2 years	5 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.087	0.087	0.052	0.045	0.061	0.097	0.093	0.059
	(2.97)	(2.94)	(2.02)	(1.72)	(2.48)	(3.59)	(3.52)	(2.35)
Female		0.043	-0.029	-0.060	-0.044	0.030	0.004	0.012
		(1.32)	(-1.02)	(-2.06)	(-1.61)	(1.01)	(0.15)	(0.44)
Age		-0.001	0.002	0.005	0.000	-0.002	-0.001	-0.010
		(-0.09)	(0.40)	(0.74)	(0.04)	(-0.31)	(-0.16)	(-1.73)
Age squared		0.000	-0.000	-0.000	-0.000	0.000	-0.000	0.000
		(0.11)	(-0.69)	(-0.89)	(-0.31)	(0.15)	(-0.06)	(1.26)
Schooling (years)		0.003	0.004	0.001	-0.005	0.018	0.016	0.013
		(0.53)	(0.69)	(0.18)	(-0.98)	(3.22)	(3.05)	(2.63)
Log household size		0.073	0.042	0.039	0.012	0.041	0.044	0.032
		(2.42)	(1.62)	(1.45)	(0.48)	(1.51)	(1.64)	(1.26)
Log distance to road		-0.009	-0.031	-0.001	-0.020	-0.051	-0.067	-0.065
		(-0.49)	(-2.00)	(-0.05)	(-1.31)	(-3.11)	(-4.15)	(-4.28)
Constant	0.236	0.113	2.686	0.629	0.898	0.277	0.301	0.621
	(11.40)	(0.49)	(13.63)	(3.12)	(4.75)	(1.33)	(1.48)	(3.24)
Observations	919	917	1,017	1,017	1,018	1,018	1,017	1,018
R-squared	0.087	0.087	0.052	0.045	0.061	0.097	0.093	0.059

All regressions are linear probability models except for quality of life which is OLS.

Table 6: Impact of SCT on time preference with control for affect, self-assessment and expectations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.087	0.078	0.082	0.079	0.069	0.068	0.075
	(2.94)	(2.69)	(2.80)	(2.69)	(2.37)	(2.31)	(2.58)
Female	0.043	0.050	0.056	0.051	0.040	0.047	0.044
	(1.32)	(1.55)	(1.71)	(1.56)	(1.24)	(1.46)	(1.37)
Age	-0.001	-0.001	-0.002	-0.001	-0.000	0.000	0.002
	(-0.09)	(-0.18)	(-0.25)	(-0.09)	(-0.03)	(0.00)	(0.33)
Age squared	0.000	0.000	0.000	0.000	0.000	0.000	-0.000
	(0.11)	(0.25)	(0.30)	(0.14)	(0.09)	(0.06)	(-0.22)
Schooling (years)	0.003	0.002	0.003	0.004	-0.000	0.000	0.000
	(0.53)	(0.33)	(0.47)	(0.66)	(-0.04)	(0.02)	(0.04)
Log household size	0.073	0.064	0.067	0.069	0.065	0.064	0.066
	(2.42)	(2.17)	(2.25)	(2.32)	(2.20)	(2.17)	(2.22)
Log distance to road	-0.009	-0.002	-0.007	-0.005	-0.001	0.001	0.003
	(-0.49)	(-0.10)	(-0.37)	(-0.29)	(-0.07)	(0.07)	(0.14)
Quality of life		0.181					
		(5.11)					
Satisfied with health			0.156				
			(4.54)				
Generally happy				0.136			
Expect life to be better:				(3.70)			
1 year					0.200		
					(5.76)		
2 years						0.199	
						(5.59)	
5 years							0.219
							(5.73)
Constant	0.113	-0.370	0.018	-0.012	0.057	0.040	-0.041
	(0.49)	(-1.51)	(0.08)	(-0.05)	(0.25)	(0.18)	(-0.18)
Observations	917	916	916	917	917	917	917
R-squared	0.019	0.046	0.041	0.033	0.053	0.051	0.053

Linear probability models; dependent variable is whether individual would wait one month for ZK300,000 rather than accepting ZK200,000 immediately. T-statistics in parentheses below coefficients.

Table 7: Impact of SCT on time preference with control for negative circumstances

	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.087	0.084	0.088	0.085	0.090	0.087
	(2.94)	(2.84)	(2.98)	(2.47)	(2.65)	(2.96)
Female	0.043	0.048	0.046	0.019	0.019	0.044
	(1.32)	(1.46)	(1.38)	(0.51)	(0.51)	(1.33)
Age	-0.001	-0.001	-0.001	0.002	0.002	-0.001
	(-0.09)	(-0.18)	(-0.12)	(0.24)	(0.24)	(-0.08)
Age squared	0.000	0.000	0.000	-0.000	-0.000	0.000
	(0.11)	(0.17)	(0.14)	(-0.23)	(-0.23)	(0.09)
Schooling (years)	0.003	0.002	0.003	0.005	0.007	0.003
	(0.53)	(0.26)	(0.56)	(0.69)	(0.98)	(0.53)
Log household size	0.073	0.049	0.073	0.051	0.072	0.074
	(2.42)	(1.45)	(2.42)	(1.42)	(2.00)	(2.44)
Log distance to road	-0.009	-0.008	-0.010	-0.011	-0.009	-0.009
	(-0.49)	(-0.45)	(-0.55)	(-0.51)	(-0.41)	(-0.47)
Lowest wealth decile		-0.067				
		(-1.54)				
Bad rain last season			-0.015			
			(-0.41)			
Persistent crop failure				0.064		
				(1.32)		
Crop failure last season					0.093	
					(2.67)	
Death in household						-0.019
						(-0.34)
Constant	0.113	0.188	0.130	0.083	0.001	0.109
	(0.49)	(0.80)	(0.56)	(0.31)	(0.00)	(0.47)
Observations	917	917	915	697	702	917
R-squared	0.019	0.021	0.019	0.017	0.027	0.019

Linear probability models; dependent variable is whether individual would wait one month for ZK300,000 rather than accepting ZK200,000 immediately. T-statistics in parentheses below coefficients.

Table 8: Impact of SCT on time preference with control for health status

	(1)	(2)	(3)	(4)
Treated	0.087	0.086	0.088	0.086
	(2.94)	(2.91)	(2.96)	(2.92)
Health improved in last year		-0.030		
		(-0.68)		
Activities of daily living index			-0.002	
			(-0.34)	
Fit to work				-0.030
				(-0.93)
Constant	0.112	0.124	0.144	0.125
	(0.49)	(0.54)	(0.60)	(0.54)
Observations	917	917	913	917
R-squared	0.019	0.019	0.019	0.020

Linear probability models; dependent variable is whether individual would wait one month for ZK300,000 rather than accepting ZK200,000 immediately. T-statistics in parentheses below coefficients. Models include individual and household level covariates but are not reported in table.

Table 9: Falsification test of impact of treatment status on time preference, subjective self-assessment and future expectations among untreated households

Dependent variable:	Will wait one month for ZK 300,000		Quality of life index	Satisfied with health?	Generally happy?	Do you expect your life to be better in ….?		
						1 year	2 years	5 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.017	0.017	-0.015	0.011	-0.005	0.040	0.022	0.009
	(0.60)	(0.62)	(-0.74)	(0.53)	(-0.26)	(1.60)	(0.89)	(0.37)
Female		0.009	-0.049	-0.038	-0.032	-0.047	-0.031	0.001
		(0.29)	(-2.23)	(-1.72)	(-1.58)	(-1.75)	(-1.19)	(0.05)
Age		-0.004	-0.010	-0.010	-0.007	0.001	-0.002	-0.012
		(-0.77)	(-2.41)	(-2.38)	(-1.77)	(0.27)	(-0.49)	(-2.49)
Age squared		0.000	0.000	0.000	0.000	-0.000	-0.000	0.000
		(0.52)	(1.68)	(1.73)	(1.16)	(-0.99)	(-0.23)	(1.84)
Schooling (years)		0.004	0.001	-0.005	-0.003	0.011	0.012	0.011
		(0.90)	(0.30)	(-1.39)	(-1.00)	(2.54)	(2.91)	(2.76)
Log household size		-0.034	0.034	0.026	0.020	-0.007	-0.002	0.027
		(-1.14)	(1.50)	(1.16)	(0.97)	(-0.27)	(-0.07)	(1.03)
Log distance to road		-0.006	-0.054	-0.016	-0.024	-0.038	-0.037	-0.037
		(-0.33)	(-4.24)	(-1.28)	(-2.01)	(-2.43)	(-2.44)	(-2.47)
Constant	0.317	0.511	3.204	1.181	1.146	0.408	0.471	0.612
	(15.92)	(3.18)	(27.79)	(10.24)	(10.79)	(2.90)	(3.38)	(4.50)
Observations	1,141	1,135	1,275	1,276	1,276	1,275	1,275	1,275
R-squared	0.000	0.005	0.048	0.021	0.019	0.048	0.049	0.046

All regressions are linear probability models except for quality of life which is OLS. Sample is original treatment households who did not enter into the program and 'unmatched' control households.