BIG PUSH PRO-POOR TRANSFERS AND PERCEPTIONS OF THE RICH AND POOR[*]

NICOLAS CERKEZ   ADNAN Q.KHAN   IMRAN RASUL   ANAM SHOAIB

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Abstract

Does exposure to big push pro-poor interventions impact household perceptions of their own economic standing, village inequality, and their views towards the rich and poor? We study the issue using a field experiment tracking $15,000$ households over four years in rural Pakistan. Villages are randomly assigned to receive an intervention where the poor are either offered a one-time asset transfer of value $620 or an equivalent valued one-time unconditional cash transfer. In treated villages we randomize which of the poor receive the transfer, and then track treated poor, not treated poor and not poor households at two and four years post-intervention. The transfers cause large and persistent economic gains to the treated poor, and reduce village-level consumption inequality. Despite these measurable changes, we document a wedge between economic reality and household's perceptions of their own economic standing and village inequality. On perceptions towards broader classes of households, two years post-intervention, all households, irrespective of their beneficiary status, are more likely to view the rich as deserving, and are significantly less likely to view poverty as being driven by structural factors that the poor are helpless against. Results four years post-intervention highlight how exposure to economically effective big push pro-poor interventions is however unlikely to persistently shift perceptions, even as economic gains to the treated poor are sustained. *JEL: I30, O12, P10.*

1 Introduction

The last few decades have witnessed a steady rise in policy interventions that provide transfers directly to the poor [Banerjee *et al.* 2024]. Among the most successful of such interventions – as measured by economic impacts on beneficiaries – take the form of 'big push' in-kind or cash transfers. At least 119 low-income countries have implemented unconditional cash transfer programs, and in-kind livestock asset transfers have been implemented as part of poverty graduation interventions in over 50 programs worldwide [CGAP 2016, Handa *et al.* 2017]. A body of evidence shows large and persistent impacts of such one-off and high-valued transfers on the economic lives of the poor [Banerjee *et al.* 2015, Haushofer and Shapiro 2016, Bandiera *et al.* 2017, Blattman *et al.* 2020, Balboni *et al.* 2022, Egger *et al.* 2022].

This paper goes beyond the study of economic impacts, to understand whether direct and indirect exposure to real world big push pro-poor interventions impact household perceptions of *specific* economic outcomes – their own standing and village inequality – and their *broader* perceptions of entire classes of rich and poor households. We examine these issues using a large-scale and long-term randomized control trial, tracking beneficiary and non-beneficiary households, where the pro-poor interventions take the form of either high-valued in-kind asset transfers or equivalent valued unconditional cash transfers. Our analysis sheds light on whether there are wedges between economic reality and households' perceptions of economic circumstances of themselves and households around them, and whether exposure to such big push interventions alters how households view the rich and poor more generally. Our study context are small, close-knit villages in rural Pakistan, where the economic impacts of interventions should *a priori* be noticeable to others, leaving less scope for misperceptions of gains to beneficiaries to persist [Alesina *et al.* 2021].

For both big push interventions considered, eligibility was determined by households lying below a poverty threshold and being identified as poor. In a first treatment arm, poor households in a village were offered productive assets in-kind. They could choose any combination of assets off a menu, up to a total value of PKR50K (500USD in 2012 prices). In conjunction with these assets, households were offered training of value PKR12K. Hence the total value of transfers and training offered was 620USD. We refer to this treatment as T1. The second intervention was identical to the first but with one more listed option on the menu: a one-off unconditional cash transfer of 620USD. We refer to this treatment as T2. The treatments are big push interventions in the sense that the value of transferred assets/cash is very high relative to the baseline assets of the poor. In both treatment arms there is near 100% take-up. In T1, 50% of eligibles chose combinations of livestock; 37% chose assets to set-up a small-scale retail business or engage in petty trade. In T2, 91% of households chose the unconditional cash transfer over any in-kind asset transfer – so reveal prefer cash over asset transfers.

To establish village-wide intervention impacts on economic outcomes and perceptions of beneficiaries and non-beneficiaries, we use a two-stage randomization design. In the first, we randomly

assign villages to T1, T2 or control. At a second stage, within treated villages, we randomly assign the offer of treatment among eligible households. Half of those eligible are actually offered treatment. Among the poor in treated villages we thus distinguish between the treated poor (TP) and the not treated poor (NTP). Our design and data collection allows us to evaluate causal impacts on beneficiaries (TP), on those overtaken in economic standing (NTP), and spillovers to those never eligible (NP). Tracking $15,000$ households – from the TP, NTP and NP groups – at two-years (midline) and four-years (endline) post intervention, we establish the dynamic evolution of economic outcomes, general equilibrium effects, and how these translate into perceptions of beneficiaries and non-beneficiaries.

Our first set of results establish impacts of the interventions on actual economic circumstances. We document large and persistent gains on noticeable economic outcomes for the TP. For example, using the within-village randomization we find gains to the TP in terms of livestock ownership, the value of livestock owned, and consumption of own produced milk, relative to the NTP in the same village. The magnitude of the effects are of economic significance and hard to miss in these tight-knit villages. For example, for the TP in T1, livestock ownership increases by 20pp, a 35% increase over the baseline mean for the poor in controls, the value of livestock owned increases by between 10-15% across periods, and by the four-year endline, the consumption of own produced milk increases by around 25%. We find no evidence of economic spillovers to the NTP or NP along these margins of noticeable outcomes.

As TP and NTP households are balanced at baseline, these gains to the TP imply that many of the NTP are overtaken by their treated neighbors. These changes in relative standing can shape perceptions of the NTP if they have concerns for their relative standing or exhibit last place aversion [Duesenberry 1949, Luttmer 2005, Card *et al.* 2012, Kuziemko *et al.* 2014].

Using the between village randomization, we document significant reductions in village level consumption inequality two- and four-years post intervention, driven by a rising left tail of the outcome distribution. These changes, if perceived, can also alter perceptions of others.

Finally, we note that both big push interventions have similar impacts on noticeable economic outcomes over time. Hence we pool T1 and T2 treatments for the bulk of the analysis.

Given this backdrop of persistently changed economic circumstances in treated villages, our core analysis sheds light on whether direct and indirect exposure to the interventions shift perceptions of own standing, economic inequality and attitudes towards the rich and poor. Our experimental design reveals the following insights on these non-economic outcomes.

First, perceptions of own economic standing are shifted by big push economic interventions targeting the poor, but these shifts are far more muted than actual measurable changes in economic standing. For example, the TP – direct beneficiaries of the interventions – have little change in perception of their economic standing, while non-beneficiaries report significant falls in their standing at midline. This is in line with findings from higher income settings that individual well-being can fall when individuals observe changes in wealth/income in people around them

[Luttmer 2005, Card *et al.* 2012, Perez-Truglia 2020, Cullen and Perez-Truglia 2022]. At the same time, there are very muted impacts on households perceptions of changes in village inequality as a whole, despite large and persistent reductions in village consumption inequality in treated villages relative to controls. In short, there exists a wedge between economic reality – as measured by the econometrician – and household's perceptions of their economic standing and village inequality.

Second, moving to broader perceptions of entire classes of households, we consider views towards the rich and poor. We find that exposure to the big push interventions has pronounced changes at midline in perceptions towards each class of household. In particular, all households in treated villages, irrespective of their own beneficiary status, perceive the rich to be more deserving of their status. On perceptions of the poor, the big push interventions do not shift households' views of the character of the poor, but they do shift perceptions of the root causes of poverty. Specifically, at midline TP and NTP households are significantly less likely to view poverty as being driven by structural factors that the poor are helpless against – these factors include exploitation of the poor by the rich, society failing to help them, the unequal distribution of land, or a lack of opportunities.

Big push pro-poor interventions hold immense promise for pulling the world's poor out of poverty. Our core contribution to is to advance understanding of whether the economic gains from such interventions are recognized by households directly and indirectly exposed to them, and how they shape their wider perceptions of the rich and poor. By documenting a wedge between actual changes in the levels, rankings and inequality of economic outcomes in village economies, and whether households perceive such changes, we address a basic but largely unanswered question in the literature: whether beneficiary and non-beneficiary households actually recognize the measured changes in economic circumstances around them, that are central to measuring the welfare impacts or cost-effectiveness of such interventions [Smith *et al.* 2021].

By documenting how exposure to big push interventions shift perceptions towards the rich and poor, we highlight changes in social dynamics that anti-poverty interventions trigger. Perceptions about others shape a wide array of attitudes, behaviors and social norms. For example, by making a first inroad into understanding how exposure to such interventions change views of others, our results link to the vast literature studying the impacts of economic shocks on preferences for redistribution [Meltzer and Richard 1981, Alesina and Giuliano 2011].

Beyond these specifics, three common themes emerge across our findings.

First, shifts in perceptions of others do not depend on whether an individual is an actual beneficiary of the intervention or not – rather they are driven by common village-wide exposure to pro-poor policies. Our experiment thus addresses a long-standing issue in the literature studying how economic attitudes respond to economic shocks, suggesting in our context, attitudes are driven by sociotropic concerns that relate to wider community well-being, rather than narrow self-interest. This has been emphasized in the political science literature largely in the context of redistributive preferences [Margalit 2019] and the nascent literature on how policy views are

formed [Stantcheva 2024]. Our experiment reveals that all groups – the TP, NTP and NP – have changed non-economic outcomes at midline in response to big push interventions. This is despite the very different impacts of the interventions on the economic outcomes across these groups. *A fortiori*, such policies do not polarize perceptions, or create backlash within villages – in nearly all cases impacts on the poor and non poor are of the same sign and similar magnitude.

Second, the evidence suggests that shifts in perception that occur at the two-year midline, often do not persist until the four-year endline, despite long-lasting impacts on the actual economic circumstances of the treated poor and village inequality. These dynamics could reflect salience effects of big push interventions, genuine updating that is subsequently reversed, or the fact that the economic gains to beneficiaries are achieved within a year of the intervention taking place and then plateau so that perceptions are driven by changes in economic outcomes. Understanding dynamics of non economic outcomes in response to pro-poor interventions remain an open area for future studies.

Third, our results hold irrespective of whether transfers to the poor are made in-kind or via cash transfers. The choice between in-kind and cash transfers has long been discussed through the lenses of public economics and political economy [Musgrave 1959, Atkinson and Stiglitz 1976, Akerlof 1978, Nichols and Zeckhauser 1982, Besley 1988, Coate *et al.* 1994, Benhassine *et al.* 2015]. Our results show that exposure to either form of pro-poor transfer generates similar measurable changes in outlook among beneficiaries and non-beneficiaries of the world around them.

Section 2 describes our context, interventions and research design. Section 3 examines impacts on noticeable economic outcomes and village inequality. Section 4 details how perceptions of self, the village and others are shifted by the interventions. Section 5 concludes by discussing differential impacts of cash and asset transfers, external validity and directions for future work.

2   Context, Interventions and Design

2.1   Context

Our evaluation covers 88 villages in semi-arid regions of four districts in southern Punjab: Bahawalpur, Bahawalnagar, Lodhran and Muzaffargarh. Households are almost all Muslim, and pre-intervention, the main activities heads of household engage in are cropping/farming (38%), unskilled laboring (19%) and livestock rearing (12%).

2.2   Interventions

Interventions take two forms. The first offered households productive assets in-kind. To determine the assets to offer, in each village we initially conducted a market assessment of those assets likely to generate high returns. These included livestock, assets to start a retail business (e.g. grocery

shop, fruit stall), crop farming, and other forms of self-employment (e.g. tailoring). Figure A1 shows a stylized representation of an asset menu. Households were free to choose any combination of assets off the menu up to a total value of PKR50K (500USD in 2012 prices). In conjunction with these transfers, households were offered micro-enterprise training, as well as skills specific to the chosen asset(s). The value of training was fixed at PKR12K. Hence the total value of transfers and training offered was PKR62K (around 620USD). We refer to this as treatment T1.[1]

The second intervention is identical to the first but with one more listed option on the menu: to take a one-off unconditional cash transfer of PKR62K. To mimic the timing of transfers and training in T1, the delivery of cash transfers was staggered as an up-front payment of PKR50K followed by PKR12K a month later. We refer to this as treatment T2.

Both treatments were implemented in collaboration with quasi-government agencies: the Pakistan Poverty Alleviation Fund (PPAF) and their government field partners, FDO and NRSP. Each intervention is thus best perceived as a government delivered program.[2]

The interventions are big push, representing high-valued resource transfers to the poor. The value of transfers corresponds to the equivalent of eight months of food consumption at baseline. Such resource injections are large enough to persistently uplift the economic well-being of the poor, do so in noticeable ways to others in these village economies, and have the potential to reduce village consumption and asset inequality.[3]

Eligibility   To establish eligibility, we first conducted a census of 35,522 households in our villages. Each was assigned a 0-100 poverty score based on characteristics proxying household's permanent income. Households with a score of 0-18 are deemed to be poor and hence eligible for the interventions. The interquartile range of poverty scores is 19 to 37, with the highest decile of households scoring above 46. The poverty score construction is similar to that used to target welfare programs to the rural poor in Pakistan, including the Benazir Income Support Programme. This is the most widespread social protection program in Pakistan, reaching nearly five million

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[1]The asset prices shown are indicative and include travel costs to markets. If households chose a combination of assets valued at more than PKR50K they self-finance the excess.

[2]The intervention partners used the same standardized modes of delivery for both treatments. For livestock asset transfers, beneficiaries were accompanied by field partners to local livestock markets. Beneficiaries selected the desired asset, field partners helped ensure quality assets were procured, and to negotiate down prices. Vendors were then paid in cash on the spot. For non-livestock asset transfers, beneficiaries were also assisted by field partners who would typically obtain multiple quotes for assets and then select the lowest price vendor. For households choosing the unconditional cash transfer in T2, bank accounts were simultaneously opened for recipients. Cash recipients were informed they could use the accounts as a saving device, and about the timing of the second tranche of cash. Transfers were made via cheque in private ceremonies.

[3]The value of transfers is in line with earlier evaluations of the economic impacts of asset and cash transfers. On livestock asset transfers, Banerjee *et al.* [2015] present a meta-analysis of such interventions across six countries, with the value of asset transfers being between approximately PPP$437 and PPP$1228. This included one study that was also with our intervention partner, PPAF, but in Sindh province of Pakistan, where the value of asset transfers delivered was $1043. Bandiera *et al.* [2017] offer ultra-poor women in Bangladesh assets and training similar to ours valued at $560. In terms of unconditional cash transfers, Haushofer and Shapiro [2016] evaluate the offer of one-time cash payments ranging from $400 to over $1000.

households in 2012. Households are thus familiar with the kind of poverty score used to determine eligibility. Not treated poor households were made aware by PPAF that the treated poor were randomly selected among eligibles, and given no promise of future treatment. Not poor households were aware they were never going to be eligible.[4]

2.3 Research Design

Randomization, Sampling, Take-Up and Timeline   To assess impacts on economic and non-economic outcomes of beneficiaries and non-beneficiaries, we follow a two-stage randomization design. In the first, we randomly assign villages to T1, T2 or control, stratified by district. At a second stage, in treated villages we randomly assign the offer of treatment among eligible households. Half of those eligible are actually offered treatment. Among the poor in treated villages, we thus distinguish between the treated poor (TP) and the not treated poor (NTP). In addition, we use our village census to draw a random sample of non poor households from across all deciles of poverty scores. We denote non poor households as NP.

We sample 6237 eligible poor households in treated and control villages (around 75% of all poor households): 3052 reside in controls, 1598 are in T1 villages (of which 854 are treated), and 1587 are in T2 villages (942 are treated). We survey 9435 non poor households (around 33% of all the non poor): 3130 reside in controls, 3306 in T1 villages, and 2999 in T2 villages.

In both treatment arms, there is near 100% take-up of the offer of transfers. In T1, 50% of eligibles offered treatment chose some combination of livestock, 22% chose assets to set-up a small-scale retail business, 15% chose assets related to petty trade, and 13% chose other assets. In T2, over 91% of households chose the unconditional cash transfer over any form of in-kind asset transfer. Hence the majority of households in T2 reveal prefer cash over assets.[5]

We conducted our census from May to July 2012, and our baseline survey from February to June 2013. Interventions were rolled out January-March 2014. We focus on the one, two and four-year follow-up surveys that were fielded May to July 2015, September/October 2016, and February/March 2018. Noticeable economic outcomes are measured at the one, two- and four-year follow ups. Perceptions are measured at the two-year midline and four-year endline.

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[4]The poverty score combines information on: (i) the number of dependents aged 18-65; (ii) the highest education level of the household head; (iii) the number of children age 5-16 in school; (iv) the number of rooms per household member; (v) the type of toilet used; (vi) asset ownership (including land and livestock). A weighting scheme within each category then combines to produce scores between 0 and 100.

[5]Given the scale of cash transfers offered, two other design features are relevant. First, after their initial choice, households were giving a two week window to finalize their choice, in case they preferred an alternative bundle after having discussed further with family and neighbors. Nearly all households stuck with their initial choice of cash transfers in T2. Second, the cash transfer is best interpreted as a labelled cash transfer because it is offered in the context of the asset menu presented, and because those taking cash transfers were asked to prepare investment plans. The vast majority stated they intended to use the cash to purchase the kinds of asset offered on the menu lists: very few households reported planning to make investments that were not originally offered, such as using the cash to migrate or invest into schooling.

Balance Table 1 shows village characteristics measured from the census, across treatment arms. Table A1 shows balance when pooling the two treatment arms. On most dimensions the samples are well balanced (whether we pool or split treatment arms). Panel A of Table 1 shows that villages are small, with 400 households in each. The average distance between treated and control villages is 13kms, with travel times to market and state infrastructures such as livestock markets or police stations being around an hour. Panel B focuses on village poverty. The average household poverty score is 29, with the standard deviation of scores across households being just under half the mean. Around 23% of households are classified as poor (and therefore eligible). Of those, around 45% are actually treated (creating the division between the TP and NTP in treated villages).

To reaffirm the potential for others to notice the economic gains to the poor from the interventions, Panel C presents descriptives on the within village locations of the poor. Taking all pairwise distances between households, the median distance between poor and non poor households is one kilometer. Almost the same distance exists between the randomly assigned TP and NTP, suggesting households are not sorted within villages by poverty status. Finally, for the NP, around 30% of households that reside within a 500m radius of their home are poor.

Table 2 shows balance on household characteristics, splitting for the across and within village randomization. Table A2 shows the same test of household balance pooling the two treatment arms. On most dimensions the samples are again well balanced on household characteristics (whether we pool or split the treatment arms). Panel A shows characteristics measured in the census: poor households have a poverty score of 13, while NP households have a score of 34 (there is far more variation in the poverty scores of the NP because they are drawn from across all deciles of poverty). Poor households are larger. Heads of household are nearly always male, aged around 41: in poor households the majority have no formal education, but even among the NP, over 40% have no formal education. 90% of household heads are engaged in some form of income generating labor activity. Panel B shows livestock ownership and consumption at baseline (that are not available for NTP households as they were not surveyed at baseline). Around 55% of poor households in controls own livestock, rising to 64% in non poor households. Monthly food expenditure per adult equivalent is around $80 for the poor, and 20% higher among the non poor.

Attrition Table A3 shows that households are more likely to attrit from treated villages irrespective of the intervention type. Poor households are 4pp to 6pp more likely to attrit from treated than control villages (of whom 5 to 7 percent attrit by endline). These magnitudes are small, in line with comparable studies, and mostly occur in the first year post intervention. In each treatment arm, we cannot reject the null that attrition is the same across all groups between midline and endline (when perceptions are measured). At the four-year endline, we cannot reject the null that attrition in each treatment arm is the same for all groups.

3 Economic Outcomes

3.1 Empirical Method

We begin our analysis by considering the initial link from the economic shock to economic outcomes. These lay the foundations for how perceptions might then be shifted by big push pro-poor interventions. We estimate intervention impacts on the following economic outcomes (y_{hvt}): whether the household owns livestock, the (log) value of livestock owned conditional on ownership, whether the household has an iron roof or has a cement roof (both of which are only measured at one year post-intervention but are durable and irreversible investments), whether the household often consumes home produced milk, and (log) monthly food and non-food expenditures. We do not claim these are the most important dimensions of impact for well-being, but they are relevant for the current study because, by leading to noticeable changes in small village economies, they leave less scope for misperceptions of intervention gains to persist [Alesina *et al.* 2021].[6]

We exploit the within-village randomization to estimate intervention gains, comparing TP and NTP households in treated villages. Such within village comparisons are a less cognitively demanding counterfactual for households to construct than between village comparisons, given the rural poor are typically subject to localized common shocks. We estimate the following within-village specification for household h in village v for period t and treatment j to trace out impacts of each intervention at one-year, the two-year midline and four-year endline:

$$y_{hvt} = \alpha + \sum_{j=1,2} \sum_{t=1,2,4} \beta_{jt} \left(T_{jv} \times W_t \times TP_h \right) + \alpha_t W_t + \lambda_s + u_{hvt}, \tag{1}$$

where TP_h is a dummy for the treated poor (the omitted group are the NTP), W_t are survey waves ($t = 1, 2, 4$), λ_s are district strata, and standard errors are clustered by village.

3.2 Noticeable Impacts

Table 3 shows the results. For the TP relative to the NTP, there are large and sustained treatment effects of each intervention on livestock ownership, the value of livestock owned and consuming own produced milk. The magnitude of impacts are of economic significance: for the TP in T1, livestock ownership increases by 20pp, a 35% increase over the baseline mean for the poor in controls, the value of livestock owned increases by between 10-15% across all periods and interventions, and by the four-year endline, the consumption of own produced milk increases by around 25%.

Two further points are of note. First, gains to the TP relative to the NTP accrue within a year post-intervention, and stabilize thereafter until the four-year endline. The treated poor thus

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[6]Even consumption might be noticeable to others – Alatas *et al.* [2012] document that rural communities in Indonesia have good information about the consumption of other village households, and place some weight on consumption when identifying the poverty of others.

experience a pattern of immediate changes in economic circumstances following the transfer of assets or cash, with gains persisting, but not accumulating further.

Second, both big push interventions have similar impacts: at the foot of table we report p-values of the equality of treatment effects by survey wave. With the exception of livestock ownership – that increases significantly more for those offered in-kind asset transfers in T1 – all other treatment effects do not differ by intervention and period. Hence we pool treatments for the remainder of the analysis. We showed earlier in Tables A1 and A2 that the samples are balanced on village and household characteristics between controls and pooled treated villages and households.

Table 4 repeats the exercise pooling treatments, allowing gains to be more precisely estimated. Across all margins, TP households have significant impacts relative to the NTP and impacts are all sustained four-years post-intervention. The TP have a 16% increase in livestock ownership (corresponding to a 29% increase over baseline), conditional on ownership, the value of livestock owned increases by around 14%, they are 4pp more likely to have an iron roof one year post-intervention (an 11% increase over baseline), 3.2pp more likely to have cement walls (a 16% increase over baseline), are around 20% more likely to have improved diets as measured through the consumption of own produced milk, gains in food consumption of around 3% over baseline (the short run fall in consumption might reflect the switch from market purchased dairy products to home production), and gains in non-food expenditures of 5% over baseline.

Given our focus on whether and how such intervention change perceptions of economic outcomes and attitudes towards the rich and poor, it is useful to examine how these impacts change the ranking of TP households relative to NTP households (that are observationally identical at baseline given the second stage of randomization in treated villages). The 16% increase in the value of livestock owned by the TP corresponds to a TP household moving from the mean (median) to the 72nd (58th) percentile among NTP households; the 3.7% increase in the value of food consumption corresponds to a TP household moving from the mean (median) to the 62nd (55th) percentile among NTP households; the 5% increase in the value of non-food consumption corresponds to a TP household moving from the mean (median) to the 77th (53rd) percentile among NTP households. An alternative way to benchmark the impacts is to consider how their magnitude corresponds to baseline gaps between poor and non-poor households. Using this approach, the 16% increase in the value of livestock owned by the TP corresponds to 33% of baseline gap with the NP at baseline. This all suggests the gains from the intervention to the TP are meaningful, and can thus potentially cause perception shifts.[7]

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[7]We can also contextualize our estimates in relation to the literature on big push interventions. Banerjee *et al.* [2015], in a meta-analysis of asset transfer programs across six countries, report a .258 standard deviation increase in an asset index two years post-intervention – comparable to the effects we observe on livestock ownership. Bandiera *et al.* [2017], study a program providing livestock assets and training to ultra-poor women in Bangladesh, finding an 11% increase in consumption expenditure after four years, larger than our estimated effect. Turning to cash transfer programs, Haushofer and Shapiro [2016] and Egger *et al.* [2022] document increases in food consumption of 19% and 5%, respectively, with the latter being comparable to our findings. Egger *et al.* [2022] also report a 26% increase in asset value, which is broadly consistent with our results.

Spillovers Given scope for economic spillovers in these tight-knit village economies, we document treatment effects on the NTP and NP households by exploiting the between village randomization by estimating the following specification for households in group $g \in \{NTP, NP\}$:

$$y_{hvt}^g = \alpha^g + \sum_{t=1,2,4} \beta_t^g \left(T_v \times W_t\right) + \alpha_t^g W_t + \lambda_s + u_{hvt}^g. \tag{2}$$

We pool both treatments j into T_v and the comparison is with group g households in control villages, λ_s are district strata, and standard errors are still clustered by village.

Table A4 presents the results: we see little evidence that economic outcomes shift for not treated poor or not poor households relative to counterfactuals in controls. The point estimates on many of the estimates are close to zero, suggesting weak within village spillovers on these specific outcomes.[8]

Given that treated and not treated poor households are balanced on observables at baseline and the lack of spillovers, the gains to the TP reinforce the idea that many of the NTP are overtaken by their TP neighbors along these margins. These changes in relative standing will be noticeable given that half of all eligibles in treated villages are actually treated. Changes in relative economic standing can shape some attitudes of the TP and NTP if they have concerns for their relative standing or last place aversion [Duesenberry 1949, Luttmer 2005, Card *et al.* 2012, Kuziemko *et al.* 2014]. The lack of economic spillovers on these margins to NTP and NP households reinforces the idea that any changes in perceptions of non-beneficiary households operate through non-economic channels, such as the demonstration of economic gains to the TP.

3.3 Village Inequality

Our results so far suggest that both big push interventions impact levels of economics outcomes in ways replicating findings in the literature [Banerjee *et al.* 2015, Haushofer and Shapiro 2016, Bandiera *et al.* 2017, Blattman *et al.* 2020, Balboni *et al.* 2022, Egger *et al.* 2022]. As a consequence, the NTP are overtaken in economic standing on a number of important margins. What has been less discussed in the literature is that such interventions can also impact overall levels of village inequality. This is especially the case in our context because villages are small and half the eligible poor, or 10% of all households (40 households per village), are actually treated. To examine the possibility, we estimate the following between village treatment effect on measures

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[8]Consistent with this, in their meta-analysis of asset transfer interventions across six countries, Banerjee *et al.* [2015] report little evidence of within village spillovers in three sites that had within and between village randomization. Repeating the exercise for the treated poor, we find the magnitude of the between village impacts to be very similar to those from the within village estimates. For example, on the likelihood of owning livestock, the between village treatment effects are .143, .163 and .160 at one, two and four years post intervention (and all are statistically significant at the 1% level).

of consumption inequality, I_{vt}, for village v in survey wave t:

$$I_{vt} = \alpha + \sum_{t=1,2,4} \beta_t \left(T_v \times W_t\right) + \alpha_t W_t + \lambda_s + u_{vt}, \tag{3}$$

where our consumption inequality measure is based on the value of adult-equivalent food expenditure, we pool treatments, and robust standard errors are reported.[9]

Table 5 presents the results for three measures of inequality. In line with the dynamic impacts on consumption of the treated poor, reductions in inequality in food expenditure take a few years to materialize, but there are statistically significant reductions in consumption inequality at two- and four-years post intervention. The magnitude of the impacts are also plausible given that 10% of households are treated. On all measures of inequality, we cannot reject equality of impacts at two and four years. Finally, as expected, reductions in village inequality are driven by a rising left tail of the outcome distribution, as can be seen from the 90-10 percentile measure (Column 3). At baseline in controls the value of food expenditure at the 90th percentile is 2.4 times higher than at the 10th percentile, and this falls by .109 (or 5% of the value at baseline in control villages) by the four-year endline.

4   Perceptions

Given this backdrop of big push pro-poor interventions impacting levels, rankings and inequality of economic outcomes, we turn to understand whether these changes feed through to the actual perceptions of household heads (that in 98% of cases are men). To do so, we exploit the between village randomization that enables us to establish impacts on TP, NTP and NP households. We estimate treatment effects using the following specification for heads of household:

$$y_{hvt}^g = \alpha^g + \sum_{t=2,4} \beta_t^g \left(T_v \times W_t\right) + \alpha_t^g W_t + \lambda_s^g + \lambda_e^g + u_{hvt}^g, \tag{4}$$

where y_{hvt}^g is the outcome reported by household head h in group $g \in \{TP, NTP, NP\}$ in village v for period t. We continue to pool interventions, and all other variables are as defined earlier. Given the nature of questions asked about perceptions, we include a full set of dummies for enumerators, λ_e. We cluster standard errors by village.[10]

Standard identifying assumptions for the treatment effects on each group are that there is random assignment, and that there are no spillovers onto controls. The effects on NTP and NP households capture their exposure to the interventions, that can operate through: (i) observing

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[9]To construct village level measures of inequality we re-weight the sample to account for the fact that a random sample of poor and non poor households are tracked at one, two and for years post-intervention, and these sampling weights vary across poor and non poor households and across villages.

[10]There are 134 enumerators with nearly all being used at midline and endline, and the majority operating across treatment and control villages. The median (mean) number of interviews conducted by each is 163 (223).

intervention impacts on the TP and village as a whole; (ii) any changes in their own economic circumstances occurring through spillovers or general equilibrium effects not captured by the margins of noticeable outcomes considered earlier; (iii) any emotional connection with beneficiaries – that is relevant given our setting is one in which there is a close proximity of poor and non poor households and a likely complex set of family and network ties between them.

Throughout we report p-values on each treatment effect, and account for multiple hypothesis testing (MHT) by presenting sharpened two-stage q-values [Benjamini *et al.* 2006, Anderson 2008]. These q-values conservatively account for the fact that for each outcome we test six hypotheses across three groups g at midline and endline $(\widehat{\beta}_2^g, \widehat{\beta}_4^g)$.[11]

4.1 Perceptions of Economic Outcomes

The first set of outcomes we consider are households' perceptions of specific economic outcomes. A basic but largely unanswered question in the literature is whether beneficiary and non-beneficiary households actually recognize the measured changes in levels, rankings and inequality of economic outcomes caused by big push interventions.

4.1.1 Own Economic Standing

We start by examining how households' perceive their own economic standing. As a precursor, we consider whether households are imperfectly informed about their own standing at baseline [Benabou and Ok 2001, Alesina and Angeletos 2005, Cruces *et al.* 2013, Hoy and Mager 2021, Hvidberg *et al.* 2023]. Figure A2 plots the difference between household' true and perceived income rank (grouped into seven bins), against their actual reported monthly income. In line with existing evidence we find that poorer households tend to overestimate their income rank (so those with the lowest monthly income have a positive difference between their true and perceived rank) and the opposite is true for higher income individuals. These misperceptions might make it harder for rich and poor households to recognize stark changes in economic reality caused by big push interventions even in small village economies.

To assess this directly we asked about their perceived standing: *On a ladder with 10 steps, where do you currently stand?* The results are in Table 6 where we show midline and endline impacts for TP, NTP and NP households, estimated from (4). Focusing first on the results for

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[11]Where relevant, we also report results that exploit the within-village randomization, where we estimate treatment effects on the perceptions of TP relative to the NTP in treated villages from the following specification for household h in village v for period t:

$$y_{hvt} = \alpha + \sum_{t=2,4} \beta_t \left(T_v \times W_t \times TP_h\right) + \alpha_t W_t + \lambda_s + \lambda_e + u_{hvt}, \tag{5}$$

where all variables are as defined earlier, we continue to include enumerator fixed effects, and cluster standard errors by village. An advantage of this specification is that it removes village-level unobservables that are common drivers of perceptions of the TP and NTP.

the TP in Column 1a, we see they report no change in their perceived own standing at midline or endline, despite measurable and persistent economic gains from the intervention to them. The 95% confidence interval at midline rules out a change larger than .096, or a 3% change over the baseline level. In contrast, the NTP and NP report significant *falls* in their perceived own standing at midline, with both results being robust to MHT – this is despite there being no change in their absolute standing given the lack of economic spillovers to them.[12]

In short, for the TP, there is a wedge between perceived economic standing and reality, while for the overtaken NTP and TP, their perceptions better reflect their relative rather than absolute standing at midline. The results are in line with findings from higher income settings that individual well-being can fall when individuals observe changes in wealth/income in people around them [Luttmer 2005, Card *et al.* 2012, Perez-Truglia 2020, Cullen and Perez-Truglia 2022]. As such, the results highlight the potential for pro-poor interventions to generate negative psychological spillovers to non-beneficiaries, although households appear to adapt to this by endline. Haushofer *et al.* [2015] are among the few other experimental studies in a low-income setting to study how exogenous changes in the wealth of neighbors impacts psychological wellbeing. They find increases in neighbors' wealth decrease life satisfaction (but with positive effects on the life satisfaction of beneficiaries), and in line with our results, also find evidence of adaptation, in that the negative spillover decreases over time.

4.1.2 Village Inequality

Moving from individual to aggregate economic outcomes, we next ask whether households perceive changes in village level inequality. To examine this we asked household heads whether: (i) inequality in their village has decreased in the last three years; (ii) the share of households in the village that do not have enough to eat has fallen. Given our study context in which the majority of households heads have no formal education, we purposively chose these questions to understand perceptions of inequality, rather than ask households to provide any quantitative assessment of changes in village inequality. The results are in the remaining Columns of Table 6.[13]

We see a near complete set of null impacts across both perceptions of inequality for the TP, NTP and NP. These null impacts are again quite precise at midline and endline. For example, on whether village inequality has decreased, the endline impact for TP households is −.011, where

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[12]To further emphasize the differences between the TP and NTP, Table A5 shows that using the within-village randomization, the TP diverge significantly from the NTP in their own standing, a divergence in perceptions that is sustained until endline. This finding is robust to MHT, and to reiterate, this specification accounts for any village-level unobservables that are common drivers of perceptions of the TP and NTP in treated villages.

[13]The wording of the first question is, *do you think that the difference in income between the few people at the top and most people at the bottom has [...] in the last three years?*, where respondents were presented with five possible answers (has decreased a lot; has decreased a little; has remained the same; has increased a little; has increased a lot). We convert this into a dummy equal to one if the respondent answers decreased a little or decreased a lot. The second outcome asks, *think of the people in your village who do not have enough to eat or sometimes may have to skip meals. Out of every 100 people, how many do you think are in that situation in your village?*.

the 95% confidence interval rules out an impact larger than .053, or 16% of the view held by the TP in controls. On the more explicit margin of others not having enough food to eat, we find generally negative point estimates but these are very small in magnitude and mostly imprecisely estimated. For example, the endline impact for TP households is −.005, and the 95% confidence interval rules out an impact smaller than −.013, or a 14% reduction relative to views held by the TP in controls.[14]

The persistent changes in village consumption inequality documented earlier thus largely do not translate into perceived changes among households of inequality changing in their village, irrespective of whether they are poor or non poor, whether they are beneficiaries, and the time frame considered. Our results build on work – mostly from high-income settings – documenting that individuals misperceive levels of economic inequality [Hauser and Norton 2017, Gimpelson and Treisman 2018]. We demonstrate that such misperceptions persist even in the face of large exogenous shifts in local economic circumstances, and in close knit communities where changes in the economic circumstances of others should be most noticeable.

4.2   Perceptions of Others

We next move beyond perceptions of *specific* economic outcomes – their own standing and village inequality – to *broader* perceptions of entire classes of households. Specifically, we consider views towards the rich and poor, that can underpin a wide array of attitudes, behaviors and social norms.

4.2.1   Perceptions of the Rich

We first examine views on the deservedness of the rich by asking household heads whether they agree/strongly agree with the statement that *the rich rightfully deserve their income*. The result is in Table 7. Around a third of poor and non poor households in controls perceive the rich to be deserving. Columns 1a to 1c show that at midline *all* households in treated villages are significantly more likely hold this view. Relative to counterfactual households in controls, the TP are 7.5pp more likely to move towards this notion of the deserving rich (a 23% increase over controls), with the corresponding impact for the NTP being 5.7pp, and the NP also increase their views of the deserving rich by 7.2pp. This represents a remarkable across the board shift at midline of households viewing the rich as deserving, irrespective of their own beneficiary status.

However as with the earlier results on perceptions of specific economic outcomes, these shifts in view towards the rich do not persist to the four-year endline.

To be clear, we cannot identify which households define as their reference group for the rich. On the one hand, it could be traditional elites in villages and that this reference group is the same across treated and control villages. On the other hand, in treated villages, households might view

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[14]The within-villages estimates confirm that perceptions of village inequality do not significantly differ between the TP and NTP.

the rich as the visible beneficiaries (TP) of the interventions, in which case the view that they rightfully deserve their income may reflect an acknowledgement that these neighbors are using their transfers productivity. To shed light on these alternative, we next consider how households perceive the reason why some households are rich.

Why are the Rich Rich? The remaining Columns of Table 7 examine specific positive and negative perceptions of how the rich in the village achieved their economic status. The positive view is elicited by asking respondents whether they believe the reason for the rich being rich are *education, intelligence or hard work*. The negative view is elicited by asking whether they believe the reason relates to ill-gotten gains through *illegal activities*.

We generally see little impact on positive perceptions of the rich – that runs counter to the idea that in treated villages, households view the TP as the rich. In contrast, negative views towards the rich decline among the poor – by endline the TP are 3.6pp less likely to think the rich are rich because of crime, relative to 11% of the poor holding this view in controls. The NTP share this change in perception: their likelihood to report a negative view of the rich falls 3.0pp by endline. The NP themselves do not have such persistent shifts in views towards the rich.

These findings highlight the value of our experimental design. If we only had data on the TP, the pattern of results could be interpreted as beliefs of beneficiaries being endogenously determined through motivated reasoning: to maintain a positive self-image, the TP become more likely to think the rich are more deserving, and their standing is not attributed to ill gotten gains. Our design however reveals similar changes in beliefs among non-beneficiaries – the NTP – suggesting broader community-wide shifts in perceptions of the poor towards the rich in response to exposure to pro-poor interventions rather than such perceptions shifting through self-serving biases.

4.2.2 Perceptions of the Poor

A natural counterpart to these results is whether perceptions of the poor are shifted by the pro-poor interventions [Andersen *et al.* 2023]. We split the analysis into how exposure to the anti-poverty interventions shift perceptions of the poor, and of the fundamental causes of poverty.

Focusing first on the character of the poor, we asked households whether they thought the poor: (i) *lack the ability to manage money or other assets*; (ii) *waste their money on inappropriate items*; (iii) *do not actively seek to improve their lives*; (iv) *are not motivated because of outside support from government/NGOs*. NP households were only asked these questions at endline. We note that 30-40% of respondents in controls at midline agree/strongly agree with each statement, irrespective of whether they are themselves poor. The strongest agreement is for the view that the poor are not motivated because of outside support from government/NGOs. However, the results in Table 8 show little evidence that these views of the character of the poor are shifted by direct or indirect exposure to the big push pro-poor interventions.

Why are the Poor Poor? We then consider perceptions of what drives poverty: structural features of the economy, or destiny. On structural causes, we asked households whether they thought the poor were poor because: (i) *they are exploited by rich people*; (ii) *society fails to help and protect the most vulnerable*; (iii) *the distribution of land between poor and rich people is uneven/unequal*; (iv) *they lack opportunities due to the fact that they come from poor families*. In each case the outcome is whether the household head agreed or strongly agreed with the statement. Table 9 shows that 70-80% of respondents in controls at midline agree/strongly agree with each statement about the structural causes of poverty, irrespective of whether they are themselves poor. The belief in structural causes of poverty is thus far more prevalent among all households than negative views of the character of the poor.

At midline, the interventions cause significant falls in the view that poverty is driven by structural factors. This holds across all four factors and magnitudes of impact vary between 5pp and 10pp, and with seven out of eight estimates being robust to MHT. However, by endline these treatment effects fade.

On poverty as destiny, we asked households whether they thought the poor were poor because: (i) *they are unlucky*; (ii) *they have encountered misfortunes*; (iii) *they have bad destiny/fate*. Table 10 shows the perception of poverty as destiny is generally less prevalent among controls than the view that poverty is down to structural causes – but for each question, the poor are more likely to hold the view than the non poor. The interventions do little to shift views of poverty as destiny among the TP or NTP. However, among the NP in treated villages, by endline we find significant increases in agreement with the view that the poor are poor because of destiny/fate ($p = .022$, $q = .124$): the magnitude of this shift, 5.2pp, amounts to half the rich-poor gap in perceptions among controls.[15]

5 Discussion

Big push pro-poor interventions hold immense promise for pulling the world's poorest out of poverty [Heil *et al.* 2026]. Our analysis moves beyond the existing evidence base of economic impacts of such interventions, to study their impacts on perceptions of the economic circumstances of households, villages, and views towards the rich and poor more broadly. Our analysis is based on a large-scale and long-term experiment combining layers of between and within village randomization, and tracking treated poor, not treated poor and not poor households. Our data and design thus allow us to go beyond the study of beneficiaries themselves, builder a rich picture of the dynamic and general equilibrium effects of such interventions across economic and non-economic outcomes.

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[15] Andersen *et al.* [2023] use a housing lottery in Ethiopia to study how an increase in wealth affects beliefs of beneficiaries about the causes of poverty. They find lottery winners become more likely to attribute poverty to character traits rather than luck, in line with a self-serving bias.

Against a backdrop of large and persistent actual economic gains to the TP, changes in relative standing of the NTP and reductions in village inequality, we document that these translate into relatively muted changes in perceptions of these specific economic changes. In contrast, more pronounced changes occur in terms of broader perceptions of classes of households: all households are more likely to believe the rich are rightfully deserving and are less likely to view poverty as driven by structural causes.

Our analysis reveals that economic self-interest does not explain our findings – many views of non-beneficiaries are similarly shifted through their exposure to the big push pro-poor interventions. Shifts in perceptions of others largely do not depend on whether an individual is an actual beneficiary of the intervention or not – rather they are driven by common village-wide exposure to pro-poor policies, in line with attitudes being driven by sociotropic concerns that relate to wider community well-being, rather than narrow self-interest. This has been emphasized in the political science literature largely in the context of redistributive preferences [Margalit 2019] and the nascent literature on how policy views are formed [Stantcheva 2024]. *A fortiori*, such interventions do not polarize perceptions – in many cases impacts on the poor and non poor are of the same sign and similar magnitude.

Finally, we note that for most margins of perception, treatment effects fade by the four-year endline. Whether this is because of salience effects of big push interventions, updating and then reversing of beliefs, or the fact that the economic gains to beneficiaries are achieved within a year of the intervention taking place and then plateau so that perceptions are driven by changes in economic outcomes – all remain to be understood.

We conclude by discussing two issues. First, whether non-economic outcomes are shifted in the same way irrespective of the metric of pro-poor transfers: cash or in-kind. Second, study features that are key to the external validity of our findings, and that each represent directions in which to extend our work.

5.1 Asset Transfers versus Revealed Preferred Cash Transfers

We exploit the treatment arms to examine whether in-kind asset transfers and reveal preferred unconditional cash transfers have similar impacts on perceptions. These results are summarized in Figures A3 and A4, in which each panel shows the estimated treatment effect $(\widehat{\beta}_{2j}^{g}, \widehat{\beta}_{4j}^{g})$ for group g and treatment arm j from the between village estimates, and we indicate whenever impacts differ across treatment arms at conventional levels of statistical difference. Treatment T1 refers to when the poor are offered a menu of in-kind asset transfers. Treatment T2 refers to when households are additionally offered the equivalent valued cash transfers, and the majority reveal prefer cash over in-kind transfers.

We find little evidence that perceptions of own standing, village inequality, and views of the rich and poor, are differentially impacted depending on the metric of transfers. Figure A3 focuses

on perceptions of own standing and village inequality, so the outcomes from Table 6. The estimates are largely the same across treatment arms, for each group of households, and across both midline and endline estimates. Figure A4 summarizes perceptions of the rich and poor, so outcomes from Tables 7 to 10. Shifts in the 14 perceptions largely do not differ depending on whether the poor are provided asset transfers, or reveal prefer cash over in-kind transfers. Perceptions on which the metric of transfers matters are: (i) that the rich are rich for positive reasons such as education/hard work, where this shift at endline is greater among the TP and NTP if the poor are provided asset transfers ($p = .012, .064$ respectively); (ii) that the poor are poor because they do not actively seek to improve their lives, where the shift at midline is greater among the TP and NTP if the poor are provided asset transfers ($p = .099, .045$ respectively).

5.2 Future Agenda and External Validity

Our results suggest a broad agenda for future work on how exposure to economic interventions translate into perceptions. A natural extension might be to supplement interventions with information provision to households – for example aiming to correct misperceptions that drive a wedge between economic reality and perceptions of specific economic outcomes [Cruces *et al.* 2013, Kuziemko *et al.* 2015]. This might be effective given our study emphasizes how the real-world demonstration of positive impacts of the transfers is not enough to correct such misperceptions, even in small village economics in which the benefits to beneficiaries are observable to all. Moreover, in our context, the fact that beneficiary and non-beneficiary households reside next to each other and are likely tied through social or economic networks might play an important role in how reality maps into perceptions.

We highlight three other areas for future work based on dimensions of our data that are likely critical for thinking through the external validity of our findings.

Setting Villages in our field experiment are close-knit and ethnically homogeneous. However, in more geographically dispersed settings, economic impacts on beneficiaries might not be so noticeable. Alternatively, in more diverse or ethnically fragmented settings, perceptions of targeting biases, or actual targeting biases of local delivery agents across groups, might be first order [Londono-Velez 2022, Bandiera *et al.* 2023]. It thus remains an open question to understand whether in such settings, pro-poor interventions are more likely to lead to polarization in perceptions or conflict than we find in our setting.

Financing Interventions Our results suggest the link between pro-poor policy interventions, economic reality and perceptions does not depend on whether households are themselves beneficiaries. Rather, these non-economic outcomes are largely driven by common village-wide exposure to such pro-poor policies. However, the big push interventions studied are financed and delivered

by a quasi-governmental NGO – they are not financed through general taxation, nor through informal local taxation. The rich (non eligibles) might be impacted very differently by pro-poor interventions when they are implicitly financing them or when they come at the expense of some other policy or local public good they favor. It remains an open question to understand how such outcomes across households might be shifted when within-village redistributive institutions, such as local taxation schemes, are used to target resources to the poor.

The Design of Social Protection Systems We have examined the non-economic impacts of one-off big push policies in the form of asset or cash transfers. However, social protection systems are designed not only to redistribute resources but also to provide social insurance. As such, a very rich policy space exists including small and frequent transfers, conditional cash transfers, universal transfers (such as UBI), indirect transfers (such as minimum wages), or insurance against shocks to earnings, health, crop failure etc. [Banerjee *et al.* 2024]. While a large literature exists to understand the economic impacts of transfers in-kind versus in cash, as well as political economy arguments in favor of one form of transfer over another, much less is known about how the design of social protection more broadly impacts perceptions of the poor and non poor. Developing an agenda along these lines would help fill knowledge gaps related to the origins of the demand for social protection, the sustainability of social protection systems, and most broadly, the link between exposure to economic policies and how households view the world around them.

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Table 1: Balance on Village Characteristics

Means, standard deviation in braces, p-values in brackets

	(1) Control	(2) T1: Asset Transfer	(3) T2: Revealed Preferred Unconditional Cash Transfer	C = T1	C = T2	T1 = T2
Number of villages	30	29	29			
Panel A: Village Aggregates						
Village size (number of households)	403	440	368	[.482]	[.541]	[.207]
	(180)	(271)	(199)			
Nearest control village (km)	14.3	11.1	12.9	[.135]	[.632]	[.491]
	(9.96)	(5.98)	(12.6)			
Travel time to nearest livestock market (mins)	67.0	64.0	74.3	[.641]	[.452]	[.289]
	(32.4)	(40.1)	(44.3)			
Travel time to nearest police station (mins)	52.7	53.4	55.9	[.895]	[.781]	[.692]
	(34.4)	(33.4)	(38.3)			
Panel B: Poverty						
Average poverty score (0-100) of households	29.2	30.6	29.0	[.193]	[.993]	[.178]
	(4.77)	(3.79)	(4.31)			
Standard deviation of poverty score of households	13.6	13.6	13.2	[.926]	[.322]	[.378]
	(2.43)	(2.43)	(2.24)			
Share of households that are eligible (poor)	.248	.202	.240	[.025]	[.558]	[.127]
Share of poor households that are treated (TP)	-	.447	.450	-	-	[.993]
Panel C: Within Village Locations of the Poor						
Median distance between:						
Poor and not poor households (km)	1.00	1.02	.951	[.740]	[.756]	[.598]
	(.580)	(.511)	(.632)			
Treated poor and not treated poor households (km)	-	.979	.884	-	-	[.500]
	-	(.556)	(.561)			
Share of poor households living within a 500m radius of not poor households	.303	.280	.310	[.490]	[.909]	[.501]

Notes: Columns 1, 2, and 3 show sample means and standard deviations (in parentheses for continuous variables) for each village characteristic as measured in the census. The p-values on the tests of equality are derived from OLS regressions of the corresponding village characteristic on a treatment dummy variable, and district fixed effects. Robust standard errors are estimated. In Panel B, the household poverty score combines information on: (i) the number of dependents aged 18-65; (ii) the highest education level of the household head; (iii) the number of children age 5-16 in school; (iv) the number of rooms per household member; (v) the type of toilet used; (vi) asset ownership (including land and livestock). A weighting scheme within each category then combines to produce scores household poverty between 0 and 100. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions.

Table 2: Balance on Household Characteristics

Means, standard deviation in parentheses, p-values in brackets

	Control		T1: Asset Transfer			T2: Revealed Preferred Unconditional Cash Transfer			Treated Poor			Not Treated Poor			Non Poor		
	(1) P	(2) NP	(3) TP	(4) NTP	(5) NP	(6) TP	(7) NTP	(8) NP	C = T1	C = T2	T1 = T2	C = T1	C = T2	T1 = T2	C = T1	C = T2	T1 = T2
Panel A. Household Characteristics (census)																	
Poverty score (1-100)	13.1	34.2	13.6	13.6	34.3	13.4	13.6	33.8	[.050]	[.221]	[.610]	[.133]	[.929]	[.258]	[.946]	[.815]	[.772]
	(3.91)	(12.6)	(3.54)	(3.72)	(11.9)	(3.84)	(3.71)	(12.0)									
Household size	7.63	5.07	7.60	7.60	4.93	7.58	7.60	5.07	[.802]	[.489]	[.752]	[.820]	[.407]	[.347]	[.837]	[.839]	[.726]
	(2.32)	(2.53)	(2.09)	(2.05)	(2.42)	(2.16)	(2.05)	(2.45)									
Female headed household	.018	.026	.010	.018	.024	.020	.018	.027	[.106]	[.705]	[.075]	[.859]	[.645]	[.487]	[.664]	[.948]	[.565]
Age of household head	41.4	42.5	41.6	40.9	41.9	41.5	40.9	42.0	[.924]	[.861]	[.935]	[.781]	[.496]	[.737]	[.818]	[.566]	[.762]
	(12.2)	(15.8)	(12.3)	(12.0)	(15.6)	(12.4)	(12.0)	(15.6)									
Household head has no formal education	.549	.433	.529	.538	.412	.586	.538	.418	[.174]	[.848]	[.121]	[.280]	[.537]	[.556]	[.569]	[.789]	[.744]
Household head is currently working	.931	.893	.934	.927	.908	.936	.927	.891	[.761]	[.432]	[.741]	[.453]	[.208]	[.552]	[.404]	[.851]	[.294]
Panel B. Household Welfare (baseline)																	
Own any livestock	.542	.638	.572		.607	.556		.605	[.450]	[.757]	[.650]				[.518]	[.285]	[.757]
Monthly food expenditure (AE, US$ PPP)	82.1	98.7	82.7		100	84.6		99.5	[.304]	[.085]	[.608]				[.516]	[.748]	[.651]
	(35.8)	(45.4)	(35.1)		(45.1)	(37.1)		(42.9)									
Non food expenditure (pc, US$ PPP)	18.1	28.0	18.2		29.7	19.8		30.5	[.641]	[.076]	[.215]				[.454]	[.194]	[.604]
	(13.4)	(24.3)	(15.2)		(28.9)	(15.2)		(29.2)									

Notes: Columns 1 to 8 show sample means and standard deviations (in parentheses for continuous variables) for each household characteristic, as measured in the census or at baseline. The p-values on the tests of equality are derived from OLS regressions of the corresponding household characteristic on a treatment dummy variable, and district fixed effects. Standard errors are clustered by village. In Panel A, the household poverty score combines information on: (i) the number of dependents aged 18-65; (ii) the highest education level of the household head; (iii) the number of children age 5-16 in school; (iv) the number of rooms per household member; (v) the type of toilet used; (vi) asset ownership (including land and livestock). A weighting scheme within each category then combines to produce scores between 0 and 100. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. In Panel B, food expenditures include cereal grains, meat, vegetables, dairy, oils, major condiments, food at ceremonies, and meals away from home or bought for visitors. We use the OECD adult equivalence scale of 1+(0.7*(number of adults-1))+(0.5*number of children). Non-food expenditures include fuel, cosmetics, toiletries, entertainment, transportation, electricity and salaries for maids, and is measured in per capita terms. All monetary values are in 2012 US$.

Table 3: Noticeable Economic Impacts

Within Village Estimates Treated Poor vs Not Treated Poor
Standard errors clustered by village in parentheses

	(1) Own Livestock	(2) Log (Value Livestock) | Own Livestock	(3) Iron Roof	(4) Cement Walls	(5) Often Consume Own Produced Milk	(6) Log (Monthly Food Expenditure)	(7) Log (Monthly Non Food Expenditure)
Treatment 1: Asset Transfer							
One year impact	.211***	.133*	.034	.052**	.082**	-.015	-.072
	(.027)	(.078)	(.029)	(.022)	(.032)	(.027)	(.049)
Two year impact	.231***	.157**			.113***	.022	-.007
	(.023)	(.060)			(.028)	(.017)	(.039)
Four year impact	.190***	.107**			.087***	.032	.032
	(.024)	(.053)			(.029)	(.021)	(.034)
Treatment 2: Revealed Preferred Unconditional Cash Transfer							
One year impact	.102**	.153*	.048	.010	.038	-.036	-.027
	(.043)	(.083)	(.046)	(.019)	(.036)	(.031)	(.053)
Two year impact	.138***	.138**			.086***	.028*	.034
	(.022)	(.057)			(.022)	(.016)	(.038)
Four year impact	.131***	.139**			.053**	.042*	.068*
	(.025)	(.060)			(.022)	(.024)	(.036)
Mean (poor, controls at baseline)	.563	2837	.360	.202	.328	83.7	19.0
p-values:							
T1=T2 (one year)	[.042]	[.867]	[.837]	[.187]	[.398]	[.687]	[.553]
T1=T2 (two year)	[.006]	[.835]			[.511]	[.814]	[.494]
T1=T2 (four year)	[.101]	[.741]			[.428]	[.810]	[.505]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	10784	6601	2340	2340	10785	10700	10684

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions utilize the sample of treated poor and not treated poor households within treated villages. All regressions include treatment dummies (for T1 and T2 separately), district (strata) and survey wave fixed effects. Standard errors are clustered by village. In Columns 3 and 4, having an iron roof or cement wall are only measured one year post-intervention. In Column 6, food expenditures include cereal grains, meat, vegetables, dairy, oils, major condiments, food at ceremonies, and meals away from home or bought for visitors. We use the OECD adult equivalence scale of 1+(0.7*(number of adults-1))+(0.5*number of children). In Column 7, non-food expenditures include fuel, cosmetics, toiletries, entertainment, transportation, electricity and salaries for maids, and is measured in per capita terms. All monetary values are in 2012 US$. At the foot of each Column we report p-values on tests of equality of treatment effects between T1 and T2 at one, two and four years post intervention.

Table 4: Noticeable Economic Impacts, Pooled Specification

Within Village Estimates Treated Poor vs Not Treated Poor
Standard errors clustered by village in parentheses

	(1) Own Livestock	(2) Log (Value Livestock) | Own Livestock	(3) Iron Roof	(4) Cement Walls	(5) Often Consume Own Produced Milk	(6) Log (Monthly Food Expenditure)	(7) Log (Monthly Non Food Expenditure)
One year impact	.160***	.142**	.040**	.032**	.061***	-.025*	-.051
	(.024)	(.055)	(.016)	(.014)	(.023)	(.014)	(.034)
Two year impact	.184***	.148***			.099***	.025**	.014
	(.016)	(.038)			(.015)	(.011)	(.024)
Four year impact	.160***	.123***			.069***	.037***	.050**
	(.017)	(.031)			(.015)	(.013)	(.023)
Mean (poor, controls at baseline)	.563	2837	.360	.202	.328	83.7	19.0
p-values:							
One year = Two year	[.329]	[.928]			[.117]	[.004]	[.095]
Two year = Four year	[.181]	[.548]			[.083]	[.346]	[.229]
One year = Four year	[.997]	[.742]			[.708]	[.002]	[.017]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	10784	6601	2340	2340	10785	10700	10684

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions utilize the sample of treated poor and not treated poor households within treated villages. All regressions include treatment dummies (pooling T1 and T2), district (strata) and survey wave fixed effects. Standard errors are clustered by village. In Columns 3 and 4, having an iron roof or cement wall are only measured one year post-intervention. In Column 6, food expenditures include cereal grains, meat, vegetables, dairy, oils, major condiments, food at ceremonies, and meals away from home or bought for visitors. We use the OECD adult equivalence scale of 1+(0.7*(number of adults-1))+(0.5*number of children). In Column 7, non-food expenditures include fuel, cosmetics, toiletries, entertainment, transportation, electricity and salaries for maids, and is measured in per capita terms. All monetary values are in 2012 US$. At the foot of each Column we report p-values on tests of equality of treatment effects at one, two and four years post intervention.

Table 5: Village Consumption Inequality

Between Village Estimates Treated vs Controls
OLS estimates, robust standard errors in parentheses

	(1) SD (log)	(2) Gini	(3) p90-10
One year impact	-.002	-.001	.018
	(.011)	(.006)	(.079)
Two year impact	-.037***	-.013**	-.184***
	(.012)	(.006)	(.065)
Four year impact	-.016*	-.009*	-.109*
	(.008)	(.005)	(.056)
Mean (controls, baseline)	.340	.188	2.37
p-values:			
One year = Two year	[.036]	[.151]	[.050]
Two year = Four year	[.156]	[.551]	[.387]
One year = Four year	[.321]	[.317]	[.191]
Strata Fixed Effects	Yes	Yes	Yes
Number of observations	264	264	264

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. The unit of observation is the village-survey wave. To construct village level measures of inequality we re-weight the sample to account for the fact that a random sample of poor and non poor households are tracked at one, two and for years post-intervention, and these sampling weights vary across poor and non poor households and across villages. All regressions include treatment dummies (pooling T1 and T2), district (strata) and survey wave fixed effects. Robust standard errors are estimated. Food expenditures include cereal grains, meat, vegetables, dairy, oils, major condiments, food at ceremonies, and meals away from home or bought for visitors. We use the OECD adult equivalence scale of 1+(0.7*(number of adults-1))+(0.5*number of children). All monetary values are in 2012 US$. At the foot of each Column we report p-values on tests of equality of treatment effects at one, two and four years post intervention.

Table 6: Own Standing and Village Inequality

Between Village Estimates (Treated vs Control)
OLS estimates, standard errors clustered by village in parentheses
p-values in brackets, FDR adjusted q-values in braces

	On a ladder with 10 steps, where do you currently stand?			Inequality decreased in the last three years			Share in village that do not have enough to eat		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP	(3a) TP	(3b) NTP	(3c) NP
Two year impact	-.119	-.206**	-.539***	.037	.011	.002	-.013	-.012	-.024**
	(.108)	(.097)	(.105)	(.031)	(.033)	(.027)	(.009)	(.009)	(.011)
	[.274]	[.036]	[.000]	[.236]	[.737]	[.934]	[.187]	[.186]	[.031]
	{.437}	{.099}	{.001}	{1.00}	{1.00}	{1.00}	{.453}	{.453}	{.229}
Four year impact	.050	-.048	-.126	-.011	-.008	-.011	-.005	-.002	-.004
	(.128)	(.139)	(.122)	(.032)	(.032)	(.028)	(.004)	(.005)	(.006)
	[.699]	[.729]	[.304]	[.744]	[.813]	[.700]	[.318]	[.619]	[.533]
	{.839}	{.839}	{.437}	{1.00}	{1.00}	{1.00}	{.598}	{.598}	{.598}
Mean Outcome, Controls	2.78		3.34	34.0%		38.8%	9.05%		10.8%
Two Year = Four Year	[.387]	[.429]	[.021]	[.378]	[.749]	[.711]	[.473]	[.405]	[.165]
Observations	8126	9382	17001	8126	9382	17004	8126	9382	17004

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a, 3a), Not Treated Poor (Columns 1b, 2b, 3b), and Not Poor (Columns 1c, 2c, 3c) households in treatment and control villages. All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave level, and 95% confidence intervals are reported in brackets. For the first outcome, respondents were shown a picture of a ladder and were told, "The top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you." We then asked "On which step of the ladder would you say you personally feel you stand at this time?" The wording for the second outcome is ""Do you think that the difference in income between the few people at the top and most people at the bottom has [...] in the last three years?" where respondents were presented with five possible answers (has decreased a lot; has decreased a little; has remained the same; has increased a little; has increased a lot). We convert this into a dummy equal to one if the respondent answers "decreased a little" or "decreased a lot." The final outcome asks "Think of the people in your village who do not have enough to eat or sometimes may have to skip meals. Out of every 100 people, how many do you think are in that situation in your village?". At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention.

Table 7: The Rich

Between Village Estimates (Treated vs Control)
OLS estimates, standard errors clustered by village in parentheses
p-values in brackets, FDR adjusted q-values in braces

	The rich rightfully deserve their income			Reason rich: education, intelligence, hard work			Reason rich: illegal activities		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP	(3a) TP	(3b) NTP	(3c) NP
Two year impact	.075***	.057*	.072***	-.005	.011	-.021	-.014	-.015	-.022**
	(.032)	(.030)	(.027)	(.022)	(.019)	(.015)	(.015)	(.015)	(.010)
	[.021]	[.062]	[.010]	[.838]	[.557]	[.170]	[.351]	[.323]	[.031]
	{.064}	{.091}	{.064}	{.786}	{.786}	{.579}	{.267}	{.267}	{.110}
Four year impact	-.017	.005	-.001	.028	.036*	.012	-.036**	-.030*	-.001
	(.030)	(.031)	(.025)	(.022)	(.019)	(.019)	(.016)	(.015)	(.011)
	[.563]	[.876]	[.976]	[.220]	[.060]	[.533]	[.033]	[.058]	[.932]
	{.731}	{.954}	{.954}	{.579}	{.563}	{.786}	{.110}	{.110}	{.728}
Mean Outcome, Controls	32.3%		31.0%	30.0%		33.5%	11.2%		11.0%
Two Year = Four Year	[.060]	[.327]	[.061]	[.268]	[.377]	[.168]	[.419]	[.533]	[.166]
Observations	8126	9382	17004	8126	9382	17004	8126	9382	17004

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a, 3a), Not Treated Poor (Columns 1b, 2b, 3b), and Not Poor (Columns 1c, 2c, 3c) households in treatment and control villages. All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered by village, and 95% confidence intervals are reported in brackets. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention.

Table 8: Character of the Poor

Between Village Estimates (Treated vs Control)
Strongly agree or agree with statements
OLS estimates, standard errors clustered by village in parentheses
p-values in brackets, FDR adjusted q-values in braces

	They lack the ability to manage money or other assets			They waste their money on inappropriate items			They do not actively seek to improve their lives			They are not motivated because of outside support from government/NGOs		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP	(3a) TP	(3b) NTP	(3c) NP	(4a) TP	(4b) NTP	(4c) NP
Two year impact	.030	.059*		.008	.036		.018	.033		.007	.014	
	(.030)	(.034)		(.030)	(.032)		(.036)	(.034)		(.039)	(.040)	
	[.321]	[.088]		[.804]	[.254]		[.608]	[.325]		[.854]	[.725]	
	{1.00}	{.786}		{1.00}	{1.00}		{1.00}	{1.00}		{1.00}	{1.00}	
Four year impact	-.021	-.004	-.004	-.003	.006	-.011	.006	.015	-.001	.008	-.004	.008
	(.026)	(.027)	(.019)	(.030)	(.032)	(.024)	(.032)	(.030)	(.021)	(.030)	(.029)	(.020)
	[.423]	[.891]	[.831]	[.919]	[.850]	[.657]	[.863]	[.629]	[.950]	[.805]	[.902]	[.700]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Mean Outcome, Controls	.330		.256	.357		.348	.362		.333	.400		.413
Two Year = Four Year	[.289]	[.247]		[.839]	[.585]		[.830]	[.743]		[.995]	[.768]	
Observations	7505	8502	8039	7537	8551	8089	7527	8530	8065	7271	8195	7757

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a, 3a, 4a), Not Treated Poor (Columns 1b, 2b, 3b, 4b), and Not Poor (Columns 1c, 2c, 3c, 4c) households in treatment and control villages. All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered by village, and 95% confidence intervals are reported in brackets. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention.

Table 9: Poverty as Driven by Structural Causes

Between Village Estimates (Treated vs Control)
Strongly agree or agree with statements
OLS estimates, standard errors clustered by village in parentheses
p-values in brackets, FDR adjusted q-values in braces

	They are exploited by rich people			Society fails to help and protect the most vulnerable			The distribution of land between poor and rich people is uneven /unequal			They lack opportunities due to the fact that they come from poor families		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP	(3a) TP	(3b) NTP	(3c) NP	(4a) TP	(4b) NTP	(4c) NP
Two year impact	-.052*	-.062**		-.075**	-.093***		-.067**	-.062**		-.057**	-.101***	
	(.028)	(.024)		(.030)	(.031)		(.028)	(.030)		(.026)	(.026)	
	[.068]	[.011]		[.014]	[.004]		[.017]	[.041]		[.029]	[.000]	
	{.158}	{.059}		{.029}	{.021}		{.093}	{.093}		{.062}	{.001}	
Four year impact	-.000	-.017	-.026	-.026	-.023	-.027	-.011	-.017	-.007	-.013	-.035	-.012
	(.025)	(.025)	(.023)	(.025)	(.025)	(.020)	(.025)	(.026)	(.022)	(.022)	(.023)	(.017)
	[.995]	[.499]	[.265]	[.310]	[.361]	[.165]	[.659]	[.513]	[.739]	[.553]	[.142]	[.484]
	{.792}	{.599}	{.361}	{.277}	{.277}	{.198}	{.797}	{.797}	{.797}	{.311}	{.166}	{.311}
Mean Outcome, Controls	.795		.767	.796		.751	.807		.762	.803		.756
Two Year = Four Year	[.252]	[.308]		[.324]	[.159]		[.238]	[.375]		[.282]	[.105]	
Observations	7522	8530	8065	7403	8353	7842	7375	8302	7816	7440	8411	7937

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a, 3a, 4a), Not Treated Poor (Columns 1b, 2b, 3b, 4b), and Not Poor (Columns 1c, 2c, 3c, 4c) households in treatment and control villages. All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered by village, and 95% confidence intervals are reported in brackets. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention.

Table 10: Poverty as Destiny

Between Village Estimates (Treated vs Control)
Strongly agree or agree with statements
OLS estimates, standard errors clustered by village in parentheses
p-values in brackets, FDR adjusted q-values in braces

	They are unlucky			They have encountered misfortunes			They have bad destiny/fate		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP	(3a) TP	(3b) NTP	(3c) NP
Two year impact	-.036	-.012		-.054	-.048		-.040	-.038	
	(.036)	(.037)		(.034)	(.036)		(.035)	(.032)	
	[.318]	[.741]		[.116]	[.186]		[.257]	[.248]	
	{.737}	{1.00}		{.870}	{.870}		{.413}	{.413}	
Four year impact	.006	.031	.045*	.012	.016	.023	.027	.015	.052**
	(.028)	(.027)	(.025)	(.028)	(.027)	(.023)	(.026)	(.026)	(.022)
	[.827]	[.267]	[.080]	[.680]	[.555]	[.315]	[.292]	[.574]	[.022]
	{1.00}	{.737}	{.667}	{.870}	{.870}	{.870}	{.413}	{.575}	{.124}
Mean Outcome, Controls	.484		.417	.489		.395	.391		.285
Two Year = Four Year	[.452]	[.458]		[.239]	[.243]		[.214]	[.334]	
Observations	7518	8532	8040	7426	8399	7926	7526	8535	8006

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a, 3a), Not Treated Poor (Columns 1b, 2b, 3b), and Not Poor (Columns 1c, 2c, 3c) households in treatment and control villages. All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered by village, and 95% confidence intervals are reported in brackets. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention.

Table A1: Balance on Village Characteristics

Means, standard deviation in braces, p-values in brackets

	(1) Control	(2) Treated	C = T
Number of villages	30	58	
Panel A: Village Aggregates			
Village size (number of households)	403	404	[.918]
	(180)	(238)	
Nearest control village (km)	14.3	12.0	[.299]
	(9.96)	(9.82)	
Travel time to nearest livestock market (mins)	67.0	69.1	[.856]
	(32.4)	(42.2)	
Travel time to nearest police station (mins)	52.7	54.6	[.928]
	(34.4)	(35.6)	
Panel B: Poverty			
Average poverty score (0-100) of households	29.2	28.9	[.489]
	(4.77)	(4.10)	
Standard deviation of poverty score of households	13.6	13.4	[.542]
	(2.43)	(2.32)	
Share of households that are eligible (poor)	.248	.221	[.119]
Share of poor households that are treated (TP)	-	.448	-
Panel C: Within Village Locations of the Poor			
Median distance between:			
Poor and not poor households (km)	1.00	.988	[.971]
	(.580)	(.571)	
Treated poor and not treated poor households (km)	-	.930	-
	-	(.556)	
Share of poor households living within a 500m radius of not poor households	.303	.295	[.701]

Notes: Columns 1 and 2 show sample means and standard deviations (in parentheses for continuous variables) for each village characteristic as measured in the census. The p-values on the tests of equality are derived from OLS regressions of the corresponding village characteristic on a treatment dummy variable, and district fixed effects. Robust standard errors are estimated. In Panel B, the household poverty score combines information on: (i) the number of dependents aged 18-65; (ii) the highest education level of the household head; (iii) the number of children age 5-16 in school; (iv) the number of rooms per household member; (v) the type of toilet used; (vi) asset ownership (including land and livestock). A weighting scheme within each category then combines to produce scores household poverty between 0 and 100. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions.

Table A2: Balance on Household Characteristics

Means, standard deviation in parentheses, p-values in brackets

	Control		Treated			Treated Poor	Not Treated Poor	Non Poor
	(1) P	(2) NP	(3) TP	(4) NTP	(5) NP	C = T	C = T	C = T
Panel A. Household Characteristics (census)								
Poverty score (1-100)	13.1	34.2	13.5	13.3	34.1	[.055]	[.340]	[.944]
	(3.91)	(12.6)	(3.70)	(3.84)	(11.9)			
Household size	7.63	5.07	7.59	7.56	4.99	[.578]	[.733]	[.950]
	(2.32)	(2.53)	(2.12)	(2.14)	(2.43)			
Female headed household	.018	.026	.015	.019	.026	[.602]	[.834]	[.823]
Age of household head	41.4	42.5	41.5	40.9	42.0	[.873]	[.594]	[.657]
	(12.2)	(15.8)	(12.4)	(12.1)	(15.6)			
Household head has no formal education	.549	.433	.559	.541	.414	[.531]	[.305]	[.611]
Household head is currently working	.931	.893	.935	.920	.901	[.517]	[.174]	[.668]
Panel B. Household Welfare (baseline)								
Own any livestock	.542	.638	.563		.606	[.551]		[.337]
Monthly food expenditure (AE, US$ PPP)	82.1	98.7	83.7		99.8	[.135]		[.581]
	(35.8)	(45.4)	(36.1)		(44.0)			
Non food expenditure (pc, US$ PPP)	18.1	28.0	19.0		30.1	[.179]		[.253]
	(13.4)	(24.3)	(15.2)		(29.0)			

Notes: Columns 1 to 5 show sample means and standard deviations (in parentheses for continuous variables) for each household characteristic, as measured in the census or at baseline. The p-values on the tests of equality are derived from OLS regressions of the corresponding household characteristic on a treatment dummy variable, and district fixed effects. Standard errors are clustered by village. In Panel A, the household poverty score combines information on: (i) the number of dependents aged 18-65; (ii) the highest education level of the household head; (iii) the number of children age 5-16 in school; (iv) the number of rooms per household member; (v) the type of toilet used; (vi) asset ownership (including land and livestock). A weighting scheme within each category then combines to produce scores between 0 and 100. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. In Panel B, food expenditures include cereal grains, meat, vegetables, dairy, oils, major condiments, food at ceremonies, and meals away from home or bought for visitors. We use the OECD adult equivalence scale of 1+(0.7*(number of adults-1))+(0.5*number of children). Non-food expenditures include fuel, cosmetics, toiletries, entertainment, transportation, electricity and salaries for maids, and is measured in per capita terms. All monetary values are in 2012 US$.

Table A3: Attrition

Dependent variable: household attrits
Standard errors clustered by village in parentheses

	Treated Poor (1)	Not Treated Poor (2)	Not Poor (3)
Treatment 1: Asset Transfer			
One year	.048***	.066***	.081***
	(.008)	(.008)	(.009)
Two year	.040***	.007	.088***
	(.009)	(.010)	(.008)
Four year	.047***	.002	.092***
	(.007)	(.010)	(.007)
Treatment 2: Revealed Preferred Unconditional Cash Transfer			
One year	.038***	.068***	.060***
	(.008)	(.008)	(.008)
Two year	.060***	.005	.088***
	(.008)	(.012)	(.008)
Four year	.062***	-.007	.090***
	(.009)	(.013)	(.008)
Strata Fixed Effects	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes
Attrition rate:			
One year	.051	.021	.075
Two year	.066	.072	.098
Four year	.073	.081	.097
p-values:			
T1=T2 (one year)	[.357]	[.366]	[.085]
T1=T2 (two year)	[.096]	[.896]	[.973]
T1=T2 (four year)	[.170]	[.520]	[.871]
T1 (one year)=T1 (two year)	[.300]	[.000]	[.378]
T1 (two year)=T1 (four year)	[.411]	[.516]	[.648]
T2 (one year)=T2 (two year)	[.011]	[.000]	[.000]
T2 (two year)=T2 (four year)	[.741]	[.133]	[.737]
Observations	11392	10446	37576

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. All regressions include treatment dummies (for T1 and T2 separately), district (strata) and survey wave fixed effects. Standard errors are clustered by village. The dependent variable is a dummy variable indicating attrition. Household controls include a dummy for whether the household head has any formal education, the age of the household head, household size, and the household poverty score. At the foot of each Column we report p-values on tests of equality of treatment effects between T1 and T2 at one, two and four years post intervention.

Table A4: Spillovers onto Not Treated Poor and Not Poor Households, Pooled Specification

Between Village Estimates: Treatment vs Control
Standard errors clustered by village in parentheses

	Not Treated Poor							Not Poor				
	(1) Own Livestock	(2) Log (Value Livestock) | Own Livestock	(3) Iron Roof	(4) Cement Walls	(5) Often Consume Own Produced Milk	(6) Log (Monthly Food Expenditure)	(7) Log (Monthly Non Food Expenditure)	(8) Own Livestock	(9) Log (Value Livestock) | Own Livestock	(10) Often Consume Own Produced Milk	(11) Log (Monthly Food Expenditure)	(12) Log (Monthly Non Food Expenditure)
One year impact	-.020	.003	.065	.050*	-.006	-.012	.033			.003	-.057	-.028
	(.039)	(.149)	(.051)	(.026)	(.046)	(.050)	(.079)			(.041)	(.036)	(.073)
Two year impact	-.028	-.044			-.049	.022	-.024	-.056*	-.014	-.036	.070***	-.068
	(.034)	(.098)			(.045)	(.025)	(.067)	(.031)	(.061)	(.028)	(.018)	(.056)
Four year impact	-.007	-.110			-.026	-.038	-.034	-.030	-.064	-.005	-.025	-.037
	(.037)	(.098)			(.045)	(.035)	(.049)	(.033)	(.058)	(.032)	(.024)	(.046)
Mean (poor, controls at baseline)	.563	2837	.360	.172	.328	83.7	18.1					
Mean (not poor, controls at baseline)								.638	4213	.421	98.7	28.0
p-values:												
One year = Two year	[.828]	[.609]			[.200]	[.527]	[.331]	[.081]		[.245]	[.001]	[.553]
Two year = Four year	[.401]	[.219]			[.402]	[.045]	[.884]	[.202]	[.317]	[.178]	[.000]	[.563]
One year = Four year	[.713]	[.203]			[.572]	[.675]	[.470]	[.365]		[.805]	[.412]	[.903]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	12325	6704	2666	2666	12326	12220	12233	17021	9317	22141	21744	21382

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions utilize the sample of not treated poor and not poor households within treated villages to examine within village spillovers. All regressions include treatment dummies (pooling T1 and T2), district (strata) and survey wave fixed effects. Standard errors are clustered by village. In Columns 3 and 4, having an iron roof or cement wall are only measured one year post-intervention - and is not measured for the not poor. In Columns 6 and 11, food expenditures include cereal grains, meat, vegetables, dairy, oils, major condiments, food at ceremonies, and meals away from home or bought for visitors. We use the OECD adult equivalence scale of 1+(0.7*(number of adults-1))+(0.5*number of children). In Columns 7 and 12, non-food expenditures include fuel, cosmetics, toiletries, entertainment, transportation, electricity and salaries for maids, and is measured in per capita terms. All monetary values are in 2012 US$. At the foot of each Column we report p-values on tests of equality of treatment effects at one, two and four years post intervention.

Table A5: Perceptions of Economic Outcomes

Within Village Estimates (Treated Poor vs Not Treated Poor)
OLS estimates, standard errors clustered by villa
p-values in brackets, FDR adjusted q-values in braces

|	Perception of Current Standing	Perceptions of Village Inequality	
	(1) Current: On a ladder with 10 steps, where do you currently stand?	**(2) Inequality decreased in the last three years**	**(3) Share in village that do not have enough to eat**
Two year impact	.121***	.018	-.001
	(.045)	(.017)	(.004)
	[.009]	[.329]	[.902]
	{.010}	{1.00}	{1.00}
Four year impact	.135***	-.012	-.002
	(.050)	(.020)	(.002)
	[.009]	[.549]	[.254]
	{.010}	{1.00}	{1.00}
Mean Outcome, Controls	2.78	34.0%	9.05%
Two Year = Four Year	[.840]	[.243]	[.764]
Observations	8126	8126	8126

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor and Not Treated Poor households within treated villages (Columns 1, 2, 3). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave level, and 95% confidence intervals are reported in brackets. For the first outcome, respondents were shown a picture of a ladder and were told, "The top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you." We then asked "On which step of the ladder would you say you personally feel you stand at this time?" The second and third outcomes measure individuals' perceptions of village inequality. The outcome in Column 2 is ""Do you think that the difference in income between the few people at the top and most people at the bottom has [...] in the last three years?" where respondents were presented with five possible answers (has decreased a lot; has decreased a little; has remained the same; has increased a little; has increased a lot). We convert this into a dummy equal to one if the respondent answers "decreased a little" or "decreased a lot." The third outcome asks "Think of the people in your village who do not have enough to eat or sometimes may have to skip meals. Out of every 100 people, how many do you think are in that situation in your village?". At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention.

Figure A1: Stylized Example of an Asset Menu

Livestock	Retail	Crop Farming	Non-Livestock Production
Goat Raising (One Goat @ 15k)	Grocery Shop (material up to 50k)	Cultivation of cotton (seeds 20k + fertilizer 15k)	Tailoring (Sewing machine 6k + table 4k)
Dairy Farming (One Cow @ 48K)	Fruit Stall (Stall @ 5k + Fruit up to 45k)	Pesticides @ 50k	
Calf Rearing (One Calf @ 25k)	General Store @ 50k		
Fodder @ 50k	Barber Shop @ 35k		
Veterinary Medical Store @ 50k	Carpenter Shop @ 30k		
Animal Breeding Shop @ 40k	Cycle Repairing Shop @ 35k		

Notes: The figure presents a stylized example of an asset list that households were shown in both treatment arms. Households were allowed to choose any combination of assets they desired, up to a total value of PKR50K.

# Figure A2: Misperceptions of Own Standing
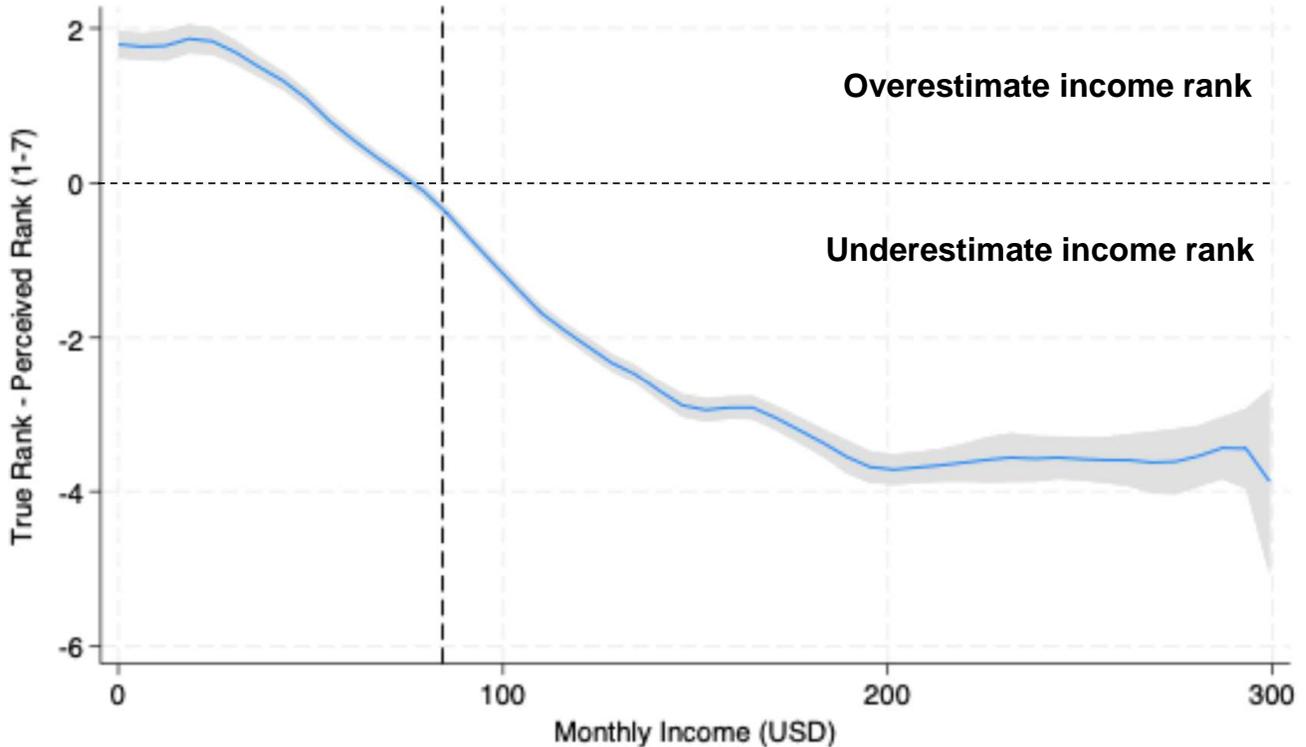
Notes: The Figure plots the difference of individuals true and perceived rank against their monthly income in USD. Both the true and perceived ranking are scored from 1 to 7, where 1 indicates the highest ranking and 7 the lowest. A negative difference thus indicates the respondent underestimates their income rank, while a positive difference indicates that the respondent overestimates their income rank.
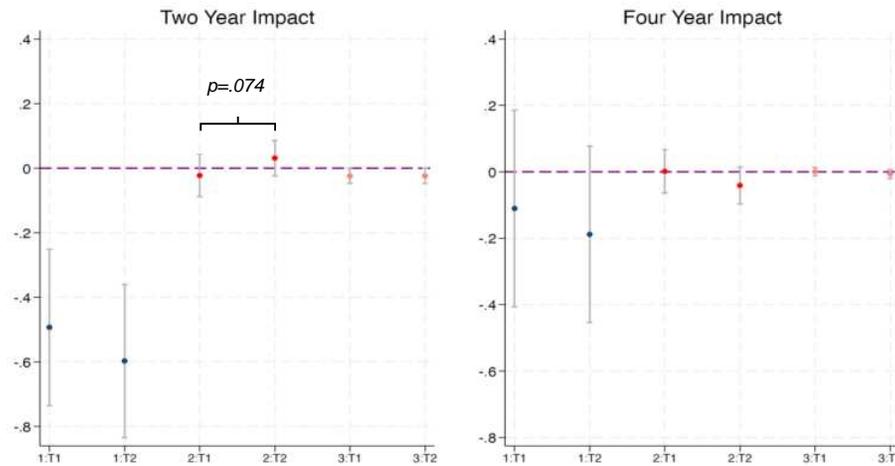
Figure A3: Perceptions, Asset versus Cash Transfers

A. Treated Poor

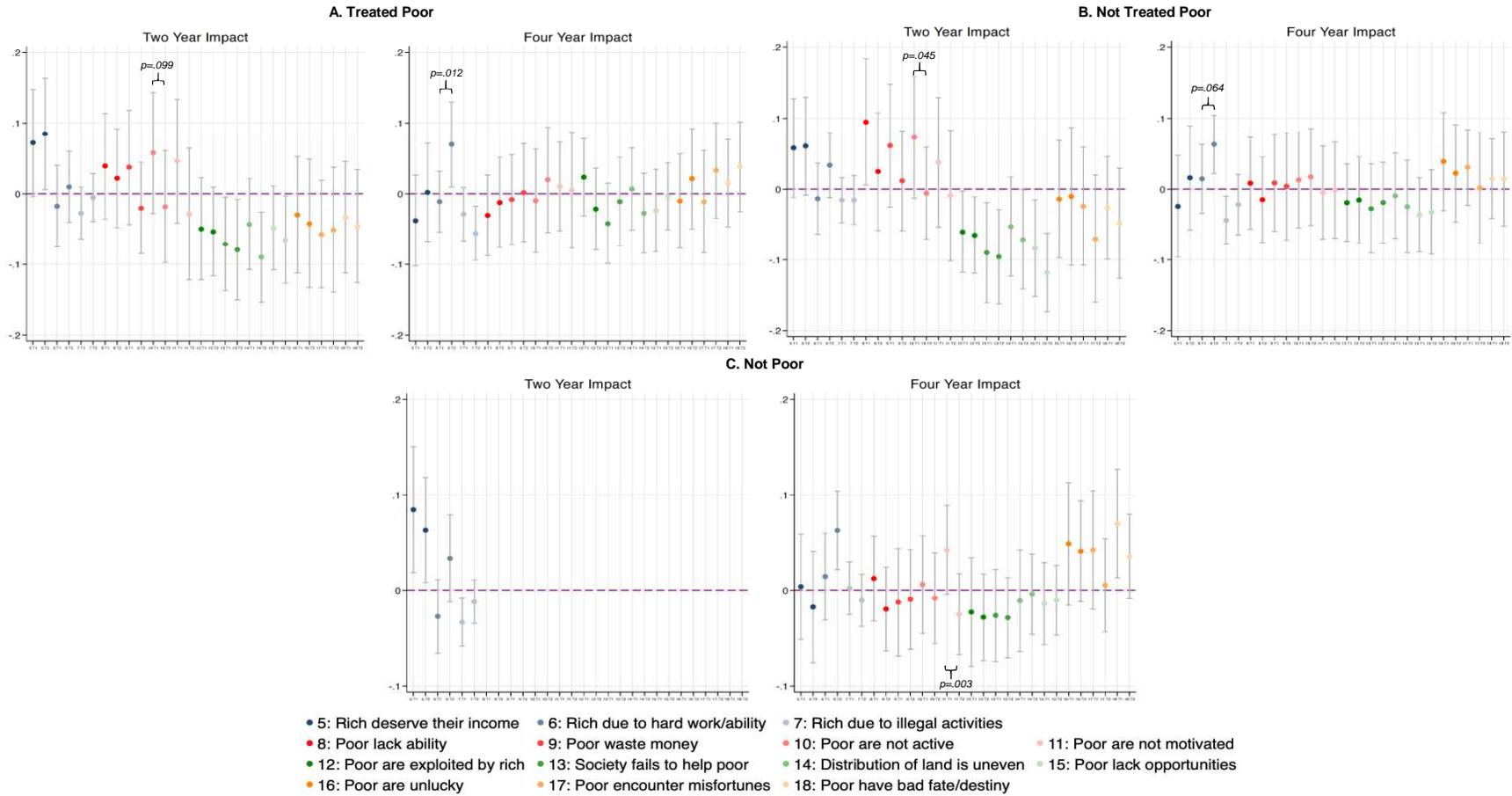## B. Not Treated Poor
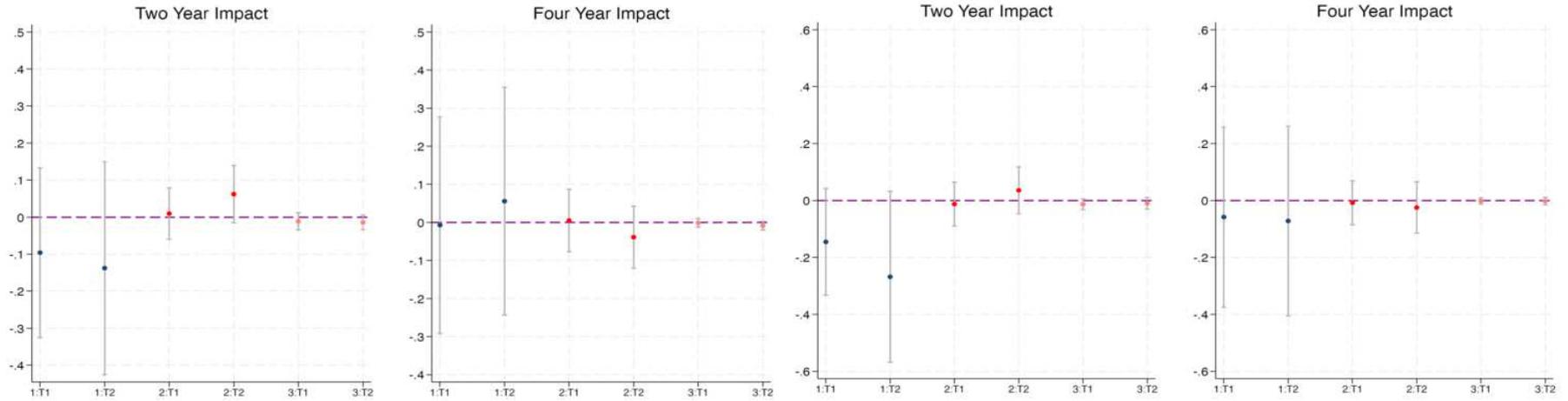
C. Not Poor

● 1: Current standing   ● 2: Inequality decrease   ● 3: Not enough to eat

Notes: Panel A (B) [C] displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households]. For each specification we report the treatment effects for T1 and T2. The outcomes are the three perceptions of economic standing reported in Table 6 and the two perceptions of inequality reported in Table 6. Wherever treatment effects differ across arms, we report the p-value on the null of equality of treatment effects.

Figure A4: Perceptions of the Rich and Poor, Asset versus Cash Transfers

A. Treated Poor

Two Year Impact — p=.099

Four Year Impact — p=.012

B. Not Treated Poor

Two Year Impact — p=.045

Four Year Impact — p=.064

C. Not Poor

Two Year Impact

Four Year Impact — p=.003

Legend:
- 5: Rich deserve their income
- 6: Rich due to hard work/ability
- 7: Rich due to illegal activities
- 8: Poor lack ability
- 9: Poor waste money
- 10: Poor are not active
- 11: Poor are not motivated
- 12: Poor are exploited by rich
- 13: Society fails to help poor
- 14: Distribution of land is uneven
- 15: Poor lack opportunities
- 16: Poor are unlucky
- 17: Poor encounter misfortunes
- 18: Poor have bad fate/destiny

Notes: Panel A (B) [C] displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households]. For each specification we report the treatment effects for T1 and T2. The outcomes are the three perceptions of the rich reported in Table 7, the four perceptions of the poor reported in Table 8, views on the four structural causes of poverty reported in Table 9, and views on the three views on poverty as destiny or fate reported in Table 10 (that are not all available for not poor households at midline). Wherever treatment effects differ across arms, we report the p-value on the null of equality of treatment effects.