Long-term Impacts of the timing of Conditional Cash Transfers on Households' Economic Mobility

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Abstract

Could the timing of an intervention affect its impact? More specifically, could a small time-differential in the entrance into a CCT program have an impact on the household's long-term welfare trajectory? This could be the case if households' binding constraints are not constant over time. These constraints are likely to be dynamic due to the heterogeneity between households. This paper exploits the randomized evaluation design of a renowned CCT program to evaluate the impact of an 18-month differential in exposure to the program on the likelihood that a household presents a path of sustained poverty or downward mobility, among other trajectories. Furthermore, I explore how the impacts of the differential timing vary according to the heterogeneity between the households (in terms of physical and human capital and exposure to shocks.) The heterogeneity analysis indicates that early receipt of the program impacts households to differing degrees according to their characteristics at baseline relating to physical capital (for land and homeowners and those close to markets) and human capital (secondary school-aged members). However, the heterogeneity between households in terms of their exposure to shocks does not seem to affect the impact of the timing of the CCT in the long-run. Overall, understanding how the timing of transfers may affect the extent of their impact can be important for targeting purposes, taking into account households' heterogenous and dynamic constraints.

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

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1. Introduction

For nearly a decade now there has been a wave of optimism about the increase in socioeconomic mobility in Latin America. According to comparable regional estimates, two in five Latin Americans were upwardly mobile between 1995 and 2010, resulting in an expansion of the middle class of approximately fifty percent over that period. Some of the cross-country descriptive evidence suggests that targeted interventions, specifically conditional cash transfers (CCTs), might have played a key role in promoting upward mobility. In contrast, general social spending (on untargeted schemes such as pensions or unemployment) showed little correlation to mobility in the region (Ferreira et al, 2013).

The focalized nature of social protection is understood to be a key component to enhance progressive socioeconomic results, such as providing opportunities for upward mobility and combating poverty persistence. Indeed, for conditional transfers there is direct evidence that targeting based on the household's characteristics affects considerably the impact and efficiency of the program (de Janvry and Sadoulet, 2006).

In addition to targeting at the household level, could the *timing* of an intervention of this sort matter for it to have an impact on socioeconomic mobility? By definition a research question examining socioeconomic mobility requires a prolonged time dimension, whether referring to mobility over time within a generation (intrageneration) or into the next generation (intergenerational). In the case of CCTs, a large literature has documented the short and medium-term impacts of the pioneering Mexican program Progresa[1] and its successors (cf. Fiszbein and Schady, 2009; and more recently Baird, et al, 2014). The evidence is now emerging about the long-run impacts though it is mixed and still limited, thus remaining high on the research agenda (Molina-Millán, et al. 2019a)[2].

Understanding how the timing of CCTs and transfers in general may affect the extent of their impact, on human capital and subsequent outcomes, can be important for targeting. This proves particularly relevant as some of the more recent transfer programs target narrower populations and objectives (e.g. Filmer and Schady, 2014 in Asia or Baird et al., 2011 in Africa). The emerging long-term evidence has accordingly laid the focus on certain critical age ranges of exposure to the intervention (Barham et al, 2013; Molina Millán, et al, 2019b; Parker and Vogl, 2018). The majority of existing evaluation studies for CCTs identify average treatment effects, even if the focus is on intergenerational impacts (Araujo, et al. 2018; Parker, Vogl, 2018). The evidence of the program's

[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 study I adhere throughout the paper to the program's original name.

[2] The review study by Molina-Millán, et al. (2019a) finds consistent positive long-term impacts on schooling but less so for cognitive skills, learning or socio-emotional skills. The results for impacts on earnings and employment are mixed, possibly because former beneficiaries were still too young. A number of the studies reviewed find estimates which are not statistically distinguishable from zero. However, the authors caution that it is often not possible to distinguish whether this is due to an actual lack of impact or the methodological challenges facing all long-term studies.

specific effect on distributional measures remains scant. A recent exception examining the impact on inequality of opportunity is the study by Van de gaer and Figueroa (2014).

This paper uses the randomly assigned differential exposure between early and late treatment groups of Mexico's renowned CCT program Progresa to evaluate the impact of the timing of the transfers on the households' socioeconomic mobility within the same generation. The hypothesis that the timing of the conditional transfer may impact the household's socioeconomic mobility follows from the premise that the household's binding constraints are not constant over time. Rather, the heterogeneity between households, in terms of their access to and accumulation of physical and human capital, as well as the unexpected shocks they may encounter, renders these constraints dynamic. As such, the specific moment at which a household enters the program may affect the extent to which the CCT has an impact on its welfare. This impact will depend on the constraints faced by the household at that given point in time and whether these constraints are effectively lifted by the CCT. Moreover, the small time differential (less than two years) may potentially have lasting effects if the CCT affects not only the household's contemporaneous welfare but rather the welfare trajectory on which the household is set.

In particular, in this paper I exploit the information from the panel's middle survey rounds to construct 3-period *welfare trajectories*. A trajectory is defined as the sequence of a household's 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, sustained downward mobility, or temporary downward mobility (or the analogous upward mobility paths). 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 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).

Furthermore, I explore how the impacts of the differential timing vary according to the heterogeneity between the households (in terms of physical and human capital and exposure to shocks.) The heterogeneity analysis indicates that early receipt of the program impacts households to differing degrees according to their characteristics at baseline relating to physical capital (for land and homeowners and those close to markets) and human capital (for primary and secondary aged members). However, the heterogeneity between households in terms of their exposure to shocks does not seem to affect the impact of the timing of the CCT in the long-run. The rest of the paper is organized as follows. Section 2 provides the basic relevant background information on the program, including the program design and data set used. In section 3 I present some definitions regarding the measure of intragenerational mobility used and the conceptual framework to motivate the hypothesis that the timing of the transfers may affect their impact on economic mobility. The empirical strategy, leveraging the experimental design and constructing welfare trajectories to evaluate the impact of timing on socioeconomic mobility, is laid out in section 4. The aggregate results for the short, mid and long-term trajectories are presented in section 5, followed by the heterogeneity analysis with focus on the long-term estimates in section 6. Concluding remarks are presented in section 7.

2. Background of the Program

2.1 Program design

As one of the most renowned and studied CCT programs the rules and evaluation design of Progresa have been extensively documented (Behrman and Todd, 1999a, 1999b; Skoufias and McClafferty, 2001). In this section I therefore limit the information to the program's most basic, relevant aspects. Progresa started operating in the most marginal rural communities in Mexico in 1997, covering approximately 300,000 beneficiary households. Subsequently, the program expanded into urban areas covering six million families by 2016[3], or about one quarter of families in Mexico. 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 provided 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[4]. 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 1.1)[5]. 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.

[3] According to official figures by 2016 the latest version of the program, including conditional and unconditional schemes (*esquema sin corresponsabilidad*), covered close to 7 million Mexican families. https://www.gob.mx/prospera/documentos/que-es-prospera
[4] In 2001 the program was extended to include high school (upper secondary) grants and the age limit increased to 21 years.
[5] 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).

2.2 Evaluation design and data

As documented in several previous studies on Progresa, the original evaluation and sample design for the program consisted of 506 rural communities[6] (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.[7]

This census, the 1997 Survey of Household Socio-Economic Conditions (ENCASEH 97), provided the pre-program data for the evaluation[8]. 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[9]. 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. (2012) 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

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

[7] There were actually two rounds of selection of eligible households in Progresa. 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 Progresa evaluation dataset since the incorporation of the *densified* households is less well documented (see for example Gertler et al., 2012; and Angelucci and Di Giorgi, 2009).

[8] 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.]

[9] 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).

to obtain a ten-year span. I mainly focus on the changes in household consumption 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 heterogeneity analysis)[10]. 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[11]. 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 all regression estimates in the paper use the sample of eligible households at each period (column 3 in Table 1.2) to measure the impact on mobility of differential exposure to the program (i.e. early versus late treatment 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)[12].

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 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

[10] I do not use Wave 5 (i.e. May 2000) since the consumption data is not available for this round.
[11] 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).
[12] 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.

electricity is the only variable which is consistently associated to a lower likelihood of attrition throughout the ten-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 to 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 mostly random, in moving forward, I will control for all these baseline characteristics[13] in the estimates and remain mindful of the selection due to sample loss over the survey rounds. Further attrition checks are presented in the Appendix section (see section A.1.1 at the end). For the sample of households under study, treatment status (early treatment or late treatment for the control group) denotes 'intention-to-treat' (ITT).

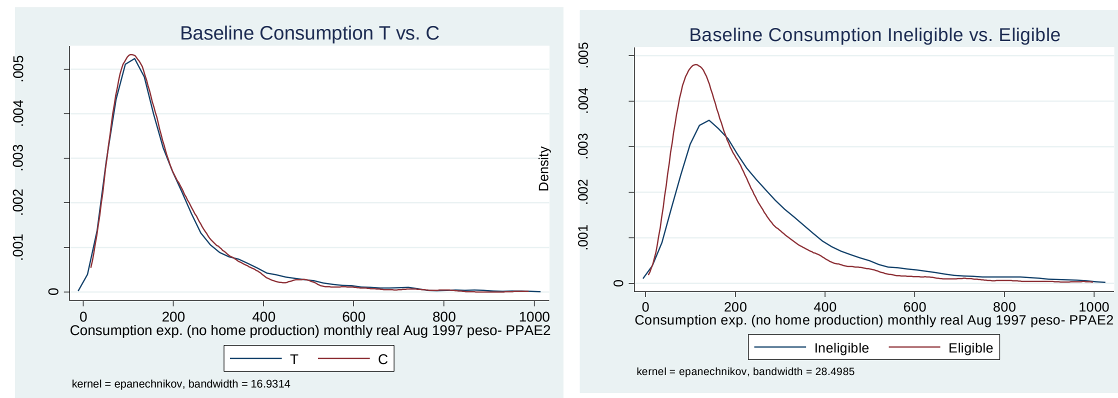
Given that I will be including the entire census of households to construct the mobility measures (i.e. using the full consumption distribution of the evaluation villages), it is important to examine how the ineligible 'non-poor' initially compare to the eligible poor. Figure 1 below plots the consumption 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[14], 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

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

[14] Indeed, throughout the analysis that follows, the validity of the intent-to-treat estimates using the evaluation design of Progresa hinges on the randomization of households into treatment status. Thus, non-compliance and/or changes in the classification of eligible households may attenuate the detectable impacts. These potential concerns are addressed in the Appendix section at the end (see section A.1.2. on compliance and the 'densification process' surrounding Progresa).

proxy for households' permanent income, and not on actual consumption. The main take-away from this comparison (showing an overlap in consumption between the eligible and non-eligible households) is that when constructing the mobility trajectories, based on a household's position along the entire consumption distribution, we can do not expect the wealthiest tercile at baseline to consist exclusively of the ineligible population. Rather, the distributions' overlap suggests there will be some marginal ineligibles even at the lowest tercile, and some eligible at the top. It will be important to take into account this initial distributional set-up when interpreting movements in the subsequent analysis.

Figure 1. Consumption distributions by Treatment and Eligibility status



3. Conceptual definitions, Framework and Hypotheses

3.1. Intragenerational Socioeconomic mobility

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 2006) 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, 2013). The *space* indicates the choice of variable in the distribution under consideration (in this study, household consumption[15]), and the *domain* indicates how far apart in time the two (or more) distributions are observed. In the present context

[15] More precisely, I use per capita adult equivalent household consumption. 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 tercile positions along the distribution.

this distance is close to ten years (between 1997 and 2007) depending on the specific welfare trajectory under study.

It is key 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 (for which 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. The unit of analysis used in this study is the same household observed at baseline and up to a decade later. Thus, the domain of mobility is strictly *intragenerational*.

Following the taxonomy of mobility measures defined by Fields (2005)[16] the underlying *concept* of mobility I adhere to in this study 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 consumption movement' or 'share movement'). The rationale for this choice of mobility concept is given by the strict focus in this study on intragenerational mobility.

As discussed in the review on mobility concepts by Ferreira et al (2013) *across* generations mobility is often associated with the notion of equality of opportunity, where a mobile society is one in which the outcomes of one generation do not substantially determine that of the next generation. As such, the concept of mobility most closely associated (in axiomatic terms) with this notion is that of origin independence (Shorrock, 1978). However, this is not necessarily the case *within* generation. For a given household, the lack of serial correlation between consumption at one period and another is not a particularly meaningful measure of mobility. There are ethical and incentive-related reasons why a certain degree of temporal persistence in the rewards to effort would be desirable within a generation (e.g. compensation for successful investments in human and physical capital). Origin independence therefore is not a suitable concept of mobility in this study. Rather, given the interest in tracing and understanding the pathways followed by the households that received the timely transfers and their standing relative to others, positional movement is arguably a more adequate measure of socioeconomic mobility.

More specifically, I build a simple outcome measure of *each* household's trajectory[17] in terms of its position (tercile) along the consumption distribution. Thus, it is important to note that in this study I am not constructing a mobility measure for a *population* as a whole, as is common for descriptive analysis on mobility. Rather, I

[16] In addition to Fields (2000 2006) and more recently Ferreira et al. (2013), other reviews of the mobility literature include Atkinson et al., (1992) and Fields and Ok (1999).
[17] The method used for the construction of the welfare trajectories is explained further below (Section 4; Estimation strategy).

construct a welfare trajectory for each *household* to obtain an outcome measure of the household's individual economic mobility[18].

Having established the space, domain and concept of intragenerational mobility used in this paper, for brevity, the term 'mobility' hereafter entails this specific definition.

3.2. Conceptual Framework and Hypotheses

In this section I lay out the conceptual underpinnings which sustain the hypothesis that the timing of the conditional transfer may impact the household's socioeconomic mobility (i.e. affect its long-term welfare trajectory). This hypothesis stems from the main premise that the household's binding constraints are not constant over time. Rather, the heterogeneity between households, in terms of their access to and accumulation of physical and human capital, as well as the unexpected shocks they may encounter, renders these constraints dynamic. As such, the specific moment at which a household enters the program may affect the extent to which the CCT has an impact on its welfare. This impact will depend on the constraints faced by the household at that given point in time and whether these constraints are effectively lifted by the CCT. Moreover, the small time differential (less than two years) may potentially have lasting effects if the timely access to the CCT affects not only the household's contemporaneous welfare but rather the welfare trajectory on which the household is set.

The premise that the households' binding constraints are dynamic over time, thereby affecting the welfare impact of the transfer program, derives from the heterogeneous conditions surrounding each household; in particular from three sources of heterogeneity:

i. Heterogeneity in Human capital

To hypothesize about the household's constraints in terms of its human capital it is useful to think about an underlying lifecycle of earnings of the household (Ben-Porath, 1967). The constraints faced by the household can be thought to be dynamic since they are shaped by the household's changing demographic composition. As modeled in a number of studies, on the production of human capital, the constraints on the household's earnings capacity is expected to change as members age (cf. Ben-Porath, 1967; and subsequently Basu and Van, 1998; and Baland and Robinson, 2000).

Taken to the rural, high poverty context where Progresa operated, for example, households with very young-aged children may be burdened to some extent, taxing their income earning capacity. Rearing new-born infants in particular may place a significant time-constraint on the caregiver attending the child's needs and the corresponding income opportunity cost of that time. This income opportunity cost could be lower during the

[18] A mobility outcome measure is needed for each individual household in order to perform the impact evaluation (of differential exposure to the CCT). Specifically, the impact evaluation in this context consists of comparing between early and late treatment households the (mean) likelihood of following a given trajectory (more details provided in Section 4; Estimation strategy).

child's primary school age, both because school fees are not as high as in secondary school (thus the lower transfer values of the CCT in primary) and because the opportunity cost of child wages are expected to be lower given the young primary- aged child's (lack of) abilities. Consequently, the constraints imposed on the household's earnings capacity would be lowest in the case of prime-aged members, having passed the stage of investment in their own human capital and being able to offer their labor supply to the household.

Indeed, early evidence on Progresa suggests the CCT had a safety net value for child schooling but not for labor (de Janvry, et al, 2010). Moreover, the heterogeneity between the households implied large differences in the impact of the conditional transfer (on enrollment) across categories of children. The largest effects detected were at the transition stage from primary school to secondary school (de Janvry & Sadoulet, 2006).

ii. Heterogeneity in Physical Capital

The prediction that the timing of the transfer may affect the program's impact, depending on the household's possession of physical capital (at the time of treatment), follows initially from the existing evidence that Progresa allowed households to invest part of the transfer in productive activities (Gertler et al 2012). This would be expected if the cash injection, for example, helped recipient households afford the start-up costs associated with entrepreneurial activities (McKenzie and Woodruff, 2006). Also, risk-averse beneficiary households could have been more willing to invest in riskier, but higher return, activities if the transfers were perceived as a secure and steady source of income. Naturally, the capacity to successfully invest the transfers would depend on the household's existing physical capital at the time of treatment (e.g. asset holdings that serve as start-up or complementary resources for the productive activity). In other words, the household's investment constraints and therefore the CCT's potential impact on welfare is expected to vary as a function of the household's initial physical capital.

In fact, the evidence for Progresa shows that, during the experimental phase, beneficiary households increased ownership of productive farm assets and that agricultural production increased faster for beneficiary households than non-beneficiary households (Gertler et al, 2012). This resulted in significantly higher agricultural income (an estimated 9.6 percent increase as a result of an 18-month exposure period). Moreover, the returns on these investments were estimated to persist (beyond the experimental period), raising long-term living standards as measured by consumption. According to the estimates by Gertler et al., four years after households in the late treatment group were incorporated into the program, consumption levels for the early treatment households were still five percent higher. This particular finding motivates the hypothesis that the timing of the transfer might affect the program's welfare impact. If the early impacts on productive activities were not sensitive to the timing of the transfers, then the late treatment households would be expected to eventually catch-up. However, the program's impact could be time-sensitive if innovative investment opportunities are seized to a greater extent by early recipients.

Furthermore, and related to productive activity, the access to markets could also emphasize the importance of the timing of the transfers. For example, it is expected that within and across localities the increased consumption demand be met by an increase in production. Whether this translated into a welfare enhancing mobility opportunity for the program recipients would depend on which households managed to seize the production opportunity raised by the increased consumption demand. If the demand was met by the wealthier ineligibles, the program beneficiaries would be less likely to move up in the welfare distribution. Alix-Garcia et al (2013) provide evidence that the production increase within localities depended on the connection to surrounding markets. Specifically, in localities with good road infrastructure there was no production-side response among local ineligible households because the demand was met by surrounding localities. In contrast, where poor infrastructure localizes economic activity the increased consumption caused by the program was indeed met by an increase in output. However, this boost in output was driven primarily by the wealthy ineligible households, thus making upward mobility *less* likely for the program beneficiaries' in these remote areas. Bearing this evidence in mind, I hypothesize that heterogeneity between household proximity to market centers may affect the program's impact on the household's welfare mobility. In particular, transfer recipients could be in better conditions to take advantage of the mobility opportunity in better-connected areas, with early beneficiaries enjoying a head-start to seize innovative investment opportunities.

iii. Heterogeneity in Exposure to Shocks

The notion that a household's response to short-run shocks may have long-term consequences on the household's welfare path rests on the premise of state-dependence in household decisions (de Janvry et al., 2006) and the evidence that temporary shocks may trigger poverty persistence (Premand and Vakis, 2010). In particular, disinvestments decisions, both in terms of human and physical capital (e.g. pulling children out of school or asset liquidation), taken in the face of transient events, may have lasting effects on households due to re-entry costs (e.g. school re-enrollment or lumpy asset expenses). In this respect, the CCT program may potentially attenuate the adverse lasting effects of shocks. This hypothesis builds on the evidence provided by de Janvry et al (2006), which suggests that Progresa had a strong mitigating effect on the school enrollment response to an income shock[19]. More recently, Adhvaryu et al. (2018) analyzed the heterogeneity of differential impacts for the Mexican program. In particular they compared the impacts between individuals who had experienced pre-program negative rainfall shocks during their first year of life, and all others. For educational and labor outcomes (i.e. labor force participation and employment stability), the authors find larger differential impacts for those exposed to negative shocks during childhood. These pieces of empirical evidence, focusing on children who were age eligible for the schooling transfer, complement the early results showing that beneficiary households managed to smooth their consumption in the face of adverse shocks (Skoufias and McClafferty, 2001).

[19] As shown in the dynamic model the authors develop households may still choose to send children to school with a negative utility for schooling but a high utility for cash as a consequence of an income shock. By contrast, since the conditionality applied to school and not to work (and the two activities were not time incompatible), the CCT did not have much of an effect in refraining parents from responding to an income shock by increasing child labor.

Thus, the heterogeneity between households in their exposure to shocks is expected to affect the CCT's impact on the welfare path the household follows. Finally, the program's impact on the household's welfare path is also expected to be time-sensitive. In this context, even a small time-differential in the receipt of the transfer could have lasting effects on the welfare trajectory followed. For a given household hit by a negative incident, receiving the cash at that moment could mitigate the adverse effect while receiving the transfer a year later would send the household on different (likely downward) trajectory.

To sum-up, the life-cycle of earnings framework and the existing evidence on the differential impact of CCTs depending on capital accumulation and shocks incidence support the postulate that households' constraints are heterogenous and dynamic over time. On the human capital plane, the rearing costs (including income opportunity costs) associated to the different stages of the household members' life-cycles are predicted to be high at early ages, possibly peaking around the transition between primary and secondary school, and falling towards prime-age (i.e. that of adult entrance into the labor market). In the case of physical capital welfare impacts of the program are predicted to vary depending on initial household wealth and the timing of the transfer could make a difference if innovative investment opportunities are seized to a greater extent by early recipients. Lastly, the CCT's shock mitigating capacity, and hence its impact on the welfare trajectory, is expected to be sensitive to the timing of the transfers in proximity to the moment when the household is hit by an unexpected negative event.

Overall, the main corollary from this framework is that the dynamic nature of the constraints faced by the household may imply that the timing of an intervention to lift some of those constraints could affect the impact of the intervention. That said, even a small difference in the time of receipt of the transfers could affect the program's potential impact on welfare if the household's conditions and constraints change over the course of that time difference.

4. Estimation Strategy

The rationale for using welfare trajectories is discussed at length by Premand and Vakis (2010). 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 the long-term panel further beyond just looking at differences between the baseline and end-line. By characterizing three-period trajectories I can retain information about the pathways leading up to the final period. This approach can be especially 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. 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 that 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 (2010) 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 it falls into. For the aggregate estimates below (section 5) I also present the welfare trajectories for the food and non-food components of consumption to understand which drives the overall welfare dynamics. Based on Engel's law and given the acute poverty levels of the population in question, for which food expenditures constitute around two thirds of total consumption, the food component is expected to drive the poverty persistence pathways.

It is worth noting that the consumption distribution used to define each household's position (tercile), at each wave, is the one based on all eligible and non-eligible households in the marginal communities in Mexico that were targeted by the program. The trajectories I construct therefore characterize each household's mobility within this specific population and given the context of exposure to the program in all communities (by the end of the experimental phase; i.e. by November 1999). The rationale for including the ineligible households in the consumption distribution is precisely that they provide a higher benchmark, in terms of living standards, against which to gauge the socioeconomic mobility of the program beneficiaries. These ineligible households, whom were too wealthy to qualify for the program, constitute - in principle - the non-vulnerable population. This non-poor population is arguably a suitable referent in welfare standards given the program's poverty alleviation aim (in the current generation). The data confirm that the ineligible households have a slightly higher mean consumption level, though there is some overlap of their consumption distribution to that of the poor (eligible)

[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.

households (as discussed in section 2; Figure 1). That said it is important, when interpreting the results, to bear in mind that the welfare trajectories describe the households' mobility within this particular (relatively) poorer part of the overall Mexican population.

Expanding beyond the negative trajectories characterized by the authors (poverty persistence and downward mobility), in this paper I am also interested in whether the conditional transfers may lead to positive patterns of welfare. Hence, I focus on two additional 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 estimates 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 six 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 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.

[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.

T is a dummy variable indicating intention-to-early-treatment status for household i and β_1 is therefore the coefficient of interest reported in each regression result capturing the impact of the differential exposure to the conditional transfer. X_i is a vector of pre-program characteristics (including household head, household demographic and community variables[22]) I will control for to gain precision.

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.

5. Aggregate Results

The regression results for the aggregate trajectories outcomes are presented in Table 2. Panel A displays the impact estimates during the short-term, experimental period (Waves 1, 2, 4). The coefficients indicate that the early treatment households are less likely to move downward (and more likely to move upward) in the distribution. 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) are presented in columns 13 through 18. The trajectories estimates indicate that in the short run the early beneficiary households 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). There are no detectable impacts on the upward mobility pattern for the three period trajectories (and in the case of downward mobility the impact is 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

[22] 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.

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. The 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 compared to those for sustained high welfare. Furthermore, in the case of total 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 B and C) suggest the early treatment group is more likely to follow a path of 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 treatment group to fall between these two time periods.

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.

The results for this final period 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 long-term estimates in Panel C (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 was 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.

The ensemble of 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 recovery). However, this mobility advantage for the late treatment group also dissipates with time.

To recap, the only impacts, from the timing of the transfers, 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 is suggestive of the importance of the timely assistance received by the early treatment group.

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 the two 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.

To synthesize, 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. In the following section I intend to delve further into the mechanism which may be driving these results.

6. Heterogeneity Analysis

Based on the conceptual framework described above the focus in the empirical examination is set on the analysis of heterogeneity. In particular, in this section I explore how the impacts of the differential timing in the transfer vary according to the initial heterogeneity between the households. This empirical specification is driven by the hypothesis that the constraints faced by a household are time dynamic; i.e. change along the household's economic lifecycle, as a function of asset accumulation and due to transient shocks. As argued in the conceptual frame, these constraints are shaped by the household's access to and formation of physical capital and human capital, as well as the household's exposure to shocks.

To investigate this empirically I perform heterogeneity analysis along a number of baseline household assets (relating to physical capital), sociodemographic characteristics (relating to human capital) and preprogram adverse events reported by the household (exposure to shocks). I focus on the long-term trajectories after phase-in of the control group; that is once both groups have received the transfers for at least a year (Waves 6,7 and 8). The reason is that I am interested in zooming-in on the conditions which may have set households on differing paths in a sustained manner; above and beyond the immediate, mechanical initial shift in consumption caused by the entrance of each group into the program (i.e. the early treatment group in Wave 2 and the late treatment by Wave 5). Rather, I am interested in detecting diverging trajectories which are maintained further in time.

The heterogeneity results for the variables relating to physical capital are presented in Table 3.1. The first panel shows the results for heterogeneity between the households in terms of their distance from market centers. The intention-to-early-treatment (itt) coefficient on its own shows the effect of receiving the transfer early for households living closer to market centers (i.e. households for which the distance dummy is 0)[23]. The significant coefficients indicate that, among the households living closer to market centers, receiving the treatment early made them (7 percentage points) less likely in the long term to follow a trajectory of downward mobility and (5 percentage points) more likely to follow an upward trend (columns 3 and 4). Importantly, the significant interaction terms indicate that the impact of the timing of the transfers is entirely reversed for households residing further away from those market centers. The differential effects are sizable (11 percentage points for the downward mobility outcome and 8 percentage points for upward mobility).

This result is consistent with the existing evidence on the market-mediated increases in local demand brought about by Progresa in the short term (during the experimental phase). In particular, Alix-Garcia et al. (2013) provide evidence that the consumption increases caused by Progresa were similar across localities with different connection to markets. However, the corresponding production increases among nearby wealthier households were not. Specifically, in localities with good road infrastructure there was no production-side response among local ineligibles. In contrast, in areas where poor infrastructure localizes economic activity the increased consumption caused by the program was indeed met by an increase in output. In other words, in well-connected localities, the richer ineligible households (which are included in the consumption distribution used in my estimates) did *not* increase their production capacity because the demand was met by neighboring markets. As such, it was in these well-connected localities that the early-program recipients managed to seize the mobility opportunity. This may explain why the early treatment had a (sizable) differential effect on the economic mobility depending on the household's proximity to markets.

Given the hypothesis that part of the transfer is invested in productive activities (Gertler et al., 2012) one might expect the asset holdings at baseline to predict the upward movers. This effect could potentially be sensitive to the timing of the transfer if investment opportunities are seized to a greater extent by early recipients. This seems to be the case with landholding and homeownership for the sustained high welfare outcome (column 2). In particular, the significant interaction terms indicate that the impact of receiving the transfer early was augmented

[23] The distance dummy is equal to 1 if the household's minimum distance to an urban center is greater than or equal to 71km (i.e. indicating households in the top 3 quintiles of the distance distribution).

for households owning more land (over 3 hectares) and for homeowners. The differential effect is larger for the homeownership variable (7 percentage points) compared to that of the land variable (only 3 percentage points). Moreover, there was no differential effect observed in the case of assets in the form of animal value.

The heterogeneity analysis along the human capital variables for the trajectories outcomes is presented in Table 3.2. In the first panel the point estimate for the itt coefficient on its own indicates the effect of receiving the transfers early for the households whose head had less than three years of education at baseline (i.e. for zero-values of the education dummy). The result in column 2 shows that, among households with less education, the timing of the transfer affected the welfare trajectory followed; the early recipient households were more likely to follow a path of sustained high welfare. Moreover, the interaction term suggests this effect of the timing of the transfer was differential depending on the household head's years of education, i.e. the effect was lower for higher-educated households (though marginally significant). Given the above results suggesting augmented mobility impacts for the households with a richer physical capital base, this other result showing lower impacts for the more educated heads is possibly indicative of the substitutability between the two forms of capital (physical and human).

The estimates in the second panel present the heterogeneity between households with and without any young children at baseline (aged between zero and seven years old). The point estimates for the itt coefficient are overall statistically insignificant. This result therefore does not support the hypothesis that households with very young-aged children are burdened to some extent, in particular due to the time constraint and the corresponding income opportunity cost faced by the child's caregiver(s) whom must attend the children's needs.

The significant itt coefficient in the third panel indicates that, among households without any primary aged children (8 to 12 years old), early treatment increases the likelihood of presenting a sustained high-welfare level (column 3). This is also the case for households without any children of secondary age, i.e. among households without children aged 13 to 17 years old at baseline, early treatment increases the chances of sustained high welfare (column 3 in the fourth panel). The interaction term for the secondary school age variable- indicates that the timing of the program indeed affected the likelihood of sustained welfare depending on the household members' age composition. For example, the impact of the timing of the CCT on the likelihood of having sustained high-welfare is *lower* for households with secondary school-aged minors with respect to the impact on households without members of this age group. This is consistent with hypothesis that the children at secondary age pose a constraint to households given the direct (e.g. schooling fees) and indirect costs (e.g. forgone wages) associated to rearing members at this stage in the life-cycle. In contrast, having members of prime-age or elderly members in the household (well beyond the age eligibility for the schooling transfers) does not affect the impact of the timing of the transfer in a meaningful way, as suggested by the coefficients in the last two panels of Table 3.2.

The heterogeneity analysis by shocks to the household is reported in Table 3.3. 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.

The estimates in Table 3.3 indicate that in the post-experimental phase, once both groups have received the program for at least a year, there are some, though few, detectable differences between the early and late program recipients related to shock exposure at baseline. For events relating to unemployment of the household head the interaction term does indicate that the impact of receiving the transfer early is augmented in some cases for households suffering this type of shock. In particular, early treatment households are more likely than late treatment households to follow a sustained high-welfare trajectory (column 2) and to recover from a downward trend (column 6). These impacts are intensified in the case of households hit by an unemployment spell at baseline. This differential impact of the timing is only detected in the case of natural disasters for the sustained poverty outcome (column 1; with marginal significance). In the case of drought, none of the point estimates for the interaction terms suggest a significant difference in the impact of timing conditional on exposure to this type of shock.

To understand further these lacks of significant results, I run the heterogeneity analysis for the short and mid-term trajectories (displayed in Table A.3 in the Appendix). Overall the short-term estimates (Panel A) yield the expected significant coefficients for the sustained welfare trajectories. As intuited, the households that experience higher shock incidence from natural disasters 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 (column 1) is mitigated for the early recipients of the CCT. Put differently the CCT's protective effect against shocks suffered at baseline is highly sensitive to the timing of the transfer. Indeed, the negative impact of natural disasters is fully mitigated for the early beneficiaries of the program. This result remains robust when using the disaggregated components of consumption, i.e. food and non-food (not displayed in Table A.3. for 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 in a trajectory spanning from the moment of the shock (baseline) up to ten years later, the differential timing still affects the magnitude of the impact, mitigating fully the effect of natural disasters for early recipients of the CCT.

In sum, heterogeneity in shock-exposure at baseline does seem to affect the impact of the timing of the transfers but only for trajectories starting at baseline. In contrast, this heterogeneity in impacts is much less evident for the long-term trajectories that trace post phase-in periods (Waves 6, 7, and 8).

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. Several variables relating to physical capital affect the timing of the program's effect on mobility. Proximity to an urban center, in particular, affects the extent to which early treatment households manage to seize productive opportunities. Heterogeneity in terms of human capital also affects the impacts of differential timing on households' trajectories. The interaction terms indicate that the timing of the program indeed affected the trajectory followed depending on the household members' age composition in the secondary school stage. Finally, heterogeneity between households in terms of their exposure to shocks seems to affect the impact of timing only temporarily, mainly for the period before the late treatment receives the program.

7. Concluding remarks

Could the timing of an intervention affect its impact? More specifically, could a small time-differential in the entrance into a CCT program have an impact on the household's long-term welfare trajectory? In this paper I examined the hypothesis that the timing of a CCT could affect its impact, provided that the households' binding constraints are not constant over time. Rather, these constraints are likely to be dynamic due to the heterogeneity between households.

To examine this hypothesis empirically, I exploit the randomized evaluation design of a renowned CCT program. I evaluate the impact of an 18-month differential in exposure to the program on the likelihood that a household presents a path of sustained poverty or downward mobility, among other trajectories. Furthermore, I explore how the impacts of the differential timing vary according to the heterogeneity between the households (in terms of physical and human capital and exposure to shocks.)

The heterogeneity analysis indicates that early receipt of the program impacts households to differing degrees according to their characteristics at baseline relating to physical capital (for land and homeowners and those close to markets) and human capital (for secondary aged members). However, the heterogeneity between households in terms of their exposure to shocks does not seem to affect the impact of the timing of the CCT in the long-run.

Understanding how the timing of CCTs, and transfers in general, may affect the extent of their impact can be important for targeting; in particular now that some of the more recent transfer programs target narrower populations and objectives. At this stage, it seems key for researchers to learn more about how interventions of this sort affect not only outcomes at a given point in the short or mid-term, but rather the long-term trajectories on which households are set. Further insight about the interplay between timed and/or targeted transfers and households' dynamic constraints, may provide a valuable input to policymakers in the design of antipoverty programs as these continue to evolve.

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Tables

Table 1.1. Progresa 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) .

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

	Sustained poverty			Sustained welfare			Downward mobility			Upward mobility			Temporary Upward			Temporary Downward		
	{ijk = 111}			{ijk = 333}			{ i ≥ j > k} or { i > j ≥ k }			{ i ≤ j < k} or { i < j ≤ k }			{ i < j > k}			{ i > j < k}		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	consump.	food	nonfood	consump.	food	nonfood	consump.	food	nonfood	consump.	food	nonfood	consump.	food	nonfood	consump.	food	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 3.1 Heterogeneity analysis for baseline characterteristics relating to Physical Capital

Estimates for Consumption Trajectories

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

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.)

Heterogeneity variable definitions

(1) distance = 1 if the minimum 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 household's animal value is in the top two quintiles of the distribution

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

Table 3.2. Heterogeneity analysis for baseline characterteristics relating to Human Capital

Estimates for Consumption Trajectories

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	-0.01	0.03***	0.01	-0.02	-0.01	0.01
	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)
Education (1)	-0.01	0.04***	0.00	-0.02	0.00	-0.02
(at least 3 years)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
itt* education	-0.02	-0.02	-0.01	0.01	0.03	-0.01
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	5,003	5,003	5,003	5,003	5,003	5,003
R-squared	0.10	0.05	0.04	0.04	0.05	0.07
Intent-to-early-treatment	0.01	0.03	-0.02	-0.05	-0.01	0.03
	(0.02)	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)
Young children (2)	0.06***	-0.04***	0.04	-0.06**	0.02	-0.04*
(aged 0-7 years old)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
itt* Young children	-0.03	-0.02	0.03	0.04	0.02	-0.03
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
Observations	5,003	5,003	5,003	5,003	5,003	5,003
R-squared	0.10	0.05	0.04	0.04	0.05	0.07
Intent-to-early-treatment	-0.00	0.03**	-0.03	-0.02	0.02	0.00
	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)
Primary school-aged children (3)	0.03*	0.00	0.01	-0.03	0.02	-0.04**
(aged 8-12 years old)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
itt* primary school-aged children	-0.02	-0.01	0.05**	0.01	-0.03	-0.00
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	5,003	5,003	5,003	5,003	5,003	5,003
R-squared	0.10	0.05	0.04	0.04	0.05	0.07
Intent-to-early-treatment	-0.02	0.03***	-0.01	-0.02	0.01	0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Secondary school-aged children (4)	-0.02	0.01	-0.00	0.00	-0.00	0.03*
(aged 13-17 years old)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
itt* secondary school-aged children	-0.01	-0.03***	0.03	0.01	-0.01	-0.01
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	5,003	5,003	5,003	5,003	5,003	5,003
R-squared	0.10	0.05	0.04	0.04	0.05	0.07
Intent-to-early-treatment	0.04	0.01	-0.04	0.10	0.03	-0.16*
	(0.04)	(0.06)	(0.06)	(0.08)	(0.04)	(0.09)
Prime aged members (5)	0.00	-0.02	0.05	0.12*	-0.03	-0.16**
(aged 18-54 years old)	(0.03)	(0.05)	(0.05)	(0.06)	(0.04)	(0.07)
itt*prime aged-members	-0.06	0.01	0.05	-0.12	-0.03	0.17*
	(0.04)	(0.06)	(0.06)	(0.08)	(0.04)	(0.09)
Observations	5,003	5,003	5,003	5,003	5,003	5,003
R-squared	0.09	0.05	0.04	0.04	0.05	0.07
Intent-to-early-treatment	-0.03*	0.02**	0.03	-0.02	0.00	-0.00
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
Elderly members (6)	-0.02	0.00	0.02	0.01	-0.02	0.01
(aged above 54 years old)	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)
itt*elderly members	0.04*	0.01	-0.08***	0.01	0.01	0.02
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
Observations	5,003	5,003	5,003	5,003	5,003	5,003
R-squared	0.10	0.05	0.04	0.04	0.05	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, head's ethnicity (language), baseline household size, 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.)

Heterogeneity variable definitions

(1) Education: dummy equal to 1 if the household head has at least three years of education

(2) Young children: dummy equal to 1 if there are any children between the ages of 0 and 7 years in the household.

(3) Primary school-aged children: dummy equal to 1 if there are any children between the ages of 8 and 12 years in the household.

(4) Secondary school-aged children: dummy equal to 1 if there are any children between the ages of 13 and 17 years in the household.

(5) Prime-aged members: dummy equal to 1 if there are any members between the ages of 18 and 54 years in the household.

(6) Elderly members: dummy equal to 1 if there are any members aged above 54 years in the household.

Table 3.3 Heterogeneity analysis by Shocks to the household

Estimates for Consumption Trajectories

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	-0.02	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
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

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 (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.)

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

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

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

Appendix sections

Appendix 1: Robustness checks: Further examination of Attrition in estimates and Compliance

A.1.1. 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 use an alternative mobility measure (see the ranks measure used in Clavijo, 2017) and re-estimate these 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[24]. 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 A.1. below. 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 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. 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 in the main text).

[24] For clarity, I run the following specification for each wave prior to wave 8 (ie. Wave 2, 4 , 6 and 7):
$\textit{rank mobility}_i = \alpha + \beta\, \text{itt}_i + \delta\, \textit{attrition}\ \text{wave 8} + \lambda\, \text{itt} * \text{att w8} + \text{baseline controls}_i + \varepsilon$

Table A.1.1. 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.

A.1.2. 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 *densification* 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 A.2. below., 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. Table A2 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.

Table A.2. 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.

Table A.3. Heterogeneity analysis by Shocks to the household

Estimates for Consumption Trajectories

	Sustained poverty	Sustained welfare	Downward mobility	Upward mobility	Temporary upward	Temporary downward
	{ijk = 111}	{ijk = 333}	{ i ≥ j > k} or { i > j ≥ k }	{ i ≤ j < k} or { i < j ≤ k }	{ i < j > k}	{ i > j < k}
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: SHORT TERM (Waves 1, 2, 4; Experimental variation period)						
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)
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)
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)
Panel B: MID TERM (Waves 1, 4, 7: Phase-in period by wave 4)						
	(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)
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)
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)
Panel C: LONG TERM (Waves 1, 4, 8: Phase-in period by wave 4)						
	(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)
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)
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. *** p<0.01, *

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.)