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Long-term Impacts of Conditional Cash Transfers on Economic Mobility: Evidence using welfare ranks and trajectories

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Abstract

There is recent optimism about the increase in economic mobility in Latin America. This paper exploits the randomized evaluation design of a renowned CCT program to measure the long-run impact on intragenerational socioeconomic mobility. In particular, I use two distinct approaches to examine the effect of differential exposure to the program (welfare ranks and trajectories). More specifically I evaluate the impact of differential exposure to the program on the likelihood that a household presents a path of sustained poverty (wealth), sustained downward (upward) mobility, or temporary downward (upward) movement. The results using the ranks approach suggest the effect on mobility was not sustained in the long-term. In contrast, the impacts from the trajectories estimates do persist into the long term. Moreover, the heterogeneity analysis suggests the program has a compensating effect in some cases (for the less connected and less educated households) and a temporary mitigating effect against adversity (for natural disaster shocks). However, there are other cases in which existing inequalities are reinforced (for households with children at critical transition ages).

Keys words: CCT, socioeconomic mobility analysis, long-term effects

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I. Introduction

A number of macro and cross country studies for the Latin American region have highlighted the important role played by government transfers in reducing inequality in the last two decades. These studies usually look for the 'proximate' causes of the decline in inequality, searching for plausible broad explanations of why labor and non-labor income inequality have fallen. They often examine at the aggregate level trends in macro policy variables (e.g. growth, trade openness, financial depth, inflation, etc.), human capital accumulation (e.g. returns to education and health service usage) and public social spending, among others. Studies of this kind commonly estimate the correlation of inequality indices (constructed at the country level) to a number of policy indicators mentioned above (see for example Behrman et al., 2001a, 2001b). Other approaches include tax benefit incidence analysis (usually using Income survey data) to gauge for example the extent to which transfers (among them government transfers) may be responsible for a reduction in the Gini coefficient (Graham, 2002).

As such, it is now widely stated that conditional cash transfers in Latin America have a potential equalizing effect on the distribution of income. Cogneau and Gignoux (2005) for example analyze the evolution of overall inequalities and, in particular, inequalities of opportunity in males' earnings in Brazil over two decades since the end of the 1970's and find the last period of their study was characterized by an education-related reduction in earnings inequalities. The authors conclude by saying their findings could give rise to optimism regarding the long-run effects of targeted programs to educate poor children such as *Bolsa Escola*.

Moreover, there is currently further optimism about the increase in socioeconomic mobility in Latin America. A flagship study by the World Bank states that two in five Latin Americans were upwardly mobile between 1995 and 2010. According to the study, for the first time ever, the number of people in poverty is equal to the middle class. While most of the middle class expansion came through faster growth, the study indicates that re-distribution through conditional cash-transfers (CCTs) and other social programs have also played a key role (Ferreira et al, 2013). However, none of these studies attempt to identify the causal effect of a single government transfer scheme on reducing inequality or raising mobility. This has been the case mainly because causal attribution remains elusive in cross-country approaches but also, in the particular case of CCTs, it is only now that sufficient time has elapsed since their inception (over a decade) to properly gauge their impact on final outcomes (e.g. wages and consumption, as well as learning in the long-run; see for example Gertler et al. 2012 and Barham et al., 2013) and hence economic mobility. In effect, the jury is still out on the long-term effects of CCTs. A recent systematic review concludes that the existing evidence to date is remains scant and mixed (Molina-Millan et al, 2016).

This paper aims to contribute both to the literature exploring the causes of socioeconomic mobility and that examining the long-term effects of CCTs. To this end I exploit the randomized evaluation design of Mexico's

renowned CCT program *Oportunidades*[1] to measure the program's long-run impact (up to a decade after it came into operation) on within generation economic mobility. Naturally the evaluation design, which was randomized at the community level, cannot serve the purpose of identifying the effect of the program on inequality at the more aggregate level (for example at the municipality, state level or national level). However, in the present rural context, the local level dynamics are of particular interest given the existing evidence of the program's locality-wide effects and spillovers to the untreated population (Angelucci and De Giorgi, 2009; Bobonis and Finan, 2009).

In particular, I use two different approaches to examine the effect of differential exposure to the program. The first approach, consisting of *welfare ranks*, uses the baseline and end-line of the program's 10-year panel dataset to measure socioeconomic mobility in terms of the change in households' consumption rank. The second approach, exploits the information from the panel's middle survey rounds to construct 3-period *welfare trajectories*. A trajectory is defined as the sequence of a households' position along the welfare distribution. More specifically I evaluate the impact of differential exposure to the program on the likelihood that a household presents a path of sustained poverty (wealth), sustained downward (upward) mobility, or temporary downward (upward) mobility.

The evaluation data allows the inclusion of the (ineligible) non-poor population in the mobility rankings (as opposed to simply gauging impacts on beneficiaries against the eligible poor in the control group). This ranking sets a higher bench mark against which to measure impacts - closer to the non-vulnerable, whom are too wealthy to qualify for the program.

The results using the ranks approach suggest that differential exposure to the CCT increased upward mobility; however this effect was not sustained in the long-term. In contrast, the trajectories estimates indicate that the beneficial welfare impact on the early recipients does persist into the long term. In particular, the households that randomly received the transfers first displayed on average a higher likelihood of sustaining high welfare levels and a lower probability of remaining stuck in poverty. Thus, the persistence effects stand the test of time while the impacts on mobility decay (upward and downward movement, sustained as well as temporary). This contrast in the long-term results (between the ranks and trajectories estimates) highlights the fact that utilizing the mid-round surveys of panel datasets provides additional insight about the pathways households follow.

Moreover, the heterogeneity analysis suggests the program has a compensating effect in some cases (for the less connected and less educated households) and a temporary mitigating effect against adversity (i.e. natural disaster shocks). However, there are other cases in which existing inequalities are reinforced (for households with children at critical transition ages). Interestingly, longer treatment favors the mobility of households which are initially more likely to send their children to school; a result which on one hand may accentuate an existing

[1] The program was initially called *Progresa* in 1997; the name was later changed to *Oportunidades* in 2002, and more recently rebranded as *Prospera*. In this paper I adhere henceforth to the name in use by the end of the period of study, i.e. *Oportunidades*.

disparity (by benefiting the less constrained households), yet on the other hand, rewards households that attribute a high value to the investment in the next generation's human capital.

The rest of the paper is organized as follows. Section II provides the basic relevant background information on the program, including the program design and data set used. In section III I present some conceptual definitions regarding the mobility measured used. Section IV displays some basic stylized facts about the difference in mobility patters between the treatment and control groups. The straight-forward empirical strategy is presented in section V while the results of the paper are presented in section VI. Finally, some robustness checks are presented in section VII before concluding in section VIII.

II. Background of the Program

II.1 Program design

As one of the most renowned (and studied) CCT programs the rules and evaluation design of *Oportunidades* have been extensively documented. In this section I therefore limit the information to the program's most basic, relevant aspects. *Oportunidades* started operating in the most marginal rural communities in Mexico in 1997, covering approximately 300,000 beneficiary households. Since then, the program has gradually expanded into urban areas and currently covers over 5 million households (about one quarter of all Mexican families). Its broad coverage and prolonged tenure - as opposed to other randomized evaluation trials (RCTs) consisting of small, temporary pilot interventions-suits well the distributional focus of this study on long-term mobility.

The program provides cash transfers to mothers, conditioned on children's enrollment in school and regular attendance (85 per cent of the time) as well as scheduled visits to health centers. Originally the program provided grants only for children between the third grade of primary and the third year of secondary school (i.e. ninth grade) aged eight to seventeen years[2]. Under the original grant structure, cash amounts (adjusted every six months for inflation) increased as children progressed to higher grades to reflect the increased opportunity cost of schooling as children grow older. In addition, at the secondary level of education (grades seventh through ninth) cash amounts were slightly higher for girls than boys (by about 13 percent; Table 1)[3]. Students benefiting from the program are allowed to fail each grade once, but if a same grade is repeated twice, the schooling grant is discontinued permanently. Finally, the program also provides subsidies for school supplies and a fixed transfer for nutritional support linked to health clinic attendance. However, in terms of magnitude the school grants represent the majority of the program benefits.

II.2 Evaluation design and data

[2] In 2001 the program was extended to include high school (upper secondary) grants and the age limit increased to 21 years.
[3] By the end of 1999 the educational grants ranged from 80 pesos (about $US8) in the third grade of primary to 265 pesos ($US26) for boys and 305 pesos ($US 30) for girls in the third year of secondary school (all nominal prices). For further details on the program rules see Skoufias and Parker (2001).

As documented in several previous studies on *Oportunidades*, the original evaluation and sample design for the program consisted of 506 rural communities[4] (localidades) of which 320 were randomly assigned to receive benefits immediately and the other 186 to receive benefits at a later point in time. The eligible households in the original treatment localities (henceforth referred to as the treatment or early treatment group) began receiving program benefits in the spring of 1998, while the control group (also referred to as the late treatment group) started receiving benefits at the end of 1999. Program eligibility depended on poverty status of the household as determined by a proxy means test. In particular, households in both treatment and control villages were classified as being eligible or ineligible according to an assessment of their permanent income from information collected in a census of localities carried out in September 1997. As a result of this selection process slightly over half of the households in the evaluation sample were initially classified as eligible in 1997.[5]

This census, the 1997 Survey of Household Socio-Economic Conditions (ENCASEH 97), provided the pre-program data for the evaluation[6]. In March 1998 before any transfers were distributed a specially designed baseline (Wave 1) evaluation survey (ENCEL survey) was applied to all households in both treatment and control communities to collect detailed information on demographics, schooling, health, employment, income and expenditures. The first follow-up ENCEL survey was conducted in October 1998 (Wave 2). From then until November 2000 ENCEL surveys (Waves 2 through 6) were applied every six months. Since control households started receiving benefits between November and December 1999 the experimental variation phase comprises Waves 2, 3 and 4. A new follow-up survey (ENCEL 2003 or Wave 7) was conducted in 2003 which included all the households that could be located in the original 320 treatment localities and the original 186 control communities. Finally, the most recent follow-up survey was carried out in 2007 (ENCEL 2007 referred to as Wave 8), though this final survey was only carried out in a subset of the original evaluation localities[7]. Given the long time-span between the base and end-line, and notably the administrative issues concerning data collection for the final round, attrition is of particular concern as I discuss next.

For this study I build on the household panel dataset used in Gertler et al. (2012a) which linked the ENCASEH97 to the ENCEL surveys between Wave 2 and 7. To this panel I added Wave 1 (the baseline ENCEL) and Wave 8 to obtain a ten-year span. I mainly focus on the changes in household consumption

[4] From the following seven states: Guerrero, Hidalgo, Michoacan, Puebla, Queretaro, San Luis Potosi, and Veracruz.

[5] There were actually two rounds of selection of eligible households in *Oportunidades*. In the first selection 52 percent of households were initially classified as eligible. A few months later, still before the program began, the list of eligible households was revised and 54 percent of the households originally classified as ineligible were added to the beneficiary group. This reclassification procedure was known as the '*densification*'. However, around 60 percent of reclassified (or *densified*) households did not receive transfers because of administrative problems. In this study I adhere to the original classification of households (i.e. the ineligible group constitutes 48 percent of the sample). This has mostly been standard practice in studies using the *Oportunidades* evaluation dataset since the incorporation of the *densified* households is less well documented (see for example Gertler et al., 2012; and Angelucci & Di Giorgi, 2008).

[6] See INSP 2006 for further details on the three successive phases of the targeting process, ie. [(i) Geographic targeting of marginal areas with adequate access to education and health facilities; (ii) targeting based on discriminant analysis applied to the ENCASEH survey; and (iii) Verification and modification of the beneficiary roster at a community assembly.]

[7] Due to budgetary and operation cost issues, only localities with more than 20 dwellings (viviendas) in 2003 were revisited in 2007. As a result, 37 of the 320 early treatment localities and 10 of the 186 late treatment localities were excluded from the survey sample in 2007 (Instituto Nacional de Salud Pública, 2007). In terms of the households from the original sample included in the ENCEL 2003, but excluded in ENCEL 2007, this amounts to 2.9 percent overall sample loss, slightly higher for the treatment group (3.1 versus 2.3 percent for the control) (see Clavijo, 2011 for further details on the survey sampled in Wave 8).

between the baseline and Waves 2 and 4 (for the short-term analysis), and the changes between baseline and Waves 6, 7 and 8 (for the long-term)[8]. I concentrate on the household as the unit of analysis primarily to address non-random attrition concerns which are more salient at the individual level[9]. The complete unbalanced panel contains 20,670 households with consumption data at baseline, of which 52 percent (i.e. 10,676 households) were originally classified as eligible.

Of these eligible households approximately 2 thirds belong to the early treatment group and the other third to late treatment. Table 1.2 details the sample of households used for the core of the analysis in this study (i.e. households, present at baseline [Wave 1] and at each follow-up, for which consumption data is available). Note that the main estimates in the Results section of the paper (Section VI) use the sample of eligible households at each period (column 3 in Table 2.1) to measure the impact on mobility of differential exposure to the program (i.e. treatment versus control households). However, the mobility outcomes (measured at the household level) are constructed using the entire consumption distribution (including all the households originally classified as ineligible (column 7) in addition to the original treatment and control households (column 3)[10].

The attrition rates indicated in Table 1.2 show there is already considerable sample loss between the first two periods. Attrition amounts to 9 percent at the aggregate level by Wave 1 and 4 and is higher for the treatment group (10 percent versus 6 percent for the control). After that this period the cumulative attrition remains stable up to Wave 7. The steepest hike in attrition (amounting to 48 percent at the aggregate level) happens between the last 2 periods; in the transition between the mid and long term. In all, attrition in the long-term panel is highest among the ineligible households (52 percent), followed by the treatment group (46 percent) and slightly lower among the control group (42 percent). However, beyond comparing the raw attrition rates, in order to understand the bias this sample loss may generate, it is necessary to determine whether there is differential attrition between the treatment groups based on their initial characteristics. Table 1.3 displays the estimates at each wave of attrition as a function of treatment status and the interaction term with a number of baseline characteristics at the head, household and community level. While attrition is associated to a few baseline characteristics (e.g. education of the head/spouse, household composition, and access to electricity), the results indicate there is no evidence of differential attrition according to treatment status. None of the point estimates for the intent-to-treat variable alone are statistically significant and, in all, less than five percent of the point estimates for the interacted terms are significant in the short term (columns 2, 4 and 6). By Wave 7 (5 and a half years since the beginning of the program) this proportion increases only slightly and is still less than 10 percent (columns 8 and 10). Access to electricity is the only variable which is consistently associated to

[8] I do not use Wave 5 (i.e. May 2000) since the consumption data is not available for this round.
[9] In previous research I conducted using the preprogram census and the last round of the ENCEL (ie. the ENCASEH linked to Wave 8) I found substantial attrition especially among the sample of youths I studied (74 percent overall attrition and 76 percent among the male youth; see Clavijo, 2011).
[10] For further clarity, when constructing the household mobility measures, I use the entire universe of households in the evaluation villages (i.e. the original eligibles plus the original ineligibles; column 9 in Table 1.2). In contrast to other studies that drop the *densified* households from their sample altogether, in this study, although I adhere to the original classification into treatment and control, I still use the information of all the original ineligibles in order to characterize the welfare distribution in these villages at each period.

a lower likelihood of attrition throughout the 10 year span, and to a greater extent for the early treatment group.

Moreover, in the long-term, households are less likely to attrite if the head is of indigenous descent (i.e. speaks an indigenous language). This sample selection due to ethnicity only appears in the final round of the survey. It is important to bear in mind that in this final wave only localities with more than 20 dwellings (*viviendas*) in 2003 were revisited in 2007. However, this administrative sample selection does not seem to be driving the ethnicity result since indigenous predominance is likely to be higher precisely in the smaller and thus excluded localities. Rather the negative association between ethnicity and attrition is more consistent with a lower likelihood of migration among the indigenous population. The interaction term indicates this effect is augmented among the early beneficiaries.

The potential biases from the two sources of non-random attrition seem to work in opposite directions. On the one hand indigenous descent is likely associated with lower levels of welfare while access to electricity correlates to higher living standards. Thus it will be important bear these factors in mind when interpreting the impact results. In any event, the ensemble of estimates for all the baseline characteristics across the five separate survey rounds suggests there is no evidence of differential attrition along treatment status, even in the long-run, despite the high rate of sample loss (48 percent) examined above. To summarize, even though the evidence suggests the attrition is random, in moving forward, I will control for all these baseline characteristics[11] in the estimates and remain mindful of the selection due to sample loss over the survey rounds.

Furthermore, given that I will be including the entire census of households to construct the mobility measures (i.e. using the full consumption distribution of the evaluation villages), it is important to examine how the ineligible 'non-poor' initially compare to the eligible poor. Figure 1 plots the consumption distributions for the households in the sample at baseline by treatment status (left panel: T vs. C) and eligibility status (right panel: Ineligibles vs. Eligibles). As expected, given the successful village randomization, treatment and control households have nearly identical consumption distributions (the kernel densities essentially overlap at all points the distribution). The consumption distribution for the ineligible households is mildly skewed to the right indicating a slightly higher mean consumption as expected given they were classified as non-poor. However, the distribution suggests they are not very well off, since there is still considerable overlap of their consumption distribution to that of the poor (eligible) household, in particular at the upper and lower tails. This similarity in terms of consumption at the extremes of the distribution reflects the fact that the program eligibility was based on a means test, to proxy for households' permanent income, and not on actual consumption.

Lastly, for the sample of households under study, treatment status (early treatment or late treatment for the control group) denotes 'intention-to-treat' (ITT) rather than actual treatment. However, because of the high

[11] Following to a large extent Gertler et al. (2012) in the choice of control variables.

marginality in the rural areas the program experienced near universal participation, thus the impacts of the program based on ITT comparisons can be relied on to approximate 'treatment-on-the-treated' (ToT) effects[12].

III. Conceptual definitions, Hypotheses and Relevant empirical evidence

As mentioned in the introduction, the recent optimism about the increase in economic mobility in Latin America has been associated in varying degrees to the advent of conditional transfer programs by most governments in the region. Up to now I have used the term mobility loosely but an important distinction must be made regarding the concept of mobility I will use in this study. As reviewed in an influential taxonomy by Fields (2000 and 2005) the literature on economic mobility is vast and the different indices are not measures of the same underlying conceptual entity. Ideally the *space, domain* and *concept* of economic mobility should be well defined (Ferreira et al, 2011). The *space* indicates the choice of variable in the distribution under consideration (in this study, household consumption[13]), and the *domain* indicates how far apart in time the two (or more) distributions are observed (in the present context, close to 10 years; between 1998 and 2007).

An exhaustive survey of the distinct mobility measures is beyond the scope of this study[14] and it should suffice to clarify that the underlying concept of mobility I adhere to is one of 'mobility as movement' (as opposed to 'mobility as origin independence' or 'mobility as equalizer of long term incomes'). In particular within this concept of 'mobility as movement' I will focus on 'positional movement'(as opposed to 'directional or non-directional income movement' or 'share movement') by building a simple outcome measure of each household's change in rank along the consumption distribution. Finally, it is important to distinguish between two very different domains of economic mobility: the intragenerational (for which the unit of observation, e.g. individuals or households, is tracked over time) and the intergenerational (the unit of observation indicating lineage is followed across generations, e.g. fathers and sons, mothers and daughters, etc). Both domains are important in their own right and the distinction is fundamental since the key desirable properties for a measure of mobility across generations may differ from those for mobility over a person's lifetime. Also, the two domains may portray diverse pictures since it is possible for a given society to exhibit high mobility within generations while remaining almost completely immobile across them, or vice versa. Since the unit of analysis used in this study is the household observed at baseline and up to a decade later, the domain of mobility is therefore strictly intragenerational.

The distinction regarding the domain of mobility is useful to hypothesize about how a conditional cash transfer program may affect economic mobility in the long-term. One way in which *intergenerational* mobility may be

[12] In contrast, participation rates among eligible households in urban areas have been much lower (close to half of the eligible population). In this case, the distinction between ITT and ToT effects is empirically important and studies looking at impacts of *Oportunidades* in the urban context must consider what actually determines program participation (Behrman, et al. 2009).
[13] More precisely, I use per capita adult equivalent household consumption.
[14] In addition to Fields (2000 and 2005) and more recently Ferreira et al. (forthcoming), other reviews of the mobility literature include Atkinson et al., (1992) and Fields and Ok (1999).

affected is via the improvements in schooling which were identified in the short-term (Schultz, 2004 and Skoufias and Parker, 2001). The underlying rationale is that these improvements in schooling may potentially translate into higher returns in the labor market, rendering young, educated beneficiaries more mobile in socioeconomic terms.

One of the few studies, to my knowledge, examining the impact of *Oportunidades* on economic mobility was conducted by Freije and Rodríguez-Oreggia (2009); though their focus is on *intergenerational* mobility. The authors examine the effect of *Oportunidades* on between-generation mobility by measuring the effect of exposure to the benefits of the program over time comparing the labor situation of the beneficiary youths to that of their parents. In particular, they compare generational differences in income levels, participation in formal employment and employment with social security benefits as well as occupational qualifications, but do not find any significant effects on the outcomes under study. Besides focusing on intergenerational rather than intragenerational mobility, the methodology used by Freije and Rodríguez-Oreggia differs significantly from the approach employed in this paper since the authors do not exploit the random assignment of eligible households from the original sample into early or late treatment. Rather, their comparison group is made-up by a small proportion of the new households incorporated into the evaluation in 2003 (from states not included in the original sample). In particular the control group only included the households from these additional states that had not been incorporated into the program by 2007. Notice the vast expansion of coverage of *Oportunidades* by 2004 meant the control group used by Freije and Rodríguez-Oreggia (2009) was very small (less than 18% of the eligible youths interviewed in 2007). Furthermore, at the time the study was conducted (as part of the 2008 Evaluations requested by the Government of Mexico) the information from the individuals who had migrated by 2007 was not available. Thus, the study by Freije and Rodríguez-Oreggia (2009) only utilized information from those youths who continued living in their villages. In any case, in recent research I largely confirmed these results incorporating the information on the members who had moved out of their households[15] since 1997, or were temporarily away (Clavijo, 2011).

Hence, there is no clear evidence that improvements in short-term outcomes from *Oportunidades* (increased years of schooling and reduced drop-out rates) crystallized into gains in final outcomes for the youth in the long-term (improved insertion in the labor market). As pointed out by Molina-Millán et al (2016), an important caveat to qualify the lack of impacts, is that given the timeframe of *Oportunidades* (and most subsequent CCTs), evidence is necessarily limited to impacts during early adulthood, and hence partly reflects the trade-off between schooling and early work experience. However, even at further stages of adulthood, if existing job opportunities are scarce (due to low economic activity and low labor demand in local markets) as may be the case in the very marginal rural areas, it is possible that the additional years of schooling do not entail more favorable future employment conditions. Nevertheless, the transfers themselves could still improve employment related outcomes in the long-run through asset creation by enhancing household' asset base and

[15] This information was collected in an additional migration module included in the ENCEL2007 questionnaire which asked an informant in the household about certain labor market outcomes and migration decisions concerning the absent member.

income diversification capacity (e.g. allowing households to diversify away from agricultural activities). Indeed, this is explicitly intended in other CCT programs like the one in Nicaragua where productive investment grants, technical assistance and basic commercial training are offered (Macours, Premand and Vakis, 2012). Even though these options are not available within the context of *Oportunidades*, this objective could potentially be achieved if the extra cash relaxed the household's budget constraint allowing for asset accumulation and income diversification. For example, the cash injection could help households afford the start-up costs associated with entrepreneurial activities (McKenzie and Woodruff, 2006). Also, risk-averse beneficiary households may be more willing to invest in riskier but higher return activities if the transfers are perceived as a secure and steady source of income.

In fact, the evidence for *Oportunidades* shows that beneficiary households increased ownership of productive farm assets and that agricultural production increased faster for beneficiary households than non-beneficiary households resulting in significantly higher agricultural income (an estimated 9.6 percent increase as a result of 18-month exposure). Moreover, the returns on these investments are estimated to persist over time and raise long-term living standards as measured by consumption. Even 4 years after households in the control group were incorporated into the program, consumption levels for the original treatment households were 5 percent higher than for the original control. In all, this is one of the scant pieces of evidence suggesting positive short-term impacts from *Oportunidades* (via returns on investments made by treatment households during the initial 18-month experimental period) did in fact translate into improvements in long-term living standards (Gertler et al, 2012). It is this finding that motivates to a large extent the research question in this study compounded with the early evidence from simulation and ex-post results suggesting the largest reductions in poverty (notably in the poverty gap and severity) which could be attributed to *Oportunidades* were achieved among the poorest of the poor population (Skoufias and DiMaro, 2006). As such, from a distributional perspective it is interesting to examine whether beyond improvements in (the mean of) long-term living standards, *Oportunidades* also improved mobility (within the same generation) in a sustained way.

One of the reasons longer-term results of RCTs may not be examined is that the control group is usually phased-in, since few programs were set up for rigorous long-term evaluation of their overall impacts. This in principle thwarts the opportunity to gauge the program's long-term impact against the counterfactual of absence of the program. This has been pointed out as one of the limitations for measuring the long-term impacts of CCTs using the experimental design However, given the focus on mobility, the dynamic perspective in this context and the catch-up process (or lack thereof) of the households that received the program late is of particular interest in this study. In principle, given the short differential exposure, full catch-up by the late treatment group might be expected. For example, given the evidence by Gertler et al. 2012 on increased investment in productive activities for the early recipients, once the late treatment households receive the transfers they should also be able to seize these investment opportunities thus catching up to the early beneficiaries.

However, it is possible that the timing of the intervention (even if delayed only a year and half) could impact a household's welfare pathway in a sustained manner, in particular in the high risk rural context under study. As such, one way in which a transfer program may have lasting effects on welfare and mobility is through its risk coping mechanism. Transient shocks may have persistent effects if the households' response (e.g. taking kids out of school or selling productive assets) sets it on a lower welfare pathway. For example for early stages of *Oportunidades*, de Janvry et al. (2010) show that there is strong state dependence in school enrollment and that the CCTs helped protect enrollment. Moreover, Premand and Vakis (2010) show that temporary shocks may trigger poverty persistence. Thus, given the evidence on the irreversible impacts of transient shocks (de Janvry, et al 2010, and de Janvry et al, 2006), it remains an open empirical question whether the late treatment households (that were exposed longer to uninsured risk) indeed manage to catch-up to the early beneficiaries. Moreover, the timing of the intervention could also affect the household's welfare path depending on the stage within the household's life-cycle at which the transfer are received (early when the burden of rearing young children is high or later when more members are close to prime age and represent less of a burden). Examining the programs differential impact along a number of heterogeneity dimension including shock exposure as well as family composition will allow me to investigate this empirically.

IV. Descriptive evidence on mobility

To recap, the space of economic mobility in this study is household consumption[16], the domain *intragenerational* and the concept is one of positional movement. For brevity, the term 'mobility' hereafter entails this definition unless otherwise stated. As a first approximation I examine positional movement using quintile mobility matrices (Table 2). In each matrix the rows indicate the quintile of consumption a household belonged to at baseline and the columns indicate the corresponding quintile at each subsequent wave (i.e. the diagonal contains the share of households that have remained immobile). The entries in each matrix are row shares, i.e. they indicate what share of households belonging to a given quintile at baseline end up in a given quintile in the subsequent year. Thus, each row sums to 100 percent. The consumption quintiles are constructed using the entire sample, including the original non-eligible population (as mentioned before the ineligibles constitute a little less than half of the households in the sample). The inclusion of the ineligibles gives the full picture of the entire welfare distribution in the evaluation villages. Note however, that the resulting mobility matrices are built using only eligible households (treatment T and control C separately) for visual comparison of the movement along the distribution of the early beneficiaries compared to their counterparts (i.e. the late beneficiaries).

[16] Household consumption includes food and nonfood expenditures. From Wave 2 onwards, the food consumption data is based on a direct question about the amount of each food item consumed and purchased. In particular, respondents are asked about consumption of 36 food items grouped into 4 types ("Fruit and Vegetables", "Cereals and Grains", "Meats, Fish and Dairy", and "Other processed foods"). However, the consumption questionnaire differed slightly in Wave 1 (food expenditures were not recorded for each item separately but aggregated by type. In any case, the comparability of the consumption variable in levels between the waves is not a particular concern since the outcomes of interest (the mobility measures) are constructed using the rank along the distribution.

The mobility matrices for the first follow-up round (between baseline and October 1998) show no statistical difference between the T and C groups (the chi-squared values that indicate the null hypothesis that the two samples are drawn from the same distribution cannot be rejected). Thus, the program initially seems to have no visible impact on economic mobility as defined previously. The highest immobility (i.e. the highest probability of lying along the diagonal or, alternatively, of remaining in the same consumption quintile) for both the treatment and the control is observed among the lowest two quintiles. A year into the program, mobility is visibly higher among the treatment group (the difference between the two groups is statistically significant at the 5 percent level). This pattern holds up to two years after the program was implemented (by wave 4). The control group exhibits less positional movement in particular among the lowest quintile (nearly half of the households in the bottom quintile at baseline in March 1998 remain there by November 1999.) As a result, the initially wealthier households in the control group are displaced from the top of the distribution. The bottom row of the matrix for the control households shows that conditional on being in the top quintile at baseline a household has about an equal chance of ending up in any other quintile (even the bottom one) by wave 4. This degree of downward mobility is not observed in the treatment group.

Is the difference in mobility between treatment and control, detected in waves 3 and 4, sustained once the experimental period is over? The matrices for wave 7 and 8 suggest this is not the case since the differences in the proportion of households lying along the diagonal are no longer statistically significant. However the distribution patterns described earlier are still observed in wave 7, meaning the bottom quintile exhibits the least mobility (two thirds of the households in the bottom quintile at baseline remain in the first or second quintile 4 years later). Mobility seems to increase considerably by 2007 (most values along the diagonal hover around 20 percent; the benchmark value of complete mobility or perfect randomness). To delve further into the patterns described above I next turn to regression analysis to examine the impact of differential exposure to the transfers over time more systematically.

To understand further the patterns described above I next turn to the formal estimation strategy to examine the impact on mobility of differential exposure to the transfers over time more systematically.

V. Estimation Strategy

Ranks approach

There is a methodological as well as a practical argument in favor of using ranks-based estimation. On the methodological side Athey and Imbens (2016) advocate for the use of the difference in means of the ranks by treatment status, in lieu of the commonly used difference in means of the outcome. The authors illustrate the gains from this transformation in terms of the robustness to outliers and in particular to the presence of zeros (Athey and Imbens, 2016, p. 15). Overall, the ranks transformation improves the power of the hypotheses tests

in settings with outliers and thick-tailed distributions. They highlight that a rank-transformation is less arbitrary than, for example, trimming a variable to deal with outliers or simply transforming it by taking logarithms. Moreover, they emphasize their preference for a rank-transformation especially in the case of outcomes where such transformations are not feasible, e.g. skewed distributions and those with a mass point at or near zero such as earnings or consumption. Finally, Imbens and Wooldridge (2008) argue that ranks- based estimation can provide more robust results for heterogeneity analysis, when sample size is low, because it allows equal weighting for outliers.

In practical terms, the ranks-approach is appealing because it allows me to exploit further the information available, in particular the data on the non-poor (and thus ineligible) population. This expands beyond the common difference in means comparison of the early and late treatment groups. In this approach the non-poor population households are included directly in the ranking setting a higher bench mark against which to measure impacts - closer to the non-vulnerable, whom are too wealthy to qualify for the program. This is arguably a more informative yardstick to use in order to avoid looming or overstating absolute gains. As Rosenzweig (2012)[17] points out in reference to the general interpretation of results from randomized control trials (RCTs): "*small absolute gains on small baseline levels produce large percentage gains, but the difference in the lives of the subjects, even in the long run can be quite small*" (Rosenzweig, 2012, p.120). The author argues that in many cases the interventions evaluated do make a difference in the lives of poor people, but nowhere near to bridging the gap between poor and non-poor in the developed world. In this respect, the ranks-based approach to impact measurement used in this paper may add to the understanding of targeted transfer impacts from a distributional perspective.

The estimation strategy for the ranks approach in this paper is straight-forward. I used the randomization design to estimate separately the short-term (waves 2 and 4), mid-term (wave 7), and long-term (wave 8) impacts of differential exposure to *Oportunidades*. More specifically, I use simple differences between the treatment households (receiving benefits in March 1998) and control households (receiving benefits up to 18 months later). I include baseline controls to increase the precision of the estimates. The basic econometric specification for all regressions is as follows:

$$M_{it} = \beta_0 + \beta_1 T_i + \beta_2 X_i + \varepsilon_i$$

Where M_{it} represents the outcome mobility variable (as described below) for each household i measured between baseline (wave 1) and a subsequent period (wave t). T is a dummy variable indicating early-intention-to-treat status for household i and β_1 is therefore the coefficient of interest reported capturing the magnitude of the impact. X_i is a vector of pre-program characteristics (including household head, household demographic and community variables[18]) I will control for to gain precision.

[17] In his review of A. Banerjee and E. Duflo's book *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty (*2011*)*
[18] The notes in the tables indicate the full set of control variables and variable definitions; I follow Gertler et al. (2012) to a large extent in the choice of control variables.

Lastly, the clustering of households within villages implies that household-specific error terms are likely to be correlated within each village (and across time). If this correlation is not taken into account it may lead to a considerable bias in the estimated standard errors of the program impact (Skoufias and Parker, 2001). The regression models I estimate therefore account for the clustered nature of the sample and report the robust standard error estimates for the impact of the program.

Mobility measures using Ranks

To examine the positional movement of households through time since the implementation of the program I define two outcome measures of mobility based on each household's rank in consumption:

(i) *Absolute mobility (AM)* is defined as the rank[19] of the change in consumption (*C*) which household *i* exhibits between two periods (the baseline year, wave 1 and a subsequent wave *t*). The change is simply the difference between the log of consumption in *t* and baseline (i.e. the percentage change in consumption).

$$AM_{it} = rank_i(\log C_t - \log C_1)$$

This can be viewed as a rank measure of a household's progress (accounting for their initial position, since the difference in logs gives the percentage change in consumption). The ranking (normalized so it ranges from 0 to 100) thus indicates how a household fares (with respect to other households including the non-poor) in terms of its own progress. However, it does not indicate the household's movement along the consumption distribution (it is therefore not a relative but an absolute measure of mobility). Note that a household that is among the richest in both periods may rank low in the *AM* measure if it has made no progress in consumption terms or experienced a decline between the two periods. Conversely, a household that is still among the poorest in both periods could have vastly improved its spending capacity thereby scoring a high *AM*. Thus, the absolute mobility consumption measure does not relate directly to the level of consumption since a low absolute mobility value does not indicate that a household has low consumption, but rather that it experiences low mobility in terms of its consumption.

(ii) *Relative mobility (RM)* is defined as the change in the rank of consumption for a household between time *t* and baseline.

$$RM_{it} = rank_i(C_t) - rank_i(C_1)$$

To build this measure the entire consumption distribution is ranked and normalized (to range from 0 to 100) at baseline (1) and follow-up (t). Thus the relative mobility measure is simply the difference between the

[19] All ranks measures are normalized so that they range from 0 to 100. When constructing the ranks equal observations are assigned the average rank.

normalized ranks at the two periods. This can be viewed as a measure of the change in a household's relative position along the consumption distribution. This rank measure indicates how a household fares in terms of mobility relative to the mobility experienced by the other households in the sample (including the non- poor). In contrast to the *AM* measure, the *RM* does not range from 0 to 100 since a household may experience downward mobility and thus fall in rank scoring a negative value. Note therefore that a household scoring a high *RM* measure will typically have climbed from a low initial position in the distribution to a high one (experiencing upward mobility), a household scoring a near zero value will have experienced no mobility, and a low scoring household will have descended from a high rank to a low one (reporting downward mobility).

In sum, the *AM* measure gauges a household's (consumption) mobility with respect to their own initial situation, while the *RM* measure gauges upward or downward mobility in terms of their relative position (with respect all other households in the sample).

Trajectories approach

The manifold rationale for using welfare trajectories is discussed at length by Premand and Vakis (2008).
From a descriptive standpoint, welfare trajectories are the most comprehensive presentation of households' mobility patterns in a three-round panel since the universe of welfare trajectories traces all possible mobility outcomes. In the present study, the trajectories approach allows me to exploit further the long-term panel beyond the 2 wave ranks estimates described above. By characterizing three period trajectories I can retain information about the pathways leading up to the final period. This approach can be specially useful in the evaluations of temporary interventions, such as CCTs, in order to gain insight about the mid-term results leading to the long-term outcomes.

On practical grounds, trajectories spanning large time-windows are employed in empirical mobility analyses in order to characterize longer-term welfare trends (Baulch and Hoddinott, (2000)). Moreover, trajectories provide a summary measure of welfare over multiple periods following for example the work on long-term poverty measurement by Calvo and Dercon (2009), which uses a single index of intertemporal poverty based on trajectories of households' standard of living.

On structural grounds, as underlined by Premand and Vakis, trajectories may prove superior to using round-to-round transition matrices, as used in the ranks approach above. In the present case, traditional two period transition matrices would only yield an appropriate representation of the underlying welfare process if all the households in a given consumption tercile have the same transition probabilities regardless of their past history (i.e. if the first order Markov assumption holds [Shorrocks, 1976].) However, this assumption is not likely to hold since there is reason to believe that a household's probability of poverty in a period $t+1$ is not only affected by its poverty state in period t but also by the household's entire history of poverty states prior to t.

Indeed, Premand and Vakis demonstrate there are visible deviations from the first-order Markov assumption in the frequency distributions of the trajectories in the 3-wave panel of their study.

Building on Premand and Vakis (2008) I also construct welfare trajectories which describe the sequence of a household's position along the welfare distribution as time unfolds. Specifically, I make use of three rounds of data (t=3) such that trajectories take the form {ijk} where i,j,k correspond to each household's position along the welfare distribution in period 1 (i), period 2 (j) and period 3 (k)[20]. The household's position in a given round is determined by the tercile of the consumption distribution if falls into.

Expanding beyond the negative trajectories characterized by the authors (poverty persistence and downward mobility), in this paper I am also interested in the whether the conditional transfers may lead to positive patterns of welfare. Hence, I focus on the following four particular welfare trajectories {ijk} over three rounds of data to characterize separately patterns of persistence and movement:

Persistence patterns

(1) Sustained Poverty: {ijk} = {111} ;

(2) Sustained High Welfare: {ijk} = {333} ;

Movement patterns

(3) Downward mobility (weak): {ijk} such that { $i \geq j > k$ or $i > j \geq k$ } ;

(4) Upward mobility (weak): {ijk} such that { $i \leq j < k$ or $i < j \leq k$ } ;

(5) Temporary upward: {ijk} such that { $i < j > k$ } ;

(6) Temporary downward: {ijk} such that { $i > j < k$ } ;

The general specification for the trajectories approach is thus:

$$traj_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \varepsilon_i$$

Where the dependent outcome $traj_i$ is a binary variable indicating whether the household *i* exhibits each of the 4 trajectories outlined above. For example, in the case of Sustained Poverty (High Welfare), $traj_i$ takes the value of 1 for households with trajectories {111} ({333}), i.e. remaining in the lowest (highest) tercile of consumption throughout the 3 survey rounds, and 0 otherwise. The first two movement patterns, (3) and (4), describe monotonic trajectories while the last two, (5) and (6), characterize changes in the direction of mobility. More specifically, for Downward (Upward) Mobility, $traj_i$ takes the value of 1 whenever a household consistently moves to a lower (higher) tercile in the consumption distribution between the first and second wave and the second and third wave and 0 otherwise. Note that in the weak version of this condition presented above, one of the two transitions (either between period 1 and 2 or between period 2 and 3) may hold with the

[20] I limit the number of waves conforming the trajectories despite having access to up to 5 rounds of information because the number of possible trajectory combinations increases excessively over longer trajectories. This renders the frequency of sustained patterns near negligible (below 5%). I therefore focus on three-period trajectories which suitably capture the short, mid and long term effects of differential exposure to the CCTs.

equality sign[21]. The Temporary Upward trajectory describes pathways in which households initially ascend in the distribution (i.e. move up between period 1 and 2) only to fall back down in the subsequent period (i.e. move downward between period 2 and 3). As such, these are households that, despite making an initial progress slip back into poverty. Conversely, the Temporary Downward trajectory describes patterns of initial decline followed by recovery. As such, these are households that are capable of escaping poverty despite an initial descent. Finally, T is a dummy variable indicating early-intention-to-treat status for household i and β_1 is therefore the coefficient of interest reported in each regression result capturing the magnitude of the impact.

VI. Results

Ranks approach

The main results for the regression analysis using the ranks approach for the short term (waves 2 and 4) are presented in Table 2.1. The results confirm to a large extent the findings previewed by the mobility matrices. Positive and significant treatment impacts are indeed observed in the short term (up to a year and half after the program started) for the consumption mobility outcomes. In particular, *Oportunidades* seems to have a significant impact on mobility as early as six months after the first transfers were delivered (wave 2); an impact which was not detected in the mobility matrices.

In terms of absolute mobility (*AM*), by wave 2 the households receiving the transfers ranked on average 4 positions higher (on a normalized scale of 100) than their control counterparts (column 2 in Panel A). This mobility measure, capturing the percentage change in log consumption between baseline and March 1998, indicates that the treated households presented on average greater improvements in their livings standards, not only with respect to the other poor (untreated) households but also with respect to the non-poor (ineligible) households in the sample. The magnitude of the impact sets the treated households at a slightly higher average position than the non-poor households (51.3 versus 49.8)[22] in terms of the welfare progress made since baseline.

In terms of relative mobility (RM), there are also positive significant impacts on consumption mobility as early as wave 2. The households receiving the transfers presented on average a change in their consumption rank 4 positions higher than the comparison (late treatment) households. As expected, the comparison households, which have not yet received transfers, have on average descended in the consumption distribution. The estimated downward movement for this late treatment groups is equivalent to 3.9 positions. Thus the magnitude of the positive impact (3.95) from the transfers on the treated households almost exactly mirrors the downward movement experienced by the late treatment households (this is not the case in subsequent waves).

[21] Again the weak condition is the preferred version presented in the main results since the strict condition, which requires both transitions (periods 1- 2 and 2-3) to hold with the strict inequality, yields a very low frequency (2%) of success cases.
[22] For clarity, the resulting average absolute mobility value for the early-treatment group (i.e. 51.3) is obtained by adding the point estimate value (3.7) to the late-treatment group's average (47. 6).

This suggests the reshuffling of ranks is occurring mainly between the eligible poor, and therefore towards the bottom of the distribution. Indeed, in contrast to the result on absolute mobility, the impact on relative mobility is not enough for the treated poor (presenting an average value of 0.03) to surpass the ineligible non-poor (whose mean value is 0.6). Taken together, the mobility measures I use indicate that early in the program's operation the proportional progress made by the very poor treated households is on average greater than that made by the wealthier ineligible households. This might not necessarily have been expected given the evidence of the program's 'spillover' effect on the ineligible population (i.e. the transfers indirectly increased the consumption of ineligibles households in treatment localities due to enhanced credit and insurance markets; Angelucci and De Giorgi, 2009). However, given that the relative standing (in consumption rank) of the treated households is still low compared to the non-poor, the progress at this point is not enough to set the treated households at the standard of the non-poor group in relative mobility terms.

Panel B presents the results a year later which indicate that the program´s impacts on mobility intensify with longer exposure. This is the last wave before the late-treatment group is phased-in and as such the period at which the advantage in mobility terms experienced by the early treatment households reaches its peak. Indeed the positive impacts both in absolute as well as relative mobility nearly double in magnitude by wave 4. At this stage, the treated households manage to surpass their non-poor counterparts no only in terms of the proportional consumption growth but also in terms of progress in their relative standing in the welfare distribution. Overall, the program's impact sets the treated households 4 positions above the ineligible middle-class mean in absolute mobility terms and 2 positions above in terms of relative mobility.

Columns 1 and 4 include the levels estimates as a reference. In these regressions the outcome variable is the per capita consumption value at wave 2 and 4 correspondingly. As such, the coefficients reported indicate the impact of the program on the early beneficiaries' standard of living (similar to the main specification used by Gertler. et al, 2012). I find it useful to include these estimates in order to underscore the additional insight provided by the ranks approach employed, in particular with regards to the interpretation of the magnitude of the impacts. Consistent with the results on mobility, the levels estimates show positive and significant impacts on the level of welfare. However, this measurement based on the difference in means by treatment status only incorporates the control (late treatment) households as a comparison group, while the ranks estimates integrate the non-poor's standard. The bottom three rows of the table display the magnitude of the impacts (for the levels and the AM estimates) as a percentage of the mean values for the (i) late-treatment and the (ii) non-poor households, and the difference between the two. The comparison of these two benchmarks is useful in order to examine whether the impacts loom large because they are gauged against low reference levels (as warned against by Rosenzweig (2012) in particular in the context of RCTs). Indeed, the values in the bottom row indicate that this looming effect, captured by the difference between (i) and (ii), is significantly larger in the levels estimates. By wave 4, when impacts reach their peak values, this gap between the two benchmarks is in the order of 6 percentage points; a sobering result when interpreting the magnitudes of the impacts. By contrast, this difference between the non-poor and the poor reference means is as low as half a percentage

point for the ranks estimates (in wave 2), indicating the ranks-based estimates may indeed yield a more conservative and informative impact measure vis-à-vis the levels estimates which may overstate absolute gains. Panel C displays the results a year after the control group starts receiving the program. The level estimate (column 7) indicates the early treatment group still have a higher living standard with respect to the control, though the lower magnitude of the coefficient indicates the difference between the two groups is starting to narrow. The estimates for the mobility measures still signal an advantage for the early beneficiaries, although the coefficients are only marginally significant.

I now examine the mid and long term results which are displayed in Table 2.2. The experimental period ends after wave 4 (November 1999) when the late treatment households are phased into the program. Thus the mid-term results for wave 7 (Panel D measure the impacts of differential exposure to the CCT (up to 5 and half years for the early treatment versus 4 years for late treatment group). The estimates indicate there are still detectable impacts in the mid-term post-experimental phase, both in terms of absolute and relative mobility (as well as the level estimate). In order words, despite the fact that the late treatment households are now also receiving transfers those who received them early still display an advantage. The households treated longer rank on average 2 positions above their counterparts in terms of their consumption growth (i.e. gains in absolute mobility) and present an average improvement in their relative standing in the distribution which is 2.4 positions higher (i.e. gains in relative mobility).

However, this mobility advantage seems to decay with time, with the magnitudes of the coefficients falling below the levels presented in wave 2. This pattern indicates that the late treatment households are starting to catch-up (Figure 1 illustrates this more clearly). This is evidenced further by the fact that their average relative mobility score is no longer negative and the gap in means between the poor and the non-poor is narrowed; an indication that some of these households are managing to remount positions along the distribution. The catch-up process to the non-poor is interesting in its own right since it is not evident from the levels estimates alone or in previous studies examining the sustained impacts of *Oportunidades*. For example, Gertler et al. do not find evidence of catch-up by the late treatment households in terms of their level of investment. The ranks approach thus provides additional insight, in particular about the late treatment groups' situation in the post-experimental period with respect to all other households in the sample, including the wealthier middle class. At any rate, the households with greater tenure in the program still exhibit on average greater mobility, placing them one position above the non-poor (both in AM and RM terms).

Nevertheless, by the last survey round (Panel E), when the program had completed a decade in operation, none of the point estimates are statistically different from zero any longer. The results suggest there is no detectable difference between the mobility (since baseline) experienced by the beneficiaries that started receiving the transfers early and those who received benefits up to 2 years later. The coefficient for the level estimate is also statistically insignificant, suggesting there is no longer a difference between the early and late treatment households in consumption capacity. In order words, the households that received the program late have

managed to fully catch-up to the early group both in terms of living standards and socioeconomic mobility. Despite this catch-up being ruled out by Gertler et al, it is still plausible that their investment hypothesis is at play. The fact that the impacts both on the levels and the mobility estimates are sustained in the mid-term may suggest that the productive assets and activities which gave the early treatment group the initial advantage required lumpy investments. Hence, given that the impacts do disappear in the long run, it is possible that the additional 5 years of transfers provided the late treatment group enough time to accrue the necessary resources to invest in similar productive assets and activities, finally allowing them to catch up to the early beneficiaries.

Indeed, the absolute mobility value for the late treatment group underpins this full catch-up process, setting them on a par with the early group (almost exactly at the median value of 50) and above two ranks above the ineligible. This mobility measure provides insight about the distributional effect of the program, namely that the long-term progress made by the original poor is on average higher than that made by the non-poor. In other words, the poor have finally managed to surpass the non-poor households in terms of their mobility capacity.

I now turn to the trajectories approach to complement the ranks approach suggesting a temporary advantage in mobility terms for the early treatment group which fades out in the long-term once the late treatment group have benefited from the program for up to four years.

Trajectories approach

The regression results for the trajectories approach are presented in Table 3. Panel A displays the impact estimates during the experimental period (Waves 1, 2, 4). Consistent with the short-term mobility results using the ranks approach, these coefficients indicate that the early treatment households are less likely to move downward (and more likely to move upward) in the distribution which, again, includes both the control as well as the ineligible non-poor. These results are consistent for the aggregate consumption trajectories as well as the subcomponents (food and non-food). Moreover, the early treatment group also presents a lower likelihood of exhibiting patterns of sustained poverty and a higher chance of remaining in the highest terciles of the consumption and the food expenditure distribution. The non-monotonic movement patterns (i.e. temporary upward and temporary downward mobility), which could not be characterized using the ranks approach, are presented in columns 13 through 18. The trajectories estimates indicate that in the short run the early beneficiary are more likely to present patterns of only temporary decline; that is trajectories in which they are able to recover (by November 1999) after suffering a fall between (March and October 1998). This result is robust for both the aggregate consumption measure as well as its subcomponents. By contrast, there are no significant early impacts of the program on the likelihood of presenting a temporary ascent.

Panel B presents the results for the welfare trajectories extending past the experimental variation period for the mid-term (Waves 1, 4, 7). In contrast to the mid-term ranks results, which indicate a positive impact on mobility for the early beneficiaries, there are no detectable impacts on the upward mobility pattern when using the three

period trajectories (and in the case of downward mobility the impact in only significant for food expenditure). However, with regards to the persistence patterns, the trajectories estimates do indicate that the households with longer program tenure are on average less likely to remain stuck in poverty and more likely to remain at the top of the consumption (and food) distribution.

The long-term trajectories estimates are presented in Panel C (Waves 1, 4, 8). Though the magnitudes of the coefficients are slightly lower, there are still a significant number of positive differential impacts for the early treatment group even for these trajectories that span well beyond the experimental period. These results are driven by the outcomes for sustained patterns (111 and 333), more so than for the movement outcomes. Thus, while the ranks approach indicate that by the last wave the households that received the treatment later succeeded in catching up to their early treatment counterparts, in their mobility capacity with respective to their baseline condition, this approach does not indicate anything about the pathway leading up to that final catch-up. In contrast, the trajectories estimates show that early treatment households are less likely to remain in sustained poverty and more likely to maintain high living standards throughout the entire 3-round period. In particular, the magnitudes of the coefficients are higher, in absolute terms, for the outcomes relating to poverty persistence as opposed to sustained high welfare. Furthermore, in the case of consumption the magnitude of the impact on poverty persistence remains stable (at -0.04 percentage points) between the mid and long term, while the impact on sustained high consumption halves.

Interestingly, and contrary to the findings for the short run, the mid and long term results (Panels C and D) suggest the early treatment group is more likely to follow a path temporary upward movement (followed by a decline) and less likely to follow a path of initial decline followed by an ascent. These results are indeed illustrative of the reshuffling of positions along the distribution once the control group starts receiving benefits. It is important to note that this phase-in occurs after the middle round in these trajectories (after period *j* of the three period trajectory {ijk}). The negative mobility effects for the early beneficiaries is explained by the fact that once their counterparts enter the program they predictably move down in the distribution between period *j* and *k*, mechanically making it more likely for the early T group to fall between these two time periods. However this mechanical effect is no longer present in the post experimental period (waves 6, 7 and 8 in Panel D). By the final three rounds, a year after phase-in, there are no detectable impacts on either of the temporary mobility patterns from prolonged exposure to the program, while the impacts on sustained poverty are maintained.

The trajectories results so far suggest that the beneficial impacts (in terms of avoiding poverty persistence and sustaining high welfare standards), which are born by the households receiving the transfers first, are maintained in the long term. However, given path dependency, it is important to ensure that these results aren't just driven by the initial effect already detected during the experimental period. To examine this point, the final panel focuses on the trajectories for the post-experimental period alone (i.e. Waves 6, 7 and 8; Panel D) when the late treatment group have been receiving transfers for up to a year.

Overall, the results for this post phase-in period also differ from those using the long term ranks approach. Ultimately, these trajectories zoom-in further into the pathways traced in the post-experimental period. Starting at Wave 6, both early and late treatment households have received the transfers for at least a year. Compared to the short term estimates for Waves 1, 4, 8, these estimates show slightly lower magnitudes of impacts, though there are still a number of significant coefficients. The results, in particular in the mid and long term, are driven by the expenditure on food items. This is expected given the acute poverty levels of the population in question for which food expenditures constitute around two thirds of total consumption. More importantly, these post phase-in results are also driven by the outcomes for sustained patterns, as oppose to the movement outcomes. Interestingly, the differential impact on downward and upward mobility paths detected in the short term, with magnitudes as high as a 0.12 percentage point reduction in the probability of descending in the welfare distribution, disappear altogether in the post-experimental phase but not the impacts on the persistence patterns. In other words the early treatment group is less likely to be chronically poor and more likely to remain at the top even once their counterparts have received the transfers. Thus, the persistence effects stand the test of time while the impacts on upward and downward mobility (both sustained and temporary) decay. Thus, the ensemble of my results suggests that once the late treatment group receives the transfers these households may manage to catch-up to the early treatment group in terms of their capacity to move upward (or avoid moving downward). Not only do they catch-up, the late beneficiaries temporarily enjoy a mobility advantage with respect to their counterparts (i.e. they are more likely to present a path of initial descent followed by recovey). However, this advantage also dissipates with time. Thus to recap, the only impacts which persist in the longer run are the impacts on sustained poverty and sustained welfare. Indeed, tracing the trajectories after all households have benefited from the program for at least a year, those who received the program early still exhibit a greater ability to escape and remain out of poverty and to consistently maintain a high standard of living. This transcending result speaks to the importance of the timely assistance received by the early treatment group.

Moreover the distinction in my results, between persistence and movement patterns, is noteworthy to the extent that it resonates with the findings by Premand and Vakis (2010). In particular, in the Nicaraguan case the authors find stronger impacts of shocks on poverty persistence than on downward mobility, in particular among the poorest of the poor. My present findings also signal that the timing of the transfers may be especially meaningful for households stuck at the bottom of the distribution. As noted by Premand and Vakis, the fact that the causes of poverty persistence differ from those for downward mobility is a key finding since it suggests they each may require distinct sets of policy options. I explore this notion further in the next section where I investigate how the impacts on persistence and movement vary according to initial heterogeneity between households (including exposure to shocks.)

To synthesize the main results in this section, the estimates using the ranks approach suggest that differential exposure to the CCT increased upward mobility only temporarily for the early beneficiaries. Thus the mobility

analysis using two round comparisons (baseline versus each of the subsequent waves) indicates that the initial advantage exhibited by the early beneficiaries (in the short and mid-term) wears out eventually, sometime after the late beneficiaries receive the program. In contrast, the trajectories estimates indicate that the beneficial welfare impact on the early recipients does persist into the long term. In particular, the households that randomly received the transfers first displayed on average a higher likelihood of sustaining high welfare levels and a lower probability of remaining stuck in poverty. I find this contrast in the long-term results (between the ranks and trajectories estimates) interesting in its own right since it highlights the fact that utilizing the mid-round surveys of the panel provides additional useful information.

Nevertheless, even in the absence of mid round surveys as if often the case, the ranks approach may prove instrumental for mobility analysis. The results above using the ranks estimates detect a catch-up process of the late treatment group to the non-poor households in the sample. This result is revealing since it is not evident from the levels estimates alone or in previous studies examining the sustained impacts of *Oportunidades*. Furthermore, the construction of ranks has the added benefit that it allows the researcher to incorporate in the analysis other relevant segments of the population (such as the middle class) besides de homogenous treatment and controls groups. In short, this straightforward approach may add some useful distributional insight to the standard impact evaluation approach.

Heterogeneity Analysis

To understand the above results further, in particular the conditions leading to the sustained mobility advantage for the early beneficiaries, in this section I explore how the impacts vary according to the initial heterogeneity between the households. More specifically, I am interested in investigating whether differential exposure to the CCT compensates for or reinforces initial inequalities in human and physical capital. To this end, I perform heterogeneity analysis along a number of baseline household assets and characteristics as well as preprogram shocks. Again, I conduct this analysis both using the ranks approach and the trajectories approach.

The ranks heterogeneity results for the variables relating to physical capital are presented in Table 4.1. The coefficient of interest displayed is the intention-to-treatment variable interacted with each of the heterogeneity dimensions. Given the investment hypothesis posited by Gertler et al. (2012) one might expect the asset holdings at baseline to predict the upward movers among the households receiving the transfer. However, the ranks heterogeneity analysis does not suggest that the transfer program had a greater impact on the mobility of the households owning more land, or draft animals. Again, addressing the investment hypothesis, the non-agricultural households could be expected to exhibit greater mobility as a result of receiving the cash transfers if for example they have a higher income diversification capacity. However, the coefficient for the interacted productive activity term is not statistically significant. The only baseline characteristic relating to physical capital for which the interaction term is significant, in the short run, is the distance to an urban center. Interestingly, this result indicates that the CCT had the greatest impact, both in terms of absolute as well as relative mobility,

among the households that were further away from the urban centers, and thus less connected to income generating venues and opportunities. This suggests the CCT compensated to some degree for this initial disadvantage in proximity to the markets. Nevertheless, overall the ranks estimates suggest the households' physical asset base does not amplify the program's impact on upward mobility; not even in the short run before the late treatment starts receiving the program.

In contrast, the heterogeneity ranks results presented in Table 4.2 do suggest that the program's impact on mobility varies according to the household's human capital. In terms of education level the negative significant coefficient for the interaction term suggests the relative mobility gains from the program were highest on average for the less educated household heads. Once again this results suggests the CCT compensates on some level for a disparity at baseline.

I also include age group dummies describing the households' demographic composition. These are meant to capture the households' human capital (or lack thereof) via its potential labor force. A salient result is that the transfers' impact on mobility (absolute and relative) is weaker for the households with very young children at baseline. This child rearing burden, which seems to mute somewhat the program's mobilizing potential, is still visible in the long-run (when the children who were newborns at baseline are still dependent). Conversely, households with children in the age range identified as the critical juncture period for abandoning school (ages 8 through 12) display greater mobility impacts from early receipt of the program. This result is only visible during the short run (Wave 4) before the control group starts receiving the program. Interestingly, program impacts on mobility in the short run were weaker for the early recipient households with members (other than the head and spouse) which are of prime age (18 - 54 years of age). This may reflect the fact that this is the age group for which no school grant is offered. Indeed, in this respect the targeted nature of the transfers proves to have a compensating effect; benefiting to a greater extent the households with children in the critical transition period between primary and secondary school and less so those with adults in the prime of their productive lives. Nevertheless, the program reinforces in some degree the disadvantage for those households bearing the early burden of rearing their young also at a critical developmental stage.

The bottom panel of Table 4.2 includes a related measure of the household's human capital base, namely the proportion of children enrolled at each school going age group. Again, the estimates yield positive and significant coefficients for the pre-teen critical period (8-12 years) and, in this case, also for the early development age group (children under 7 years old.) The coefficients for the interacted terms are large in magnitude (in the order of 10 positions along the normalized rank distribution). The proportion of members enrolled in school captures both the household's binding constraint to human capital formation as well as their valuation of an investment in education. Thus, the differential impact on upward mobility may be interpreted as reinforcing an existing disparity (by benefiting to a greater extent the less constrained households), yet rewarding the households that attribute a high value to the investment in their offspring's human capital.

The results for the rank heterogeneity analysis by shock type are presented in Table 4.3. The rationale for performing heterogeneity analysis along this dimension is that one way in which a transfer program may have lasting effects on welfare and mobility is through its risk coping mechanism. Transient shocks may have persistent effects if the households' response (e.g. taking kids out of school or selling productive assets) sets it on a lower welfare pathway. For example for early stages of *Oportunidades*, de Janvry et al. (2012) show that there is strong state dependence in school enrollment and that the CCTs helped protect enrollment. Moreover, Premand and Vakis (2010) show that temporary shocks may trigger poverty persistence. To examine whether this timely protection against risk may be driving the sustained effects on mobility in the long term I perform heterogeneity analysis using information about households' exposure to shocks. Following de Janvry et al. (2012) I consider three types of shocks; one idiosyncratic shock at the household level (unemployment of the household head) and two covariate shocks (drought and other natural shocks). Drought is grouped separately since there is a clear distinction in frequency between this shock (which affected 60% of households at least once over the course of two years) and the rest of natural disasters (flood, frost, fire, plague, earthquake and hurricane). The prevalence for this last group of shocks is around 25% of households reporting having experienced a natural disaster at least once in two years. For the covariate shocks I use the percentage of households within the locality reporting the adverse event as a measure of the severity of the shock. I also include a variable indicating whether the household suffered loss of land, harvest or animal due to a shock. The information on shocks is not collected at baseline so I use the earliest round for which the information is available, October 1998 (wave 2). For the case of the natural shocks, in the questionnaire households are asked to report the shocks experienced in the previous 6 months leading up to the survey. Thus, this period refers to the time period when only the early treatment households were receiving the program.

The results in Table 4.3 indicate that the program did not overall have differential mobility impacts on the households who had suffered shocks around the time when the program started. Indeed of the 4 shocks used only household head unemployment yields a negative and significant coefficient, suggesting the CCT's mid-term mobility enhancing potential on early beneficiaries is subdued if the head of the household has suffered an unemployment spell at the onset of the program. None of the coefficients for the other interaction term shocks are statistically significant. Nevertheless the absence of significant effects must be regarded with caution since the estimates have very low precision.

Finally, the heterogeneity analysis using the trajectories approach is presented in Table 5.1, 5.2 and 5.3 for the physical capital, human capital, and shocks variables correspondingly. Each table includes the trajectories estimates for the short term (Panel A), mid-term (Panel B), long term before phase-in of the control group (Panel C) and after the phase-in (Panel D). The results for physical capital suggest there are no detectable differential impacts by treatment status based on households initial asset holdings in the short or the mid-term (Table 5.1, Panels A and B). Indeed, the only characteristic relating to physical capital for which the trajectories estimates yield significant coefficients (for both the variable and the interaction term) is the households' productive activity. The estimates indicate that controlling for a number correlates, non-agricultural households

are on average more likely to exhibit an upward trajectory, which follows intuition since agricultural activities are more risk-prone. However, this association is lower for the program beneficiaries, possibly suggesting the cash transfers compensate to some degree for that economic disadvantage faced by agricultural households with lower income diversification capacity. The overall lack of significant differential impacts detected in the trajectories spanning from the onset of the program to the mid and long term (i.e. Waves 1,4,7 in Panel B and Waves 1,4,8 in Panel C) may be due to the fact that these trajectories comprise both the experimental and the post experimental period. Thus, any initial mobility advantage for the early treatment group might be subdued once the control group is phased-in.

Sure enough there are more detectable differential impacts for the long-term trajectory that zeroes in on the post experimental phase (Waves 6,7,8 in Panel D). For example, the coefficient for the distance variable interacted with treatment indicates that the early recipients of the transfers are less likely to experience downward mobility and that this beneficial effect is augmented for the more remotely located households. Again, this suggests the program compensates an initial inequality, in this case in terms of market proximity. This result is consistent with the estimates using the ranks approach which suggested a positive differential impact of the program on the mobility of less connected households. Note however, that the effect of distance is symmetric in the case of the upward mobility trajectory (Panel D, column 4). The estimates indicate that early treatment households are more likely to move upward in the long term post phase-in period, but to a greater extent the better connected households (as suggested by the negative significant interaction coefficient). Thus, in the case of upward mobility the program rather reinforces the existing location disadvantage.

In the case of land, homeownership and productive activity the signs of the coefficients for the variables alone are somewhat counterintuitive, i.e. having more land and being a home owner at baseline is associated with a higher probability of sustained poverty in the long-run. A possible explanation is the higher dependence on agriculture of larger landowning households in this rural, high poverty setting. Similarly, the productive activity coefficient suggests non-agricultural households have a lower likelihood of being upwardly mobile. This result may reflect the fact that the non-agricultural households are already more concentrated at the top of the distribution and thus less likely to present further mobility upward. In any event, the significant interaction terms indicate that the early receipt of the transfers largely mitigates the association between these physical capital characteristics and households' mobility patterns in the long term.

The heterogeneity analysis along the human capital variables for the trajectories outcomes is presented in Table 5.2. In this case the estimates do confirm the compensating effect of the CCT for the less educated households as suggested by the ranks approach. As for the household demographic composition, the results between the two approaches are partially consistent in the case of the 8 to 12 year old age group, i.e., the early treatment households containing members in this critical transition period (for which transfer amounts increase progressively) present higher absolute mobility (as measured in the ranks approach) and exhibit a higher likelihood of escalating in the welfare distribution (as measured in the trajectories approach). That said, the results for the two persistence trajectories ({111} and {333}) suggest a contrasting effect; namely that the

probability of remaining in poverty (sustaining high welfare) is greater (lower) for the early recipient households with members in this crucial stage of their human capital formation. Finally the estimates at the bottom suggest the program's beneficial impact on the persistence trajectories is augmented for the households with a higher proportion of children enrolled (for ages 5-7 and 13-17) while the mobility trajectories (for the same two age groups) do not support this finding.

The heterogeneity analysis by shocks to the household is reported in Table 5.3. Given that the information about shocks is self-reported a potential concern is whether there is endogenous reporting according to the treatment group. The direction of the potential bias is not a priori clear. The early treatment group may be more inclined to over-report adverse events if they think it will affect the amount of the transfers they receive. Conversely, the control group could have an incentive to over-report shocks if they are under the impression that it will accelerate the onset of the program in their locality. In any event, to account for this possibility of endogenous reporting I construct measures of the severity of the climatic shocks based on the reports by the ineligible households. Therefore, this alternative severity measure for each locality is defined as the percentage of households from the ineligible population in the locality reporting the shock. Panel A presents the estimates for the short term trajectories. Overall the estimates yield the expected significant coefficients for the sustained welfare trajectories. As intuited, the households that experience higher shock incidence between November 1998 and 1999 are more likely to remain trapped in poverty throughout this period and less likely to present a trajectory of sustained high welfare. The coefficients for the interaction terms indicate that the effect of these natural disasters on households' likelihood of remaining in poverty is mitigated by the CCT. Indeed, for aggregate consumption, the negative impact of natural disasters is fully mitigated by the program. This result remains robust when using the disaggregated components of consumption, food and non-food (not displayed in Table 5.3 for the sake of brevity).

The mitigating effect for the early recipients of the program extends beyond the short and midterm. Panel C shows the results for the long-term including the period before the control group is phased-in (Waves 1,4,8). The estimates indicate that, even ten years after the program's inception, the program still fully mitigates the effect of natural disasters for early recipients of the CCT. However, this interaction term is no longer significant in the post-experimental phase. The estimates in Panel D show that, once both groups have received the program for at least a year, there are no longer any significant differences in its capacity to mitigate the effect shock. In other words, it takes a relatively short amount of time for the program to exert its shock mitigating capacity. In the short time elapsed between phase-in the program seems to shield all households against poverty persistence to the same extent.

In contrast to natural disasters, drought is not significantly associated with sustained poverty in any of the periods. This lack of an effect on poverty is consistent with what de Janvry et al find. A possible explanation they posit is that droughts are sufficiently frequent in Mexico that households have designed ex-ante risk-coping strategies to account for these occurrences. The authors mention a similar result is found by Reardon et

al. (1988) for the risky Sahelian zone of Burkina Faso. Finally, the idiosyncratic unemployment shock is not associated in any of the periods to any of the mobility trajectories.

To recap, the heterogeneity analysis indicates that early receipt of the program impacts households to differing degrees according to their characteristics at baseline and the shocks they endure. Overall, few of the variables relating to physical capital augment the program's effect on mobility. Distance to an urban center is the only variable that shows significant interaction effects both for the ranks and the trajectories estimates. In synthesis, the results suggest that early access to the program has a compensating effect in some cases (for the less connected and less educated households) and a mitigating effect against adversity (for natural disaster shocks). While there are other cases in which existing inequalities are reinforced (for households with children at critical transition ages). Interestingly, longer treatment favors the mobility of households with a larger proportion of children enrolled; a result which on one hand may accentuate an existing disparity (by benefiting the less constrained households), yet on the other hand, rewards households that attribute a high value to the investment in the next generation's human capital.

VII. Robustness checks : Further examination of Attrition in estimates and Compliance

Further Attrition checks

Given the long period elapsed between the base-line and the final follow-up survey in 2007, attrition is a potential concern in this study. Despite the fact that the initial examination of sample loss showed no evidence non-random attrition (Table 1.3), it is worth checking whether the impact results reported in the previous section are driven by the selected sample on which they are estimated. This is particularly important given the large jump in sample loss occurring between the mid [Wave 7] and the long run [Wave 8] (from 14 to 48 percent at the aggregate level, Table 1.2). To this end I re-estimate the ranks results (for the short and mid-run) including a term to indicate whether the household (or its consumption data) is missing in the long-term plus the interaction term with the intent- to-treat variable[23]. This interaction term is the coefficient of interest in order to understand whether the attrition which may be driving the estimated impacts does so to a larger extent for the early treatment household and as such affects the interpretation of the results. The estimates are presented in Table A2. The results indicate that while the attrition in the long term does correlate significantly with the mobility measures in the mid-term (Wave, columns 7 and 8), there is not a differential attrition effect for the treatment group. Thus, there is no evidence that the impacts detected above are driven simply by sample selection. Nevertheless, the above results warrant caution given the high level of attrition (even if random) in the long term. In particular it is important to be cognizant about which type of households the long term

[23] For clarity, I run the following specification for each wave prior to wave 8 (ie. Wave 2, 4 , 6 and 7):
$rank\ mobility_i = \alpha + \beta\ itt_i + \delta\ attrition\ wave\ 8 + \lambda\ itt * att\ w8 + baseline\ controls_i + \varepsilon$

results hold for. That is, one must bear in mind the type of households that remain in the sample, as determined from the initial examination of attrition along baseline characteristics (see Section II.2 above). In the present case the remaining sample constitutes overall less privileged households. In particular since the households that are less likely to leave the sample are those with less educated and younger heads of indigenous descent, as well as those without access to electricity (review Table 1.3).

Compliance and the Densification process

The validity of the intent-to-treat estimate using the evaluation design of Progresa hinges on the randomization of households into treatment status. Thus non-compliance among the eligible households may attenuate the detectable impacts. Moreover, in my estimates, given that the mobility outcomes are constructed using the entire population of eligible and ineligible households, non-compliance or changes in the classification of the ineligibles may also attenuate the results.

Some of these factors may be at play as a result of certain administrative issues surrounding the early implementation of the program. As mentioned above, and documented by Buddelmeyer and Skoufias (2003), during the early stages of the program (i.e. during 1998) the PROGRESA beneficiary selection method led to approximately 52% of the households in the evaluation sample to be classified as eligible for the program benefits. By July 1999 PROGRESA underwent the *densifcation* process and had added new households to the list of beneficiaries since it was felt that the original selection method was biased against the elderly poor who no longer lived with their children. The revised selection procedure did not simply increase the region-specific thresholds but rather it adjusted the way household-specific discriminant scores were calculated. As a result of the revised selection process the fraction of households classified as eligible for program benefits increased from 52% of the evaluation sample to 78% of the sample. However, after the release of the payment records in late August 2000, it was discovered that in the evaluation sample, many of the households (27% of the eligible households in treatment localities) that were supposed to be added to the updated list of beneficiaries had not received any cash benefits since the start of the distribution of program benefits in these localities. According to Buddelmeyer and Skoufias (2003) it was confirmed that this was due to an administrative error and thus these households were never incorporated into the program.

Moreover, substantial delays in the implementation of the program were reported for the early treatment communities. Finally, at the beginning of 2001 a new survey to determine eligibility (a new ENCASEH survey) was launched to update the households' proxy mean scores. As a result many new entrants were admitted into the program in both the treatment and control communities. Indeed, Table A3., based on transfer information from administrative records confirms a substantial number of households (close to 18 percent) from the early treatment communities received their first transfers with delays of up to 9 months after the beginning of the program. This lag in transfer receipt may attenuate the impacts detected at the early stages of evaluation. In particular it may explain to some extent why the short term ranks results are considerably lower than the

midterm impacts. Table A3 also confirms the late entrance of a meaningful proportion of households around the early months of 2001 when the survey to determine eligibility (the ENCASEH survey) was revised.

The implementation issues mentioned above constitute a source of non-compliance with respect to the randomized treatment classification. Given the fact that these subsequent changes fell outside the randomization procedure, it has been standard practice to adhere to the original treatment status classification (albeit excluding the *densified* households altogether; see for example Angelucci and De Giorgi, 2008; and Gertler et al., 2012). In this study I adhere to the original classification for both the eligible and non-eligibles (ie. I do not exclude the *densificados* in the sense that these household that were originally classifies as ineligible are included in the consumption distribution upon which I construct the mobility measures.) However, the transfer data confirms that the proportion of households that deviated from the original treatment status is non-negligible. In this particular study, the detectable impacts may be considerably attenuated given the fact that the mobility outcomes are constructed using both the eligible and the ineligible population. The delays for the early treatment households compounded with the subsequent inclusion of some of the originally non-poor households work in the same direction against detecting a mobility advantage for the early beneficiaries. Hence, the important take-away message from examining the transfer data, which signals the extent of non-compliance, is that the detected intent-to-treat impacts using the original randomization classification must be regarded as lower bound estimates.

VIII. Conclusions

The recent optimism about the increase in economic mobility in Latin America. Several cross-country studies for the region highlight the role played by social spending, in particular conditional cash transfer programs (CCTs). However, few studies have identified the actual impact of these programs on households' mobility. Indeed, despite ample evidence about the positive short-term impacts of CCT programs, little is known about their long-term effects. This paper exploits the randomized evaluation design of Mexico's renowned CCT program Oportunidades to measure the long-run impact of the program on intragenerational socioeconomic mobility. In particular, I use two different approaches to examine the effect of differential exposure to the program. The first approach, consisting of welfare ranks, uses the baseline and endline of the program's 10-year panel dataset to measure socioeconomic mobility in terms of the change in households' consumption rank. The second approach, exploits the information from the panel's middle survey rounds to construct 3-period consumption trajectories. A trajectory is defined as the sequence of a households' position along the welfare distribution. More specifically I evaluate the impact of differential exposure to the program on the likelihood that a household presents a path of sustained poverty (wealth), sustained downward (upward) mobility, or temporary downward (upward) mobility.

The results using the ranks approach suggest that differential exposure to the CCT increased upward mobility; however this effect was not sustained in the long-term. In contrast, the trajectories estimates indicate that the

beneficial welfare impact on the early recipients does persist into the long term. In particular, the households that randomly received the transfers first displayed on average a higher likelihood of sustaining high welfare levels and a lower probability of remaining stuck in poverty. Thus, the persistence effects stand the test of time while the impacts on upward and downward mobility (sustained as well as temporary) decay. This contrast in the long-term results (between the ranks and trajectories estimates) is interesting in its own right since it highlights the fact that utilizing the mid-round surveys of panel datasets provides additional useful information.

Moreover, the heterogeneity analysis suggests the program has a compensating effect in some cases (for the less connected and less educated households) and a mitigating effect against adversity (for natural disaster shocks). However, there are other cases in which existing inequalities are reinforced (for households with children at critical transition ages). Interestingly, longer treatment favors the mobility of households with a larger proportion of children enrolled; a result which on one hand may accentuate an existing disparity (by benefiting the less constrained households), yet on the other hand, rewards households that attribute a high value to the investment in the next generation's human capital.

The results using the ranks approach suggest that differential exposure to the CCT increased upward mobility; however this effect was not sustained in the long-term. In contrast, the trajectories estimates indicate that the impact on the likelihood of presenting sustained upward mobility patterns persists into the long term. This effect outlasts the entrance of the control group into the program for over a year. Thus, one of the paper's contribution hinges on the comparison and contrast of the two methodological approaches to empirical mobility analysis. Taken together the results provide evidence of the additional insight delivered by the ranks-based estimation and underscore the importance of utilizing, when available, the mid-term rounds of long panel surveys.

Finally, I conclude from the results that there is noteworthy distinction between persistence and movement patterns. This finding dovetails with previous research finding stronger impacts of shocks on poverty persistence than on downward mobility (Premand and Vakis, 2010). The results in the present study signal that the timing of the transfers may be especially meaningful for households stuck at the bottom of the distribution. More importantly from a policy perspective, the fact that the causes of poverty persistence differ from those for downward mobility is a key finding since it suggests they each may require distinct sets of policy options.

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Table 1.1. Oportunidades monthly cash transfer schedule (nominal pesos)

	January-June 1998	July-December 1998	January-June 1999	July-December 1999
Educational grant per child 1				
Primary				
3rd grade	65	70	75	80
4th grade	75	80	90	95
5th grade	95	100	115	125
6th grade	130	135	150	165
Secondary				
1st-male	190	200	220	240
2nd-male	200	210	235	250
3rd male	210	220	245	265
1st-female	200	210	235	250
2nd-female	220	235	260	280
3rd-female	240	255	285	305
Grant for school materials per child				
Primary-September	–	In-kind	–	110
Primary-January	40	–	45	–
Secondary-September	–	170	–	205
Grant for consumption of food per household 2				
Cash transfer	95	100	115	125
Maximum grant per household	585	625	695	750

Source: Skoufias and Parker (2001)

1/ Conditioned on child school enrollment and regular attendance

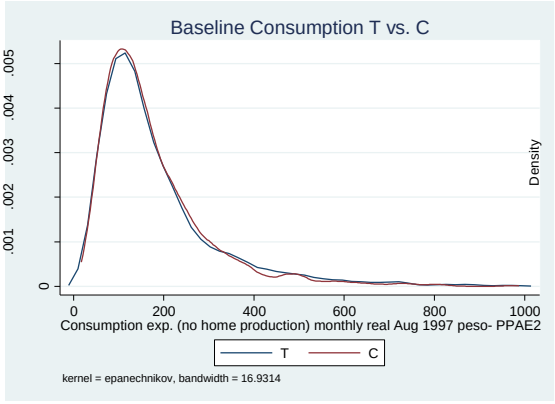
2/ Conditioned on attending scheduled visits to health centers

Table 1.2. Sample of Households used in the analysis: Attrition rates, Treatment and Eligibility status

		Eligible					Ineligible			TOTAL	
		Proportion of Eligible households by treatment status		No. of households	Attrition by treatment status (cumulative)		Proportion of total households	No. of households	Attrition (cumulative)	No. of households	Attrition (cumulative)
Survey rounds	Time Period	Treatment	Control		Treatment	Control					
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Waves 1 & 2	Mar 98 - Nov 1998	0.63	0.37	**10,676**	0%	0%	0.48	**9,994**	0%	**20,670**	0%
Waves 1 & 4	Mar 98 - Nov 1999	0.62	0.38	**9,783**	10%	6%	0.48	**8,947**	10%	**18,730**	9%
Waves 1 & 6	Mar 98 - Nov 2000	0.63	0.37	**9,692**	10%	9%	0.47	**8,443**	16%	**18,135**	12%
Waves 1 & 7	Mar 98 - Nov 2003	0.62	0.38	**9,456**	12%	10%	0.47	**8,319**	17%	**17,775**	14%
Waves 1 & 8	Mar 98 - Aug 2008	0.61	0.39	**5,960**	46%	42%	0.45	**4,785**	52%	**10,745**	48%
Balanced panel (1,2,4,6,7,8)		0.61	0.39	**5,022**	55%	50%	0.42	**3,700**	63%	**8,722**	58%

Note: The number of households (indicated in columns 3, 7 and 9) corresponds to households, present at baseline (Wave 1) and at each follow-up, for which consumption data is available. The main estimates in the Results section of the paper (Section VI) use the sample of eligible households at each period (column 3) to measure the impact on mobility of differential exposure to the program (ie. treatment vs. control households). However, the mobility outcomes (measured at the household level) are constructed using the entire consumption distribution (including all the households originally classified as ineligible (column 7) in addition to the original treatment and control households (column 3).

Figure 1. Consumption distributions by Treatment and Eligibility status



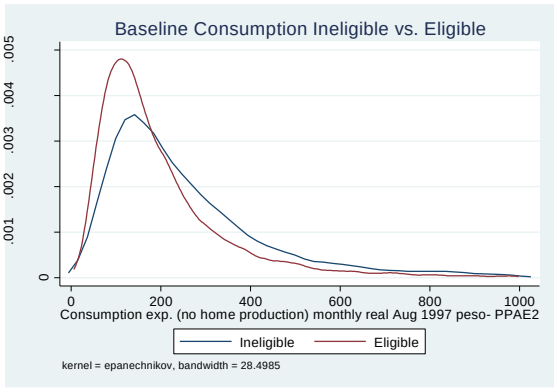


Table 1.3. Differential Attrition by Treatment Status and Baseline Characteristics

Dependent variable: Attrition	Wave 2		Wave 4		Wave 6		Wave 7		Wave 8	
(dummy=1 if hh consumption data is missing)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Intent-to-early-treatment (itt)	0.21		0.06		0.11		0.02		0.15	
	(0.15)		(0.08)		(0.14)		(0.09)		(0.17)	
X = Characteristic at baseline	**X**	**X * itt**	**X**	**X * itt**	**X**	**X * itt**	**X**	**X * itt**	**X**	**X * itt**
Head/spouse										
age of household head (hhh)	0.00	-0.00	0.00	-0.00	0.00	-0.00	0.00	-0.00	0.00*	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
female hhh	0.03**	0.01	0.02	0.03	0.02	0.02	0.00	0.02	-0.01	0.05
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.03)	(0.04)
low education hhh (1)	0.02	0.01	0.02	0.01	0.02	0.00	0.04**	-0.02	0.05*	-0.05
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)	(0.04)
ethnicity of hhh (2)	0.03	-0.04	0.02	-0.01	0.02	-0.03	0.01	-0.02	-0.10*	0.14*
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.06)	(0.07)
age of spouse	-0.00	0.00	-0.00	0.00	-0.00*	0.00	-0.00***	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
low education spouse (1)	0.04**	-0.01	0.03*	-0.01	0.02	-0.01	0.04***	-0.02	0.03	0.00
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.03)	(0.03)
Household										
household size	-0.00	-0.00	-0.00	0.00	0.00	-0.00	0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
age 0-7 (3)	-0.01	-0.01	-0.00	-0.01	-0.01	-0.01	-0.01	-0.01	-0.02	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)
age 8-17 (3)	-0.02	-0.00	-0.03**	0.01	-0.03**	0.01	-0.02	0.01	-0.03	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)
age 18-54 (3)	-0.07*	0.00	-0.05	-0.01	-0.05*	-0.02	-0.07**	0.04	-0.05	-0.04
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.05)	(0.06)
homeowner (4)	-0.01	-0.00	0.00	-0.01	-0.00	-0.01	-0.01	-0.01	-0.02	-0.05
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.05)	(0.06)
dirtfloor (5)	0.01	0.02	0.01	0.01	0.01	0.02	0.00	0.03*	0.02	-0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.05)
electricity (6)	-0.04*	0.07**	-0.05*	0.07**	-0.04*	0.06**	-0.03	0.05**	-0.13**	0.12*
	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)	(0.06)
non-agricultural hh	0.04	-0.02	0.02	-0.01	0.03	-0.02	0.03	-0.00	0.02	-0.00
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
large farm (> 3 ha of land)	-0.02	0.01	-0.03	0.02	-0.02	0.01	-0.02	0.00	-0.02	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.05)
draft animals (7)	0.00	0.00	0.00	-0.00	0.00	-0.00	0.00	-0.00	-0.03**	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
productive animals (7)	-0.00	0.00	-0.00	0.00	-0.00	0.00	-0.00	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
land (total owned in hectares)	0.00	-0.01	0.00	-0.01	0.00	-0.00	0.00	-0.00	-0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Community										
community organization (8)	-0.01	-0.08	-0.03	-0.05	-0.02	-0.06	-0.04	-0.03	0.08	-0.11
	(0.04)	(0.07)	(0.04)	(0.06)	(0.04)	(0.06)	(0.03)	(0.06)	(0.07)	(0.09)
distance to a large urban center	0.00*	-0.00	0.00**	-0.00	0.00*	-0.00	0.00*	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
male community wage (log)	0.00	-0.02	0.00	0.00	-0.00	-0.00	-0.01*	-0.00	-0.01	0.01
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
Obs.	11,555		10,462		10,361		10,218		10,218	
R-squared	0.04		0.03		0.03		0.04		0.04	

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

Estimates correspond to a single regression per wave: $\text{attrition}_i = \alpha + \beta\, \text{itt}_i + \delta \mathbf{X}_i + \lambda\, \text{itt}_i * \mathbf{X}_i + \varepsilon$

(1) dummy =1 if education < = incomplete primary

(2) ethnicity = 1 if hhh speaks indigenous language

(3) dummy = 1 if hh has a member in this age group

(4) homeowner = 1 if the home is owned by the one of its members

(5) dirtfloor = 1 if hh floor material is dirt

Table 2. Quintile Mobility Matrices

Mar 98 vs Oct 98
Wave 2

CONTROL

		1	2	3	4	5	Total
Baseline	1	44	23	16	10	6	100
	2	37	28	19	11	5	100
	3	27	27	22	16	9	100
	4	19	22	23	19	16	100
	5	15	20	21	20	24	100

Obs. 4,349

TREATMENT

		1	2	3	4	5	Total
Baseline	1	38	25	18	12	7	100
	2	28	28	23	15	6	100
	3	19	25	25	20	10	100
	4	12	21	25	24	18	100
	5	11	17	21	26	25	100

Obs. 7,252

Total obs. 11,601

Chi2 Test (p-values)

All cells	0.28	
Diagonal	0.66	

Mar 98 vs May 99
Wave 3

CONTROL

	1	2	3	4	5	Total
1	49	21	15	8	7	100
2	41	26	17	11	6	100
3	31	28	20	15	7	100
4	21	23	24	19	13	100
5	19	21	16	21	23	100

Obs. 3,923

TREATMENT

	1	2	3	4	5	Total
1	31	26	20	14	9	100
2	24	26	24	17	9	100
3	18	23	27	21	13	100
4	12	21	23	25	18	100
5	13	15	21	25	25	100

Obs. 6,690

Total obs. 10,613

Chi2 Test (p-values)

All cells	0.00	**
Diagonal	0.01	**

Mar 98 vs Nov 99
Wave 4

CONTROL

	1	2	3	4	5	Total
1	48	21	15	11	5	100
2	40	26	17	12	5	100
3	35	24	20	15	7	100
4	21	25	21	20	14	100
5	20	20	23	20	17	100

Obs. 4,012

TREATMENT

	1	2	3	4	5	Total
1	32	27	21	12	8	100
2	24	27	24	17	8	100
3	17	26	25	19	13	100
4	12	21	21	28	18	100
5	12	18	21	24	25	100

Obs. 6,492

Total obs. 10,504

Chi2 Test (p-values)

All cells	0.00	**
Diagonal	0.01	**

Mar 98 vs Nov 00
Wave 6

CONTROL

	1	2	3	4	5	Total
1	40	24	18	11	6	100
2	32	27	20	14	7	100
3	26	24	21	19	10	100
4	18	22	22	22	16	100
5	17	16	25	23	19	100

Obs. 3,932

TREATMENT

	1	2	3	4	5	Total
1	34	24	20	15	6	100
2	27	25	20	17	10	100
3	22	23	23	20	13	100
4	15	22	23	23	18	100
5	15	21	21	22	22	100

Obs. 6,472

Total obs. 10,404

Chi2 Test (p-values)

All cells	0.88	
Diagonal	0.79	

Mar 98 vs Nov 03
Wave 7

CONTROL

	1	2	3	4	5	Total
1	33	27	19	14	7	100
2	27	26	21	16	11	100
3	22	24	24	20	11	100
4	19	21	23	23	13	100
5	19	18	21	26	15	100

Obs. 0

TREATMENT

	1	2	3	4	5	Total
1	33	24	19	14	10	100
2	24	25	23	17	11	100
3	19	25	21	21	14	100
4	16	19	23	23	18	100
5	17	21	20	20	22	100

Obs. 0

Total obs. 0

Chi2 Test (p-values)

All cells	0.87	
Diagonal	0.66	

Mar 98 vs Aug 07
Wave 8

CONTROL

	1	2	3	4	5	Total
1	23	40	14	10	14	100
2	24	35	14	15	12	100
3	24	29	16	16	15	100
4	20	25	18	16	21	100
5	27	18	16	16	22	100

Obs. 0

TREATMENT

	1	2	3	4	5	Total
1	20	40	14	11	15	100
2	23	34	18	12	14	100
3	25	27	15	15	18	100
4	29	21	15	14	22	100
5	27	21	14	15	22	100

Obs. 0

Total obs. 0

Chi2 Test (p-values)

All cells	0.92
Diagonal	0.92

Note: In all matrices the rows indicate the quintile of consumption a household belonged to at baseline and the columns indicate the corresponding quintile at each subsequent wave. The entries in each matrix are row shares, i.e. they indicate what share of households belonging to a given quintile at baseline end up in a given quintile in the subsequent year. Thus, each row sums to 100 percent.

The consumption quintiles are constructed using the entire sample, including the original non-eligible population (which constitute 48 percent of the sample). The resulting mobility matrices are then built using only the eligible households (T vs. C separately).

Wave 5 (i.e. May 2000) is not included because the consumption data is not available for this round.

Table 2.1. Impacts of differential exposure to the CCT on Economic mobility in the short-term

	Panel A: Wave 2 (Oct. 1998)			Panel B: Wave 4 (Nov. 1999)			Panel B: Wave 6 (Nov. 2000)		
	Level Estimate	Normalized Rank estimates		Level Estimate	Normalized Rank estimates		Level Estimate	Normalized Rank estimates	
Dep. Var: ***Consumption***		*Absolute Mobility (AM)*	*Relative Mobility (RM)*		*Absolute Mobility (AM)*	*Relative Mobility (RM)*		*Absolute Mobility (AM)*	*Relative Mobility (RM)*
		Rank of change	**Change in rank**		**Rank of change**	**Change in rank**		**Rank of change**	**Change in rank**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Intent-to-early-treatment	11.41***	3.73***	3.95***	21.16***	7.04***	8.09***	7.73**	1.96 +	2.42 +
	(3.71)	(1.28)	(1.34)	(3.25)	(1.26)	(1.41)	(3.43)	(1.30)	(1.52)
Number of Observations	11,532	10,632	10,632	10,755	9,773	9,773	10,352	9,650	9,650
Mean (late treatment hhs)	142.19	47.62	-3.92	128.64	46.32	-5.73	133.7	49.41	-0.87
Mean (non-poor hhs)	216.39	49.88	0.57	195.89	49.24	0.69	190.52	48.85	1.23
St.Dev(control hhs)	95.99	28.88	31.35	79.75	28.42	32.43	81.89	28.16	32.87
Min(late treatment hhs)	3.07	0	-98.05	5.92	0.02	-97.91	5	0	-96
Max(late treatment hhs)	1,071.67	99.98	94.62	860.69	99.95	91.97	943.98	99.99	93.34
(i) Impact (% late treatment mean)	8%	8%		16%	15%		6%	4%	
(ii) Impact (% non-poor mean)	5%	7%		11%	14%		4%	4%	
(i)-(ii)	3%	0.4%		6%	1%		2%	0%	

Table 2.2. Impacts of differential exposure to the CCT on Economic mobility in the mid and long-term

	Panel C: Wave 7 (Nov. 2003)			Panel D: Wave 8 (Aug. 2007)		
	Level Estimate	Normalized Rank estimates (Eligibles and Noneligibles)		Level Estimate	Normalized Rank estimates (Eligibles and Noneligibles)	
Dep. Var: ***Consumption***		*Absolute Mobility (AM)*	*Relative Mobility (RM)*		*Absolute Mobility (AM)*	*Relative Mobility (RM)*
		Rank of change	**Change in rank**		**Rank of change**	**Change in rank**
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-early-treatment	13.84***	2.08*	2.44*	6.45	0.16	-0.02
	(5.14)	(1.18)	(1.44)	(11.71)	(1.34)	(1.59)
Number of Observations	10,415	9,441	9,441	6,520	5,956	5,956
Mean (late treatment hhs)	183.1	48.88	0.05	770.85	50.74	-0.8
Mean (non-poor hhs)	259.71	49.7	1.54	882.81	48.73	4.54
Min(late treatment hhs)	0.48	0.01	-96.91	153.57	0.01	-97.89
Max(late treatment hhs)	2,730.55	99.98	96.27	4,611.45	99.94	98.61
Impact (% late treatment mean) (i)	8%	4%		1%	0%	
Impact (% non-poor mean) (ii)	5%	4%		1%	0%	
(i)-(ii)	2%	0%		0%	0%	

Notes (for Tables 2.1. and 2.2): Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1, + p<0.2.

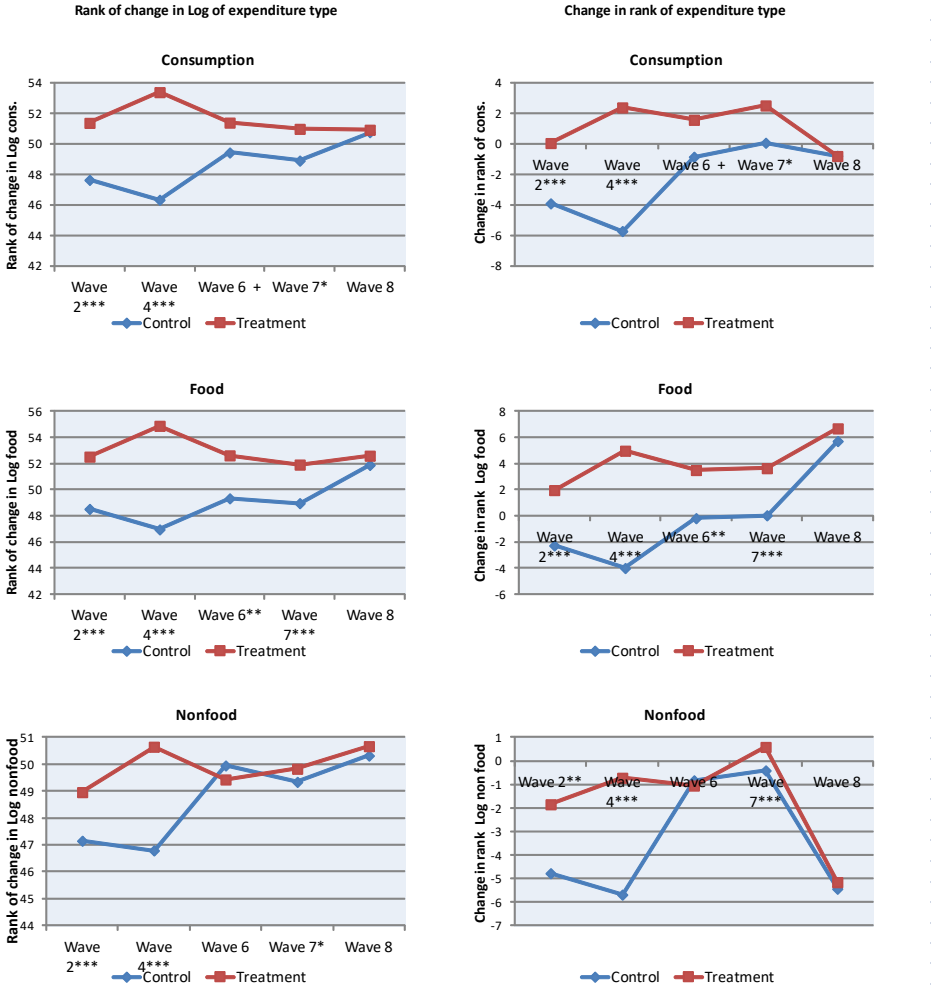
Absolute mobility (*AM*) is defined as the rank of the change in log consumption that a household exhibits between two periods (the baseline year and a subsequent wave). The ranking, which includes all the households in the sample (including the ineligible), is normalized so it ranges from 0 to 100.

Relative mobility (*RM*) is defined as the change in the rank of consumption for a household between time t and baseline. To build this measure the entire consumption distribution is ranked and normalized (to range from 0 to 100) at baseline and follow-up. Thus the relative mobility measure is simply the difference between the normalized ranks at the two periods.

All regressions include the following controls: head's and spouse's age, age squared and baseline education dummies*, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draf t and production animals, ha of land, farmsize, homeowership, dirt floor and electricity) and baseline community characteristics (community organizations, distance to urban center and wages).

Per capita expenditures in food and nonfood items are expressed in adult equivalent units and October 1997 pesos. Homeproduction, imputed using community level prices, is included.

Figure 2. Summary of the Ranks estimates
Economic mobility by treatment status at each wave



Note: The asterisks next to each wave number indicate the significance level of the difference between T and C: + p< 0.2, *p<0.1, ** p<0.05, *** p<0.01. Consumption data is not available for Wave 5 (Nov. 2000)

Table 3. Impacts of differential exposure to the CCT on households' Welfare Trajectories

	Sustained poverty {ijk = 111}			Sustained welfare {ijk = 333}			Downward mobility { i ≥ j > k} or { i > j ≥ k }			Upward mobility { i ≤ j < k} or { i < j ≤ k }			Temporary Upward { i < j > k}			Temporary Downward { i > j < k}		
	(1) consump.	(2) food	(3) nonfood	(4) consump.	(5) food	(6) nonfood	(7) consump.	(8) food	(9) nonfood	(10) consump.	(11) food	(12) nonfood	(13) consump.	(14) food	(15) nonfood	(16) consump.	(17) food	(18) nonfood
Panel A: SHORT TERM (Waves 1, 2, 4; Experimental variation period)																		
Intent-to-early-treatment	-0.06***	-0.05***	-0.03**	0.01	0.02**	0.01	-0.08***	-0.09***	-0.05***	0.08***	0.09***	0.04**	0.00	-0.00	0.01	0.03***	0.02*	0.02*
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293
R-squared	0.11	0.09	0.07	0.07	0.07	0.06	0.03	0.03	0.02	0.02	0.03	0.02	0.01	0.01	0.01	0.01	0.01	0.01
Panel B: MID TERM (Waves 1, 4, 7: Phase-in period by wave 4)																		
Intent-to-early-treatment	-0.04***	-0.04***	-0.02	0.02**	0.02**	0.01	-0.01	-0.02	-0.02	0.02	0.03*	-0.00	0.05***	0.05***	0.04***	-0.05***	-0.05***	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293
R-squared	0.09	0.06	0.08	0.06	0.06	0.07	0.05	0.04	0.03	0.05	0.04	0.03	0.01	0.01	0.01	0.01	0.01	0.01
Panel C: LONG TERM (Waves 1, 4, 8: Phase-in period by wave 4)																		
Intent-to-early-treatment	-0.04***	-0.02	-0.02*	0.01	0.02***	0.00	0.00	-0.03*	0.01	0.02	0.00	0.03*	0.06***	0.06***	0.02	-0.04***	-0.04***	-0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293	5,293
R-squared	0.09	0.06	0.07	0.06	0.05	0.04	0.04	0.04	0.07	0.03	0.03	0.07	0.03	0.02	0.04	0.03	0.01	0.05
Panel D: LONG TERM (Waves 6, 7, 8: Post phase-in period) both groups have benefited from transfers from the onset for up to a year																		
Intent-to-early-treatment	-0.02	-0.03**	-0.01	0.02**	0.02***	0.00	0.01	0.01	0.01	-0.01	-0.03	0.02	0.01	0.01	0.00	0.00	0.00	-0.02
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Observations	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.10	0.05	0.07	0.06	0.04	0.05	0.04	0.03	0.07	0.04	0.02	0.09	0.05	0.01	0.05	0.07	0.02	0.06

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

All regressions are OLS estimates including the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draf t and production animals, hectares of land, farmsize, homeowership, dirt floor and electricity) and baseline community characteristics (community organizations, distance to urban center and wages).

Each regression estimates the Intent-to-early-treatment impact on an outcome dummy variable indicating whether the household presents a specific welfare trajectory (e.g. In column (1) the mobility outcome is a dummy equal to 1 if the household remained in the lowest consumption tercile over the three wave period. In column (8) the mobility outcome is a dummy equal to 1 if the household remained in the highest food tercile over the three wave period. In column (12) the mobility outcome is a dummy equal to 1 if the household exhibited a pattern of upward mobility along the non-food distribution as explained in section VI, which describes the estimation strategy in detail.)

Table 4.1. Heterogeneity analysis for baseline characterteristics relating to Physical Capital

Ranks Estimates

	Panel A: Wave 2 (Oct. 1998)		Panel B: Wave 4 (Nov. 1999)		Panel C: Wave 7 (Nov. 2003)		Panel D: Wave 8 (Aug. 2007)	
Dependent variable in all regressions: Consumption mobility; Absolute Mobility (AM) or Relative Mobility (RM)								
Heterogeneity by: **DISTANCE TO AN URBAN CENTER**								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt* distance (1)	0.02	-0.01	0.08**	0.06*	0.02	0.02	0.03	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
Obs.	10,632	10,632	9,973	9,973	9,641	9,641	6,056	6,056
Heterogeneity by: **LAND OWNERSHIP**								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt* land (2)	-1.08	-1.51	-2.09	-1.19	1.62	1.63	-2.24	-0.45
	(1.83)	(1.98)	(1.96)	(2.25)	(2.62)	(3.22)	(2.27)	(2.74)
Obs.	10,635	10,635	9,976	9,976	9,641	9,641	6,056	6,056
Heterogeneity by: **HOME OWNERSHIP**								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt* homeowner (3)	1.36	0.26	0.85	0.08	0.13	1.31	2.54	2.88
	(2.46)	(2.68)	(2.58)	(3.05)	(1.99)	(2.44)	(2.92)	(3.50)
Obs.	10,632	10,632	9,973	9,973	9,643	9,643	6,037	6,037
Heterogeneity by: **ANIMAL VALUE**								
itt* animal value (4)	-0.36	-0.43	-1.2	-2.07	0.3	0.48	0.61	0.01
	(1.61)	(1.71)	(1.60)	(1.82)	(1.51)	(1.83)	(1.78)	(0.05)
Obs.	10,632	10,632	9,973	9,973	9,643	9,643	6,037	6,037
Heterogeneity by: **PRODUCTIVE ACTIVITY**								
itt*non-agriculture (5)	2.01	3.39	-1.66	-0.67	-1.71	-2.98	0.53	-0.64
	(2.08)	(2.26)	(2.19)	(2.70)	(2.18)	(2.65)	(2.23)	(2.75)
Obs.	10,632	10,632	9,973	9,973	9,641	9,641	6,037	6,037

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

AM and *RM* stand for *Absolute Mobility* and *Relative Mobility* as defined in the estimation strategy (Section V.)

All regressions include the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language) and baseline household size.

(1) distance = 1 if the minumum distance to an urban center ≥ 71km (top 3 quintiles of the distribution)

(2) land = 1 if the household owns/rents more than 3 hectares of land (top 2 quintiles of the distribution)

(3) homeowner= 1 if the home where the households head lives is owned by the one of its members

(4) animal value = 1 if the value of the productive animals owned by the household is in the top 2 quintiles of the distribution

(5) non-agriculture = 1 if the household's main productive activity is outside of agriculture

Table 4.2. Heterogeneity analysis for baseline characterteristics relating to Human Capital

Ranks Estimates

	Panel A: Wave 2 (Oct. 1998)		Panel B: Wave 4 (Nov. 1999)		Panel C: Wave 7 (Nov. 2003)		Panel D: Wave 8 (Aug. 2007)	
Dependent variable in all regressions: Consumption mobility; Absolute Mobility (AM) or Relative Mobility (RM)								
Heterogeneity by: **EDUCATION LEVEL**								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt*education (1)	-1.88	-2.04	-1.98	-2.65*	-0.35	-0.42	-1.46	-2.36
	(1.46)	(1.53)	(1.44)	(1.59)	(1.42)	(1.72)	(1.79)	(2.33)
Obs.	10,632	10,632	9,973	9,973	9,641	9,641	6,056	6,056
Heterogeneity by: **HOUSEHOLD DEMOGRAPHIC COMPOSITION** (2)								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt * age 0-7	-2.81*	-4.07**	-4.79***	-6.82***	-4.59***	-6.38***	-4.75**	-6.01**
	(1.49)	(1.65)	(1.46)	(1.75)	(1.60)	(2.02)	(2.08)	(2.65)
itt * age 8-12	1.86	1.83	2.38*	1.57	1.21	1.64	1.12	1.39
	(1.35)	(1.43)	(1.32)	(1.51)	(1.33)	(1.58)	(1.58)	(1.90)
itt * age 13-17	0.57	0.33	-0.25	-0.5	-2.02	-2.27	0.66	0.49
	(1.30)	(1.39)	(1.41)	(1.59)	(1.40)	(1.63)	(1.62)	(2.03)
itt * age 18-54	-6.08*	-8.84**	-2.26	-3.78	-4.17	-5.4	-1.73	-1.84
	(3.53)	(3.98)	(3.38)	(3.89)	(3.62)	(4.63)	(5.66)	(6.28)
itt * age≥55	-1.37	-2.34	-1.82	-2.53	-0.12	-0.84	-0.6	0.39
	(1.44)	(1.53)	(1.49)	(1.65)	(1.41)	(1.69)	(1.69)	(2.11)
Obs.	10,632	10,632	9,973	9,973	9,641	9,641	6,037	6,037
Heterogeneity by: **PROPORTION OF CHILDREN ENROLLED IN SCHOOL BY AGE GROUP** (3)								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt * % school age: 5-7	8.77	7.67	10.02*	7.35	1.47	2.46	3.35	2.18
	(5.77)	(6.37)	(5.32)	(6.09)	(5.79)	(7.10)	(7.47)	(9.16)
itt * % school age: 8-12	6.38	5.94	9.51**	12.20***	4.29	5.93	2.13	1.5
	(4.13)	(4.22)	(4.13)	(4.48)	(3.93)	(4.81)	(5.18)	(6.20)
itt * % school age: 13-17	1.08	2.2	1.6	1.1	0.21	-1.17	-5.31	-6.06
	(2.71)	(2.93)	(3.03)	(3.32)	(2.71)	(3.30)	(3.37)	(4.36)
Obs.	2,041	2,041	1,958	1,958	1,910	1,910	1,247	1,247

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

AM and *RM* stand for *Absolute Mobility* and *Relative Mobility* as defined in the estimation strategy (Section V.)

All regressions include the following controls: head's and spouse's age, age squared, head's ethnicity (language) and baseline household size.

(1) education = 1 if household head has at least primary education

(2) each age group is a dummy variable equal to 1 if there is a household member in that age range

(3) each school age group is a dummy variable equal to 1 if at least half of the members in that age range are enrolled in school

Table 4.3. Heterogeneity analysis by Shocks to the household

Ranks Estimates

	Panel A: Wave 2 (Oct. 1998)		Panel B: Wave 4 (Nov. 1999)		Panel C: Wave 7 (Nov. 2003)		Panel D: Wave 8 (Aug. 2007)	
Dependent variable in all regressions: Consumption mobility; Absolute Mobility (AM) or Relative Mobility (RM)								
Type of shock: **HOUSEHOLD HEAD UNEMPLOYMENT** (1)								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt*unemployment	0.03	0.19	-1.37	-0.5	-3.18	-4.28*	-0.96	-1.64
	-2.02	-2.19	(1.91)	(2.16)	(1.96)	(2.38)	(2.49)	(3.13)
Obs.	10,632	10,632	9,745	9,745	9,421	9,421	5,924	5,924
Type of shock: **DROUGHT SEVERITY** (2)								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt*drought severity	-2.96	-5.27	-3.93	-6.24	-1.25	-1.95	0.24	-0.66
	(4.49)	(4.77)	(4.71)	(5.51)	(4.48)	(5.40)	(5.19)	(6.09)
Obs.	10,632	10,632	9,745	9,745	9,421	9,421	5,924	5,924
Type of shock: **NATURAL DISASTER SEVERITY** (3)								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt*nat.diaster severity	5.71	0.06	1.28	-2.00	4.33	3.93	3.99	3.42
	(6.87)	(7.13)	(6.44)	(7.48)	(6.44)	(7.65)	(7.58)	(8.56)
Obs.	10,632	10,632	9745	9745	9,421	9,421	5,924	5,924
Type of shock: **LOSS DUE TO NATURAL DISASTERS** (4)								
	AM	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
itt*nat.disaster loss	0.56	0.05	0.04	-0.94	-1.00	-0.9	-0.6	-0.65
	(1.72)	(1.83)	(1.68)	(1.96)	(1.75)	(2.12)	(1.98)	(2.28)
Obs.	10,632	10,632	9,745	9,745	9,421	9,421	5,924	5,924

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

AM and *RM* stand for *Absolute Mobility* and *Relative Mobility* as defined in the estimation strategy (Section V.)

All regressions include the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language) and baseline household size.

(1) Household head unemployment reported in Wave 2 (October 1998)

(2) Severity of drought is calculated as the percentage of households within the locality reporting the shock

(3) Natural disaster include: flood, frost, fire, plague, earthquake and hurricane. Severity is calculated as the percentage of households within the locality reporting the shock

(4) Loss of land, harvest or animal due to the shock

Table 5.1 Heterogeneity analysis for baseline characterteristics relating to Physical Capital

Estimates for Consumption Trajectories

Panel A: SHORT TERM (Waves 1, 2, 4; Experimental variation period)

	Sustained poverty {ijk = 111}	Sustained welfare {ijk = 333}	Downward mobility {i ≥ j > k} or {i > j ≥ k}	Upward mobility {i ≤ j < k} or {i < j ≤ k}	Temporary Upward {i < j > k}	Temporary Downward {i > j < k}
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-early-treatment	-0.03	0.02	-0.08*	0.00	0.04*	0.02
	(0.02)	(0.02)	(0.05)	(0.04)	(0.02)	(0.02)
distance	0.07***	-0.01	-0.03	-0.04	0.04**	-0.04**
	(0.02)	(0.02)	(0.05)	(0.04)	(0.02)	(0.02)
itt* distance (1)	-0.02	-0.01	-0.01	0.09**	-0.03	0.02
	(0.03)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.10	0.06	0.02	0.02	0.00	0.01
Intent-to-early-treatment	-0.04**	0.01	-0.09***	0.07***	0.01	0.03**
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
land	0.00	0.01	-0.01	-0.02	0.02	-0.00
	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)
itt* land (2)	-0.04	-0.00	0.03	0.03	-0.02	0.01
	(0.03)	(0.02)	(0.03)	(0.04)	(0.03)	(0.02)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.09	0.06	0.02	0.02	0.00	0.01
Intent-to-early-treatment	-0.08**	-0.01	-0.08	0.07	-0.02	0.10**
	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)
homeowner	0.01	-0.01	-0.03	0.02	-0.01	0.02
	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)
itt* homeowner (3)	0.03	0.02	-0.01	0.01	0.04	-0.07
	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.09	0.06	0.02	0.02	0.00	0.01
Intent-to-early-treatment	-0.04**	0.01	-0.10***	0.07***	0.01	0.05***
	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.01)
animal value	0.02	0.02*	-0.02	0.01	-0.01	0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
itt* animal value (4)	-0.03	0.00	0.04	0.01	0.00	-0.04**
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.09	0.06	0.02	0.02	0.00	0.01
Intent-to-early-treatment	-0.05***	0.01	-0.09***	0.08***	0.01	0.04***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
non-agriculture	-0.06**	0.00	-0.01	0.07*	-0.02	0.01
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)
itt*non-agriculture (5)	0.04	0.04	0.03	-0.09*	-0.00	-0.03
	(0.03)	(0.03)	(0.04)	(0.05)	(0.03)	(0.03)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.09	0.06	0.02	0.02	0.00	0.01

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

All regressions are OLS estimates including the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, and baseline community characteristics (community organizations, and wages).

Each regression estimates the Intent-to-early-treatment impact on an outcome dummy variable indicating whether the household presents a specific welfare trajectory (e.g. In column (1) the mobility outcome is a dummy equal to 1 if the household remained in the lowest consumption tercile over the three wave period. In column (4) the mobility outcome is a dummy equal to 1 if the household exhibited a pattern of upward mobility along the consumption distribution as explained in section VI, which describes the estimation strategy in detail.)

Table 5.1 Heterogeneity analysis for baseline characterteristics relating to Physical Capital (cont.)

Estimates for Consumption Trajectories

Panel B: MID TERM (Waves 1, 4, 7: Phase-in period by wave 4)

	Sustained poverty {ijk = 111}	Sustained welfare {ijk = 333}	Downward mobility {i ≥ j > k} or {i > j ≥ k}	Upward mobility {i ≤ j < k} or {i < j ≤ k}	Temporary Upward {i < j > k}	Temporary Downward {i > j < k}
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-early-treatment	0.00	0.02	0.01	0.00	-0.02	-0.05
	(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)
distance	0.08***	-0.02	-0.03	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)
itt* distance (1)	-0.03	-0.00	-0.04	0.02	0.08**	0.00
	(0.03)	(0.02)	(0.05)	(0.05)	(0.03)	(0.04)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.07	0.05	0.04	0.04	0.01	0.01
Intent-to-early-treatment	-0.02	0.01	-0.01	0.02	0.04***	-0.06***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
land	-0.03	-0.01	0.03	0.02	-0.02	-0.01
	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)
itt* land (2)	0.00	0.03*	-0.05	0.01	-0.01	0.04
	(0.03)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.06	0.05	0.04	0.04	0.01	0.01
Intent-to-early-treatment	-0.02	-0.01	0.00	-0.07	0.10**	-0.02
	(0.03)	(0.03)	(0.06)	(0.05)	(0.04)	(0.04)
homeowner	0.01	-0.01	-0.04	-0.03	0.06**	0.02
	(0.03)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)
itt* homeowner (3)	0.00	0.02	-0.02	0.09	-0.06	-0.03
	(0.03)	(0.03)	(0.06)	(0.05)	(0.04)	(0.04)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.06	0.04	0.04	0.04	0.01	0.01
Intent-to-early-treatment	-0.02	0.02	-0.02	0.01	0.05***	-0.06***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
animal value	0.00	0.00	0.00	-0.00	0.00	-0.00
	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)
itt* animal value (4)	-0.00	0.00	-0.01	0.01	-0.02	0.02
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.06	0.04	0.04	0.04	0.01	0.01
Intent-to-early-treatment	-0.03	0.01	-0.02	0.02	0.05***	-0.05***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
non-agriculture	-0.05**	-0.01	-0.01	-0.00	0.06**	0.02
	(0.02)	(0.01)	(0.04)	(0.03)	(0.03)	(0.02)
itt*non-agriculture (5)	0.03	0.04*	0.04	-0.06	-0.03	-0.04
	(0.03)	(0.02)	(0.05)	(0.04)	(0.04)	(0.03)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.06	0.05	0.04	0.04	0.01	0.01

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

All regressions are OLS estimates including the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, and baseline community characteristics (community organizations, and wages).

Each regression estimates the Intent-to-early-treatment impact on an outcome dummy variable indicating whether the household presents a specific welfare trajectory (e.g. In column (1) the mobility outcome is a dummy equal to 1 if the household remained in the lowest consumption tercile over the three wave period. In column (4) the mobility outcome is a dummy equal to 1 if the household exhibited a pattern of upward mobility along the consumption distribution as explained in section VI, which describes the estimation strategy in detail.)

Table 5.1 Heterogeneity analysis for baseline characterteristics relating to Physical Capital (cont.)

Estimates for Consumption Trajectories

Panel C: LONG TERM (Waves 1, 4, 8: Phase-in period by wave 4)

	Sustained poverty {ijk = 111}	Sustained welfare {ijk = 333}	Downward mobility { i ≥ j > k} or { i > j ≥ k }	Upward mobility { i ≤ j < k} or { i < j ≤ k }	Temporary upward { i < j > k}	Temporary downward { i > j < k}
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-early-treatment	-0.04*	0.02	-0.03	0.02	0.03	-0.02
	(0.02)	(0.01)	(0.04)	(0.03)	(0.03)	(0.03)
distance	0.03*	0.00	-0.11***	0.05	-0.01	0.01
	(0.02)	(0.01)	(0.04)	(0.03)	(0.03)	(0.02)
itt* distance (1)	0.01	-0.01	0.02	0.00	0.04	-0.03
	(0.03)	(0.02)	(0.05)	(0.04)	(0.03)	(0.03)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.09	0.07	0.04	0.03	0.03	0.03
Intent-to-early-treatment	-0.02*	0.01	-0.01	0.02	0.05***	-0.05***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
land	0.00	-0.01	0.02	0.00	-0.02	-0.01
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
itt* land (2)	-0.02	0.01	-0.03	-0.02	0.02	0.04*
	(0.03)	(0.01)	(0.04)	(0.03)	(0.03)	(0.02)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.08	0.07	0.03	0.03	0.03	0.03
Intent-to-early-treatment	-0.06*	-0.02	0.02	-0.04	0.12***	-0.06
	(0.03)	(0.03)	(0.06)	(0.05)	(0.04)	(0.04)
homeowner	-0.01	-0.02	-0.00	-0.02	0.06***	-0.00
	(0.03)	(0.02)	(0.05)	(0.04)	(0.02)	(0.03)
itt* homeowner (3)	0.04	0.03	-0.03	0.06	-0.06	0.02
	(0.04)	(0.03)	(0.06)	(0.05)	(0.04)	(0.04)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.08	0.07	0.03	0.03	0.03	0.03
Intent-to-early-treatment	-0.02	0.01	-0.01	0.02	0.06***	-0.05***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
animal value	-0.00	0.00	0.00	0.00	0.01	-0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
itt* animal value (4)	-0.01	0.00	0.01	-0.00	-0.02	0.02
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.08	0.07	0.03	0.03	0.03	0.03
Intent-to-early-treatment	-0.03**	0.01	-0.01	0.02	0.06***	-0.04***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
non-agriculture	-0.06***	-0.01	0.04	-0.03	0.07	-0.02
	(0.02)	(0.01)	(0.04)	(0.03)	(0.04)	(0.03)
itt*non-agriculture (5)	0.02	0.03	0.05	-0.02	-0.06	-0.01
	(0.02)	(0.02)	(0.05)	(0.04)	(0.05)	(0.04)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.08	0.07	0.03	0.03	0.03	0.03

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

All regressions are OLS estimates including the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, and baseline community characteristics (community organizations, and wages).

Each regression estimates the Intent-to-early-treatment impact on an outcome dummy variable indicating whether the household presents a specific welfare trajectory (e.g. In column (1) the mobility outcome is a dummy equal to 1 if the household remained in the lowest consumption tercile over the three wave period. In column (4) the mobility outcome is a dummy equal to 1 if the household exhibited a pattern of upward mobility along the consumption distribution as explained in section VI, which describes the estimation strategy in detail.)

Table 5.1 Heterogeneity analysis for baseline characterteristics relating to Physical Capital (cont.)

Estimates for Consumption Trajectories

Panel D: LONG TERM (Waves 6, 7, 8: Post phase-in period) both groups have benefited from transfers for up to a year)

	Sustained poverty {ijk = 111}	Sustained welfare {ijk = 333}	Downward mobility {i ≥ j > k} or {i > j ≥ k}	Upward mobility {i ≤ j < k} or {i < j ≤ k}	Temporary Upward {i < j > k}	Temporary Downward {i > j < k}
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-early-treatment	0.00	-0.00	-0.07**	0.05*	0.03	-0.02
	(0.03)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)
distance	0.04	-0.02	-0.10***	0.09***	-0.00	-0.02
	(0.03)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)
itt* distance (1)	-0.02	0.02	0.11**	-0.08**	-0.03	0.03
	(0.03)	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.09	0.05	0.04	0.04	0.04	0.07
Intent-to-early-treatment	-0.01	0.01	0.01	-0.01	0.00	0.00
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
land	0.00	-0.02**	-0.01	-0.02	0.02	0.01
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)
itt* land (2)	-0.02	0.03*	-0.00	-0.00	0.00	-0.01
	(0.03)	(0.01)	(0.04)	(0.03)	(0.04)	(0.02)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.09	0.05	0.03	0.03	0.04	0.07
Intent-to-early-treatment	0.04	-0.05	0.01	0.05	-0.09**	0.01
	(0.03)	(0.04)	(0.06)	(0.05)	(0.05)	(0.04)
homeowner	0.06**	-0.06**	0.02	0.06*	-0.05	-0.04
	(0.02)	(0.03)	(0.05)	(0.04)	(0.04)	(0.03)
itt* homeowner (3)	-0.06*	0.07**	-0.00	-0.07	0.10**	-0.01
	(0.03)	(0.03)	(0.07)	(0.05)	(0.05)	(0.05)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.09	0.05	0.03	0.03	0.04	0.07
Intent-to-early-treatment	-0.01	0.02*	-0.00	-0.01	0.00	0.00
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
animal value	0.02	0.00	-0.04*	0.01	0.02	-0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
itt* animal value (4)	-0.02	-0.00	0.02	-0.01	0.00	-0.00
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	5,005	5,005	5,005	5,005	5,005	5,005
R-squared	0.09	0.05	0.03	0.03	0.04	0.07
Intent-to-early-treatment	-0.02	0.02**	0.01	-0.02	0.01	0.00
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
non-agriculture	-0.02	0.00	0.08*	-0.10***	0.01	0.01
	(0.02)	(0.01)	(0.05)	(0.03)	(0.03)	(0.03)
itt*non-agriculture (5)	0.04	-0.01	-0.04	0.07*	-0.04	-0.00
	(0.03)	(0.02)	(0.06)	(0.04)	(0.03)	(0.04)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.09	0.05	0.03	0.04	0.04	0.07

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

All regressions are OLS estimates including the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, and baseline community characteristics (community organizations, and wages).

Each regression estimates the Intent-to-early-treatment impact on an outcome dummy variable indicating whether the household presents a specific welfare trajectory (e.g. In column (1) the mobility outcome is a dummy equal to 1 if the household remained in the lowest consumption tercile over the three wave period. In column (4) the mobility outcome is a dummy equal to 1 if the household exhibited a pattern of upward mobility along the consumption distribution as explained in section VI, which describes the estimation strategy in detail.)

Table 5.2. Heterogeneity analysis for baseline characterteristics relating to Human Capital

Trajectories Estimates

Dependent variables: Persistent poverty, persistent high welfare, downward mobility, upward mobility

Heterogeneity by: **EDUCATION LEVEL**				
	cons {111}	**cons {333}**	**downward**	**upward**
itt*education (1)	-0.01	-0.01	-0.01	-0.07*
	(0.03)	(0.02)	(0.04)	(0.04)
Obs.	5,009	5,009	5,009	5,009
Heterogeneity by: **HOUSEHOLD DEMOGRAPHIC COMPOSITION** (2)				
	cons {111}	**cons {333}**	**downward**	**upward**
itt * age 0-7	-0.08	-0.02	-0.13	0.01
	(0.07)	(0.06)	(0.13)	-0.13
itt * age 8-12	0.83***	-0.11**	0.32	0.84***
	(0.20)	(0.06)	(0.24)	(0.24)
itt * age 13-17	-0.01	-0.04**	0.01	-0.01
	(0.02)	(0.01)	(0.03)	(0.03)
itt * age 18-54	-0.84***	0.08	-0.53**	-0.77***
	(0.18)	(0.06)	(0.21)	(0.22)
itt * age≥55	0.02	0.03	-0.00	0.03
	(0.07)	(0.03)	(0.08)	(0.09)
Obs.	5,009	5,009	5,009	5,009
Heterogeneity by: **PROPORTION OF CHILDREN ENROLLED IN SCHOOL BY AGE GROUP** (3)				
	cons {111}	**cons {333}**	**downward**	**upward**
itt * % school age: 5-7	0.03	0.07*	0.24*	0.06
	(0.11)	(0.04)	(0.14)	(0.14)
itt * % school age: 8-12	0.06	-0.01	0.15	-0.08
	(0.07)	(0.03)	(0.10)	(0.09)
itt * % school age: 13-17	-0.10*	-0.01	-0.03	-0.15**
	(0.05)	(0.02)	(0.06)	(0.07)
Obs.	1,070	1,070	1,070	1,070

Notes: Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

AM and *RM* stand for *Absolute Mobility* and *Relative Mobility* as defined in the estimation strategy (Section V.)

All regressions include the following controls: head's and spouse's age, age squared, head's ethnicity (language) and

(1) education = 1 if household head has at least primary education

(2) each age group is a dummy variable equal to 1 if there is a household member in that age range

(3) each school age group is a dummy variable equal to 1 if at least half of the members in that age range are enrolled in school

Table 5.3 Heterogeneity analysis by Shocks to the household

Estimates for Consumption Trajectories

Panel A: SHORT TERM (Waves 1, 2, 4; Experimental variation period)

	Sustained poverty {ijk = 111}	Sustained welfare {ijk = 333}	Downward mobility { i ≥ j > k} or { i > j ≥ k }	Upward mobility { i ≤ j < k} or { i < j ≤ k }	Temporary upward { i < j > k}	Temporary downward { i > j < k}
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-early-treatment	-0.01	0.01	-0.08***	0.06**	-0.00	0.03
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
natural disaster severity	0.06***	-0.02*	0.00	-0.02	0.02	-0.01
	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)
nat.disaster*itt	-0.08**	-0.00	-0.01	0.03	0.02	0.01
	(0.03)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.12	0.07	0.03	0.02	0.01	0.01
Intent-to-early-treatment	-0.08***	0.01	-0.06**	0.06**	0.01	0.06***
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
drought severity	-0.01	-0.02***	0.04*	-0.02	-0.00	0.02*
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
drought*itt	0.04*	0.01	-0.02	0.02	-0.01	-0.03
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.11	0.07	0.03	0.02	0.01	0.01
Intent-to-early-treatment	-0.05***	0.01*	-0.09***	0.07***	0.00	0.04***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Unemployment (household head)	0.02	0.00	-0.01	-0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Unemployment*itt	-0.01	-0.00	0.01	0.01	0.01	-0.00
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.11	0.07	0.03	0.02	0.01	0.01

Notes: Robust standard errors in parentheses clustered at the community level.

All regressions are OLS estimates including the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draf t and production animals, hectares of land, farmsize, homeowership, dirt floor and electricity) and baseline community characteristics (community organizations, distance to urban center and wages).

Each regression estimates the Intent-to-early-treatment impact on an outcome dummy variable indicating whether the household presents a specific welfare trajectory (e.g. In column (1) the mobility outcome is a dummy equal to 1 if the household remained in the lowest consumption tercile over the three wave period. In column (4) the mobility outcome is a dummy equal to 1 if the household exhibited a pattern of upward mobility along the consumption distribution as explained in section VI, which describes the estimation strategy in detail.)

Table 5.3 Heterogeneity analysis by Shocks to the household (cont.)

Estimates for Consumption Trajectories

Panel B: MID TERM (Waves 1, 4, 7: Phase-in period by wave 4)

	Sustained poverty {ijk = 111}	**Sustained welfare {ijk = 333}**	**Downward mobility { i ≥ j > k} or { i > j ≥ k }**	**Upward mobility { i ≤ j < k} or { i < j ≤ k }**	**Temporary upward { i < j > k}**	**Temporary downward { i > j < k}**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Intent-to-early-treatment	0.02	0.02	-0.04	-0.00	0.05**	-0.06***
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
natural disaster severity	0.06***	-0.01	-0.02	-0.01	-0.01	-0.00
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
nat.disaster*itt	-0.08***	-0.01	0.03	0.03	-0.01	0.02
	(0.03)	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.09	0.06	0.05	0.05	0.01	0.01
Intent-to-early-treatment	-0.05**	0.03**	-0.01	-0.01	0.05**	-0.02
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
drought severity	-0.02	-0.01	0.02	-0.03**	0.02	0.02*
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
drought*itt	0.02	-0.01	0.00	0.02	-0.00	-0.02
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.09	0.06	0.05	0.05	0.01	0.01
Intent-to-early-treatment	-0.03**	0.02***	-0.02	0.01	0.04***	-0.05***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
Unemployment (household head)	0.00	0.00	-0.01	0.02	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Unemployment*itt	-0.01	-0.01	0.01	0.01	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.09	0.06	0.05	0.05	0.01	0.01

Notes: Robust standard errors in parentheses clustered at the community level.

All regressions are OLS estimates including the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draf t and production animals, hectares of land, farmsize, homeowership, dirt floor and electricity) and baseline community characteristics (community organizations, distance to urban center and wages).

Each regression estimates the Intent-to-early-treatment impact on an outcome dummy variable indicating whether the household presents a specific welfare trajectory (e.g. In column (1) the mobility outcome is a dummy equal to 1 if the household remained in the lowest consumption tercile over the three wave period. In column (4) the mobility outcome is a dummy equal to 1 if the household exhibited a pattern of upward mobility along the consumption distribution as explained in section VI, which describes the estimation strategy in detail.)

Table 5.3 Heterogeneity analysis by Shocks to the household (cont.)

Estimates for Consumption Trajectories

Panel C: LONG TERM (Waves 1, 4, 8: Phase-in period by wave 4)

	Sustained poverty {ijk = 111}	Sustained welfare {ijk = 333}	Downward mobility { i ≥ j > k} or { i > j ≥ k }	Upward mobility { i ≤ j < k} or { i < j ≤ k }	Temporary upward { i < j > k}	Temporary downward { i > j < k}
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-early-treatment	-0.00	0.02	-0.03	0.04	0.03	-0.04*
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
natural disaster severity	0.03**	-0.01	-0.04	0.05*	-0.03	0.01
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
nat.disaster*itt	-0.05**	-0.01	0.04	-0.05	0.04	0.00
	(0.02)	(0.01)	(0.04)	(0.04)	(0.03)	(0.03)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.10	0.07	0.04	0.04	0.03	0.03
Intent-to-early-treatment	-0.06***	0.01	0.02	0.01	0.05**	-0.04*
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
drought severity	-0.02*	-0.00	0.01	0.00	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
drought*itt	0.02*	-0.00	-0.03	0.00	0.01	-0.00
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.10	0.07	0.04	0.03	0.03	0.03
Intent-to-early-treatment	-0.03**	0.00	-0.00	0.00	0.06***	-0.04***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
Unemployment (household head)	-0.00	-0.00	-0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Unemployment*itt	0.00	0.02*	0.00	0.02	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.10	0.07	0.04	0.03	0.03	0.03

Notes: Robust standard errors in parentheses clustered at the community level.

All regressions are OLS estimates including the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draf t and production animals, hectares of land, farmsize, homeowership, dirt floor and electricity) and baseline community characteristics (community organizations, distance to urban center and wages).

Each regression estimates the Intent-to-early-treatment impact on an outcome dummy variable indicating whether the household presents a specific welfare trajectory (e.g. In column (1) the mobility outcome is a dummy equal to 1 if the household remained in the lowest consumption tercile over the three wave period. In column (4) the mobility outcome is a dummy equal to 1 if the household exhibited a pattern of upward mobility along the consumption distribution as explained in section VI, which describes the estimation strategy in detail.)

Table 5.3 Heterogeneity analysis by Shocks to the household (cont.)

Estimates for Consumption Trajectories

Panel D: LONG TERM (Waves 6, 7, 8: Post phase-in period) both groups have benefited from transfers from the onset for up to a year)

	Sustained poverty {ijk = 111}	Sustained welfare {ijk = 333}	Downward mobility { i ≥ j > k} or { i > j ≥ k }	Upward mobility { i ≤ j < k} or { i < j ≤ k }	Temporary upward { i < j > k}	Temporary downward { i > j < k}
	(1)	(2)	(3)	(4)	(5)	(6)
Intent-to-early-treatment	0.00	0.02	-0.02	0.01	0.00	-0.00
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
natural disaster severity	0.03	-0.02**	-0.08***	0.06*	0.02	-0.01
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
nat.disaster*itt	-0.04	-0.00	0.03	-0.03	0.01	0.00
	(0.03)	(0.01)	(0.04)	(0.04)	(0.03)	(0.02)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.10	0.06	0.05	0.05	0.05	0.07
Intent-to-early-treatment	-0.02	0.01	0.00	0.01	0.00	-0.00
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
drought severity	-0.01	-0.01**	0.00	0.02	-0.02*	0.01
	(0.01)	(0.00)	(0.02)	(0.02)	(0.01)	(0.01)
drought*itt	0.00	0.00	0.01	-0.03	-0.01	0.01
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.10	0.06	0.04	0.04	0.05	0.07
Intent-to-early-treatment	-0.02	0.01	0.02	-0.01	0.01	-0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Unemployment (household head)	-0.00	-0.01	0.02	-0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Unemployment*itt	0.01	0.02**	-0.01	-0.02	-0.01	0.02*
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Observations	4,990	4,990	4,990	4,990	4,990	4,990
R-squared	0.10	0.06	0.04	0.04	0.05	0.07

Notes: Robust standard errors in parentheses clustered at the community level.

All regressions are OLS estimates including the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draf t and production animals, hectares of land, farmsize, homeowership, dirt floor and electricity) and baseline community characteristics (community organizations, distance to urban center and wages).

Each regression estimates the Intent-to-early-treatment impact on an outcome dummy variable indicating whether the household presents a specific welfare trajectory (e.g. In column (1) the mobility outcome is a dummy equal to 1 if the household remained in the lowest consumption tercile over the three wave period. In column (4) the mobility outcome is a dummy equal to 1 if the household exhibited a pattern of upward mobility along the consumption distribution as explained in section VI, which describes the estimation strategy in detail.)

Appendix

Table A.1 Ranks estimates by wave for all consumption components

	Levels Estimates			Normalized Rank estimates (Eligibles Only)						Normalized Rank estimates (Eligibles and Noneligibles)					
Panel A: Wave 2 (Oct. 1998)				*Absolute mobility (AM)*			*Relative mobility (RM)*			*Absolute mobility (AM)*			*Relative mobility (RM)*		
				Rank of change			Change in Rank			Rank of change			Change in Rank		
	(1)			(2)			(3)			(4)			(5)		
	cons	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**
Intent-to-treat	11.41***	8.18**	2.60*	3.72***	3.97***	1,83	4.63***	4.78***	3.36**	3.73***	4.01***	1,81	3.95***	4.24***	2.93**
	(3.71)	(3.20)	(1.45)	(1.27)	(1.23)	(1.14)	(1.43)	(1.36)	(1.39)	(1.28)	(1.24)	(1.13)	(1.34)	(1.29)	(1.31)
Number of Observations	11.532	11.532	11.532	10.632	10.579	10.314	10.632	10.632	10.632	10.632	10.579	10.314	10.632	10.632	10.632
Mean (control hhs)	142,19	110,27	36,94	47,5	47,42	48,76	-2,88	-2,98	-1,93	47,62	48,52	47,15	-3,92	-2,28	-4,78
Mean (non-poor hhs)	216,39	156,43	67,86	.	.	.	.	.	.	49,88	48,8	51,74	0,57	-1,37	2,92
St.Dev(control hhs)	95,99	95,38	46,05	28,79	28,85	28,95	33	33,01	36,4	28,88	29,17	28,73	31,35	31,52	34,62
Min(control hhs)	3,07	0	0	0	0,02	0	-97,8	-99,07	-97,49	0	0,03	0,01	-98,05	-97,68	-97,71
Max(control hhs)	1.071,67	2.965,70	658,66	99,98	99,97	100	96,28	94,85	99,86	99,98	99,98	99,99	94,62	90,93	99,39
Impact (% control mean) (1)	8%	7%	7%	8%	8%	4%				8%	8%	4%			
Impact (% non-poor mean) (2)	5%	5%	4%							7%	8%	3%			
1- (2)/(1)	0,34	0,30	0,46							0,05	0,01	0,09			
Panel B: Wave 4 (Nov. 1998)				*Absolute mobility*			*Relative mobility*			*Absolute mobility*			*Relative mobility*		
				Rank of change			Change in Rank			Rank of change			Change in Rank		
	(1)			(2)			(3)			(4)			(5)		
	cons	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**
Intent-to-treat	21.16***	16.11***	4.68***	7.09***	7.99***	3.92***	9.16***	9.79***	5.84***	7.04***	7.94***	3.86***	8.09***	8.99***	4.97***
	(3.25)	(2.86)	(1.43)	(1.27)	(1.37)	(1.07)	(1.53)	(1.64)	(1.38)	(1.26)	(1.37)	(1.06)	(1.41)	(1.56)	(1.27)
Number of Observations	10.755	10.755	10.755	9.973	9.944	9.788	9.742	9.742	9.742	9.973	9.944	9.788	9.973	9.973	9.973
Mean (control hhs)	128,64	90,45	40,21	45,6	45,17	47,47	-5,37	-5,34	-3,43	46,32	46,95	46,79	-5,73	-4,01	-5,69
Mean (non-poor hhs)	195,89	129,16	68,84	.	.	.	.	.	.	49,24	48,09	50,76	0,69	-1,14	3,11
St.Dev(control hhs)	79,75	77,26	42,88	28,55	28,63	28,83	34,34	34,03	36,56	28,42	28,54	28,42	32,43	32,28	34,79
Min(control hhs)	5,92	0	0	0,01	0,02	0,01	-98,51	-99,9	-98,91	0,02	0,05	0,01	-97,91	-99,73	-98,72
Max(control hhs)	860,69	3.351,55	635,73	99,96	100	100	90,25	91,09	98,84	99,95	99,99	100	91,97	92,31	97,13
Impact (% control mean) (1)	16%	18%	12%	16%	18%	8%				15%	17%	8%			
Impact (% non-poor mean) (2)	11%	12%	7%							14%	17%	8%			
1- (2)/(1)	0,34	0,30	0,42							0,06	0,02	0,08			
Panel C: Wave 7 (Nov. 2003)				*Absolute mobility*			*Relative mobility*			*Absolute mobility*			*Relative mobility*		
				Rank of change			Change in Rank			Rank of change			Change in Rank		
	(1)			(2)			(3)			(4)			(5)		
	cons	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**
Intent-to-treat	13.84***	8,53	4,22	2.10*	2.99***	0,49	2.70*	3.95***	1,19	2.08*	2.96***	0,49	2.44*	3.61***	1
	(5.14)	(5.40)	(2.68)	(1.18)	(1.14)	(1.22)	(1.54)	(1.46)	(1.62)	(1.18)	(1.13)	(1.21)	(1.44)	(1.39)	(1.51)
Number of Observations	10.415	10.415	10.415	9.641	9.520	9.429	9.417	9.417	9.417	9.641	9.520	9.429	9.641	9.641	9.641
Mean (control hhs)	183,1	120,35	72,95	48,62	48,08	49,55	-0,5	-1,67	0,23	48,88	48,96	49,34	0,05	0,03	-0,41
Mean (non-poor hhs)	259,71	170,12	110,5	.	.	.	.	.	.	49,7	49,04	50,21	1,54	0,09	2,61
St.Dev(control hhs)	152,79	237,73	80,33	28,48	28,41	28,82	36,31	36,21	37,66	28,38	28,31	28,62	34,73	34,59	36,04
Min(control hhs)	0,48	0	0	0	0,03	0	-96,73	-99,37	-98,14	0,01	0,07	0,01	-96,91	-98,46	-97,78
Max(control hhs)	2.730,55	9.104,37	1.635,34	100	100	100	96,85	94,27	97,78	99,98	99,99	100	96,27	95,38	96,85
Impact (% control mean) (1)	8%	7%	6%	4%	6%	1%				4%	6%	1%			
Impact (% non-poor mean) (2)	5%	5%	4%							4%	6%	1%			
1- (2)/(1)	0,29	0,29	0,34							0,02	0,00	0,02			
Panel D: Wave 8 (Aug. 2007)				*Absolute mobility*			*Relative mobility*			*Absolute mobility*			*Relative mobility*		
				Rank of change			Change in Rank			Rank of change			Change in Rank		
	(1)			(2)			(3)			(4)			(5)		
	cons	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**	**cons**	**food**	**nonfood**
Intent-to-treat	6,45	5,77	0,68	0,22	0,73	0,39	-0,05	1,3	0,03	0,16	0,71	0,35	-0,02	0,97	0,28
	(11.71)	(5.20)	(8.87)	(1.37)	(1.47)	(1.36)	(1.66)	(1.94)	(1.73)	(1.34)	(1.46)	(1.33)	(1.59)	(1.87)	(1.67)
Number of Observations	6.520	6.520	6.520	6.056	5.983	5.972	5.938	5.938	5.938	6.056	5.983	5.972	6.056	6.056	6.056
Mean (control hhs)	770,85	155,01	615,84	49,68	49,55	49,36	1,66	1,9	-0,2	50,74	51,89	50,32	-0,8	5,72	-5,44
Mean (non-poor hhs)	882,81	171,14	711,67	.	.	.	.	.	.	48,73	47,12	48,84	4,54	-0,58	5,39
St.Dev(control hhs)	442,88	121,34	383,04	28,29	28,95	28,43	35,43	38,49	38,44	27,72	28,59	27,9	36,78	37,18	39,99
Min(control hhs)	153,57	0	117,3	0	0,03	0	-96,81	-97,38	-93,13	0,01	0,07	0,01	-97,89	-94,91	-97,02
Max(control hhs)	4.611,45	1.979,22	4.157,84	99,95	99,97	99,98	98,33	97,62	98,59	99,94	99,98	99,93	98,61	98,09	98,43
Impact (% control mean) (1)	1%	4%	0%	0%	1%	1%				0%	1%	1%			
Impact (% non-poor mean) (2)	1%	3%	0%							0%	2%	1%			
1- (2)/(1)	0,13	0,09	0,13							-0,04	-0,10	-0,03			

Notes: Robust standard errors in parentheses clustered at the locality level. *** p<0.01, ** p<0.05, * p<0.1. All values refer to intent-to-treat.
Per capita expenditures in food an nonfood items expressed in adult equivalent unit and October 1997 pesos. Homeproduction, imputed using community level prices, is included. All regressions include the following controls: head's and spouse's age, age squared and baseline education dummies, head's ethnicity (language), baseline household size, dummies controlling for household demographics at baseline, baseline assets (number of draf t and production animals, ha of land, farmsize, homeowership, dirt floor and electricity) and baseline community characteristics (community organizations, distance to urban center and wages).

Table A2. Robustness check: Estimates of Mobility Ranks results as a function of long -term attrition

	Wave 2		Wave 4		Wave 6		Wave 7	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variables								
AM= Absolute Mobility /1	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**	**AM**	**RM**
RM= Relative Mobility /2								
itt	4.201***	4.168**	8.180***	9.085***	1.799	1.785	1.889	2.040
	(1.586)	(1.648)	(1.593)	(1.782)	(1.653)	(1.940)	(1.501)	(1.793)
attrition in 8	-0.430	-0.662	-1.381	-1.613	-1.003	-1.313	-2.759*	-3.088*
	(1.508)	(1.574)	(1.491)	(1.661)	(1.451)	(1.733)	(1.514)	(1.799)
attrition_w8 * itt	-1.034	-0.427	-2.214	-2.029	0.593	1.619	1.070	1.277
	(1.940)	(2.038)	(1.922)	(2.188)	(1.910)	(2.283)	(1.993)	(2.436)
Constant	-6.310	-58.22***	10.14	-35.91***	19.83**	-28.39***	23.47***	-30.19***
	(13.85)	(15.09)	(8.012)	(9.422)	(8.578)	(9.976)	(4.519)	(5.770)
Observations	10,643	10,665	9,752	9,757	9,660	9,668	9,427	9,508
R-squared	0.030	0.032	0.044	0.045	0.043	0.049	0.065	0.071

Notes:

1/ Absolute mobility (*AM*) is defined as the rank of the change in log consumption that a household exhibits between two periods (the baseline year and a subsequent wave). The ranking, which includes all the households in the sample (including the ineligible), is normalized so it ranges from 0 to 100.
2/ Relative mobility (*RM*) is defined as the change in the rank of consumption for a household between time t and baseline. To build this measure the entire consumption distribution is ranked and normalized (to range from 0 to 100) at baseline and follow-up. Thus the relative mobility measure is simply the difference between the normalized ranks at the two periods.

Each column corresponds to a single regression: $mobility_i = \alpha + \beta\ itt_i + \delta\ attrition\ wave\ 8 + \lambda\ itt * att\ w8 + baseline\ controls_i + \varepsilon$

Robust standard errors in parentheses clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

Table A3. Administrative information on Date of first transfer

(Frequency of households at each month)

		Original classification		New entrants	
Date(month/year)		**C**	T		**Total**
Apr	1998	0	5,113	0	5,113
Jun	1998	0	303	0	303
Aug	1998	0	281	0	281
Dec	1998	0	528	0	528
Feb	1999	0	1	0	1
Aug	1999	0	0	4	4
Nov	1999	2,579	0	1	2,580
Dec	1999	1,761	0	0	1,761
Feb	2000	99	0	20	119
Apr	2000	517	0	2	519
Jun	2000	5	0	0	5
Aug	2000	1	0	255	256
Nov	2000	1	0	6	7
Feb	2001	0	0	1	1
Apr	2001	0	0	848	848
Jun	2001	0	0	13	13
Aug	2001	0	0	16	16
Dec	2001	0	0	2	2
Feb	2002	0	0	4	4
Aug	2002	0	0	4	4
Nov	2002	0	0	1	1
Feb	2003	0	0	1	1
Total		4963	6226	1178	12,367

Source: Own calculations based on administrative transfer records up to 2003.