Big Push Pro-poor Policies and Economic Circumstances: Reality, Perceptions and Attitudes*

Nicolas Cerkez Adnan Q.Khan Imran Rasul Anam Shoaib

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Abstract

Are large and persistent changes in economic circumstances caused by big push pro-poor policies actually perceived by households, and do they result in changed attitudes or voting behaviors? We study the issue using a partial population experiment tracking $15,000$ rural households in Punjab, 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-off unconditional cash transfer. Within treated villages, we randomize which of the poor receive the transfer. We track treated poor, not treated poor and not poor households over four years. The interventions cause the treated poor to have large and persistent economic gains, and lead to persistent reductions in village consumption inequality. Perceptions of poor and non poor households are shifted similarly by the interventions, but these impacts are more muted than measurable changes in economic standing and village inequality. Most impacts on perceptions – of current standing, village inequality, and views towards rich and poor classes more generally – also fade four years post-intervention. The wedge between economic reality and perceptions means that redistributive attitudes of households remain inelastic to exposure to these interventions. Finally, although the interventions increase political participation, this does not differ by political affinity. Our results highlight that even in small close-knit village economies, the experience or demonstration of welfare enhancing big push anti-poverty policies is unlikely to alter households' perceptions of economic outcomes or for them to become advocates for such interventions. *JEL: O12.*

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1 Introduction

The last few decades have witnessed a steady rise in programs providing direct transfers to the poor [Banerjee *et al.* 2022]. Among the most successful forms such interventions have taken are big push in-kind or cash transfers. 119 low-income countries now having implemented unconditional cash transfer programs, and in-kind livestock asset transfers being 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].[1]

This paper goes beyond the study of economic impacts, to understand whether the changed economic circumstances caused by such big push policies are actually perceived by households, and whether they result in changed attitudes or voting behaviors. This helps shed light on a fundamental issue of whether those that benefit or experience effective pro-poor policies in their communities, recognize their effectiveness on the kinds of economic outcomes that evaluations focus on. If so, this can spark individuals and communities benefitting from welfare enhancing and cost effective interventions to potentially advocate for them, starting a causal chain of demand for good anti-poverty policies.

We examine the issue using a large-scale and long-term randomized control trial, where the pro-poor interventions take the form of either high-valued in-kind asset transfers or equivalent valued unconditional cash transfers. We use a partial population experiment tracking $15,000$ households for four years in small, close-knit villages in rural Pakistan. We consider how these pro-poor interventions change economic circumstances: the level of economic outcomes of beneficiaries, changes in the relative economic standing of near poor non-beneficiaries, and changes in levels of village inequality. The core of our analysis examines how these changes in economic circumstances translate into how the poor and non poor perceive their economic standing in their village, what has happened to inequality in their village, and how they perceive the rich and poor more generally. Given that perceptions, not just actual circumstances, matter for redistributive preferences [Alesina *et al.* 2012, Cruces *et al.* 2013, Alesina *et al.* 2018], at a final stage we consider how exposure to the big push policies translate into attitudes towards redistribution and voting behaviors.

For both big push interventions considered, eligibility was determined by households lying below a poverty threshold and identified as poor. In a first treatment arm, poor households in a

[1] The choice between in-kind and cash transfers has long been discussed. Cash transfers are more efficient in the presence of perfect markets and standard decision making, because it is always possible to perfectly replicate outcomes from in-kind transfers using cash [Atkinson and Stiglitz 1976]. Arguments for in-kind transfers include: they generate greater positive externalities [Coate *et al.* 1994], they provide access to certain goods as a right [Besley 1988], they can be easier to target given incomplete information on who is poor [Akerlof 1978, Nichols and Zeckhauser 1982], paternalism towards the poor [Musgrave 1959], or endorsement effects [Benhassine *et al.* 2015].

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 asset transfers, households were also 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 considered big push interventions in the sense that the value of transferred assets or cash is very high relative to the value of baseline assets or wealth 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 households reveal prefer cash over asset transfers.

Our evaluation covers 88 villages in rural southern Punjab. These villages are small, comprising 400 households on average. Hence, economic gains accruing to the poor are noticeable to others, leaving little apparent scope for misperceptions of the intervention gains or their distributional impacts to persist.

Our field experiment follows 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 actual 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). This design allows us to evaluate the causal impacts of the interventions on beneficiaries (TP), impacts on those overtaken in economic standing (NTP) and wider spillovers to those never eligible (NP).

We randomly sample 75% of poor households in treated and control villages. This covers 6237 households: 3052 reside in control villages, 1598 are in T1 villages (of which 854 are treated), and 1587 are in T2 villages (of which 942 are treated). Following a partial population experiment design, we draw a random sample of non poor (never eligible) households from all deciles of baseline household poverty scores. We survey 9435 non poor (NP) households (around 33% of all non poor households): 3130 reside in controls, 3306 in T1 villages, and 2999 in T2 villages.

We exploit the within and between village randomizations to trace the dynamic economic impacts of these interventions, and the evolution of perceptions and attitudes by tracking households two-years post intervention (midline) and four-years post intervention (endline).

On the impacts of the interventions on economic circumstances, we first document large and persistent gains on noticeable economic outcomes for the TP – those margins most noticeable to others in the village. For example, using the within-village randomization we document 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. 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%.

As treated and not treated poor households are balanced on observables at baseline, the magnitudes of these gains imply that many of the NTP are overtaken by their treated poor neighbors. These changes in relative standing can shape the perceptions and attitudes 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 statistically significant reductions in village level consumption inequality two- and four-years post intervention. These changes in local economic inequality, if perceived, can also alter economic attitudes across households.

Finally, we note that both big push interventions have similar impacts on noticeable economic outcomes over time. Hence we pool treatments T1 and T2 for the remainder of the analysis. We later confirm impacts on perceptions and attitudes do not substantively differ depending on whether the TP receive asset or cash transfers.

Given this backdrop of changes in economic circumstances in treated villages, the core of our analysis exploits our partial population experiment to understand whether and how these interventions shift perceptions and economic attitudes across the TP, NTP and NP. We do so among household heads, that are nearly always male (for their spouses, we collected only a subset of perception and attitudinal measures).

Our long run partial population experiment design reveals four core insights.

First, perceptions are shifted by big push economic interventions targeting the poor, but these impacts are far more muted than measurable changes in economic standing and village inequality. Most impacts on perceptions fade four years post-intervention, despite far more persistent changes in economic circumstances. For example, the TP – direct beneficiaries of the interventions – have little change in perception of their current 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.

Second, we find exposure to the big push interventions has more pronounced changes at midline in perceptions towards the rich and poor more generally. In particular, all households in treated villages perceive the rich to be more deserving. We further examine perceptions of how the rich in the village attained their economic status. While we find little impact on positive perceptions towards the rich, negative views towards the rich decline across groups. More precisely, by endline the TP are 3.6pp less likely to think the rich are rich because of ill-gotten gains through illegal activities, relative to 11% of the poor holding this view in controls. Households do not change their views about the character of the poor, but TP and NTP households both change their views of the causes of poverty – at midline they are significantly less likely to view poverty as being driven by structural factors that the poor are helpless against, such as exploitation by the rich, society failing to help them, the unequal distribution of land, or a lack of opportunities.

The wedge between economic reality and perceptions can be a reason why redistributive attitudes remain inelastic to these real-world big push interventions, even in small tight-knit village economies [Alesina *et al.* 2012, Alesina *et al.* 2018]. Our third set of results examine this directly, considering how changed economic circumstances and perceptions translate into attitudes towards redistribution. While there are many potential ways to measure redistributive preferences, we anchor our results by following the influential work of Kuziemko *et al.* [2015], to construct the same index of redistributive attitudes based on views related to whether the rich should give part of their income to the poor, how windfall gains should be treated, concerns over societal inequality, and on the deservedness of the rich.

We find households hold more redistributive attitudes on the first component of the index, when asked, *should the rich give part of their income to the poor?* Although the vast majority agree with this statement in controls, we find: (i) at midline, the NTP and NP nudge forward in being more likely to hold this view. The magnitude of impacts is 2.0pp for the NTP and 3.0pp for the NP ($p = .043$, .018 respectively); (ii) at endline, the TP nudge forward on this view by 1.6pp ($p = .052$). However, this effect towards more pro-redistributive attitudes is offset by another component of the index – perceptions towards the rich – that shift at midline in a direction that makes households hold less redistributive attitudes. Overall, we find little shift in the index of redistributive attitudes of any group in either time period. For example, among the TP at midline we can rule an increase in the redistributive attitudes index greater than .105 or 3% of its baseline level in controls.

Finally, we consider whether such big push interventions have more persistent impacts through increased engagement of households with political processes. We probe this using self-reported data on past voting – between baseline and midline high stakes local elections were held in our study region. We find all groups become significantly more likely to report voting in these elections: the TP are 5.8pp more likely to vote, and the NTP are 5.1pp more likely – both impacts significant at the 1% level. However, the largest point estimate increase is among the NP (9.2pp). To examine whether vote shares for political parties might be swayed by the interventions, we exploit the fact that at baseline, we asked TP and NP households their affinities with political party platforms. We use this information to classify them as left-leaning, centrist or right-leaning. We find household heads of all political affinities significantly increase their likelihood to vote. Among the TP the largest effects are among left- and right-leaning households, although the impacts are not significantly different. Among the NP, the largest point estimate is for right-leaning households (11.4pp) but again these are not different from impacts on left-leaning households ($p = .208$). Overall the evidence suggests that although effective pro-poor interventions increase political participation, this does not differ by political affinities expressed at baseline.

Our work has implications for two sets of literatures that have not been closely connected in prior work. We first extend work evaluating pro-poor interventions, taking a first step in mapping the large and persistent impacts on economic circumstances of big push interventions, to more muted and temporary shifts in households' perceptions of these changes. We do so in terms of household heads perceptions of current and future economic standing, village inequality, and views of the rich and poor more generally. The partial population experiment reveals that all groups – the TP, NTP and NP – do alter their perceptions at midline in response to big push interventions. This is despite the very different intervention impacts on 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. Yet at the same time we find little evidence of persistent changes in perceptions of economic circumstances, despite long-lasting impacts on actual economic circumstances.

Inevitably, given the novelty in empirically linking these types of outcomes to exposure to big push pro-poor interventions, there is far less guidance from theory on how beneficiary and non-beneficiary households could respond. Without developing a formal theory, we try to offer potential explanations on these links throughout, and view our findings as opening a broader agenda to formally model whether and how exposure to policy interventions can impact perceptions of economic outcomes and views towards other classes.

Second, we contribute to long-standing debates over what shapes redistributive preferences – where theory offers far more guidance on what shapes such preferences, stemming back to the seminal work of Meltzer and Richard [1981]. We discuss that body of work as we present findings from our field experiment. Our analysis builds on much of the earlier evidence that is based on lab experiments [Fisman *et al.* 2007, Fisman *et al.* 2021], non-experimental studies on how such attitudes are impacted by job loss, home ownership and welfare receipt [Margalit 2013, Fisman *et al.* 2015, Margalit 2019, Andersen *et al.* 2023], and a burgeoning body of work using survey experiments to understand how redistributive attitudes are shaped by information about the extent of inequalities, or one's position in the income distribution [Ciani *et al.* 2021, Stantcheva 2022].

We extend this body of work by examining how attitudes are shaped by real world big push interventions, using a large-scale and long-term field experiment that reveals whether and how attitudes differentially shift among beneficiaries of pro-poor interventions, those whose relative economic standing falls because of the interventions, and wealthier never eligible households. We show attitudinal shifts do not depend on whether the poor are assisted in cash or in-kind, nor do they depend on whether an individual is an actual beneficiary of the intervention or not – rather they are driven by common village-wide exposure to such pro-poor policies. Our experiment thus addresses a key issue in the wider 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 – as has been emphasized in the political science literature largely in the context of redistributive preferences [Margalit 2019].

Drawing together these contributions, our work shows that there is a wedge between the reality of changed economic circumstances and perceptions among those benefitting from or experiencing effective pro-poor policies in their communities. The demonstration of welfare enhancing and cost effective anti-poverty policies is unlikely to prompt households to become advocates for such interventions, or start a causal chain of demand for good and more effective anti-poverty policies. The demand for good anti-poverty policies might then need to be founded in roots other than those who benefit or experience such policies – for example the presentation of evidence to policy makers directly [Hjort *et al.* 2021].

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 and economic attitudes are shifted by the interventions. Section 5 discusses impacts on voting, differential impacts of cash and asset transfers, external validity and directions for future work. The Appendix presents additional results and checks.

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

The interventions we study take two forms. The first offered households productive assets in-kind. To determine the menu of assets to offer, in each village we initially conducted an assessment of assets likely to generate high returns. These typically 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 in-kind asset transfers, households were offered training providing skills to run a micro-enterprise, 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.[2]

[2] The asset prices shown are indicative and include travel costs to markets. For livestock, actual asset values depend on the age and breed of the animal. If households chose a combination of assets valued at more than PKR50K they self-finance the excess.

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.[3]

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 shift forward levels of economic well-being of the poor, do so in noticeable ways to others in these small village economies, and they have the potential to reduce village consumption and asset inequality.[4]

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, that we collected in the census. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The interquartile range of poverty scores is 19 to 37, with the highest decile of households having a score above 46. The poverty score construction is similar to that used to target welfare programs to the rural poor in Pakistan, including the prominent Benazir Income Support Programme. This is the most widespread social protection program in Pakistan, reaching nearly five million households in 2012. Households are thus familiar with the kind of poverty score construction used to determine eligibility. Not treated poor households were given no promise of future treatment. Not poor households were aware they were never going to be eligible.[5]

[3]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.

[4]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.

[5]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.

2.3 Research Design

Randomization We follow a two-stage randomization design. In the first, we randomly assign villages to T1, T2 or control. Randomization is stratified by district. At a second stage, within treated villages, we randomly assign the actual 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).

Sampling We sample 6237 eligible poor households in treated and control villages (so 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 use our 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 survey 9435 non poor households in total (so around 33% of all non poor households): 3130 reside in controls, 3306 in T1 villages, and 2999 in T2 villages.

Take-Up In both treatment arms, there is near 100% take-up of the offer of transfers. In T1, 50% of eligibles chose some combination of livestock, 22% chose assets to set-up a small-scale retail business, and 15% chose assets related to petty trade. 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.[6]

Timeline We conducted our household census from May to July 2012, and our baseline household survey from February to June 2013. Interventions were rolled out January-March 2014. In this paper 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 respectively. Noticeable economic outcomes are measured at the one, two- and four-year follow ups. Perceptions and economic attitudes are measured at the two-year midline and four-year endline.

Balance Table 1 shows samples are balanced on 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

[6] 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.

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.

As the intervention is delivered by a quasi-government agency, Panel C shows attitudes towards the government, NGOs and the private sector. Pre-intervention, only a quarter of households think government is effective, with similar attitudes expressed towards NGOs and the private sector. Only 20% of households think the government represents people like them, but a slightly higher share believe that people can affect government policies.

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 and attitudes 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 Circumstances

3.1 Empirical Method

To lay the foundations for how perceptions and economic attitudes are shifted by these kinds of big push pro-poor intervention, we estimate intervention impacts on a subset of 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 (that is only measured at one year post-intervention but is a durable and irreversible investment), whether the household often consumes home produced milk, and (log) monthly food expenditure. We do not claim these are the most important dimensions of impact for well-being, but they are more relevant for the current study because, by leading to highly noticeable changes in small village economies, they potentially leave less scope for misperceptions of intervention gains to persist [Alesina *et al.* 2021], and thus can drive changes in perceptions and attitudes.

We exploit the within-village randomization to estimate intervention gains, comparing TP and NTP households in treated villages. Such within village comparisons are 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:

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

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 other points are of note. First, gains to the TP relative to the NTP accrue within a year post-intervention, and stabilize thereafter until endline. The treated poor thus 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 for the purpose of studying economic preferences, 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 estimated more precisely in each wave. We find that across all margins, TP households have significant impacts relative to the NTP. The TP have a 16% increase in livestock ownership (corresponding to a 29% increase over baseline), 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), are around 20% more likely to have improved diets as measured through the consumption of own produced milk, and have 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).

Given the scope for potential spillovers, we also document treatment effects on the NTP and NP households by exploiting the between village randomization by estimating the following specification for households in group $g \in \{NTP, NP\}$:

$$y^g_{hvt} = \alpha^g + \sum\nolimits_{t=1,2,4} \beta^g_t \left(T_v \times W_t\right) + \alpha^g_t W_t + \lambda_s + u^g_{hvt}. \tag{2}$$

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 spillover 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.[7]

Given that treated and not treated poor households are balanced on observables at baseline and the lack of spillovers onto others, the magnitudes of the gains to the TP imply 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].

[7] 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).

3.3 Village Inequality

Our results so far that big push interventions impact levels of economics outcomes closely replicate the earlier 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 of consumption inequality, I_{vt}, for village v in survey wave t:

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

where our consumption inequality measure is based on the value of adult-equivalent food expenditure, we pool treatments, and robust standard errors are reported.[8]

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 and Attitudes

Given this backdrop of big push pro-poor interventions having causal impacts on changes in levels, rankings and inequality of economic outcomes, we now turn to understanding how these changes in economic circumstances feed through to shift perceptions and attitudes of household heads (that in 98% of cases are men). To do so, we exploit both the between and within village randomizations.

Focusing first on the between village randomization, we estimate treatment effects on the perceptions of the TP, NTP and NP using the following specification for heads of household in

[8] 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.

group $g \in \{TP, NTP, NP\}$:

$$y^g_{hvt} = \alpha^g + \sum\nolimits_{t=2,4} \beta^g_t \left(T_v \times W_t\right) + \alpha^g_t W_t + \lambda^g_s + \lambda^g_e + u^g_{hvt}, \tag{4}$$

where y^g_{hvt} is the perception of household head h in village v for period t. These outcomes relate to how they perceive their own economic standing in their village, what has happened to inequality in their village, and how they perceive the rich and poor more generally. 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.[9]

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 the perceptions of the NTP and NP capture their exposure to the pro-poor interventions, that can operate through them: (i) observing intervention impacts on the TP and village outcomes as a whole; (ii) any changes in their own economic circumstances occurring through spillovers or general equilibrium effects; (iii) any emotional connection with beneficiaries. As we come back to in our concluding discussion, all these channels are likely relevant given the close proximity of poor and non poor households and the likely complex set of family and economic network ties between them.

Exploiting the within-village randomization, 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:

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

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

Throughout we report p-values on treatment effects at midline and endline, and also account for multiple hypothesis testing (MHT) by also 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 eight hypotheses, six related to the between village estimates $(\widehat{\beta}^g_2, \widehat{\beta}^g_4)$ across group g at midline and endline, and two related to the within-village estimates $(\widehat{\beta}_2, \widehat{\beta}_4)$.

4.1 Perceptions of Current and Future Standing

Current Standing Motivated by an existing literature using non-experimental data to document households are imperfectly informed about their own relative standing [Benabou and Ok

[9] 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).

2001, Alesina and Angeletos 2005, Hoy and Mager 2021, Hvidberg *et al.* 2023], we start by examining how households' perceived own current economic standing is impacted by the interventions. This is perhaps the most closely linked perception to the reality of changed economic circumstances for the TP. We consider their perceived current standing by asking, *On a ladder with 10 steps, where do you currently stand?* The results are in Table 6 where Panel A shows midline and endline impacts for TP, NTP and NP households as estimated from the between village specification (4). Panel B shows midline and endline impacts on the TP using the within village specification (5). Focusing first on the results for 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 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]. 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. Panel B highlights that within-village, 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.[10]

Future Standing Motivated by the literature emphasizing that perceived prospects for upward mobility (POUM) can shape redistributive demands [Piketty 1995, Benabou and Ok 2001, Fong 2001, Alesina and La Ferrara 2005, Alesina *et al.* 2018], we next consider whether exposure to big push interventions affects household perceptions of their *future* economic standing. We did so by asking household heads: *On a ladder with 10 steps, what is the best life you can achieve?* We estimate whether views of future standing across groups are impacted by the pro-poor interventions. The results are in the remaining Columns of Table 6. As Column 2a shows, the interventions have no impact on beneficiaries perceived social mobility. This is not true for the other groups. For the overtaken NTP in treated villages, by endline they have significantly higher expectations for their future than the poor in controls ($p = .037, q = .421$). For the NP the results differ again: they have significant declines in their future expected standing at midline, although these recover significantly by endline.

[10] 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 also find increases in neighbors' wealth decrease life satisfaction (but with positive effects on the life satisfaction of beneficiaries), and also find evidence of adaptation, in that the negative spillover decreases over time.

4.2 Perceptions of Village Inequality

We next ask whether households perceive the changes in village level inequality caused by the big push interventions. 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. The results are in Table 7.[11]

Panel A shows 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. For example, on whether village inequality has decreased, the endline impact for TP households is $-.011$, where 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 noticeable margin of others not having enough food to eat, we find generally negative point estimates but these are not significant except for the NP at midline. The endline impact for TP households is $-.005$, and the 95% confidence interval rules out an impact larger than .005, or 6% of the view held by the TP in controls.

Panel B confirms that within villages, perceptions of village inequality do not significantly differ between the TP and NTP.

The measurable and persistent changes in village consumption inequality documented earlier thus largely do not translate into perceived changes among households of how inequality has changed in their village, irrespective of whether they are poor or non poor, irrespective of whether they are beneficiaries of these big push pro-poor interventions, and irrespective of 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] – to demonstrate that such misperceptions persist even in the face of large shifts in local economic circumstances.

4.3 Perceptions of the Rich

We have so far mapped the economic impacts of the interventions – through changes in the level, relative standing and inequality of outcomes across households – to perceptions of these changes. We now move to consider perceptions towards groups of households more widely: this goes beyond social preferences towards others, but rather the deservedness of the rich, and the causes of their status. More precisely, we first examine whether exposure to the interventions impacts how households perceive the rich. To do so we asked whether *the rich rightfully deserve*

[11] The exact 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?*.

their income, where the outcome is whether the household head agreed/strongly agreed with the statement. Around a third of poor and non poor households in controls perceive the rich to be deserving. The result in Columns 1a to 1c of Table 8 shows 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 for the NP we find a 7.2pp increase in this notion of the deserving rich.

Why are the Rich Rich? We probe the issue further in the remaining Columns of Table 8 by examining positive and negative opinions 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*. While we generally see little impact on positive perceptions towards the rich, in contrast, negative views towards the rich decline across groups – 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 belief: their likelihood to report a negative view of the rich falls 3.0pp by endline. Panel B confirms that within villages, views of the rich do not diverge significantly between the TP and NTP.

These findings highlight the value of our partial population experiment 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 the NTP and NP, suggesting community-wide shifts in perceptions towards the rich in response to exposure to pro-poor interventions rather than them being shifting through self-serving biases.

4.4 Perceptions of the Poor

A natural counterpart is whether and how perceptions of the poor are shifted by the pro-poor interventions [Andersen *et al.* 2023]. As with perceptions towards the rich, we split the analysis into how exposure to the big push anti-poverty interventions shift perceptions of the poor, and perceptions of the fundamental causes of poverty.

Focusing first on perceptions of 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*. The non poor were only surveyed on these questions at endline. Table 9 shows the results where the outcome is whether the household head agreed or strongly agreed with each statement about the poor. To begin with 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, we find little evidence that perceptions of the character of the poor are shifted by the big push pro-poor interventions.

Why are the Poor Poor? Considering perceptions of the causes of poverty, we divide these causes as structural features of the economy leading to poverty, versus the view of poverty as destiny/fate. 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*. Table 10 shows the results. In each case the outcome is whether the household head agreed or strongly agreed with the statement. We see 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.

As Panel A shows, at midline, the big push interventions cause significant falls in the view that the causes of poverty are structural. This holds across all four causes and magnitudes of impacts vary between 5pp and 9pp, and with a number of these impacts being robust to MHT. However, by endline these treatment effects fade. Panel B shows that within villages there are few divergences in beliefs between the treated poor and not treated poor on structural causes of poverty. The one exception is that at midline the TP are 3.6pp more likely to report the poor lack opportunities due to their background ($p = .039$, $q = .085$).

On poverty as destiny/fate, we asked households whether they thought the poor were poor because: (i) *they are unlucky*; (ii) *they have encountered misfortunes*; (iii) *they have bad fate/destiny*. Table 11 shows the results. The perception that poverty is one's destiny is generally less prevalent among controls than the view that poverty is down to structural causes. The interventions do little to shift perceptions of poverty as destiny/fate among the TP or NTP. However, among the NP, by endline we find significant increases in agreement with the view that the poor are poor because of being unlucky or having bad fate/destiny.[12]

4.5 Attitudes Towards Redistribution

The backdrop of economic gains to the TP, changes in relative standing of the NTP and reduction in inequality in treated villages, translate into relatively muted changes in perceptions of house-

[12] Andersen *et al.* [2023] use a housing lottery in Ethiopia to study how an increase in wealth affects beliefs 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.

holds own economic standing, their relative standing, and of reductions in village inequality. More pronounced changes occur in terms of the perceptions towards the rich, and perceptions of the causes of poverty. In a final set of results, we build on these findings to examine how the big push pro-poor interventions translate into shifts in attitudes towards redistribution.

Contrary to the earlier results linking big push interventions and perceptions of economic circumstances, the theoretical foundations for how such interventions shape redistributive preferences are far more established. The workhorse framework for understanding redistributive preferences is Meltzer and Richard [1981] (MR). Their model assumes self-interested individuals and has the basic predictions that: (i) pre-intervention, the poor (relative to the mean income group) should be more in favor of redistribution; (ii) the redistributive preferences of the treated poor should weaken as they benefit from pro-poor interventions.

We next take these predictions to data. While there are many potential ways to measure redistributive preferences, we anchor our results by following the influential work of Kuziemko *et al.* [2015], to construct an index of redistributive preferences based on four questions. The first is a blanket statement of views on redistribution: *do you think the rich in your village should give a part of their income to the poor in some form?*. The second is framed in terms of redistribution towards the poor when others receive a substantial windfall. We asked, *one year ago, a person's monthly income increased to PKR 250'000 as a result of luck. Should (s)he be taxed by the government to raise funds for the poor?* Third, in terms of concerns for societal inequality we asked, *do you think inequality is one of the larger socioeconomic issues of Pakistan?* The final question relates to perceptions towards the rich, using the earlier question in which we asked respondents whether they agreed with the statement, *the rich rightfully deserve their income*. We sum the number of affirmative answers (reversing the reply to the fourth question on the deserving rich) to create a 0-4 index, where a higher index value indicates an individual who holds more redistributive preferences because they are more likely to believe the rich should redistribute to the poor, that windfall gains should be redistributed to the poor, because inequality is a major societal concern, and/or the rich do not rightfully deserve their income.

At midline, the poor hold relatively pro-redistributive preferences, with an average score of 3.14. There is considerable variation across households, with 3% having a score of one or zero, 18% having a score of two, 40% having a score of three and 39% scoring four.[13]

The results are in Table 12. Using either the between village specification reported in Panel A or the within village specification reported in Panel B, we find little shift in the redistributive

[13] Two other points are of note. First, there is a positive time trend among controls in each dimension, of similar magnitude for poor and non-poor households. From midline to endline these correspond to around a 4% increase in the redistributive preferences index. Our study period is one in which Pakistan experienced steady growth in income per capita. Second, in line with existing cross country evidence, we do not find evidence that redistributive preferences vary across poverty deciles. For example, households in the lowest (highest) poverty decile have an index score of 3.13 (3.08). Hoy and Mager [2021] present evidence from a randomized survey experiment with $30,000$ subjects in 10 countries. They also find generally flat profiles of redistributive preferences across income deciles of households.

attitudes of any group in either time period. For example, among the TP at midline we can rule out an increase in the redistributive attitudes index greater than .105 or 3% of its baseline level in controls. The null impacts on the index overall are despite the fact that we have shown earlier that one component of the index – related to perceptions towards the rich – do shift at midline in a direction that makes them hold less redistributive attitudes. To understand whether this shift towards less redistributive attitudes is offset by other components of the index, the remaining Columns of Table 12 show results for the other three components of the index.

The first component of the index is based on the question, *should the rich give part of their income to the poor?* Although the vast majority agree with this statement in controls, we find: (i) at midline, the NTP and NP nudge forward in being more likely to hold this view. The magnitude of impacts is 2.0pp for the NTP and 3.0pp for the NP ($p = .043$, .018 respectively); (ii) at endline, the TP nudge forward on this view by 1.6pp ($p = .052$), while the NTP and NP no longer differ from controls; (iii) Panel B confirms that within villages, we observe no differential responses between the TP and NTP in either period.

The second additional component of the index of redistributive preferences was framed in terms of redistributive responses towards the poor when others receive a substantial windfall. We asked, *one year ago, a person's monthly income increased to PKR 250'000 as a result of luck. Should (s)he be taxed by the government to raise funds for the poor?* At midline the TP and NP are significantly more likely to believe large windfalls should be taxed to redistribute towards the poor, but these changes are not sustained at endline.[14]

The final component of our index of redistributive attitudes asked respondents whether they view inequality as a major concern in Pakistan as a whole. Across groups, point estimates of treatment effects at midline are positive, and at endline they are negative. Indeed, NTP and NP households are significantly less likely to view inequality as a societal concern at endline relative to midline ($p = .100$, .080 respectively).

Overall then, in the long run, redistributive attitudes are inelastic to exposure to the kinds of big push pro-poor interventions we study. Slight nudges forward on the first component that align with households holding more redistributive attitudes are offset by less redistributive attitudes being held because of changed perceptions towards the rich. In consequence, the effective experience or demonstration of pro-poor policies even in these small village economies – a context with low levels of asymmetric information between the poor and non poor, and non-eligibles have emotional connections with beneficiaries – does not in itself generate demand for more/less redistribution.[15]

Revisiting these results through the lens of theory, we note that MR has the basic prediction that the redistributive preferences of the TP should weaken as they economically gain from receipt

[14] If the respondent replied they should be taxed, we asked a follow up question on the how much they should be taxed to derive an implied desired average tax rate on windfalls. Throughout, we find no evidence that any group changes their desired average tax rates for recipients of large windfalls – and again, these null impacts are precise.

[15] Andersen *et al.* [2023] use a housing lottery in Ethiopia to study how an increase in wealth affects support for redistribution. They also find attitudes toward redistribution are insensitive to economic circumstances.

of the asset/cash transfers. This is exactly in line with their response at midline. However, our partial population experiment reveals similar shifts occur among the NTP and NP, in contradiction of the MR model, and more in line with community-wide attitudinal shifts shaped by exposure to the interventions rather than beneficiary status *per se*. Moreover, the long run impacts we estimate establish that attitudinal shifts do not persist, again counter to the MR model.

Given that many earlier studies have found results counter to the basic MR intuition, a large literature has extended the MR framework to help explain redistributive preferences of the rich and poor [Alesina and Giuliano 2011]. In the Appendix we present additional results exploring the idea that redistributive attitudes are shaped by whether: (i) luck or effort are viewed as responsible for individual success [Piketty 1995, Bénabou and Ok 2001, Fong 2001, Alesina and Angeletos 2005, Cappelen *et al.* 2013]; (ii) beliefs over the effectiveness of government [Alesina and Giuliano 2011, Sapienza and Zingales 2013, Kuziemko *et al.* 2015, Alesina *et al.* 2018].

4.6 Ideal Income Distribution

To gauge redistributive preferences from another societal perspective, we asked households about their ideal income distribution. Panel A of Figure 1 shows the choices presented to households, alongside a description of each. The choices vary the position of the modal household, ranging from Distribution A – where a mass of the population remains poor, through to the top heavy Distribution E. Panel B shows the ideal distributions reported in controls at midline, splitting reports by the poor and non poor. Preferences across distributions are similar across groups. The most favored distribution is D (chosen by 35%): where the modal household resides in the middle classes, and there are few households in the tails of the distribution. Bottom heavy Distributions A and B are the least preferred (chosen by fewer than 10%).[16]

We estimate between village treatment effects on each distribution being reported as the ideal one. Panel C summarizes the results – we find null impacts throughout. For any group g in either time period, the y-axis shows that the 95% confidence intervals rule out changes of more than a few percentage points on any given income distribution being viewed as ideal.

5 Discussion

Big push pro-poor interventions hold immense promise for pulling the world's poorest out of poverty. In this paper we move beyond the existing evidence base of economic impacts of such interventions, to study their impacts on perceptions of changed economic circumstances in their village, and related attitudes towards redistribution. We do so using a partial population experiment that combines layers of between and within village randomization, tracking over $15,000$ rural

[16] These graphical descriptions stem from the International Social Survey Program [Gimpelson and Treisman 2018]. Distribution B is closest to the actual income distribution in Pakistan in the 2010s.

households that are either the treated poor, not treated poor or not poor, for four years.

Our data and design reveals three core insights.

First, perceptions are shifted by big push economic interventions targeting the poor, but these impacts are far more muted than measurable changes in economic standing and village inequality. Most impacts on perceptions fade four years post-intervention, despite far more persistent changes in economic circumstances. This wedge between economic reality and perceptions can be a reason why redistributive attitudes of households remain inelastic even to these big push interventions [Alesina *et al.* 2012, Alesina *et al.* 2018].[17]

Second, although we find a weak link between changed economic circumstances and perceptions of economic standing, relative standing and inequality, we find more pronounced changes at midline in perceptions related to the rich and poor more generally. All households perceive the rich to be more deserving, and all change their views of the causes of poverty – in particular, being significantly less likely to view poverty as being driven by structural factors that the poor are helpless against, such as exploitation by the rich, society failing to help them, the unequal distribution of land, or a lack of opportunities.

Third, the partial population experiment shows that in most cases, when perceptions are shifted by the interventions, the impacts are similar across all groups of households – the treated poor, the (overtaken) not treated poor and the not poor. This is despite the very different intervention impacts on economic outcomes across groups. The evidence suggests shifts in perception and attitudes in response to pro-poor interventions 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 the pro-poor interventions – in line with attitudes being driven by sociotropic concerns rather than narrow self-interest [Margalit 2019]. *A fortiori*, such policies do not polarize attitudes – in nearly all cases impacts on the poor and non poor are of the same sign.

We conclude by discussing three issues. First, we consider whether big push pro-poor interventions have more persistent impacts via changes in engagement with political processes. Second, we discuss whether perceptions and attitudes respond in the same way irrespective of the metric of pro-poor transfers: cash or in-kind. Third, we discuss study features that are key to the external validity of our findings, that each represent important directions in which to extend our work.

5.1 Voting

Between baseline and midline high stakes local elections were held across our study region. We thus probe the possibility of lasting impacts of the big push interventions occurring through political

[17] We show big push interventions can drive perceptions and attitudes even when those experiences occur late in life – our household heads are aged in their early 40s at baseline. However, we do not find evidence that such shifts in perceptions and attitudes persist. This complements work emphasizing how experiences in formative years are more likely to determine long run attitudes and behaviors [Malmendier 2021, Margalit 2019, Giuliano and Spilimbergo 2023].

processes – rather than stated perceptions or attitudes – using self-reported data on turnout in these elections. Of course such self-reports are is likely upwards biased, but if this bias does not differ between treated and control villages, the estimated treatment effects remain informative. The results are in Table 13. We find all groups become significantly more likely to report voting in local elections: the TP are 5.8pp more likely, and the NTP are 5.1pp more likely – both impacts significant at the 1% level and robust to MHT. However, the largest increase is seen among the NP, who are 9.2pp more likely to self-report having voted.[18]

As non-eligibles are likely to outnumber those eligible for any pro-poor intervention, the median voter will typically be from a non-eligible household. Hence it is important to consider the possibility that across groups, votes for political parties might be swayed by the interventions – even if stated redistributive attitudes themselves are largely inelastic in the long run. To probe this, we exploit the fact that at baseline, for TP and NP households, we asked them their affinity with platforms of political parties in Pakistan. Although imperfect in this context, we can still classify parties on a left-centre-right spectrum and use each respondent's affinity with party platforms to classify household heads as left-leaning, centrist or right-leaning. Our classification suggests that in controls, around 14% of poor household heads are left leaning, 69% are centrist and 16% are right leaning.[19]

The remaining Columns in Table 13 show heterogeneous impacts on voting by political affinity expressed at baseline. Household heads of all political affinities significantly increase their likelihood to vote. Among the TP, the largest effects are among left- and right-leaning households, although the impacts are not significantly different across political preferences. Among the NP, the largest point estimate is for right-leaning households, that increase their voting by 11.4pp, but again these are not different from the impacts on left-leaning households ($p = .208$). Overall, while the evidence suggests interventions increase political participation across the board, this does not differ by political affinities expressed at baseline.

[18] As a benchmark, Gine and Mansuri [2018] find that a voter awareness campaign in Pakistan increased female turnout by 11pp. Evidence on voting behavior from exposure to CCT programs exists, for example, from Romania [Pop-Eleches and Pop-Eleches 2012], Uruguay [Manacorda *et al.* 2011] and Mexico [De la O 2013].

[19] The main political parties in Pakistan are the PPP, PMLN, PTI, PMLQ and JUI. The PPP and JUI are classifiable as having platforms on the left and right of the political spectrum respectively. The PPP are clearly pro-redistribution, while the JUI are a religion-based party who do not favor redistribution. Other parties are somewhat harder to classify. The PTI's voter base is in central and northern Punjab and the Khyber Pakhtunkhwa province, with many young people being among its strongest supporters, but on many issues (e.g. support to the military, social issues) it is to the right of centre, at least during the duration of this project. The PTI initially wanted to end the BISP social assistance program, but ended up sustaining it, though rebranding it as the *Ehsaas* program. Among the main parties, the PMLN used to be a right of centre alternative to the PPP, but in recent years the PMLN has become more centrist on some issues. The PMLN has continued the BISP social assistance program, and substantially increased its funding. The PMLQ is the King's Party of former PMLN politicians that was hobbled by General Musharraf to counter the PMLN in Punjab. The party is generally socially conservative. We thus classify parties on a left-right spectrum as PPP-PMLN-PTI-PMLQ-JUI.

5.2 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 and attitudes. These results are summarized in Figures A2 to A4. 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 $(\widehat{\beta}_{2j}, \widehat{\beta}_{4j})$ from the within-village estimates, and we indicate whenever impacts differ across treatment arms. 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.

On most dimensions, we find little differential impact on perceptions and attitudes, for any group and in either time period, between when the poor are assisted with asset or cash transfers. More precisely, Figure A2 focuses on perceptions of own standing and perceptions of inequality, so outcomes considered in Tables 6 and 7. For the four perceptions considered, we see the between and within village estimates are largely the same across treatment arms, and this is the case for each group of households, and across both midline and endline estimates. The most notable difference is for the perception of *future* economic standing, where at midline this is higher for the TP and the NTP if the poor receive assets rather than cash ($p = .065$, $.029$ respectively).

Figure A3 summarizes perception of the rich and poor, so outcomes considered in Tables 8 to 11. Shifts in the 14 perceptions and views considered largely do not differ depending on whether the poor are provided asset transfers, or reveal prefer cash over in-kind transfers. The views on which the metric of transfers matters most 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).

Finally, Figure A4 summarizes the results for attitudes towards redistribution and voting, the five outcomes in Tables 12 and 13. Nearly all of these margins have impacts that do not statistically differ depending on the form of big push assistance to the poor.

5.3 Future Agenda and External Validity

In future work on this project, we plan to explore the economic impacts of the interventions in far more detail – expanding the set of outcomes considered beyond those most noticeable to others, to understand how labor supply, patterns of consumption, saving, investment and interhousehold transfers are impacted, and whether and how these differ depending on whether the poor are assisted via cash or asset transfers. More closely tied to the current paper is our future plan to understand how the interventions shift the pro-market beliefs of households. Given the interventions enable the poor to deepen their engagement in labor, capital and financial markets, the pro-market beliefs of the poor could shift, with there being knock-on effects on the beliefs of non beneficiaries as a result of them observing changes in behavior of the treated poor.

Our results also suggest a far broader agenda for future work. As highlighted throughout, there is the need to develop theory to microfound the link between whether and how large noticeable changes in economic circumstances translate into perceptions of those changes. In our context, the fact that beneficiary and non beneficiary households reside next to each other and are likely tied through social networks or networks of economic exchange 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 to other settings and interventions.

Setting Villages in our field experiment are close-knit and ethnically homogeneous. This makes them an almost ideal setting in which to study the link between changes in economic circumstances and perceptions of those changes: there are large and persistent real world shifts in noticeable economic gains, changes in relative economic standing, and reductions in village inequality. 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 or conflict in perceptions and attitudes than we find in our study 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 our partial population experiment reveals that perceptions 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 perceptions and attitudes of 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 perceptions across households might be shifted when within-village redistributive institutions, such as local taxation schemes, are used to target resources to the poor, and whether such financed pro-poor interventions are more likely to lead to polarization or conflict in perceptions and attitudes than we find in our setting.

The Design of Social Protection Systems We have examined the 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.* 2022]. 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 perception and attitudes 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, and how households view the need for particular policies.

A Appendix

Luck versus Merit Redistributive attitudes might depend on whether luck or effort are viewed as responsible for individual success [Piketty 1995, Bénabou and Ok 2001, Fong 2001, Alesina and Angeletos 2005].[20] To consider this, we follow the approach of Almås *et al.* [2020] in asking household heads questions related to a redistributive task, where we vary whether income differences between individuals arise because of luck or merit. We inform respondents that *two people have randomly been allocated PKR 5'000 and PKR 15'000. The recipients have been told about the allocation.* We then ask, *should the government forcefully reallocate the money?* We then repeat the exercise but initially inform respondents, *two people have been allocated PKR 5'000 and PKR 15'000 based on test scores (where a higher test score implies higher reward).* The contrast in wording is designed to change the circumstances under which this inequality has been created: luck or merit, and to capture distributional preferences without the confounding influence of material self-interest. The results are in Table A5. We see little evidence that behavior in the redistributive task of any group, at either midline or endline, is impacted by the intervention irrespective of whether inequalities are initially framed as being driven by luck or merit.

Effectiveness of Government Redistributive attitudes might be easier to shift among those who hold greater belief in the effectiveness of government [Sapienza and Zingales 2013, Kuziemko *et al.* 2015, Alesina *et al.* 2018]. While much of the evidence related to this is taken from cross country data, findings from information experiments remain mixed – but this channel might be especially relevant in low state capacity context like Pakistan [Acemoglu *et al.* 2020].[21]

We can examine the issue in our context given both treatments were implemented in collaboration with quasi-government agencies, and so the interventions are best perceived as government delivered programs. Table A6 shows the results, where we estimate treatment effects on the index

[20] In lab experiments using dictator games, individuals redistribute less when income is earned rather than determined by luck [Cappelen *et al.* 2007, Cappelen *et al.* 2013].

[21] Kuziemko *et al.* [2015] show using an experiment that priming subjects to be less confident in government has a negative effect on the demand for redistribution. Peyton [2020] uses experiments about political corruption to identify the effect of trust in government on support for redistribution – finding largely null impacts.

of redistributive attitudes by baseline views on the effectiveness of government. Recall that around a quarter of household heads believe government is effective (Table 2). Irrespective of households' pre-intervention beliefs over the effectiveness of government, we replicate the broad findings on redistributive attitudes documented earlier. In no case do we find significant differences in intervention responses based on beliefs on government effectiveness. This holds across TP, NTP and NP households, at midline and endline.[22]

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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)									
Panel C. Attitudes (census)																	
Government is effective	.271	.256	.265	.238	.257	.275	.238	.295	[.919]	[.836]	[.921]	[.784]	[.926]	[.763]	[.888]	[.468]	[.718]
NGOs are effective	.274	.276	.231	.248	.248	.280	.248	.319	[.710]	[.707]	[.426]	[.712]	[.420]	[.285]	[.657]	[.544]	[.302]
Private sector is effective	.196	.183	.154	.181	.196	.182	.181	.216	[.686]	[.985]	[.633]	[.854]	[.710]	[.611]	[.830]	[.566]	[.843]
Government represents people like me	.196	.213	.163	.198	.225	.131	.199	.182	[.349]	[.059]	[.449]	[.812]	[.324]	[.621]	[.992]	[.385]	[.610]
People can affect government policies	.310	.269	.288	.331	.294	.253	.331	.282	[.666]	[.291]	[.524]	[.992]	[.326]	[.389]	[.739]	[.876]	[.827]

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

	(1) Own Livestock	(2) Log (Value Livestock) | Own Livestock	(3) Iron Roof	(4) Often Consume Own Produced Milk	(5) Log (Monthly Food Expenditure)
Treatment 1: Asset Transfer					
One year impact	.211***	.133*	.034	.082**	-.015
	(.027)	(.078)	(.029)	(.032)	(.027)
Two year impact	.231***	.157**		.113***	.022
	(.023)	(.060)		(.028)	(.017)
Four year impact	.190***	.107**		.087***	.032
	(.024)	(.053)		(.029)	(.021)
Treatment 2: Revealed Preferred Unconditional Cash Transfer					
One year impact	.102**	.153*	.048	.038	-.036
	(.043)	(.083)	(.046)	(.036)	(.031)
Two year impact	.138***	.138**		.086***	.028*
	(.022)	(.057)		(.022)	(.016)
Four year impact	.131***	.139**		.053**	.042*
	(.025)	(.060)		(.022)	(.024)
Mean (poor, controls at baseline)	.563	2836	.360	.328	83.7
p-values:					
T1=T2 (one year)	[.042]	[.867]	[.837]	[.398]	[.687]
T1=T2 (two year)	[.006]	[.835]		[.511]	[.814]
T1=T2 (four year)	[.101]	[.741]		[.428]	[.810]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of observations	10784	6601	2340	10785	10700

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 Column 3, having an iron roof is only measured on year post-intervention. In Column 5, 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$. 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

	(1) Own Livestock	(2) Log (Value Livestock) | Own Livestock	(3) Iron Roof	(4) Often Consume Own Produced Milk	(5) Log (Monthly Food Expenditure)
One year impact	.160***	.142**	.040**	.061***	-.025*
	(.024)	(.055)	(.016)	(.023)	(.014)
Two year impact	.184***	.148***		.099***	.025**
	(.016)	(.038)		(.015)	(.011)
Four year impact	.160***	.123***		.069***	.037***
	(.017)	(.031)		(.015)	(.013)
Mean (poor, controls at baseline)	.563	2836	.360	.328	83.7
p-values:					
One year = Two year	[.329]	[.928]		[.117]	[.004]
Two year = Four year	[.181]	[.548]		[.083]	[.346]
One year = Four year	[.997]	[.742]		[.708]	[.002]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of observations	10784	6601	2340	10785	10700

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 Column 3, having an iron roof is only measured on year post-intervention. In Column 5, 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$. 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

	(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: Perception of Current and Future Standing

OLS estimates, standard errors clustered by village in parantheses
p-values in brackets, FDR adjusted q-values in braces

	Current: On a ladder with 10 steps, where do you currently stand?			Future: On a ladder with 10 steps, what is the best life you can achieve?		
	(1a) TP	**(1b) NTP**	**(1c) NP**	**(2a) TP**	**(2b) NTP**	**(2c) NP**
A. Between Village Estimates (Treated vs Control)						
Two year impact	-.119	-.206**	-.539***	-.035	-.055	-.193*
	(.108)	(.097)	(.105)	(.118)	(.125)	(.114)
	[.274]	[.036]	[.000]	[.769]	[.648]	[.095]
	{.255}	{.048}	{.001}	{.926}	{.913}	{.499}
Four year impact	.050	-.048	-.126	.171	.242**	.064
	(.128)	(.139)	(.122)	(.117)	(.114)	(.104)
	[.699]	[.729]	[.304]	[.149]	[.037]	[.542]
	{.574}	{.574}	{.255}	{.533}	{.421}	{.913}
Two Year = Four Year	[.387]	[.429]	[.021]	[.274]	[.108]	[.118]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)						
Two year impact	.121***			.068		
	(.045)			(.068)		
	[.009]			[.321]		
	{.022}			{.671}		
Four year impact	.135***			-.024		
	(.050)			(.055)		
	[.009]			[.668]		
	{.022}			{.913}		
Two Year = Four Year	[.840]			[.299]		
Mean Outcome, Controls	2.78		3.34	7.08		7.21
Observations: Panel A	8126	9382	17001	8126	9382	17001
Observations: Panel B	8262			8262		

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 in Panel A compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Columns 1b, 2b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a). 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 outcome is based on a similar ladder of life wording as the first, except respondents are then asked to name the highest rung of the ladder they could achieve in future. 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: Perceptions of Village Inequality

OLS estimates, standard errors clustered by village in parantheses
p-values in brackets, FDR adjusted q-values in braces

	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**
A. Between Village Estimates (Treated vs Control)						
Two year impact	.037	.011	.002	-.013	-.012	-.024**
	(.031)	(.033)	(.027)	(.009)	(.009)	(.011)
	[.236]	[.737]	[.934]	[.187]	[.186]	[.031]
	{1.00}	{1.00}	{1.00}	{.775}	{.775}	{.330}
Four year impact	-.011	-.008	-.011	-.005	-.002	-.004
	(.032)	(.032)	(.028)	(.004)	(.005)	(.006)
	[.744]	[.813]	[.700]	[.318]	[.619]	[.533]
	{1.00}	{1.00}	{1.00}	{.803}	{.995}	{.995}
Two Year = Four Year	[.378]	[.749]	[.711]	[.473]	[.405]	[.165]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)						
Two year impact	.018			-.001		
	(.017)			(.004)		
	[.329]			[.902]		
	{1.00}			{1.00}		
Four year impact	-.012			-.002		
	(.020)			(.002)		
	[.549]			[.254]		
	{1.00}			{.801}		
Two Year = Four Year	[.243]			[.764]		
Mean Outcome, Controls	34.0%		38.8%	9.05%		10.8%
Observations: Panel A	8126	9382	17004	8126	9382	17004
Observations: Panel B	8262			8262		

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 in Panel A compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Columns 1b, 2b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a). 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. The outcomes are variables measuring individuals' perceptions of village inequality. The first 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?”. 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: Perceptions of the Rich

OLS estimates, standard errors clustered by village in parantheses
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
A. Between Village Estimates (Treated vs Control)									
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]
	{.087}	{.142}	{.087}	{1.00}	{1.00}	{1.00}	{.541}	{.541}	{.153}
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]
	{.603}	{.954}	{.954}	{1.00}	{.924}	{1.00}	{.153}	{.153}	{1.00}
Two Year = Four Year	[.060]	[.327]	[.061]	[.268]	[.377]	[.168]	[.419]	[.533]	[.166]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)									
Two year impact	.017			-.010			.002		
	(.023)			(.017)			(.009)		
	[.472]			[.563]			[.849]		
	{.601}			{1.00}			{1.00}		
Four year impact	-.024			-.002			-.005		
	(.016)			(.015)			(.012)		
	[.145]			[.914]			[.663]		
	{.222}			{1.00}			{1.00}		
Two Year = Four Year	[.208]			[.707]			[.611]		
Mean Outcome, Controls	32.3%		31.0%	30.0%		33.5%	11.2%		11.0%
Observations: Panel A	8126	9382	17004	8126	9382	17004	8126	9382	17004
Observations: Panel B	8262			8262			8262		

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 in Panel A 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. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a). 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: Perceptions of the Character of the Poor

Strongly agree or agree with statements

OLS estimates, standard errors clustered by village in parantheses

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
A. Between Village Estimates (Treated vs Control)												
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}	{1.00}		{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}
Two Year = Four Year	[.289]	[.247]		[.839]	[.585]		[.830]	[.743]		[.995]	[.768]	
B. Within Village Estimates (Treated Poor vs Not Treated Poor)												
Two year impact	-.021			-.019			-.006			.002		
	(.015)			(.017)			(.016)			(.018)		
	[.174]			[.257]			[.719]			[.926]		
	{1.00}			{1.00}			{1.00}			{1.00}		
Four year impact	-.007			.001			-.000			.020		
	(.016)			(.015)			(.020)			(.018)		
	[.644]			[.963]			[.990]			[.252]		
	{1.00}			{1.00}			{1.00}			{1.00}		
Two Year = Four Year	[.616]			[.456]			[.842]			[.486]		
Mean Outcome, Controls	.330		.256	.357		.348	.362		.333	.400		.413
Observations: Panel A	7505	8502	8039	7537	8551	8089	7527	8530	8065	7271	8195	7757
Observations: Panel B	7499			7544			7527			7204		

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 in Panel A 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. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a, 4a). 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 Driven by Structural Causes

Strongly agree or agree with statements

OLS estimates, standard errors clustered by village in parantheses
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
A. Between Village Estimates (Treated vs Control)												
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]	
	{.257}	{.084}		{.044}	{.029}		{.136}	{.141}		{.085}	{.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]
	{1.00}	{1.00}	{.792}	{.565}	{.565}	{.380}	{1.00}	{1.00}	{1.00}	{.331}	{.166}	{.331}
Two Year = Four Year	[.252]	[.308]		[.324]	[.159]		[.238]	[.375]		[.282]	[.105]	
B. Within Village Estimates (Treated Poor vs Not Treated Poor)												
Two year impact	.003			.015			-.006			.036**		
	(.017)			(.019)			(.018)			(.017)		
	[.848]			[.435]			[.730]			[.039]		
	{1.00}			{.569}			{1.00}			{.085}		
Four year impact	.008			-.005			.008			.014		
	(.015)			(.015)			(.012)			(.016)		
	[.582]			[.743]			[.514]			[.372]		
	{1.00}			{.738}			{1.00}			{.331}		
Two Year = Four Year	[.828]			[.397]			[.544]			[.393]		
Mean Outcome, Controls	.795		.767	.796		.751	.807		.762	.803		.756
Observations: Panel A	7522	8530	8065	7403	8353	7842	7375	8302	7816	7440	8411	7937
Observations: Panel B	7526			7332			7285			7399		

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 in Panel A 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. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a, 4a). 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 11: Poverty as Destiny or Fate

Strongly agree or agree with statements

OLS estimates, standard errors clustered by village in parantheses
p-values in brackets, FDR adjusted q-values in braces

	They are unlucky			They have encountered misfortunes			They have bad fate/destiny		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP	(3a) TP	(3b) NTP	(3c) NP
A. Between Village Estimates (Treated vs Control)									
Two year impact	-.036	-.012		-.054	-.048		-.040	-.038	
	(.036)	(.037)		(.034)	(.036)		(.035)	(.032)	
	[.318]	[.741]		[.116]	[.186]		[.257]	[.248]	
	{.956}	{.956}		{1.00}	{1.00}		{.540}	{.540}	
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]
	{.956}	{.956}	{.956}	{1.00}	{1.00}	{1.00}	{.540}	{.692}	{.183}
Two Year = Four Year	[.452]	[.458]		[.239]	[.243]		[.214]	[.334]	
B. Within Village Estimates (Treated Poor vs Not Treated Poor)									
Two year impact	-.018			-.002			.001		
	(.019)			(.024)			(.020)		
	[.349]			[.924]			[.942]		
	{.956}			{1.00}			{.692}		
Four year impact	-.019			.002			.018		
	(.017)			(.018)			(.014)		
	[.275]			[.934]			[.206]		
	{.956}			{1.00}			{.540}		
Two Year = Four Year	[.975]			[.908]			[.533]		
Mean Outcome, Controls	.484		.417	.489		.395	.391		.285
Observations: Panel A	7518	8532	8040	7426	8399	7926	7526	8535	8006
Observations: Panel B	7530			7373			7537		

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 in Panel A 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. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a). 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 12: Redistributive Attitudes

OLS estimates, standard errors clustered by village in parantheses, p-values in brackets, FDR adjusted q-values in braces

	Redistributive Attitudes Index: Kuziemko et al. [2015]			Should the rich give part of their income to the poor?			A year ago a person's monthly income increased to PKR 250K due to luck. Should (s)he be taxed by the government to raise funds for the poor?			Inequality is a serious problem in Pakistan?		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP	(3a) TP	(3b) NTP	(3c) NP	(4a) TP	(4b) NTP	(4c) NP
A. Between Village Estimates (Treated vs Control)												
Two year impact	.007	.017	.055	.012	.020**	.030**	.060*	.039	.071**	.013	.017	.027*
	(.049)	(.043)	(.043)	(.011)	(.010)	(.013)	(.033)	(.035)	(.029)	(.016)	(.015)	(.015)
	[.883]	[.695]	[.203]	[.279]	[.043]	[.018]	[.067]	[.258]	[.018]	[.416]	[.275]	[.084]
	{1.00}	{1.00}	{1.00}	{.288}	{.161}	{.161}	{.307}	{.557}	{.169}	{1.00}	{1.00}	{1.00}
Four year impact	.053	.044	.028	.016*	.016	.005	.028	.034	.029	-.012	-.021	-.010
	(.051)	(.050)	(.048)	(.008)	(.010)	(.009)	(.034)	(.036)	(.034)	(.018)	(.018)	(.014)
	[.304]	[.388]	[.560]	[.052]	[.107]	[.535]	[.417]	[.337]	[.394]	[.492]	[.253]	[.487]
	{1.00}	{1.00}	{1.00}	{.161}	{.161}	{.441}	{.557}	{.557}	{.557}	{1.00}	{1.00}	{1.00}
Two Year = Four Year	[.565]	[.712]	[.690]	[.806]	[.834]	[.177]	[.522]	[.919]	[.393]	[.260]	[.100]	[.080]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)												
Two year impact	-.020			-.006			.010			-.009		
	(.038)			(.007)			(.017)			(.011)		
	[.603]			[.447]			[.577]			[.394]		
	{1.00}			{.425}			{.763}			{1.00}		
Four year impact	.000			.002			-.017			.003		
	(.025)			(.006)			(.013)			(.009)		
	[.991]			[.782]			[.209]			[.784]		
	{1.00}			{.643}			{.557}			{1.00}		
Two Year = Four Year	[.668]			[.438]			[.231]			[.432]		
Mean in Controls	3.13		3.16	95.2%		93.8%	64.7%		66.9%	85.5%		86.1%
Observations: Panel A	7800	8988	16278	8126	9382	17004	7800	8988	16279	8126	9382	17004
Observations: Panel B	7910			8269			7910			8262		

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 in Panel A 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. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a, 4a). 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.

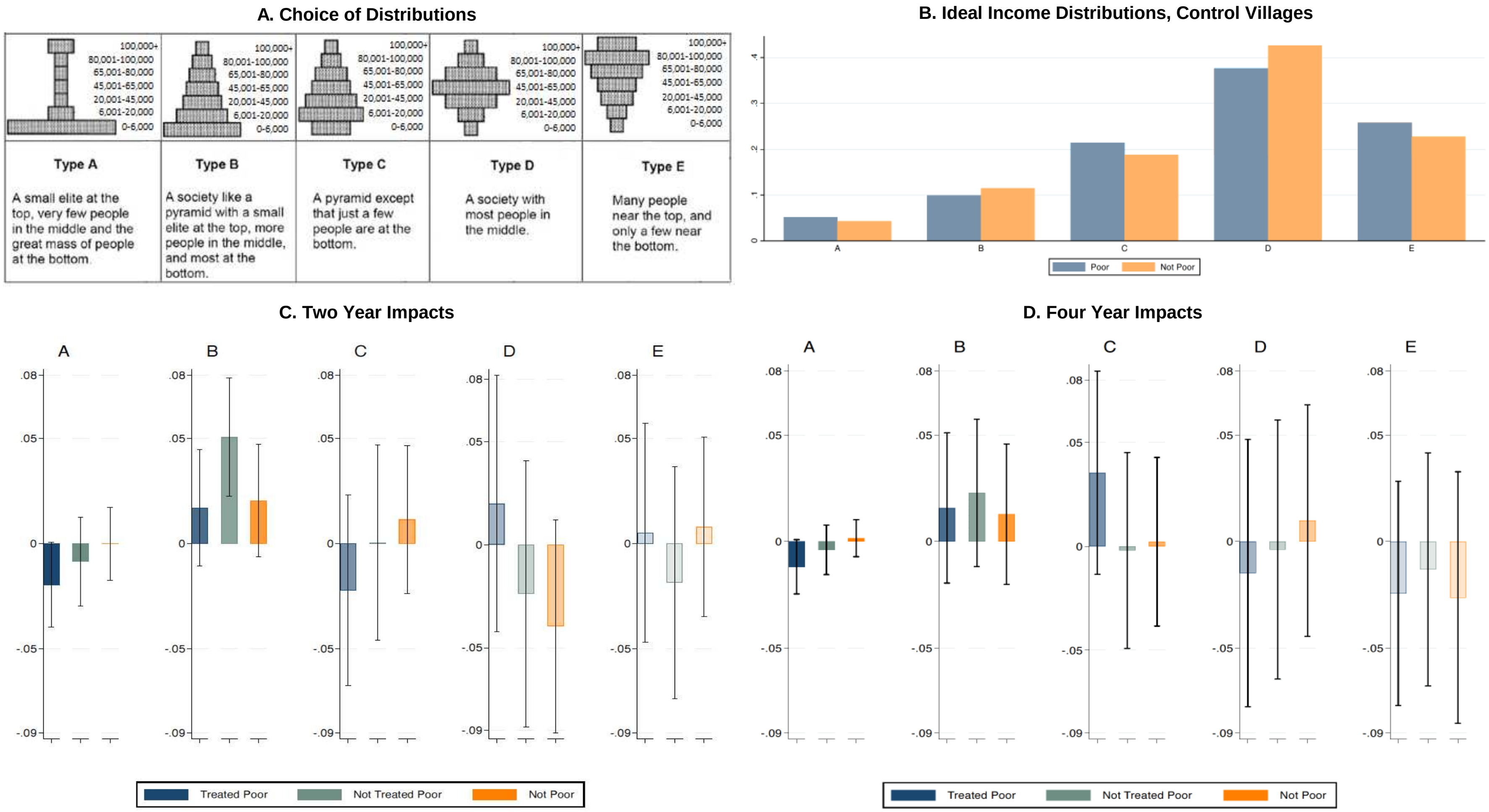
Table 13: Voting

Outcome: voted in past local election
OLS estimates, standard errors clustered by village in parantheses
p-values in brackets, FDR adjusted q-values in braces

	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2c) NP
A. Between Village Estimates (Treated vs Control)					
Two year impact	.058***	.051***	.092***		
	(.011)	(.011)	(.025)		
	[.000]	[.000]	[.000]		
	{.001}	{.001}	{.001}		
Two year impact | left leaning				.097***	.072***
				(.026)	(.025)
				[.000]	[.006]
				{.001}	{.004}
Two year impact | centrist				.065***	.075***
				(.019)	(.027)
				[.001]	[.008]
				{.001}	{.005}
Two year impact | right leaning				.091**	.114***
				(.038)	(.024)
				[.018]	[.000]
				{.009}	{.001}
B. Within Village Estimates (Treated Poor vs Not Treated Poor)					
Two year impact	.012				
	(.008)				
	[.145]				
	{.021}				
Mean Outcome, Controls	89.1%		84.6%	89.1%	84.6%
p-values:					
Left leaning = Centrist				[.224]	[.912]
Left leaning = Right leaning				[.891]	[.208]
Centrist = Right leaning				[.529]	[.113]
Observations: Panel A	4043	4677	8489	1589	5341
Observations: Panel B	4144				

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 in Panel A compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Column 1b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Column 1a). All regressions include treatment dummies (pooling T1 and T2), district (strata) and survey wave fixed effects. Standard errors are clustered by village, and 95% confidence intervals are reported in brackets. In each Panel, 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 1: Ideal Income Distributions



Notes: Panel A shows the income distributions respondents were shown, including the monthly income ranges (in PKR) that correspond to every level of the distribution. Respondents were then asked, "Independent of your position [in the distribution], which of these do you think is the ideal income distribution?" Panel B shows the share of household heads in control villages, split by poor and non-poor households, who pick each distribution from Panel A as their ideal. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. Panel C presents treatment effects comparing treated poor, not treated poor and non-poor households in treatment and control villages. All regressions treatment dummies (pooling T1 and T2), include district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered by village and we report 95% confidence intervals.

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)			
Panel C. Attitudes (census)								
Government is effective	.271	.256	.270	.256	.274	[.849]	[.903]	[.663]
NGOs are effective	.274	.276	.256	.299	.280	[.985]	[.773]	[.991]
Private sector is effective	.196	.183	.168	.204	.205	[.810]	[.913]	[.680]
Government represents people like me	.196	.213	.147	.181	.206	[.112]	[.498]	[.713]
People can affect government policies	.310	.269	.270	.301	.289	[.399]	[.569]	[.760]

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 in parentheses clustered by village

	Treated Poor	Not Treated Poor	Not Poor
	(1)	(2)	(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. The regressions utilize the sample of treated poor and not treated poor households within treated villages using date from baseline, the one-, two and four-year follow ups. 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

	Not Treated Poor					Not Poor			
	(1) Own Livestock	(2) Log (Value Livestock) | Own Livestock	(3) Iron Roof	(4) Often Consume Own Produced Milk	(5) Log (Monthly Food Expenditure)	(8) Own Livestock	(9) Log (Value Livestock) | Own Livestock	(10) Often Consume Own Produced Milk	(11) Log (Monthly Food Expenditure)
One year impact	-.020	.003	.065	-.006	-.012			.003	-.057
	(.039)	(.149)	(.051)	(.046)	(.050)			(.041)	(.036)
Two year impact	-.028	-.044		-.049	.022	-.056*	-.014	-.036	.070***
	(.034)	(.098)		(.045)	(.025)	(.031)	(.061)	(.028)	(.018)
Four year impact	-.007	-.110		-.026	-.038	-.030	-.064	-.005	-.025
	(.037)	(.098)		(.045)	(.035)	(.033)	(.058)	(.032)	(.024)
Mean (poor, controls at baseline)	.563	2836	.360	.328	83.7	.638	4213	.421	98.7
p-values:									
One year = Two year	[.828]	[.609]		[.200]	[.527]	[.081]		[.245]	[.001]
Two year = Four year	[.401]	[.219]		[.402]	[.045]	[.202]	[.317]	[.178]	[.000]
One year = Four year	[.713]	[.203]		[.572]	[.675]	[.365]		[.805]	[.412]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	12325	6704	2666	12326	12220	17021	9317	22141	21744

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 Column 3, having an iron roof is only measured on year post-intervention - and is not measured for the not poor. In Columns 5 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). 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: Luck versus Merit

OLS estimates, standard errors clustered by village in parantheses
p-values in brackets, FDR adjusted q-values in braces

	LUCK **: Two people have randomly been allocated PKR 5'000 and PKR 15'000. The recipients have been told about the allocation.**			***MERIT*** **: Two people have been allocated PKR 5'000 and PKR 15'000 based on test scores (higher test score implies higher reward)**		
	Should the government forcefully reallocate the money?					
	(1a) TP	**(1b) NTP**	**(1c) NP**	**(2a) TP**	**(2b) NTP**	**(2c) NP**
A. Between Village Estimates (Treated vs Control)						
Two year impact	-.079	-.036	-.057	-.064	-.052	-.010
	(.084)	(.089)	(.067)	(.108)	(.141)	(.100)
	[.348]	[.690]	[.398]	[.553]	[.716]	[.918]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Four year impact	.007	.014	-.016	.014	.024	.006
	(.027)	(.035)	(.030)	(.026)	(.033)	(.025)
	[.801]	[.683]	[.600]	[.599]	[.471]	[.829]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Two Year = Four Year	[.398]	[.654]	[.628]	[.534]	[.645]	[.890]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)						
Two year impact	-.034			-.001		
	(.037)			(.068)		
	[.362]			[.990]		
	{1.00}			{1.00}		
Four year impact	-.006			-.008		
	(.015)			(.013)		
	[.674]			[.533]		
	{1.00}			{1.00}		
Two Year = Four Year	[.513]			[.920]		
Mean Outcome, Controls	41.8%		37.8%	48.2%		40.7%
Observations: Panel A	4793	5725	10328	4536	5298	9479
Observations: Panel B	5118			4652		

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 in Panel A compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Columns 1b, 2b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a). 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. In the “luck” scenario, the exact wording of the vignette is as follows: "Two people in your village, A & B, have been allocated PKR 5,000 and PKR 15,000 respectively based on a coin toss. The recipients know that they have been allocated PKR 5,000 and 15,000 respectively.” In the “merit” scenario, the exact wording of the vignette is, "The initial allocation was based on the recipients score in a school test instead of a coin toss. The higher scorer was given the higher award and lower scorer was given the smaller award." In both cases, we report the answer to the question “Should the government forcefully reallocate the money?” 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 A6: Belief in Government Effectiveness

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

	Redistributive Attitudes Index: Kuziemko et al. [2015]		
	(1a) TP	(1b) NTP	(1c) NP
Two year impact | Government Ineffective	.007	-.004	.059
	(.054)	(.049)	(.048)
	[.902]	[.938]	[.227]
	{1.00}	{1.00}	{1.00}
Two year impact | Government Effective	.008	.071	.042
	(.072)	(.060)	(.043)
	[.904]	[.240]	[.329]
	{1.00}	{1.00}	{1.00}
Four year impact | Government Ineffective	.064	.030	.018
	(.056)	(.055)	(.051)
	[.257]	[.588]	[.719]
	{1.00}	{1.00}	{1.00}
Four year impact | Government Effective	.021	.080	.056
	(.070)	(.065)	(.059)
	[.768]	[.224]	[.345]
	{1.00}	{1.00}	{1.00}
Two Year = Four Year | Government Ineffective	[.978]	[.286]	[.708]
Two Year = Four Year | Government Effective	[.548]	[.451]	[.481]
Mean in Controls | Government Ineffective	3.12		3.15
Mean in Controls | Government Effective	3.16		3.17
Observations	7800	8988	16279

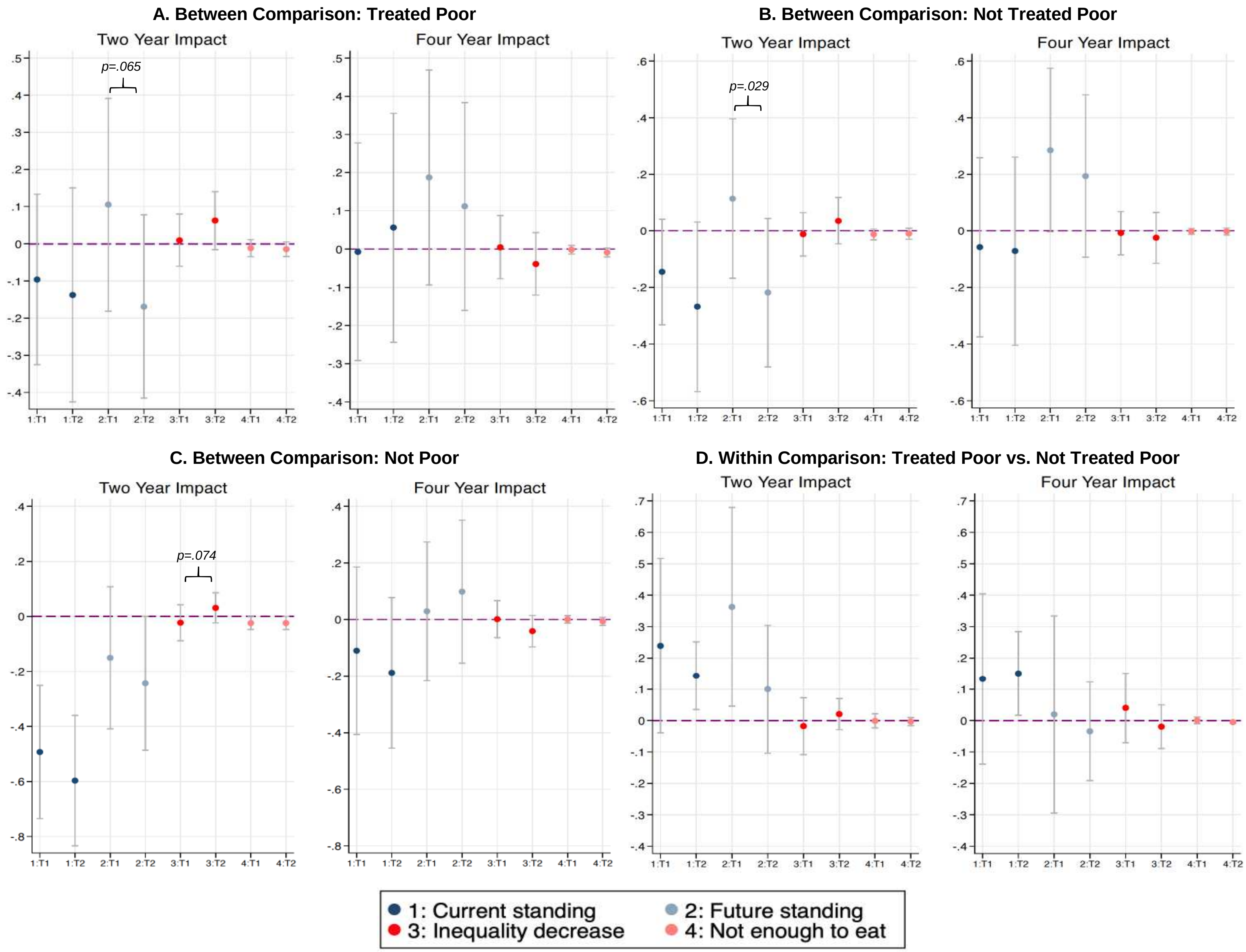
Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Column 1a), Not Treated Poor (Column 1b), and Not Poor (Column 1c) 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 level, 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 within each view of government effectiveness.

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		

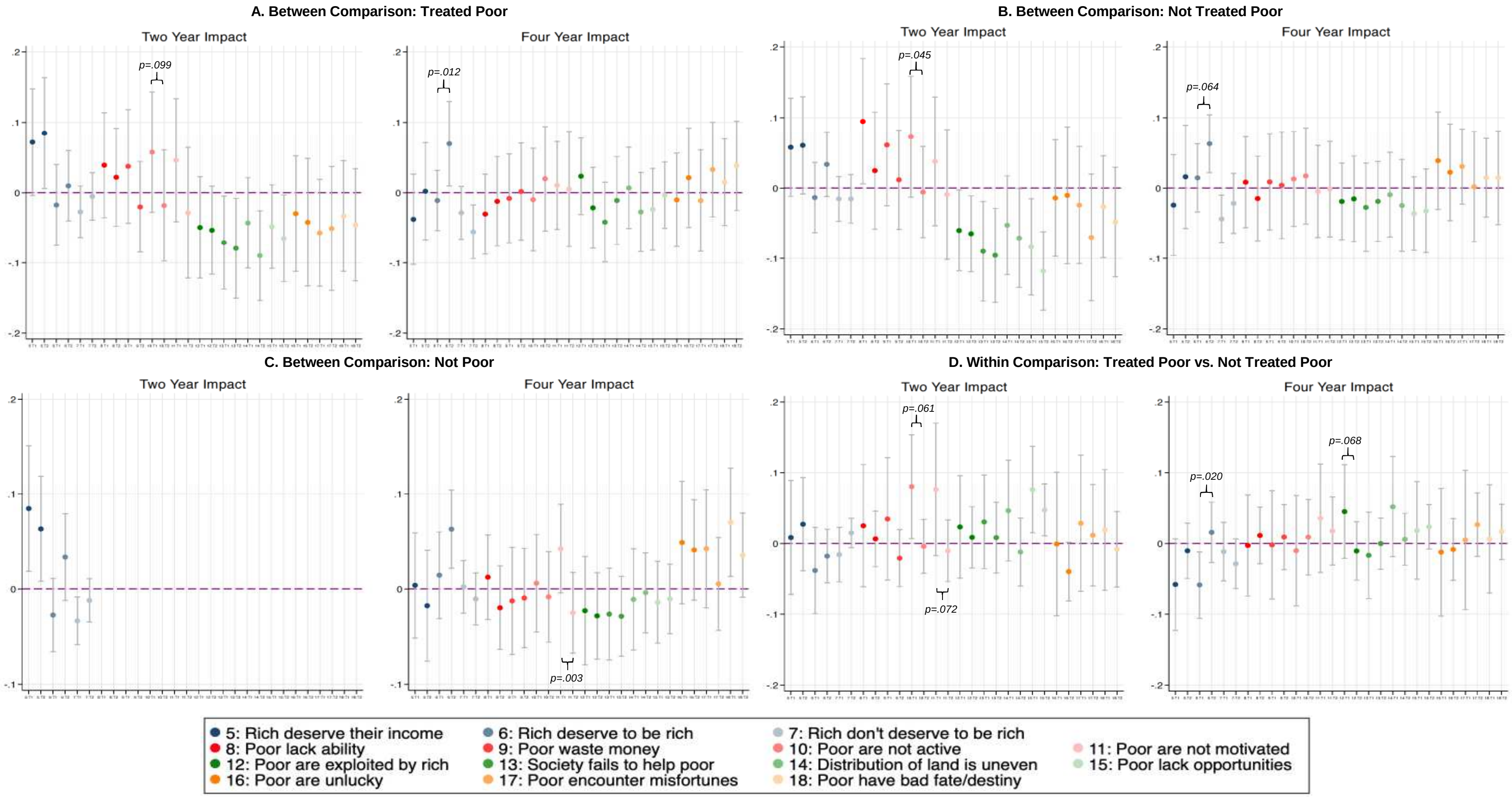
Notes: The figure presents a stylized example of an asset list that households were shown in both treatment arms. Households were allowed to choose any combination of assets they desired, up to a total value of PKR50K.

Figure A2: Perceptions, Asset versus Cash Transfers



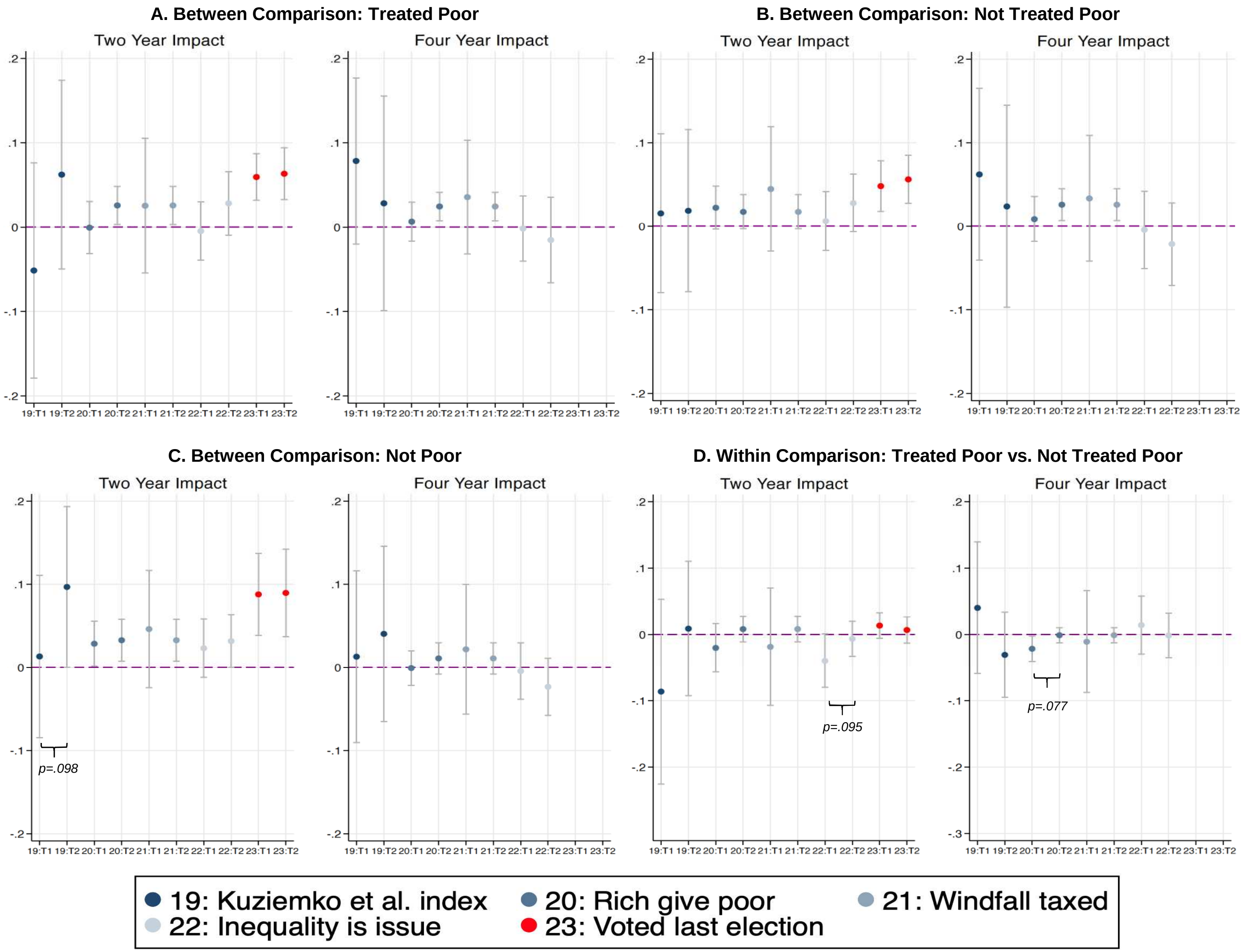
Notes: Panel A (B) [C] {D} displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households] {within estimates for the treated poor and not treated poor households}. For each specification we report 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 7. Wherever treatment effects differ across arms, we report the p-value on the null of equality of treatment effects.

Figure A3: Perceptions of the Rich and Poor, Asset versus Cash Transfers



Notes: Panel A (B) [C] {D} 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] {within estimates for the treated poor and not treated 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 8, the four perceptions of the poor reported in Table 9, views on the four structural causes of poverty reported in Table 10, and views on the three views on poverty as destiny or fate reported in Table 11 (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.

Figure A4: Redistributive Attitudes and Voting, Asset versus Cash Transfers



Notes: Panel A (B) [C] {D} 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] {within estimates for the treated poor and not treated poor households}. For each specification we report the treatment effects for T1 and T2. The outcomes are the index of redistributive preferences and its first three components as reported in Table 12, and self-reported voting as described in Table 13 (that are not available at endline). Wherever treatment effects differ across arms, we report the p-value on the null of equality of treatment effects.