Impacts of Pro-poor Transfers on Perceptions, Mindsets, Political Views and Participation of the Rich and Poor*

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Abstract

Does exposure to big push pro-poor interventions impact the political participation of the rich and poor, and is this driven by shifts in perceptions, mindsets and policy views? We study the issue using a field experiment tracking $15,000$ households over four years in rural Pakistan. Villages are randomly assigned to receive an intervention where the poor are either offered a one-time asset transfer of value \$620 or an equivalent valued one-time unconditional cash transfer. In treated villages we randomize which of the poor receive the transfer, and then track treated poor, not treated poor and not poor households at two and four years post-intervention. The transfers cause large and persistent economic gains to the treated poor, and reduce village-level consumption inequality. Despite these measurable changes, we document a wedge between economic reality and household's perceptions of their economic standing and village inequality. Two years post-intervention, all households, irrespective of their beneficiary status, more strongly view the rich as deserving, hold stronger pro-market mindsets and become more trusting of neighbors, while redistributive preferences remain unchanged. Finally, political participation of households rises in local elections. For all households, pro-market mindsets mediate this outcome, but for the treated poor, political participation is also mediated by their improved economic standing. Results four years post-intervention highlight how exposure to economically effective big push pro-poor interventions is however unlikely to persistently shift perceptions, mindsets and policy preferences of the rich and poor, even as economic gains to the treated poor persist. *JEL: I30, O12, P10.*

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1 Introduction

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

This paper goes beyond the study of economic impacts, to understand whether changed economic circumstances caused by such policies translate into shifts in deeper non-economic outcomes. Our theory of change – set out in Figure 1 – is motivated by recent work summarized in Stantcheva [2024], on how individuals form policy preferences and view the world around them more generally. We adopt this framework to consider how direct exposure to real world big push pro-poor interventions impacts perceptions, mindsets and policy preferences of the rich and poor, and whether these channels are important for moving the dial on political participation, that it turn can drive longer term social change.

We examine these issues 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. Our study context are small, close-knit villages in rural Pakistan, where the economic impacts of interventions should *a priori* be noticeable to others, leaving less scope for misperceptions of gains to beneficiaries to persist [Alesina *et al.* 2021].

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

To establish the general equilibrium impacts of the interventions, we use a two-stage randomization design. In the first, we randomly assign villages to T1, T2 or control. At a second stage, within treated villages, we randomly assign the 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). Our design and data collection allows us to evaluate causal impacts on beneficiaries (TP), impacts on those overtaken in economic standing (NTP) and wider spillovers to those never eligible (NP). We exploit the within and between village randomizations to trace the dynamic evolution of economic and non-economic outcomes by tracking households two-years (midline) and four-years post intervention (endline). We evaluate the general equilibrium effects of interventions on the outcomes linked in our theory of change by tracking $15,000$ households –from the TP, NTP and NP groups – over four years.

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

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

Using the between village randomization, we document significant reductions in village level consumption inequality two- and four-years post intervention. These changes in local inequality, if perceived, can also alter perceptions, mindsets and policy preferences across households.

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

Given this backdrop of changed economic circumstances in treated villages, our core analysis sheds light on the central part of our theory of change: whether the interventions shift perceptions, mindsets and policy preferences of TP, NTP and NP households. Our experimental design reveals the following insights on these three classes of non-economic outcome.

First, perceptions of the economic standing of households are shifted by big push economic interventions targeting the poor, but these shifts are far more muted than actual measurable changes in economic standing. For example, the TP – direct beneficiaries of the interventions – have little change in perception of their economic standing, while non-beneficiaries report significant falls in their standing at midline. This is in line with findings from higher income settings that individual well-being can fall when individuals observe changes in wealth/income in people around them [Luttmer 2005, Card *et al.* 2012, Perez-Truglia 2020, Cullen and Perez-Truglia 2022]. At the same time, there are very muted impacts on households perceptions of changes in village inequality as a whole. In short, there exists a wedge between economic reality and household's perceptions of their economic standing and village inequality.

We then move to broader perceptions of entire classes of households. Specifically, we consider views towards the rich and poor, that might in turn drive political participation. We find that exposure to the big push interventions has pronounced changes at midline in perceptions towards the rich and poor. In particular, all households in treated villages perceive the rich to be more deserving. On perceptions of the poor, at midline TP and NTP households are significantly less likely to view poverty as being driven by structural factors that the poor are helpless against – these factors include exploitation of the poor by the rich, society failing to help them, the unequal distribution of land, or a lack of opportunities.

Our second set of results examine mindsets of the rich and poor – a cognitive lens through which individuals interpret information and form judgements [Stantcheva 2024]. We consider two broad mindsets: (i) market-orientated beliefs; (ii) trust in neighbors. We document large shifts in mindset at midline for households exposed to the interventions. Specifically, all households, irrespective of their beneficiary status, become significantly more pro-market – holding stronger beliefs in meritocracy, materialism, and generalized trust in others. All households also hold more pro-social mindsets in that they are more trusting of neighbors – holding stronger beliefs that in their village the rule of law operates, that crime is down, and of feeling safe.

Third, we consider how exposure to the big push interventions translate into policy preferences – a third channel through which our theory of change suggests that such interventions can change political participation. We consider preferences for redistribution, that have long been studied [Meltzer and Richard 1981]. These impacts could go in either direction. For example: (i) if pro-poor interventions generate positive spillovers, support for future redistribution might develop across a village; (ii) on the other hand, redistribution might create a group of discontented citizens (due to relative decline in their standing), so support for future redistribution might decline. 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.

Overall we find that redistributive preferences remain inelastic to exposure to big push economic interventions. This is because of offsetting effects on components of the redistributive index, the wedge between economic reality and household's perceptions of those economic changes, and that as described above, households tend to view the rich as being more deserving, and become more likely to view markets as the means by which to allocate resources.

At a final stage of analysis we come to the last link in the causal chain of our theory of change: the impact that big push interventions have on political participation. *A priori* the effect of positive economic shocks on political participation of the poor is ambiguous – it could provide bandwidth for them to engage, or they might reduce engagement as they have less of a stake in political outcomes as their well-being rises [Margalit 2019].

We examine 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. However, the largest point estimate increase is among the NP (9.2pp). To examine whether vote shares for political parties might be swayed by exposure to the interventions, we exploit the fact that at baseline, we asked TP and NP households their affinities with party platforms. We use this 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 underlying political affinities.

We then tie together the theory of change by examining how perceptions, mindsets and policy preferences mediate impacts on political participation, as well as how improvements in economic well-being among beneficiaries raise political participation directly, in line with standard resource based channels long explored in the literature [Brady *et al.* 1995, Glaeser and Ward 2006, Margolit 2019]. Given political participation is measured at midline, the potential mediators we consider are those that are shifted over the same time frame: (i) perceived economic standing; (ii) deservedness of the rich; (iii) poverty as driven by structural factors; (iv) pro-market mindsets; (v) pro-social mindsets in the form of trust in neighbors. For the TP, pro-market mindsets are the strongest mediator for participation (accounting for 71% of the total mediated effect). However, once economic mediators – such as consumption and livestock ownership – are added, then in line with resource-based theories of voting, these are the most important mediator: livestock ownership accounts for 57% of the total mediated effect, while the remainder largely comes through pro-market mindsets and perceptions of the rich. Overall these results highlight the importance of economic *and* non-economic channels for political participation of the rich and poor. Given lasting economic impacts of the interventions on the TP, the results leave open the prospect that because of the resource channel, in the long run the TP become relatively more likely to engage in political processes than the NTP or NP, but we cannot validate this because no such high-stakes processes take place between midline and endline.

Big push pro-poor interventions hold immense promise for pulling the world's poor out of poverty. Our core contribution to is to advance understanding of the economic and non-economic impacts of such interventions, in changing levels, rankings and inequality of economic outcomes in village economies, shifting perceptions, mindsets and policy preferences of the rich and poor, and ultimately moving the dial on political participation that might then lead to wider social change. We do so using a two-stage experimental design in small village economies where we track the treated poor, the not treated poor and non poor households over four years to construct a detailed picture of the general equilibrium impacts of big push pro-poor interventions on how beneficiary and non beneficiary households view the world around them.

Our results provide multiple insights on the non-economic impacts of big push pro-poor interventions. In Figure 1 we distinguish those links that have been much studied (light blue) from those more novel to our study (dark blue). Long-standing literatures we build on across the social sciences include: (i) what shapes redistributive preferences [Meltzer and Richard 1981]; (ii) whether greater engagement in anonymized market exchange risks crowding out trust in others [Heβ *et al.* 2021, Margalit and Shayo 2021]. On (i) our findings advance earlier evidence based on lab experiments [Fisman *et al.* 2007, Fisman *et al.* 2021], non-experimental studies on how 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]. On (ii) we document that in our context experimentally induced changes in pro-market mindsets and trust in neighbors move together: we find no evidence that increasing one crowds out the other. In other words, markets and communities are not seen as substitutes or a zero sum game. We detail our link to these various literatures as we present our results, especially emphasizing insights provided by our data and design that covers beneficiary and non beneficiary households.

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

First, shifts in perception, mindsets, policy preferences and political participation largely do not depend on whether an individual is an actual beneficiary of the intervention or not – rather they are driven by common village-wide exposure to pro-poor policies. Our experiment thus addresses a long-standing issue in the literature studying how economic attitudes respond to economic shocks, suggesting in our context, attitudes are driven by sociotropic concerns that relate to wider community well-being, rather than narrow self-interest – as has been emphasized in the political science literature largely in the context of redistributive preferences [Margalit 2019] and the nascent literature on how policy views are formed [Stantcheva 2024]. Our experiment reveals that all groups – the TP, NTP and NP – have changed non-economic outcomes at midline in response to big push interventions. This is despite the very different impacts of the interventions on the economic outcomes across these groups. *A fortiori*, such policies do not polarize perceptions, or create backlash within villages – in nearly all cases impacts on the poor and non poor are of the same sign and similar magnitude.

Second, we find little evidence that shifts in perceptions, mindsets or policy preferences are persistent, despite long-lasting impacts on the actual economic circumstances of the treated poor and village inequality. Understanding dynamics of non economic outcomes in response to pro-poor interventions – either the causes of dynamic wedges between economic reality and perceptions, or whether long run wedges appear in the political participation of beneficiaries and non beneficiaries – remain key open for future studies to consider.

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

Section 2 describes our context, interventions and research design. Section 3 examines impacts on noticeable economic outcomes and village inequality. Section 4 details how perceptions, mindsets and redistributive preferences are shifted by the interventions. Section 5 discusses impacts on political participation, and how this is mediated through the channels considered in our theory of change. Section 6 concludes by discussing 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

Interventions take two forms. The first offered households productive assets in-kind. To determine the assets to offer, in each village we initially conducted a market assessment of those assets likely to generate high returns. These included livestock, assets to start a retail business (e.g. grocery shop, fruit stall), crop farming, and other forms of self-employment (e.g. tailoring). Figure A1 shows a stylized representation of an asset menu. Households were free to choose any combination of assets off the menu up to a total value of PKR50K (500USD in 2012 prices). In conjunction with these transfers, households were offered micro-enterprise training, as well as skills specific to the chosen asset(s). The value of training was fixed at PKR12K. Hence the total value of transfers and training offered was PKR62K (around 620USD). We refer to this as treatment T1.[1]

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

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

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

Eligibility To establish eligibility, we first conducted a census of $35,522$ households in our villages. Each was assigned a 0-100 poverty score based on characteristics proxying household's permanent income. Households with a score of 0-18 are deemed to be poor and hence eligible for the interventions. The interquartile range of poverty scores is 19 to 37, with the highest decile of households scoring above 46. The poverty score construction is similar to that used to target welfare programs to the rural poor in Pakistan, including the Benazir Income Support Programme. This is the most widespread social protection program in Pakistan, reaching nearly five million households in 2012. Households are thus familiar with the kind of poverty score used to determine eligibility. Not treated poor households were made aware by PPAF that the treated poor were randomly selected among eligibles, and given no promise of future treatment. Not poor households were aware they were never going to be eligible.[4]

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

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

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

[4] The poverty score combines information on: (i) the number of dependents aged 18-65; (ii) the highest education level of the household head; (iii) the number of children age 5-16 in school; (iv) the number of rooms per household

2.3 Research Design

Randomization, Sampling, Take-Up and Timeline 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).

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

In both treatment arms, there is near 100% take-up of the offer of transfers. In T1, 50% of eligibles 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.[5]

We conducted our census from May to July 2012, and our baseline survey from February to June 2013. Interventions were rolled out January-March 2014. We focus on the one, two and four-year follow-up surveys that were fielded May to July 2015, September/October 2016, and February/March 2018. Noticeable economic outcomes are measured at the one, two- and four-year follow ups. Perceptions, mindsets and policy preferences are measured at the two-year midline and four-year endline. Between baseline and midline, high stakes local elections were held across our study region, enabling us to examine impacts on political participation.

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

member; (v) the type of toilet used; (vi) asset ownership (including land and livestock). A weighting scheme within each category then combines to produce scores between 0 and 100.

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

treated and control villages is 13kms, with travel times to market and state infrastructures such as livestock markets or police stations being around an hour. Panel B focuses on village poverty. The average household poverty score is 29, with the standard deviation of scores across households being just under half the mean. Around 23% of households are classified as poor (and therefore eligible). Of those, around 45% are actually treated (creating the division between the TP and NTP in treated villages).

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

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

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 Outcomes

3.1 Empirical Method

We begin our analysis by considering the initial link in our theory of change from Figure 1: from the economic shock to economic outcomes. This lays the foundations for how perceptions, mindsets and policy preferences might then be shifted by big push pro-poor interventions, and ultimately how these channels feed through to shifting political participation. We estimate intervention impacts on the following economic outcomes (y_{hvt}): whether the household owns livestock, the (log) value of livestock owned conditional on ownership, whether the household has an iron roof or has a cement roof (both of which are only measured at one year post-intervention but are durable and irreversible investments), whether the household often consumes home produced milk, and (log) monthly food and non-food expenditures. We do not claim these are the most important dimensions of impact for well-being, but they are relevant for the current study because, by leading to noticeable changes in small village economies, they leave less scope for misperceptions of intervention gains to persist [Alesina *et al.* 2021].[6]

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

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

Two further points are of note. First, gains to the TP relative to the NTP accrue within a year post-intervention, and stabilize thereafter until 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 we pool treatments for the remainder of the analysis. We showed earlier in Tables A1 and A2 that the samples are balanced on village and household characteristics between controls and pooled treated villages and households.

Table 4 repeats the exercise pooling treatments, allowing gains to be more precisely estimated. Across all margins, TP households have significant impacts relative to the NTP. 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), 3.2pp more likely to have cement walls (a 16% increase over baseline), are around 20% more likely to have improved diets as measured through the consumption of own produced milk, gains in food consumption of around 3% over baseline (the short run fall in consumption might reflect the switch from market purchased dairy products to home production), and gains in non-food expenditures of 5% over baseline.

To get a sense of how these impacts change the ranking of TP households relative to NTP households (that are observationally identical at baseline given the second stage of randomization in treated villages), we note the 16% increase in the value of livestock owned by the TP corresponds to a TP household moving from the mean (median) to the 72nd (58th) percentile among NTP households; the 3.7% increase in the value of food consumption corresponds to a TP household moving from the mean (median) to the 62nd (55th) percentile among NTP households; the 5% increase in the value of non-food consumption corresponds to a TP household moving from the mean (median) to the 77th (53rd) percentile among NTP households. An alternative way to benchmark the impacts is to consider how their magnitude corresponds to baseline gaps between poor and non-poor households. Using this approach, the 16% increase in the value of livestock owned by the TP corresponds to 33% of baseline gap with the NP at baseline. This all suggests the gains from the intervention to the TP are meaningful, and can thus potentially cause shifts in political participation via shifts in perceptions, mindsets and policy preferences.[7]

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

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

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

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

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

3.3 Village Inequality

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

consistent with our results.

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

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

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

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

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

4 Perceptions, Mindsets and Policy Preferences

Given this backdrop of big push pro-poor interventions impacting levels, rankings and inequality of economic outcomes, we can turn to the next link in our theory of change laid out in Figure 1, to understand whether these changes feed through to shift perceptions, mindsets and policy preferences of household heads (that in 98% of cases are men). These are some of the potential mechanisms through which the interventions might ultimately shift engagement in political processes and drive forward wider change.

To do so, we exploit the between village randomization that enables us to establish impacts on TP, NTP and NP households. We estimate treatment effects using the following specification for heads of household in group $g \in \{TP, NTP, NP\}$:

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

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

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

[10] There are 134 enumerators with nearly all being used at midline and endline, and the majority operating across

Standard identifying assumptions for the treatment effects on each group are that there is random assignment, and that there are no spillovers onto controls. The effects on NTP and NP households capture their exposure to the interventions, that can operate through: (i) observing intervention impacts on the TP and village as a whole; (ii) any changes in their own economic circumstances occurring through spillovers or general equilibrium effects not captured by the margins of noticeable outcomes considered earlier; (iii) any emotional connection with beneficiaries – that is relevant given our setting is one in which there is a close proximity of poor and non poor households and a likely complex set of family and network ties between them.

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

4.1 Perceptions of Economic Outcomes

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

4.1.1 Own Economic Standing

We start by examining how households' perceive their own economic standing. We do so by asking, *On a ladder with 10 steps, where do you currently stand?* The results are in Table 6 where we show midline and endline impacts for TP, NTP and NP households, estimated from (4). Focusing first on the results for 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.

Table A5 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

treatment and control villages. The median (mean) number of interviews conducted by each is 163 (223).

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

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

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

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.[12]

In short, for the TP and NP, there is a wedge between perceived economic standing and reality, while for the overtaken NTP, their perceptions better reflect reality at least at midline.

The results are in line with findings from higher income settings that individual well-being can fall when individuals observe changes in wealth/income in people around them [Luttmer 2005, Card *et al.* 2012, Perez-Truglia 2020, Cullen and Perez-Truglia 2022]. As such, the results highlight the potential for pro-poor interventions to generate negative psychological spillovers to non-beneficiaries, although households appear to adapt to this by endline.

Misperceptions can stem from households being imperfectly informed about their own relative standing [Benabou and Ok 2001, Alesina and Angeletos 2005, Cruces *et al.* 2013, Hoy and Mager 2021, Hvidberg *et al.* 2023]. To examine this issue, in Figure A2 we plot the difference between household' true and perceived income rank (grouped into seven bins), against their actual reported monthly income. In line with existing evidence we find that poorer households tend to overestimate their income rank (so those with the lowest monthly income have a positive difference between their true and perceived rank) and the opposite is true for higher income individuals. These misperceptions might make it harder for rich and poor households to recognize stark changes in economic reality caused by big push interventions even in small village economies.

4.1.2 Village Inequality

We next ask whether households perceive changes in village level inequality. To examine this we asked household heads whether: (i) inequality in their village has decreased in the last three years; (ii) the share of households in the village that do not have enough to eat has fallen. The results are in the remaining Columns of Table 6.[13]

We see a near complete set of null impacts across both perceptions of inequality for the TP, NTP and NP. These null impacts are again quite precise. 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 mostly imprecisely estimated. The endline impact for TP households is

[12] 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.

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

$-.005$, and the 95% confidence interval rules out an impact larger than .005, or 6% of the view held by the TP in controls.[14]

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

4.2 Perceptions of Others

We next move beyond perceptions of *specific* economic outcomes – their own standing and village inequality – to *broader* perceptions of entire classes of households. Specifically, we consider views towards the rich and poor, that might in turn drive political participation.

4.2.1 Perceptions of the Rich

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

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

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

to 11% of the poor holding this view in controls. The NTP share this change in perception: their likelihood to report a negative view of the rich falls 3.0pp by endline.

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

4.2.2 Perceptions of the Poor

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

Focusing first on the character of the poor, we asked households whether they thought the poor: (i) *lack the ability to manage money or other assets*; (ii) *waste their money on inappropriate items*; (iii) *do not actively seek to improve their lives*; (iv) *are not motivated because of outside support from government/NGOs*. NP households were only surveyed on these questions at endline. 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, the results in Table 8 show little evidence that views of the character of the poor are shifted by exposure to the big push pro-poor interventions.

Why are the Poor Poor? We then consider perceptions of what drives poverty: structural features of the economy or destiny/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 9 shows that in each case the outcome is whether the household head agreed or strongly agreed with the statement. 70-80% of respondents in controls at midline agree/strongly agree with each statement about the structural causes of poverty, irrespective of whether they are themselves poor. The belief in structural causes of poverty is thus far more prevalent among all households than negative views of the character of the poor.

At midline, the interventions cause significant falls in the view that poverty is driven by structural factors. This holds across all four factors and magnitudes of impacts vary between 5pp and 9pp, and with seven out of eight estimates being robust to MHT. However, by endline these treatment effects fade.

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 10 shows that 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 views 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 ($p = .022$, $q = .124$).[15]

4.2.3 Taking Stock

The backdrop of large and persistent actual economic gains to the TP, changes in relative standing of the NTP and reductions in village inequality translate into relatively muted changes in perceptions of these economic changes. In contrast, more pronounced changes occur in terms of broader perceptions of classes of households: all households are more likely to believe that the rich are rightfully deserving and are less likely to view poverty as driven by structural causes. This all suggests exposure to individuals generating further wealth after being given a transfer raises the perception that wealth is earned. These changes in outlook can in turn shape mindsets, and political participation, as we come to when completing our theory of change.

4.3 Mindsets

We now consider how changes in economic circumstances feed through to mindsets – a cognitive lens through which individuals interpret information and form judgements [Stantcheva 2024] – and the second channel in our theory of change linking economic interventions to political participation [Enke 2024].We consider two broad types of mindset: (i) market-orientated beliefs; (ii) pro-social views in the form of trust in neighbors.[16]

4.3.1 Pro-Market Mindsets

The occupational choice of beneficiaries is transformed through the interventions, enabling them to combine their labor with capital, and engage to a greater extent day-to-day in market transactions through the sales of livestock produce for example. The pro-market views of the TP can thus be shifted through such intervention impacts. Pro-market mindsets of the NTP and NP can also shift if there is any demonstration effect of beneficial impacts of market engagement of the TP.

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

[16] Enke [2024] puts forward an argument for the re-integration of moral psychology and political economy, overviewing the literature demonstrating that economic outcomes shape moral views, views of economy policy and redistribution, and how moral views or mindsets can shape political engagement.

To bridge to the earlier literature, we measure pro-market mindsets as in Di Tella *et al.* [2007], by creating a 0-4 index capturing beliefs over individualism, meritocracy, materialism, and generalized trust (with this last component being included because trust in others is a foundation for anonymized market exchange). We sum positive answers to the following questions: (i) *do you believe that it is possible to be successful on your own or do you need a large group that supports each other?*; (ii) *in general, people who put a lot of effort in working end up much better than those who do not put an effort?*; (iii) *do you believe that having money is important to be happy?*; (iv) *in general, in our country, would you say that one can trust other people?*

Columns 1a to 1c in Table 11 shows how the pro-market index overall is impacted. All groups of household hold significantly stronger pro-market mindsets at midline. The impact on the TP is .198 ($p = .000$, $q = .001$) from baseline level of 2.4 among controls. The magnitudes of impact on the NTP and NP are similar. However, for each group, we see a significant decline in these views by endline ($p = .008$, .022 and .050 respectively).

Table A6 shows how each component of the pro-market index shift across groups. Changes in the aggregate index are driven by more strongly held beliefs in meritocracy, materialism, and generalized trust in others. The TP are 6pp more likely than controls at midline to report effort is important for success, they are also 6pp more likely to report that money is important for happiness, and the largest proportionate increases are for generalized trust in others – where the TP are 6.4pp more likely to report trusting other people in Pakistan than controls, relative to a baseline of 42.9pp.[17]

4.3.2 Trust in Neighbors

A long-standing concern expressed across social sciences is that greater engagement in anonymized market exchange risks crowding out informal exchange and forms of social capital [Bowles 1998, Attanasio and Ríos-Rull 2000, Attanasio *et al.* 2015, Heβ *et al.* 2021]. We examine the issue through considering mindsets of trust in neighbors. To do so we construct a 0-4 index measuring trust in neighbors by summing positive answers to: (i) *suppose you are walking down the road and without your noticing, your wallet with ID card falls to the ground. Someone finds your wallet and can trace you. Will they return the wallet to you?*; (ii) *do you feel the rule of law operates?*; (iii) *compared to the situation three years ago, do you think the level of crime in your village has decreased a lot?*; (iv) *do you feel safe in your village?* Treatment effects on each group of households on this index are shown in Columns 2a to 2c of Table 11, while Table A7 shows how each component of this index shifts.

The aggregate index of trust in neighbors significant increases for all groups at midline. The impact on the TP is .179 ($p = .001$, $q = .006$). At midline, the magnitude of impact is similar

[17] Margalit and Shayo [2021] present evidence from a field experiment in England to evaluate the impact of engagement in financial markets on beliefs over merit, deservingness, personal responsibility, and equality. They also find treated subjects shift right on policy, driven by growing familiarity with, and trust of, markets.

across the TP, NTP and NP, but they fade by endline. For the NTP and NP, these declines over time are statistically significant ($p = .058$, $.003$ respectively).

To understand what drives these changes, Table A7 shows that for all groups, changes in the index are driven by a stronger view that the rule of law operates, that crime is down relative to three years ago, and feeling safe. For example, the treated poor are 4.4pp more likely at midline to report the rule of law operates in their village, and they are 6pp more likely to report that crime is down relative to three years ago. At midline, the non poor report crime being down by 10.2pp (that represents a larger proportionate change relative to controls). Finally, the treated poor are 3.5pp more likely to report feeling safe in their village. This is a dimension along which intervention impacts are sustained at endline: four-years post intervention, the treated poor remain 2.3pp more likely than the poor in controls to feel safe in their village.[18]

These changes are all in the same direction as pro-market beliefs. We find no evidence that increasing one crowds out the other. In other words, markets and communities are not seen as substitutes or a zero sum game. One reason these mindsets can shift together is that they both relate to motivations to exert productive effort (consistent with the earlier documented rise in views of the deserving rich). Specifically, the first two components of the pro-market beliefs index can be seen as encouraging productive effort and activity. Similarly, some components of the trust in neighbors index can also be seen as encouraging productive effort because individuals perceive themselves to be more secure and their returns to effort are less likely to be expropriated.

In turn, these changes in pro-market and pro-community mindset can drive political participation, that we turn to below when we complete the links in our theory of change.

4.4 Redistributive Preferences

The third route through which our theory of change proposes big push interventions can change political participation is by shifting policy preferences. Specifically, we consider preferences for redistribution. The workhorse framework for understanding redistributive preferences is Meltzer and Richard [1981] (MR). Their model assumes self-interested individuals and predicts 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 their economic well-being improves pro-poor interventions. More generally: (i) if pro-poor interventions generate positive spillovers, broad support for future redistribution might develop across a village; (ii) on the other hand, redistribution might create a group of discontented citizens (due to relative decline in their standing), so support for future redistribution might decline.

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

[18] Given the remoteness of these villages from state institutions – they are on average an hour travel time away from the nearest police station – these changes are likely coming from the perceived behavior of other households, not responses of law enforcement to the resource injections into villages from the interventions.

redistributive preferences based on four questions.[19]

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 the perception that *the rich rightfully deserve their income*, discussed earlier. 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 in controls hold relatively pro-redistributive preferences, with an average score of 3.13. However, 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.[20]

Table 12 shows redistributive preferences are relatively inelastic across groups and time. Among the TP at midline, we can rule out an increase in the redistributive preferences index greater than .105 or 3% of its baseline level in controls. To understand whether the null impact on the index masks underlying offsetting changes, the remaining Columns show results for each component of the index. On the first, although the vast majority of controls agree with the statement that *the rich should give a part of their income to the poor*, 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.

The second component of the index of redistributive preferences was framed in terms of redistributive responses towards the poor when others receive a substantial windfall. At midline the TP and NP are significantly more likely to believe large windfalls should be taxed to redistribute

[19] The elicitation of redistributive preferences broadly falls into two categories: experimental and non-experimental approaches. Experimental methods are typically implemented in lab settings [Cappelen *et al.* 2007, Fisman *et al.* 2007, Cappelen *et al.* 2013, Fisman *et al.* 2015], or through online platforms where participants complete tasks [Almas *et al.* 2020]. These approaches are behavioral, in that researchers observe and measure participants' actions to infer their redistributive preferences. In contrast, non-experimental approaches [Okun 1975, Kuziemko *et al.* 2015, Andersen *et al.* 2023] rely on more direct questioning and are thus considered non-behavioral.

[20] 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.

towards the poor, but these changes are not sustained at endline.

The third component 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).

The final component relates to views of the rich, that were described earlier and show a big shift towards across all groups viewing the rich as being deserving.

Overall then, in the long run, redistributive preferences are inelastic to exposure to big push pro-poor interventions. Slight nudges forward on the first component are offset by more favorable perceptions of 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 beneficiaries have emotional connections with beneficiaries – does not in itself generate demand for more/less redistribution.[21]

The results again highlight the value of our experimental design. Viewed through the lens of theory, MR has the basic prediction that the redistributive preferences of the TP should weaken as they economically gain from receipt of the asset/cash transfers. This is in line with their response at midline. However, our design reveals similar shifts occur among the NTP and NP, in contradiction of the MR model based on self-interest alone, and more in line with community-wide attitudinal shifts shaped by exposure to the interventions rather than beneficiary status *per se*.

4.4.1 Robustness

Given that many earlier studies have also found results counter to the basic MR intuition, a 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 preferences are shaped by whether: (i) luck or effort are viewed as responsible for individual success – that is relevant in our context given the exogenously timed and targeted interventions [Piketty 1995, Bénabou and Ok 2001, Fong 2001, Alesina and Angeletos 2005, Cappelen *et al.* 2013]; (ii) beliefs over the effectiveness or representativeness of government, NGOs and the private sector – that is relevant in our context given the intervention is delivered by a quasi-governmental NGO and households may be concerned over corruption or leakage [Alesina and Giuliano 2011, Sapienza and Zingales 2013, Kuziemko *et al.* 2015, Alesina *et al.* 2018]; (iii) actual levels of village inequality. Finally, to gauge redistributive preferences from another perspective, we asked households about their ideal income distribution in society, following the graphical approach of Gimpelson and Treisman [2018].

[21] 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.

5 Political Participation

We now come to the final link in the causal chain from our theory of change: the impact big push interventions have on political participation. We first examine this outcome directly, and then study how various perceptions, mindsets and policy preferences mediate impacts, as well as how improvements in economic well-being raise political participation directly as has been emphasized in the long-standing literature focused on resource-based theories of voting [Brady *et al.* 1995, Glaeser and Ward 2006, Margolit 2019].

We can study the issue because 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 processes using self-reported data on turnout in these elections. Of course such self-reports are likely biased, but if this bias does not differ between treated and control villages, the estimated treatment effects remain informative.[22]

The results are in Table 13. 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. In controls, the poor have turnout rates that are 4.5pp higher than those of the NP, hence the big push interventions effectively almost entirely close the gap in political participation between the poor and non poor.[23]

As the median voter will typically be from a non-eligible household, it is important to consider the possibility that across groups, votes for political parties might be swayed by the interventions. To probe this, we exploit the fact that at baseline we asked TP and NP households 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

[22] Two further points are of note in relation to participation in these elections. First, local government elections were first established by the Musharraf regime, with elections taking place in 2001 and 2005. However, local government elections were then not held again until 2015 – the ones we study – when as a result of continuous pressure from courts to force civilian governments to comply with the requirements of the Constitution, they were reinstated [Liaqat *et al.* 2018]. The rarity of these elections might then contribute to higher than normal turn out rates. Moreover, there are no incumbents candidates and so we do not study whether exposure to the interventions changes support for incumbents as has been considered by other studies examining the electoral impacts of cash transfers [Manacorda *et al.* 2011]. Second, on validating turnout rates – that are for male heads of household – we first note that no official data on turnout by gender in these local elections exists. Liaqat *et al.* [2018] report that overall turnout rates for these elections was 61%. As a point of comparison we note that in the 2013 general election overall turnout in Punjab was 60%, with male (female) turnout being 64% (55%) and the gender gap in rural Punjab being 8-12pp and larger in southern and less developed districts – our study context. These factors could combine to plausibly suggest male turnout rates above 80% for the local elections we study.

[23] 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], and from UCT programs in the US [Brookman *et al.* 2024].

and 17% are right leaning.[24]

The remaining Columns in Table 13 show heterogeneous impacts on voting by political affinity expressed at baseline. Among the TP and NP, 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 baseline political affinity for either the rich or the poor.

5.1 Mediation

We consider the theory of change together by using mediation analysis to understand the relative importance of the mechanisms, following the approach of Gelbach [2016]. The basic intuition is that the treatment effect of intervention T on outcome Y can be decomposed as operating through as set of mediators, m_j:

$$\frac{dY}{dT} = \sum_{j=1}^{k} \frac{\partial Y}{\partial m_j} \frac{\partial m_j}{\partial T} + R, \tag{6}$$

where R is the part of the treatment effect which cannot be attributed to any observed mediator. The method has the advantage that it is invariant to the order in which mediators are considered.

Given voting outcomes are measured at midline, the baseline set of mediators we consider are those perceptions, mindsets and component of redistributive preferences that are shifted over the same time frame: (i) perceived standing; (ii) pro-market beliefs; (iii) trust in neighbors; (iv) deservedness of the rich. We present results both pooling all households, and separately for TP, NTP and NP households. We then extend the analysis in two ways: (i) to examine resource-base theories of political participation by adding consumption and livestock ownership as potential economic mediators; (ii) we allow views on poverty as driven by structural factors to be a mediator (as that is also shifted at midline), but then conduct the analysis for TP and NTP households

[24] 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.

only (as these perceptions were not asked for NP households at midline).

Panel A of Figure 2 shows the results using the baseline mediators. Pooling all households we see that political participation is mediated through non-economic channels such as pro-market mindsets and trust in neighbors. Splitting across households, for the TP, pro-market mindsets are the most important mediator (accounting for 71% of the total mediated effect), while trust in neighbors plays no role. For the NTP, pro-market mindsets are also the most important mediator, but trust in neighbors also mediates some of the effect. For NP households, pro-market beliefs more weakly mediate impacts (accounting for 20% of the total mediated effect), and perceptions of their current standing strongly reduce political participation.

In all cases we note that the only a small share of the total impact of the interventions on political participation is mediated through the set of baseline mediators – this is not altogether surprising given the binary outcome and the underlying assumption that the mediators do not impact each other. By ignoring any complex interactions within the mechanisms in Figure 1 – we leave more of the overall treatment effect unexplained.[25]

Panel B considers additional mediators related to economic channels, so allowing for a direct link between the economic impacts of the interventions and political participation, as shown in Figure 1. When we consider the full set of mediators, we find that for the TP livestock ownership is the single most important mediator – accounting for 57% of the total mediated effect (pro-market mindsets still account for 27%, and trust in neighbors play no role for the TP). For the NTP, these economic mediators play little role over and above the non-economic mediators considered before. Finally, for NP households, measures of economic well-being and perceptions of their current standing strongly reduce the likelihood of participating in political processes. In other words, while the exogenous positive shock to economic well-being received by beneficiaries positively mediates their political participation, such a resource based channel does not operate for non-beneficiaries – and in fact works in the opposite direction.

Panels C and D repeat the analysis adding perceptions of poverty as being driven by structural factors as a mediator. We see that: (i) from Panel C, pro-market beliefs remain the most important mediator for political participation among the TP; (ii) viewing poverty as a structural factor has no mediating effect for the TP but for the NTP, this view of the world reduces political participation; (iii) from Panel D, adding economic mediators, for the TP, livestock ownership remains the most important mediator, accounting for 53% of the total mediated effect.

Overall these result highlight the importance of economic and non-economic channels for political participation of the rich and poor. Given lasting economic impacts of the interventions on the TP (Table 3), the results leave open the prospect that because of this resource channel, the

[25] In our context such interactions might be important – for example, views over current economic standing might itself shape redistributive demands through prospects for upward mobility (POUM) [Piketty 1995, Benabou and Ok 2001, Fong 2001, Alesina and La Ferrara 2005, Alesina *et al.* 2018]. Similarly, views towards the rich and poor and the causes of their relative standing might also in turn shape policy preferences and *vice versa*.

TP become relatively more likely to engage in political processes over time than the NTP or NP, but we cannot validate this in our data because no such high-stakes elections take place between midline and endline.

6 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 specific economic circumstances and of the rich and poor more broadly, mindsets and redistributive attitudes, ultimately with a view of understanding whether such channels mediate political participation that can lead to longer term changes. We structure our analysis tightly around the theory of change in Figure 1, where we distinguish those links that have been much studied in earlier literature (light blue) from those more novel to our study (dark blue). Our analysis is based on a large-scale and long-term experiment combining layers of between and within village randomization, and tracking treated poor, not treated poor and not poor households to build a rich picture of the dynamic and general equilibrium effects of such interventions across the economic and non-economic outcomes highlighted in our theory of change.

Our data and design allows us to go beyond the study of beneficiaries themselves. This reveals that economic self-interest does not explain our findings – many non-economic outcomes of non beneficiaries are similarly shifted through their exposure to the big push pro-poor interventions underlying our study. Shifts in perception, mindsets, policy preferences and political participation largely do not depend on whether an individual is an actual beneficiary of the intervention or not – rather they are driven by common village-wide exposure to pro-poor policies, in line with attitudes being driven by sociotropic concerns that relate to wider community well-being, rather than narrow self-interest – as has been emphasized in the political science literature largely in the context of redistributive preferences [Margalit 2019] and the nascent literature on how policy views are formed [Stantcheva 2024]. *A fortiori*, such 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.[26]

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

[26] Our findings also suggest that big push interventions can drive perceptions and mindsets even when those experiences occur late in life – our household heads are aged in their early 40s at baseline. This complements work emphasizing how experiences in formative years are more likely to determine long run attitudes and behaviors [Malmendier 2021, Giuliano and Spilimbergo 2023].

6.1 Asset Transfers versus Revealed Preferred Cash Transfers

We exploit the treatment arms to examine whether in-kind asset transfers and reveal preferred unconditional cash transfers have similar impacts on perceptions, mindsets and policy preferences. These results are summarized in Figures A4 to A7, in which each panel shows the estimated treatment effect $(\widehat{\beta}_{2j}^{g}, \widehat{\beta}_{4j}^{g})$ for group g and treatment arm j from the between village estimates, and we indicate whenever impacts differ across treatment arms at conventional levels of statistical difference. Treatment T1 refers to when the poor are offered a menu of in-kind asset transfers. Treatment T2 refers to when households are additionally offered the equivalent valued cash transfers, and the majority reveal prefer cash over in-kind transfers.

On most dimensions, we find little differential impact, for any group and in either time period, between when the poor are assisted with asset or cash transfers. More precisely, Figure A4 focuses on perceptions of own standing and village inequality, so the outcomes from Table 6. The estimates are largely the same across treatment arms, for each group of households, and across both midline and endline estimates. Figure A5 summarizes perceptions of the rich and poor, so outcomes from Tables 7 to 10. Shifts in the 14 perceptions considered largely do not differ depending on whether the poor are provided asset transfers, or reveal prefer cash over in-kind transfers. Perceptions on which the metric of transfers matters are: (i) that the rich are rich for positive reasons such as education/hard work, where this shift at endline is greater among the TP and NTP if the poor are provided asset transfers ($p = .012$, $.064$ respectively); (ii) that the poor are poor because they do not actively seek to improve their lives, where the shift at midline is greater among the TP and NTP if the poor are provided asset transfers ($p = .099$, $.045$ respectively).

Figure A6 shows how shifts in mindsets differ by treatment arm, so the outcomes from Tables 11, A6 and A7. For the TP, pro-market beliefs push forward more strongly at endline for those offered cash transfers ($p = .009$) – driven by the components of effort being important for success ($p = .003$) and money being important for happiness ($p = .003$). Figure A7 shows results for redistributive preferences and voting, the outcomes in Tables 12 and 13. Nearly all of these margins have impacts that do not differ depending on the form of assistance to the poor.

Finally, Figure A8 shows the mediation analysis for the baseline set of mediators, split by T1 and T2: the results are qualitatively similar across treatment arms. However, a noticeable difference is that among the TP, livestock ownership is a relatively more important mediator when asset transfers are provided – in line with livestock ownership being pushed forward more by T1 than T2 (as shown in Table 3).

6.2 Future Agenda and External Validity

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

Financing Interventions Our results suggest the link between pro-poor policy interventions, economic reality, perceptions, mindsets and political participation does not depend on whether households are themselves beneficiaries – rather our experiment reveals that these non-economic outcomes are largely driven by common village-wide exposure to such pro-poor policies. However, the big push interventions studied are financed and delivered by a quasi-governmental NGO – they are not financed through general taxation, nor through informal local taxation. The perceptions, mindsets and political participation 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 such outcomes across households might be shifted when within-village redistributive institutions, such as local taxation schemes, are used to target resources to the poor.

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

A Appendix

A.1 Redistributive Preferences

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].[27] 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 A8. 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 and Other Actors Redistributive attitudes might be easier to shift among those who hold stronger beliefs of government effectiveness [Sapienza and Zingales 2013, Kuziemko *et al.* 2015, Alesina *et al.* 2018]. This might be especially relevant in low state capacity context like Pakistan [Acemoglu *et al.* 2020].[28]

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 A9 shows the results, where we estimate treatment effects on the index 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'

[27] 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].

[28] 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.

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. We find similar uniform impacts on redistributive preferences examining other measures of belief in government, such as whether respondents report the government represents people like them (Table A10). Finally, for completeness we consider heterogeneous responses by views of the effectiveness of NGOs and the private sector. These results in Tables A11 and A12 show largely homogenous impacts across these views.

Other Dimensions We also examine whether the response of redistributive preferences to the interventions are shaped by village inequality. We do so consider the 90-10 percentile consumption ratio (Column 3 of Table 5) measured one year post-intervention (that does not change from baseline) and estimate heterogeneous effects on redistributive preferences at midline and endline of the village being above/below the median of this. The results in Table A13 show there are no such heterogenous impacts on redistributive preferences – they are inelastic to the interventions irrespective of prior levels of village inequality. We also note a similar result if we use the other measures of consumption inequality.

A.2 Ideal Income Distribution

To gauge redistributive preferences from another perspective we asked households about their ideal income distribution. Panel A of Figure A3 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%).[29]

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.

[29] 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.

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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 in parentheses

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

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

Table 4: Noticeable Economic Impacts, Pooled Specification

Within Village Estimates Treated Poor vs Not Treated Poor

Standard errors clustered by village in parentheses

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

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

Table 5: Village Consumption Inequality

Between Village Estimates Treated vs Controls

OLS estimates, robust standard errors in parentheses

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

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

Table 6: Perception of Own Standing and Village Inequality

Between Village Estimates (Treated vs Control)

OLS estimates, standard errors clustered by village in parentheses

p-values in brackets, FDR adjusted q-values in braces

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

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

Table 7: Perceptions of the Rich

Between Village Estimates (Treated vs Control)

OLS estimates, standard errors clustered by village in parentheses

p-values in brackets, FDR adjusted q-values in braces

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

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

Table 8: Perceptions of the Character of the Poor

Between Village Estimates (Treated vs Control)

Strongly agree or agree with statements

OLS estimates, standard errors clustered by village in parentheses

p-values in brackets, FDR adjusted q-values in braces

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

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

Table 9: Perceptions of Poverty as Driven by Structural Causes

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

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

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

Table 10: Perceptions of Poverty as Destiny or Fate

Between Village Estimates (Treated vs Control)

Strongly agree or agree with statements

OLS estimates, standard errors clustered by village in parentheses

p-values in brackets, FDR adjusted q-values in braces

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

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

Table 11: Mindsets

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

	Pro Market Beliefs Index			Trust in Neighbors		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP
Two year impact	.198***	.196***	.174***	.179***	.152***	.199***
	(.054)	(.060)	(.057)	(.056)	(.055)	(.045)
	[.000]	[.001]	[.002]	[.002]	[.006]	[.000]
	{.001}	{.003}	{.004}	{.006}	{.009}	{.001}
Four year impact	-.027	.002	.023	.070	-.002	.016
	(.065)	(.062)	(.054)	(.062)	(.064)	(.041)
	[.675]	[.980]	[.669]	[.261]	[.971]	[.706]
	{.681}	{.961}	{.681}	{.244}	{.644}	{.644}
Two Year = Four Year	[.008]	[.022]	[.050]	[.187]	[.058]	[.003]
Mean Outcome, Controls	2.40		2.40	2.75		2.67
Observations	8126	9382	17004	8126	9382	17003

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Columns 1b, 2b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave level, and 95% confidence intervals are reported in brackets. The pro-market beliefs index consists of four components: (i) “do you believe that it is possible to be successful on your own or do you need a large group that supports each other?”; (ii) “in general, people who put a lot of effort in working end up much better, the same or worse than those who do not put an effort?”, presenting respondents with three possible answers (worse than those that do not put in effort; the same; much better than those that do not put in effort) – we convert these into a dummy equal to one for households that answered "much better"; (iii) “do you believe that having money is important to be happy?”; (iv) “in general, in our country, would you say that one can trust other people or that people cannot be trusted?” We follow Di Tella et al. [2007] in combining these components using a sum so this index takes values 0 to 4. The trust in neighbors index has four components: (i) “suppose you are walking down the road and without your noticing, your wallet with ID card falls to the ground. Someone finds your wallet and can trace you by the address on your ID card. Will they return the wallet to you?”, presenting respondents with four possible answers (will definitely give it back; will give it back if requires some effort; will give it back if it requires little or no effort; will not give it back) – we convert answers into a dummy equal to one for respondents answering "will definitely give it back" or "will give it back if it requires some effort."; (ii) “do you feel the rule of law is operative in your environment?”; (iii) “compared to the situation 3 years ago, do you think that the level of crime in your locality has [increased a lot, increased, stayed the same, decreased, decreased a lot]?” – we convert answers into a dummy equal to one if crime decreased or decreased a lot; and (iv) “do you feel safe in your village?” We sum across these outcomes to create our index, ranging from 0 to 4. 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 Preferences

Between Village Estimates (Treated vs Control)

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

	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?			The rich rightfully deserve their income		
	(1a) TP	**(1b) NTP**	**(1c) NP**	**(2a) TP**	**(2b) NTP**	**(2c) NP**	**(3a) TP**	**(3b) NTP**	**(3c) NP**	**(4a) TP**	**(4b) NTP**	**(4c) NP**	**(5a) TP**	**(5b) NTP**	**(5c) NP**
Two year impact	.007	.017	.055	.012	.020**	.030**	.060*	.039	.071**	.013	.017	.027*	.075***	.057*	.072***
	(.049)	(.043)	(.043)	(.011)	(.010)	(.013)	(.033)	(.035)	(.029)	(.016)	(.015)	(.015)	(.032)	(.030)	(.027)
	[.883]	[.695]	[.203]	[.279]	[.043]	[.018]	[.067]	[.258]	[.018]	[.416]	[.275]	[.084]	[.021]	[.062]	[.010]
	{1.00}	{1.00}	{1.00}	{.192}	{.117}	{.117}	{.202}	{.386}	{.122}	{.969}	{.969}	{.969}	{.064}	{.091}	{.064}
Four year impact	.053	.044	.028	.016*	.016	.005	.028	.034	.029	-.012	-.021	-.010	-.017	.005	-.001
	(.051)	(.050)	(.048)	(.008)	(.010)	(.009)	(.034)	(.036)	(.034)	(.018)	(.018)	(.014)	(.030)	(.031)	(.025)
	[.304]	[.388]	[.560]	[.052]	[.107]	[.535]	[.417]	[.337]	[.394]	[.492]	[.253]	[.487]	[.563]	[.876]	[.976]
	{1.00}	{1.00}	{1.00}	{.117}	{.117}	{.218}	{.386}	{.386}	{.386}	{.969}	{.969}	{.969}	{.731}	{.954}	{.954}
Mean in Controls	3.13		3.16	95.2%		93.8%	64.7%		66.9%	85.5%		86.1%	32.3%		31.0%
Two Year = Four Year	[.565]	[.712]	[.690]	[.806]	[.834]	[.177]	[.522]	[.919]	[.393]	[.260]	[.100]	[.080]	[.060]	[.327]	[.061]
Observations	7800	8988	16278	8126	9382	17004	7800	8988	16279	8126	9382	17004	8126	9382	17004

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

Table 13: Political Participation

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

	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2c) NP
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}	{.005}
Two year impact | centrist				.065***	.075***
				(.019)	(.027)
				[.001]	[.008]
				{.002}	{.005}
Two year impact | right leaning				.091**	.114***
				(.038)	(.024)
				[.018]	[.000]
				{.010}	{.001}
Mean Outcome, Controls	89.1%		84.6%	89.1%	84.6%
Baseline support:					
left leaning				14.2%	16.9%
centre				69.2%	69.3%
right leaning				16.6%	13.8%
p-values:					
Left leaning = Centrist				[.224]	[.912]
Left leaning = Right leaning				[.891]	[.208]
Centrist = Right leaning				[.529]	[.113]
Observations	4043	4677	8489	1589	5341

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Column 1b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. 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: Theory of Change

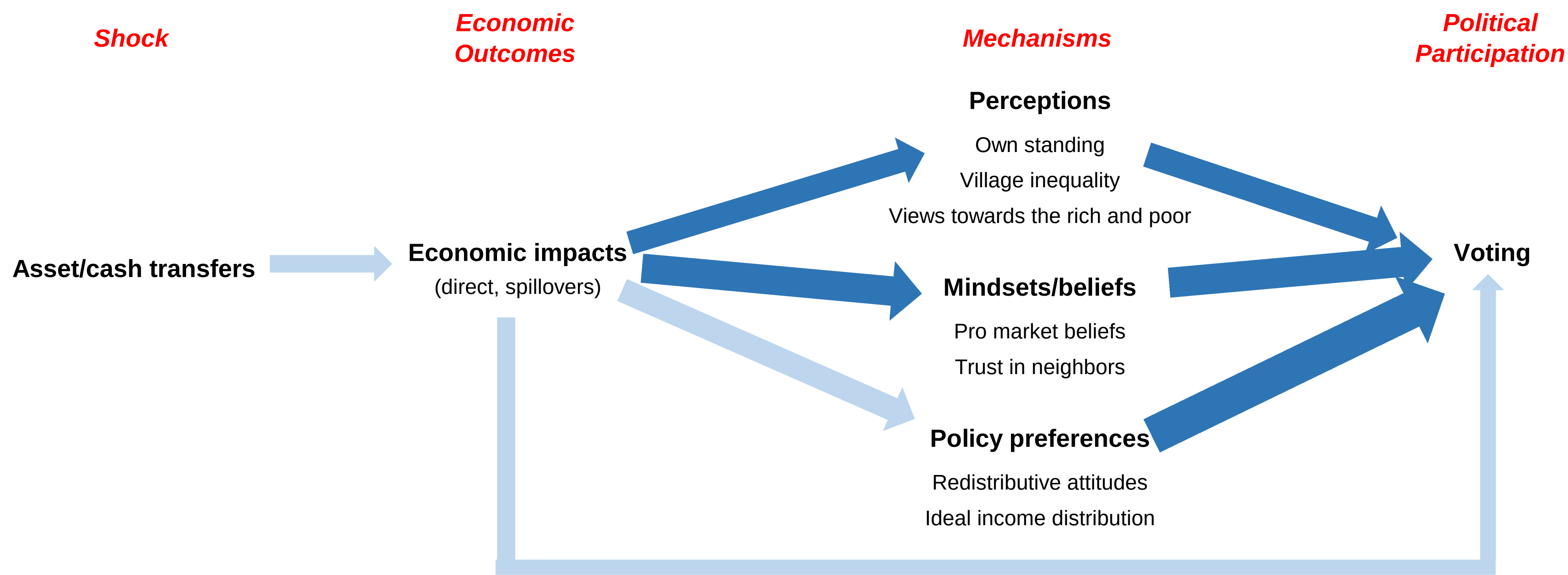
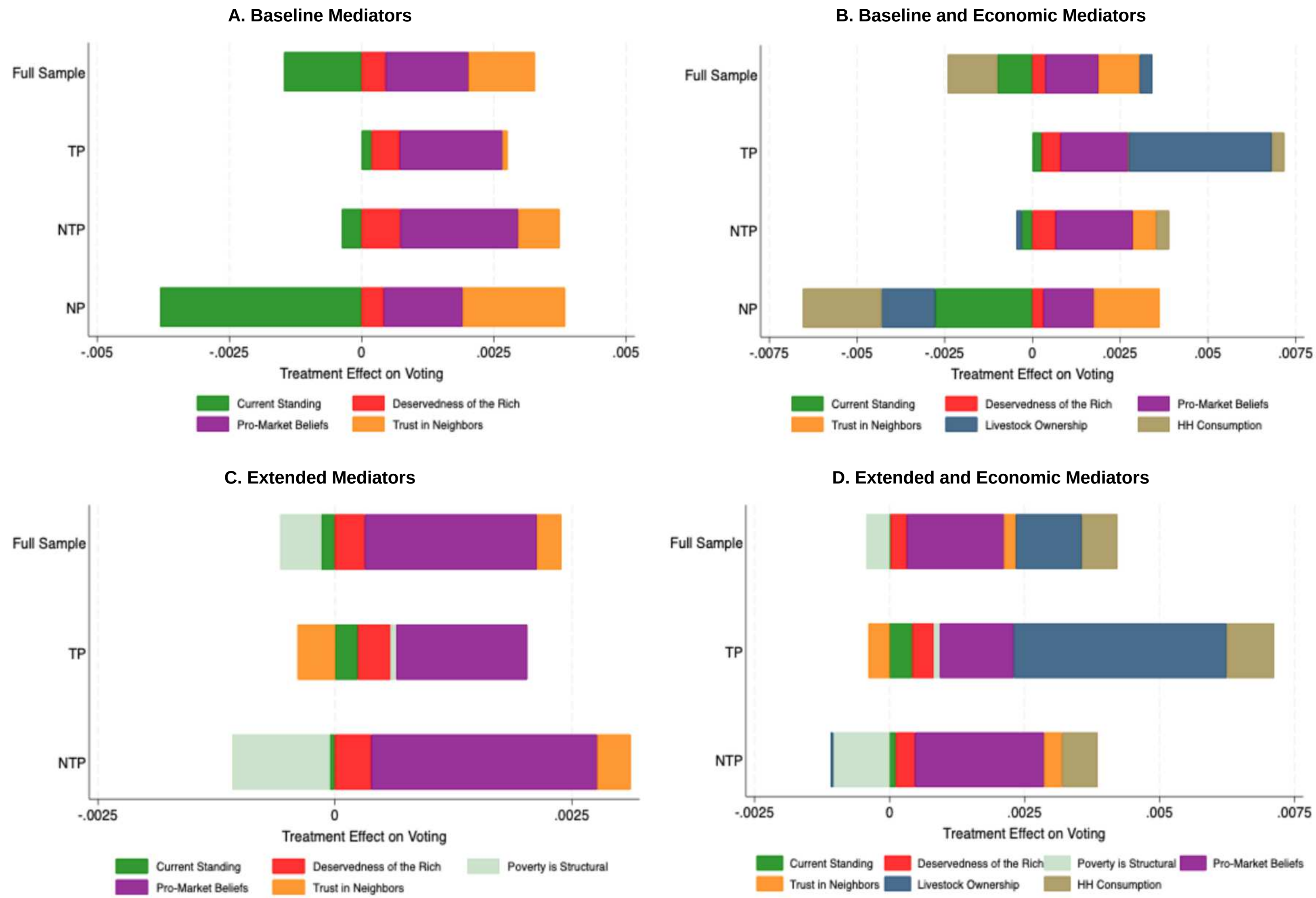


Figure 2: Mediation Analysis



Notes: The Panels reports results from a mediation analysis following Gelbach [2016]. We show results for all households pooled, as well as for the treated poor (TP), not treated poor (NTP) and not poor (NP) sepatrately. The outcome is a dummy variable indicating whether the respondent voted in the previous local election. The restricted base regression corresponds to the baseline specification shown in Table 13, while the unrestricted full regression augments this specification with the mediators listed in each Panel. The Panels show how much of the difference between the restricted and unrestricted regressions is explained by each mediator. The mediators include: perceived current standing (Table 6, Column 1), beliefs about the deservedness of the rich (Table 8, Column 1), beliefs about poverty being driven by structural factors (an index from 0 to 4 based on the outcomes in Table 10), the pro-market beliefs index (Table 13, Column 1), and the trust in neighbors index (Table 13, Column 2). In addition, we consider two economic mediators: livestock ownership (Tables 3 and 4, Column 1) and the log of monthly food expenditure per adult equivalent (Tables 3 and 4, Column 6).

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 clustered by village in parentheses

	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 in parentheses

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

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

Table A5: Perceptions of Economic Outcomes

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

	Perception of Current and Future Standing		Perceptions of Village Inequality	
	(1) Current: On a ladder with 10 steps, where do you currently stand?	**(2) Future: On a ladder with 10 steps, what is the best life you can achieve?**	**(3) Inequality decreased in the last three years**	**(4) Share in village that do not have enough to eat**
Two year impact	.121***	.068	.018	-.001
	(.045)	(.068)	(.017)	(.004)
	[.009]	[.321]	[.329]	[.902]
	{.010}	{1.00}	{1.00}	{1.00}
Four year impact	.135***	-.024	-.012	-.002
	(.050)	(.055)	(.020)	(.002)
	[.009]	[.668]	[.549]	[.254]
	{.010}	{1.00}	{1.00}	{1.00}
Mean Outcome, Controls	2.78	7.08	34.0%	9.05%
Two Year = Four Year	[.840]	[.299]	[.243]	[.764]
Observations	8126	8126	8126	8126

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor and Not Treated Poor households within treated villages (Columns 1, 2, 3, 4). 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. The third and fourth outcomes measure individuals' perceptions of village inequality. The outcome in Column 3 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 fourth outcome asks “Think of the people in your village who do not have enough to eat or sometimes may have to skip meals. Out of every 100 people, how many do you think are in that situation in your village?”. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention.

Table A6: Pro-Market Index Components

Between Village Estimates (Treated vs Control)

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

	Is it possible to be successful on your own (vs with a group)?			Is effort important for a successful life?			Is money important for happiness?			Do you trust other people 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
Two year impact	.014	-.002	-.027	.060**	.044*	.056**	.060***	.062***	.069***	.064**	.093***	.076***
	(.028)	(.026)	(.028)	(.024)	(.025)	(.023)	(.022)	(.021)	(.023)	(.026)	(.025)	(.022)
	[.624]	[.928]	[.335]	[.014]	[.083]	[.014]	[.006]	[.004]	[.003]	[.013]	[.000]	[.001]
	{1.00}	{1.00}	{1.00}	{.044}	{.125}	{.044}	{.013}	{.013}	{.013}	{.018}	{.001}	{.003}
Four year impact	.004	.013	.042	-.000	.011	-.002	.026	.016	-.000	-.040	-.038	-.016
	(.030)	(.027)	(.033)	(.030)	(.031)	(.027)	(.020)	(.021)	(.024)	(.025)	(.025)	(.027)
	[.662]	[.628]	[.202]	[.989]	[.723]	[.942]	[.201]	[.448]	[.988]	[.116]	[.138]	[.545]
	{1.00}	{1.00}	{1.00}	{.979}	{.979}	{.979}	{.178}	{.368}	{.492}	{.091}	{.091}	{.199}
Two Year = Four Year	[.503]	[.678]	[.108]	[.110]	[.392]	[.108]	[.267]	[.119]	[.035]	[.004]	[.000]	[.007]
Mean Outcome, Controls	51.7%		54.8%	66.4%		67.5%	78.5%		73.0%	42.9%		45.1%
Observations	8126	9382	17004	8126	9382	17004	8126	9382	17004	8126	9382	17004

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a, 3a, 4a), Not Treated Poor (Columns 1b, 2b, 3b, 4b), and Not Poor (Columns 1c, 2c, 3c, 4c) households in treatment and control villages. All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave 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.

Table A7: Trust in Neighbors Index Components

Between Village Estimates (Treated vs Control)

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

	If you lose your wallet, will someone return it?			Do you feel the rule of law is operative?			Crime is Down Relative to Three Years Ago			Do you feel safe in your village?		
	(1a) TP	**(1b) NTP**	**(1c) NP**	**(2a) TP**	**(2b) NTP**	**(2c) NP**	**(3a) TP**	**(3b) NTP**	**(3c) NP**	**(4a) TP**	**(4b) NTP**	**(4c) NP**
Two year impact	.040	.016	.003	.044***	.035**	.055***	.060**	.063**	.102***	.035***	.038***	.038***
	(.027)	(.029)	(.021)	(.016)	(.014)	(.014)	(.027)	(.028)	(.031)	(.010)	(.011)	(.009)
	[.143]	[.586]	[.875]	[.006]	[.015]	[.000]	[.028]	[.025]	[.001]	[.001]	[.001]	[.000]
	{1.00}	{1.00}	{1.00}	{.016}	{.021}	{.001}	{.049}	{.049}	{.007}	{.002}	{.002}	{.001}
Four year impact	.011	-.001	.005	-.009	-.015	-.010	.044*	.001	.021	.023*	.013	-.000
	(.031)	(.033)	(.024)	(.019)	(.018)	(.012)	(.025)	(.026)	(.031)	(.012)	(.014)	(.008)
	[.729]	[.969]	[.825]	[.632]	[.406]	[.408]	[.080]	[.979]	[.506]	[.057]	[.336]	[.958]
	{1.00}	{1.00}	{1.00}	{.462}	{.325}	{.325}	{.064}	{.485}	{.254}	{.045}	{.156}	{.470}
Two Year = Four Year	[.467]	[.686]	[.947]	[.026]	[.028]	[.000]	[.664]	[.091]	[.055]	[.457]	[.162]	[.001]
Mean Outcome, Controls	38.2%		38.7%	86.4%		83.9%	58.6%		51.7%	91.6%		92.3%
Observations	8126	9382	17003	8126	9382	17003	8126	9382	17003	8126	9382	17003

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a, 3a, 4a), Not Treated Poor (Columns 1b, 2b, 3b, 4b), and Not Poor (Columns 1c, 2c, 3c, 4c) households in treatment and control villages. All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave 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.

Table A8: Luck versus Merit

Between Village Estimates (Treated vs Control)

OLS estimates, standard errors clustered by village in parentheses

p-values in brackets, FDR adjusted q-values in braces

	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**
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]
Mean Outcome, Controls	41.8%		37.8%	48.2%		40.7%
Observations	4793	5725	10328	4536	5298	9479

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Columns 1b, 2b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered by village, and 95% confidence intervals are reported in brackets. 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 A9: Belief in Government Effectiveness

Between Village Estimates (Treated vs Control)

OLS estimates, standard errors clustered by village in parentheses

p-values in brackets, FDR adjusted q-values in braces

	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.

Table A10: Heterogeneity by Government Represents People Like Me

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

	Redistributive Attitudes Index: Kuziemko et al. [2015]		
	(1) TP	**(2) NTP**	**(3) NP**
Two year impact | Government Does Not Represent	.007	.042	.038
	(.054)	(.048)	(.049)
	[.894]	[.356]	[.434]
	{1.00}	{1.00}	{1.00}
Two year impact | Government Represents	.016	-.098	.116*
	(.070)	(.062)	(.060)
	[.821]	[.109]	[.061]
	{1.00}	{1.00}	{1.00}
Four year impact | Government Does Not Represent	.041	.033	.024
	(.053)	(.054)	(.051)
	[.440]	[.518]	[.634]
	{1.00}	{1.00}	{1.00}
Four year impact | Government Represents	.097	.082	.036
	(.069)	(.060)	(.060)
	[.163]	[.172]	[.547]
	{1.00}	{1.00}	{1.00}
Two Year = Four Year | Government Does Not Represent	[.906]	[.043]	[.274]
Two Year = Four Year | Government Represents	[.379]	[.396]	[.841]
Mean in Controls | Government Does Not Represent	3.12		3.15
Mean in Controls | Government Represents	3.19		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 1), Not Treated Poor (Column 2), and Not Poor (Column 3) 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 representativeness.

Table A11: Heterogeneity by NGO Effectiveness

Between Village Estimates (Treated vs Control)

OLS estimates, standard errors clustered by village in parentheses

p-values in brackets, FDR adjusted q-values in braces

	Redistributive Attitudes Index: Kuziemko et al. [2015]		
	(1) TP	**(2) NTP**	**(3) NP**
Two year impact | NGO Ineffective	.009	.010	.084*
	(.056)	(.051)	(.048)
	[.876]	[.836]	[.084]
	{1.00}	{1.00}	{.859}
Two year impact | NGO Effective	.005	.031	-.023
	(.062)	(.041)	(.051)
	[.942]	[.448]	[.642]
	{1.00}	{1.00}	{1.00}
Four year impact | NGO Ineffective	.040	.014	.013
	(.056)	(.057)	(.052)
	[.476]	[.792]	[.808]
	{1.00}	{1.00}	{1.00}
Four year impact | NGO Effective	.086	.119**	.071
	(.066)	(.057)	(.052)
	[.198]	[.034]	[.178]
	{1.00}	{.690}	{1.00}
Two Year = Four Year | NGO Ineffective	[.952]	[.682]	[.061]
Two Year = Four Year | NGO Effective	[.508]	[.094]	[.243]
Mean in Controls | NGO Ineffective	3.11		3.11
Mean in Controls | NGO Effective	3.18		3.27
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 1), Not Treated Poor (Column 2), and Not Poor (Column 3) 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 NGO effectiveness.

Table A12: Heterogeneity by Private Sector Effectiveness

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

	Redistributive Attitudes Index: Kuziemko et al. [2015]		
	(1) TP	(2) NTP	(3) NP
Two year impact | Private Sector Ineffective	.026	.038	.051
	(.051)	(.042)	(.048)
	[.610]	[.368]	[.294]
	{1.00}	{1.00}	{1.00}
Two year impact | Private Sector Effective	-.084	-.057	.058
	(.075)	(.063)	(.055)
	[.264]	[.369]	[.294]
	{1.00}	{1.00}	{1.00}
Four year impact | Private Sector Ineffective	.030	.030	.017
	(.054)	(.052)	(.050)
	[.586]	[.563]	[.734]
	{1.00}	{1.00}	{1.00}
Four year impact | Private Sector Effective	.115	.096*	.078
	(.073)	(.049)	(.055)
	[.117]	[.053]	[.161]
	{1.00}	{1.00}	{1.00}
Two Year = Four Year | Private Sector Ineffective	[.129]	[.128]	[.913]
Two Year = Four Year | Private Sector Effective	[.265]	[.245]	[.272]
Mean in Controls | Private Sector Ineffective	3.10		3.15
Mean in Controls | Private Sector Effective	3.25		3.21
Observations	7594	8741	15919

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 1), Not Treated Poor (Column 2), and Not Poor (Column 3) 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 private sector effectiveness.

Table A13: Heterogeneity by Village Inequality

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

	Redistributive Attitudes Index: Kuziemko et al. [2015]		
	(1) TP	**(2) NTP**	**(3) NP**
Two year impact | Inequality Below Median	-.020	.012	.040
	(.066)	(.053)	(.058)
	[.764]	[.814]	[.494]
	{1.00}	{1.00}	{1.00}
Two year impact | Inequality Above Median	.024	.023	.029
	(.064)	(.057)	(.048)
	[.703]	[.690]	[.538]
	{1.00}	{1.00}	{1.00}
Four year impact | Inequality Below Median	.019	.035	.032
	(.054)	(.052)	(.057)
	[.718]	[.508]	[.584]
	{1.00}	{1.00}	{1.00}
Four year impact | Inequality Above Median	.076	.054	-.014
	(.072)	(.067)	(.053)
	[.295]	[.426]	[.789]
	{1.00}	{1.00}	{1.00}
Two Year = Four Year | Inequality Below Median	[.665]	[.755]	[.908]
Two Year = Four Year | Inequality Above Median	[.581]	[.732]	[.573]
Mean in Controls | Inequality Below Median	3.13		3.14
Mean in Controls | Inequality Above Median	3.13		3.19
Observations	7797	8985	16277

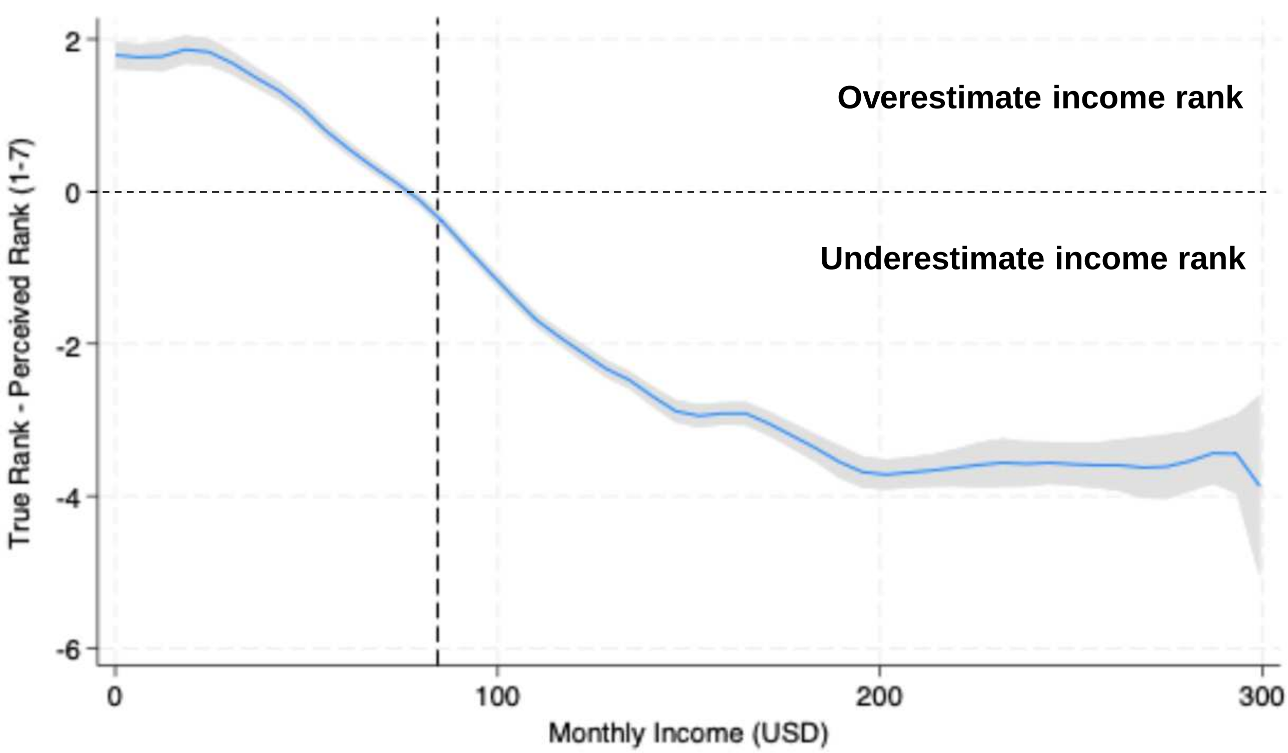
Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Column 1), Not Treated Poor (Column 2), and Not Poor (Column 3) households 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 different levels of village inequality.

Figure A1: Stylized Example of an Asset Menu

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

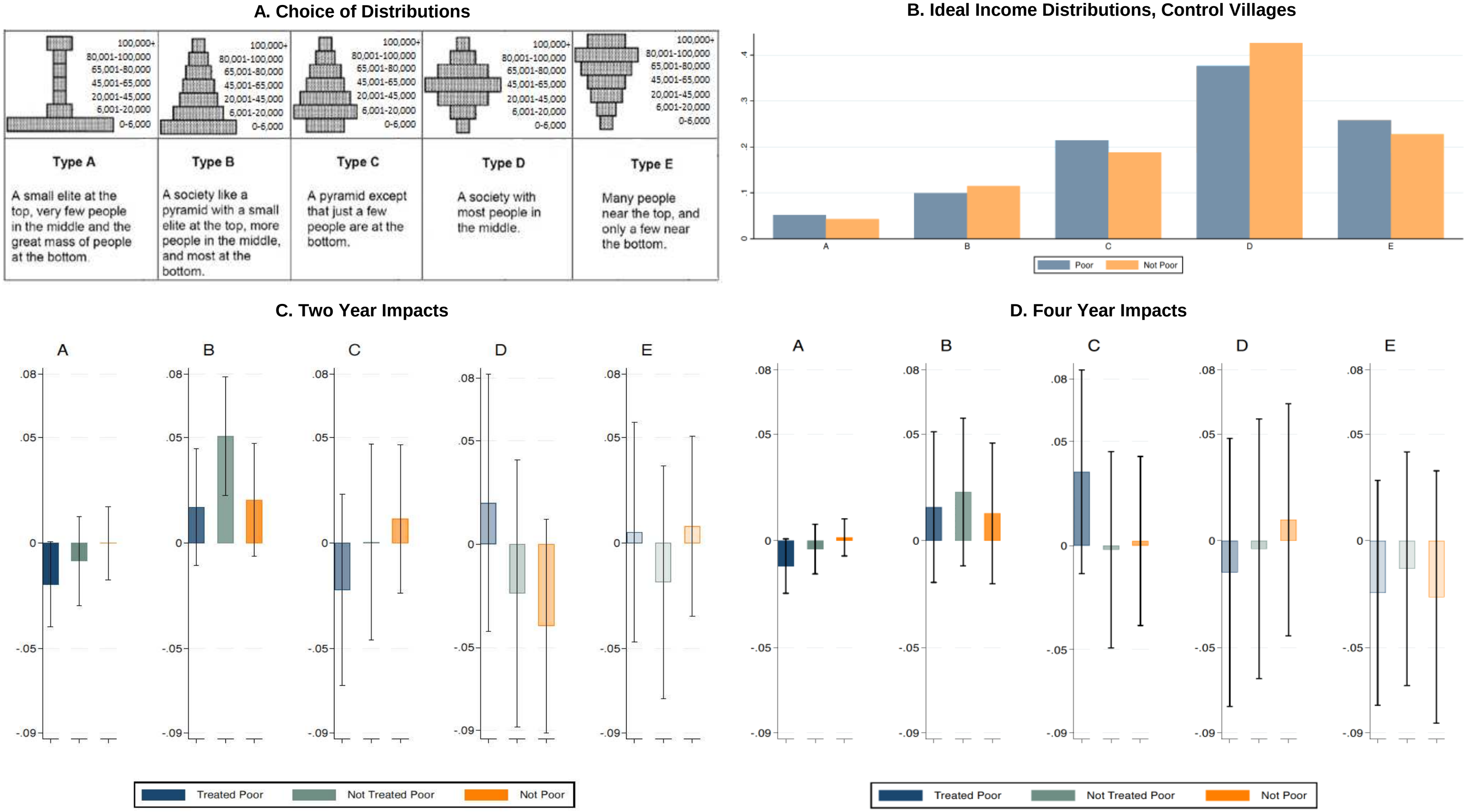
Notes: The figure presents a stylized example of an asset list that households were shown in both treatment arms. Households were allowed to choose any combination of assets they desired, up to a total value of PKR50K.

Figure A2: Misperceptions of Own Standing



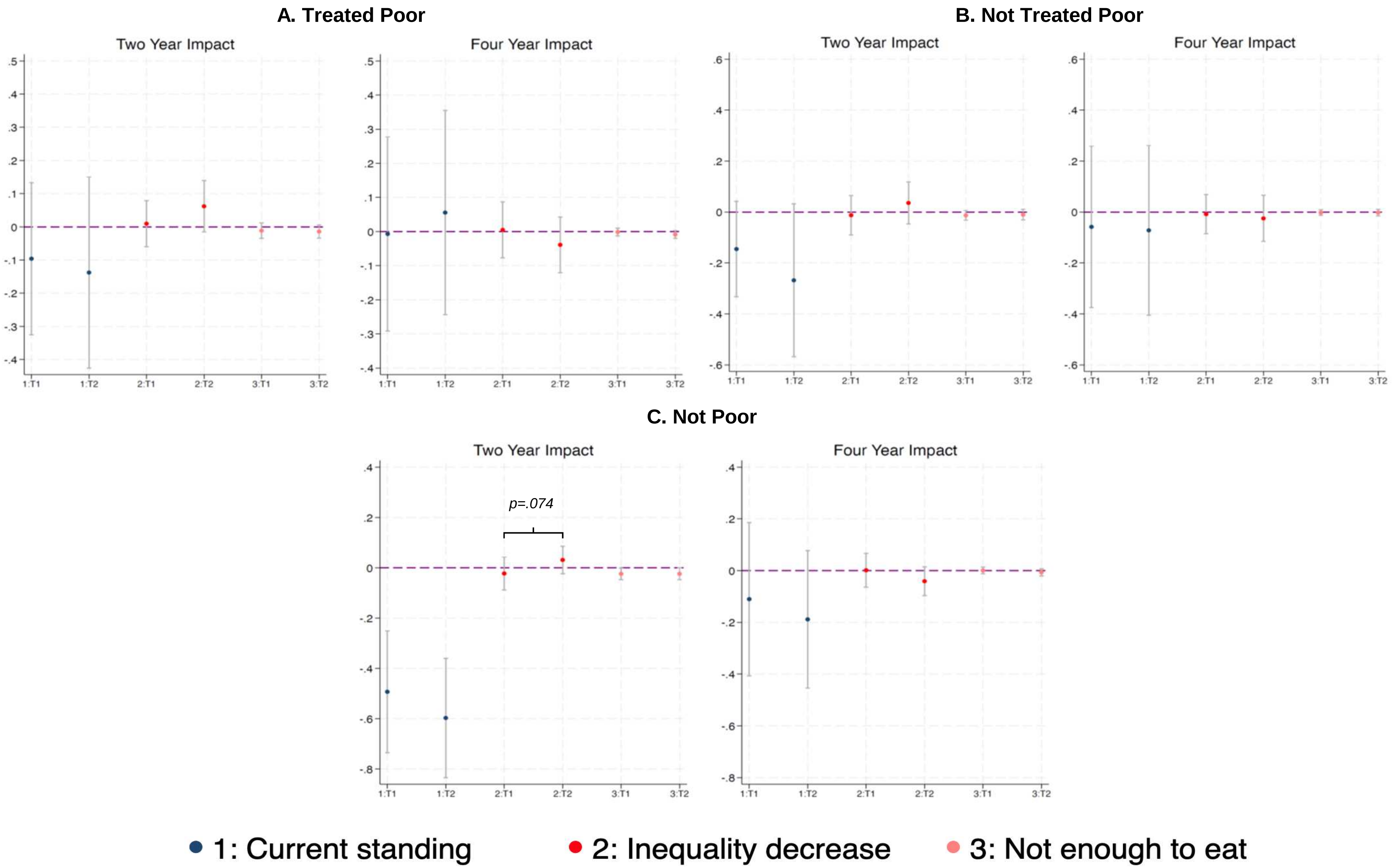
Notes: The Figure plots the difference of individuals true and perceived rank against their monthly income in USD. Both the true and perceived ranking are scored from 1 to 7, where 1 indicates the highest ranking and 7 the lowest. A negative difference thus indicates the respondent underestimates their income rank, while a positive difference indicates that the respondent overestimates their income rank.

Figure A3: Ideal Income Distributions



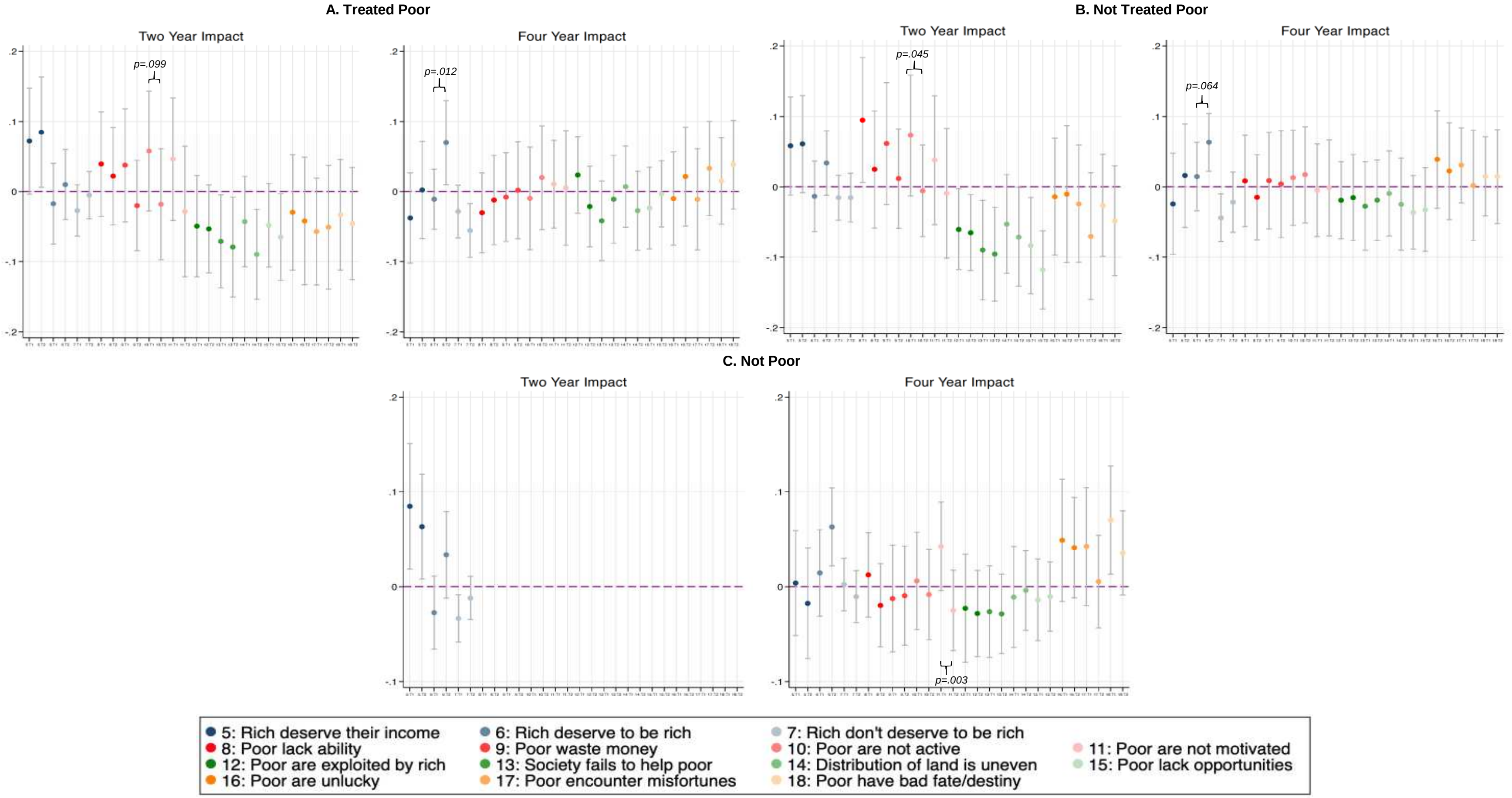
Notes: Panel A shows the income distributions respondents were shown, including the monthly income ranges (in PKR) that correspond to every level of the distribution. Respondents were then asked, "Independent of your position [in the distribution], which of these do you think is the ideal income distribution?" 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.

Figure A4: Perceptions, Asset versus Cash Transfers



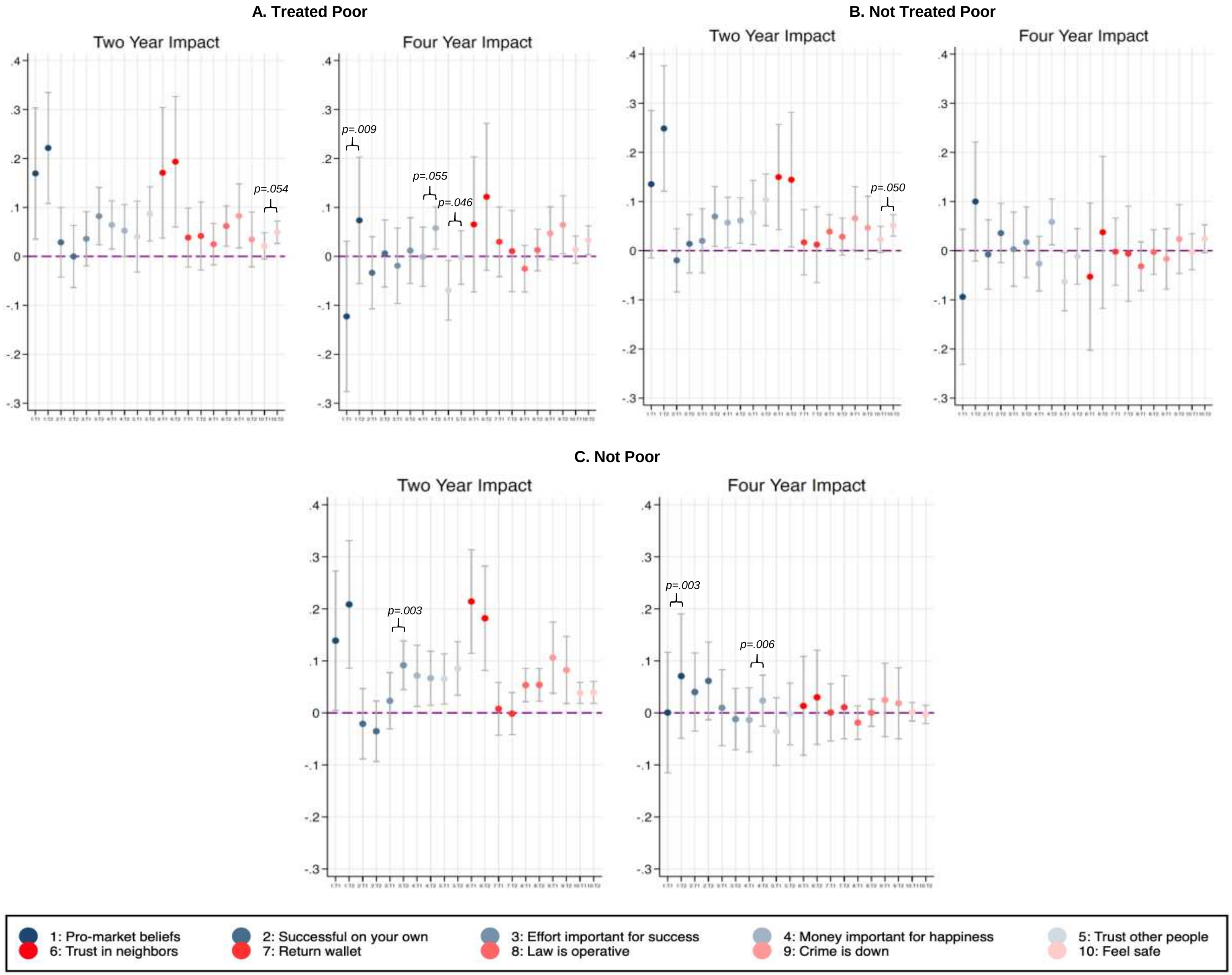
Notes: Panel A (B) [C] displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households]. For each specification we report the treatment effects for T1 and T2. The outcomes are the three perceptions of economic standing reported in Table 6 and the two perceptions of inequality reported in Table 7. Wherever treatment effects differ across arms, we report the p-value on the null of equality of treatment effects.

Figure A5: Perceptions of the Rich and Poor, Asset versus Cash Transfers



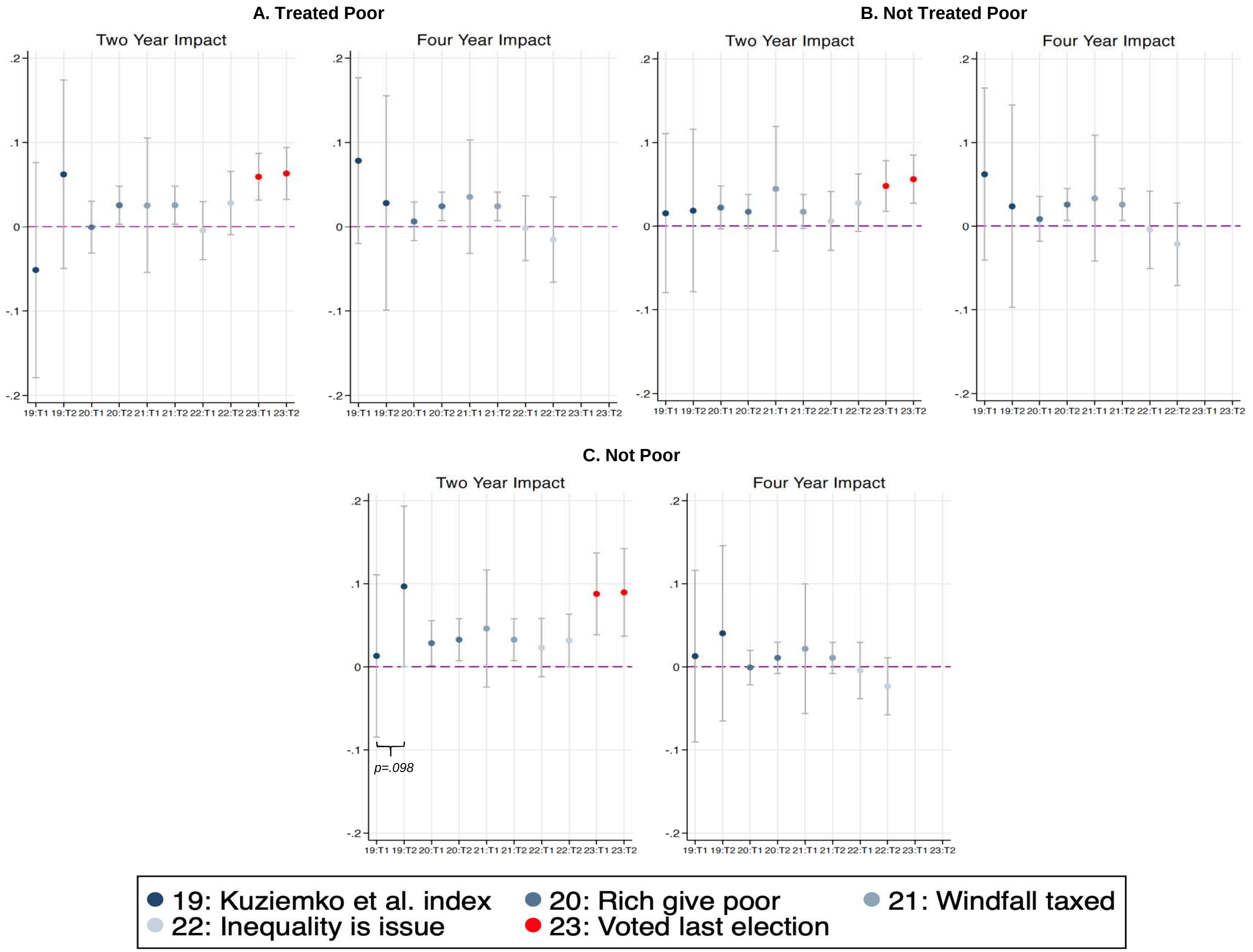
Notes: Panel A (B) [C] displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households]. For each specification we report the treatment effects for T1 and T2. The outcomes are the three perceptions of the rich reported in Table 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 A6: Pro-Market Beliefs and Trust in Neighbors, Asset versus Cash Transfers



Notes: Panel A (B) [C] displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households]. For each specification we report the treatment effects for T1 and T2. The outcomes are the pro-market beliefs index from Table 12 and its four components from Table A6, as well as the trust in neighbors index from Table 12 and its four components from Table A7. Wherever treatment effects differ across arms, we report the p-value on the null of equality of treatment effects.

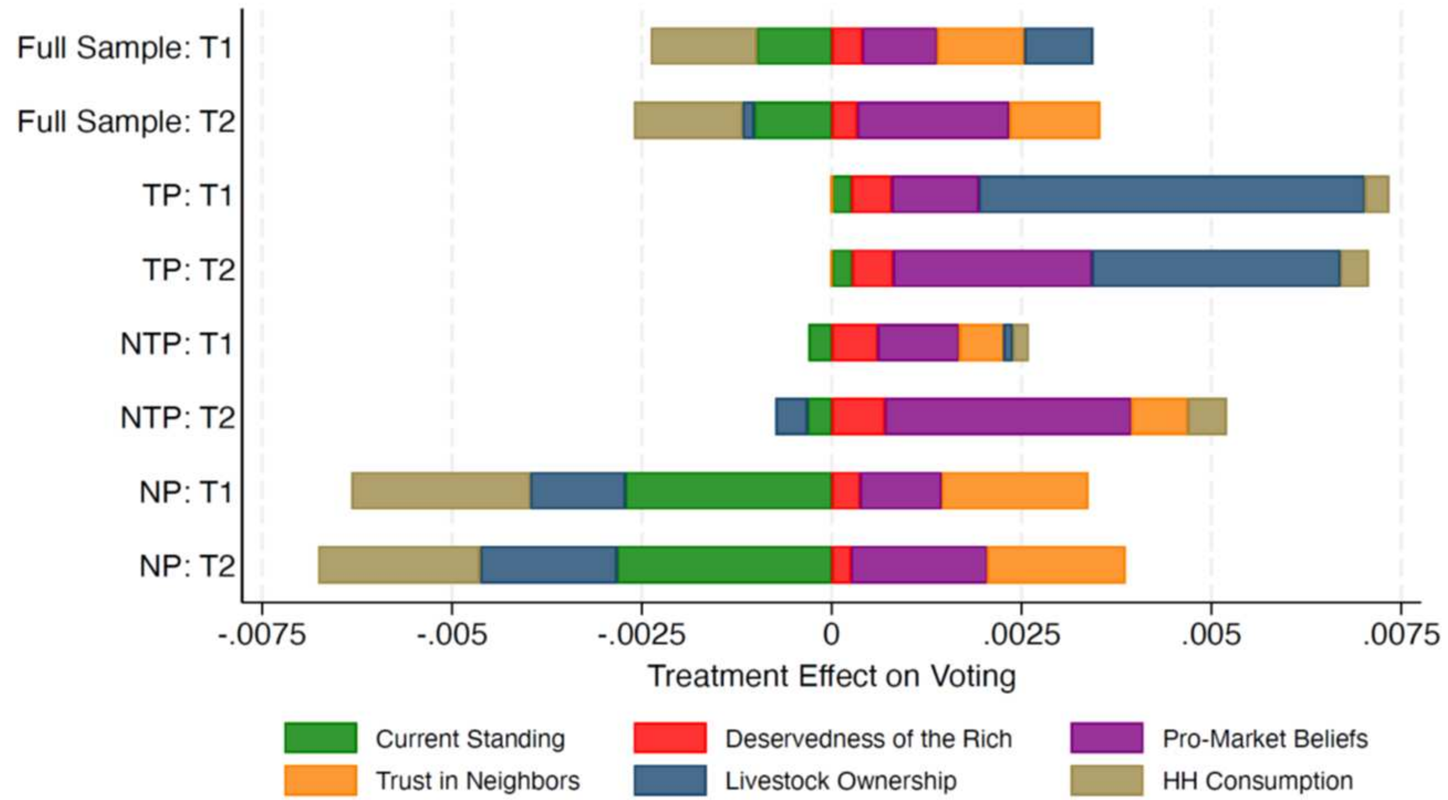
Figure A7: Redistributive Attitudes and Voting, Asset versus Cash Transfers



Notes: Panel A (B) [C] displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households]. For each specification we report the treatment effects for T1 and T2. The outcomes are the index of redistributive preferences and its first three components as reported in Table 13, and self-reported voting as described in Table 14 (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.

Figure A8: Mediation Analysis, Asset versus Cash Transfers

Baseline and Economic Mediators



Notes: The Figure reports results from a mediation analysis following Gelbach [2016]. We show results for all households pooled, as well as for the treated poor (TP), not treated poor (NTP) and not poor (NP) sepatrately. The outcome is a dummy variable indicating whether the respondent voted in the previous local election. The restricted base regression corresponds to the baseline specification analgous to that shown in Table 13 except we control for each treatment arm, while the unrestricted full regression augments this specification with the mediators listed. The Figure shows how much of the difference between the restricted and unrestricted regressions is explained by each mediator. The mediators include: perceived current standing (Table 6, Column 1), beliefs about the deservedness of the rich (Table 8, Column 1), beliefs about poverty being driven by structural factors (an index from 0 to 4 based on the outcomes in Table 10), the pro-market beliefs index (Table 13, Column 1), and the trust in neighbors index (Table 13, Column 2). In addition, we consider two economic mediators: livestock ownership (Tables 3 and 4, Column 1) and the log of monthly food expenditure per adult equivalent (Tables 3 and 4, Column 6).