Big Push Pro-poor Policies and Economic Preferences: Evidence from a Partial Population Experiment*

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Abstract

Big push pro-poor policies have been shown to cause lasting improvements in the economic outcomes of beneficiaries. In this paper we move beyond economic impacts to study whether such interventions impact three interlinked economic preferences: redistributive preferences, pro-market beliefs, and trust in neighbors. We do so using an experiment tracking $15,600$ rural households in Punjab, Pakistan. Villages are randomly assigned to receive an intervention where the poor are either offered a one-time asset transfer of value $620 or an equivalent valued one-off unconditional cash transfer. Within villages, we randomize which of the poor receive the transfer. Our partial population experiment tracks treated poor, not treated poor and not poor households for four years. The treated poor have immediate gains in economic outcomes following the transfers, with gains persisting, but not accumulating further. The interventions also cause persistent reductions in village consumption inequality. Given this backdrop, we examine impacts on economic preferences. Two-years post intervention, the treated poor are less likely to favor redistribution, hold stronger pro-market beliefs, and increase trust in neighbors. This pattern of impacts also holds for the not treated poor (despite them being overtaken by the treated poor in economic standing) and the not poor. Hence shifts in economic preferences do not depend on whether households are direct beneficiaries, but are rather shaped by village-wide exposure to pro-poor policies. Four-years post intervention, the preferences of all groups no longer differ from controls. Hence there is no virtuous cycle feeding back from shifting preferences to driving forward economic outcomes. We provide suggestive evidence that shifts in economic preferences do not persist because they are driven by changes in economic outcomes, not their levels. *JEL: O12.*

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1 Introduction

The last few decades have witnessed a steady rise in programs providing direct transfers to the poor [Banerjee *et al.* 2022]. Among the most successful forms of such interventions are big push in-kind or cash transfers. A body of evidence shows large and persistent impacts of such one-off and high-valued transfers on the economic lives of the poor [Banerjee *et al.* 2015, Haushofer and Shapiro 2016, Bandiera *et al.* 2017, Blattman *et al.* 2020, Balboni *et al.* 2022, Egger *et al.* 2022].[1]

This paper goes beyond the study of economic outcomes to understand whether and how such big push pro-poor interventions impact a bundle of interlinked economic preferences of household heads. We do so using a large-scale and long-term randomized control trial, where the pro-poor interventions take the form of either high-valued in-kind asset transfers or equivalent valued unconditional cash transfers. We use a partial population experiment tracking $15,000$ households for four years in small, close-knit but unequal villages in rural Pakistan. This design allows us to consider how pro-poor interventions impact economic preferences of direct beneficiaries, and those not eligible but who observe others in their village benefitting.

We consider preferences and beliefs along three dimensions: (i) redistributive preferences; (ii) pro-market beliefs; (iii) trust in neighbors. We focus on these dimensions because, first, redistributive preferences might naturally respond to big push policies that impact the economic standing of the poor and reduce village inequality. Given the interventions enable the poor to deepen engagement in labor, capital and financial markets, the pro-market beliefs of the poor and non-poor can also shift, as well as feeding back into preferences for whether resources should be allocated through market or interventionist mechanisms. Finally, a long-standing concern with economic interventions is they can crowd out systems of informal exchange, altering the social fabric of village economies, and reducing trust in neighbors.[2]

Our study sheds light on whether the experience or demonstration of effective pro-poor policies: (i) generates a virtuous cycle of support for redistributive measures, whether such policies create backlash or polarization in communities, or whether redistributive preferences remain inelastic to actual policy outcomes; (ii) how pro-market beliefs change as the poor engage in market exchange; (iii) whether this crowds in or crowds out trust in neighbors. We assess whether this bundle of economic preferences influences economic behaviors and voter demands, hence shaping the future path of anti-poverty and other economic policies.

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

[2] Economic philosophers since David Hume and Adam Smith have debated causal links between engagement in markets and moral sentiments – that is whether experience of market exchange might lead to greater self-interest and weaker responsibilities towards others or whether it fosters greater altruism through generating broader social ties and reducing isolationism.

Figure A1 provides evidence from the World Values Survey (WVS) on a few of the types of economic preference/belief we study. To highlight features of our study context, we use the WVS to draw comparisons between Pakistan, India, the US and Sweden. Panel A considers a redistributive preference, where respondents are asked whether *incomes should be made more equal* (on a 1-10 scale, where 1 reflects a view that *incomes should be made more equal*, and 10 reflects a view that *we need larger income differences as incentives*). Pakistanis hold more strongly anti-redistributive preferences than respondents in the US, Sweden and India. Panel B shows views related to meritocracy in that *hard work brings success* (again on a 1-10 scale). The median Pakistani and Swedish respondents express similar meritocratic beliefs, while meritocratic beliefs in the US and India are lower. On generalized trust, required to underpin anonymized exchange in markets, WVS respondents were asked, *generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?* Panel C shows that Pakistan (and India) are low-trust societies, while generalized trust is higher in the US and Sweden. Finally, a measure of trust in neighbors in the WVS is a question that asks, *could you tell me how secure do you feel these days in your neighborhood?* (with answers recorded on a four point Likert scale). Panel D reports the share of respondents that report feeling very/quite secure. Among Pakistani respondents this share is 82%, similar to the US, but lower than in India or Sweden.

Against this backdrop, we consider responses to two big push anti-poverty interventions in small but unequal rural village economies in Punjab, Pakistan. Eligibility was determined by households being below a poverty threshold and so being identified as ultra-poor. The first intervention offered poor households productive assets in-kind. They could choose any combination of assets off a menu, up to a total value of PKR50K (500USD in 2012 prices). In conjunction with these asset transfers, households were also offered training of value PKR12K. Hence the total value of transfers and training offered was 620USD. We refer to this treatment as T1. The second intervention was identical to the first but with one more listed option on the menu: a one-off unconditional cash transfer of 620USD. We refer to this treatment as T2. In both treatment arms there is near 100% take-up. In T1, 50% of eligibles chose some combination of livestock, and 37% chose assets related to setting-up a small-scale retail business or in petty trade. In T2, 91% of households chose the unconditional cash transfer over any form of in-kind asset transfer – so households reveal prefer cash over asset transfers.

Our evaluation covers 88 villages in rural southern Punjab. These villages are small, comprising 400 households on average. Hence economic gains accruing to the poor are noticeable to others, leaving little scope for misperceptions of intervention gains to persist.[3]

Our field experiment follows a two-stage randomization design. In the first, we randomly assign villages to T1, T2 or control. At a second stage, within treated villages, we randomly assign the

[3]Perceptions, not just actual outcomes, matter for redistributive preferences [Alesina *et al.* 2012, Cruces *et al.* 2013, Alesina *et al.* 2018]. Local neighborhoods, where social interactions are concentrated, are likely key determinants of perceptions.

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 randomly sample around 75% of all eligibles in treated and control villages. This covers 6237 poor households: 3052 reside in control villages, 1598 are in T1 villages (of which 854 are treated), and 1587 are in T2 villages (of which 942 are treated). Following a partial population experiment design, we draw a random sample of non poor (hence never eligible) households from across all deciles of baseline household poverty scores. We survey 9435 non poor (NP) households (so around 33% of all non poor households): 3130 reside in controls, 3306 in T1 villages, and 2999 in T2 villages. We trace the evolution of preferences/beliefs by tracking households two-years post intervention (midline) and four-years post-intervention (endline).

In a companion paper we study the economic impacts of these interventions in far greater detail. In this paper we focus on a more limited set of the most noticeable economic outcomes that are informative of how such pro-poor interventions can shape economic preferences. We find large and persistent noticeable gains to the TP. For example, using the within-village randomization we document gains to the TP in terms of livestock ownership, the value of livestock owned, and consumption of own produced milk, relative to the NTP in the same village. The magnitude of the effects are of economic significance. For example, for the TP in T1, livestock ownership increases by 20pp, a 35% increase over the baseline mean for the poor in controls, the value of livestock owned increases by between 10-15% across all periods and interventions, and by the four-year endline, the consumption of own produced milk increases by around 25%.

Gains to the TP accrue within a year post-intervention and stabilize thereafter until the four-year endline. The TP poor thus experience a pattern of immediate changes in economic circumstances following the transfer of assets/cash, with gains persisting, but not accumulating further.

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

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

Given this backdrop of changes in economic well-being across households in treated villages, the core of our analysis exploits our partial population experiment to understand whether and how these interventions shift redistributive preferences, pro-market beliefs, and trust in neighbors. We focus on the economic preferences of household heads (that are nearly always male). Following

Kuziemko *et al.* [2015], we construct an index of redistributive preferences based on views related to whether the rich should give part of their income to the poor, the deservedness of the rich, how windfall gains should be treated, and concerns over societal inequality. On pro-market beliefs, we follow Di Tella *et al.* [2007] and create an index capturing beliefs over individualism, meritocracy, materialism, and generalized trust. On trust in neighbors, we construct an index based on localized trust, feeling safe, and perceptions of crime and the rule of law.

Given the multiple dimensions of economic preferences considered, we start by using cluster analysis to identify distinct bundles of preferences/beliefs across household heads [Chowdhury *et al.* 2022]. This reveals that households can be assigned to one of two types: 'left' and 'right' types. Relative to left-types, right-types have weaker redistributive preferences, are more pro-market, and trust their neighbors to a greater extent. We then estimate how the interventions change the preference type of household heads.

Focusing first on direct beneficiaries we find: (i) at midline, the TP are significantly more likely to be in the right-type cluster of preferences/beliefs – so be less redistributive, be more pro-market and more trusting of neighbors; (ii) the magnitude of this effect is 8.9pp (relative to 61% of the TP being in the right-type preference cluster in controls); (iii) this shift is not sustained over time: by the four-year endline, the TP are no more likely to be right-types than the poor in controls.

However, the between village randomization reveals a similar shift in the economic preferences of non beneficiaries (relative to counterfactuals in controls). The likelihood the NTP are right-types significantly increases by 8.5pp at midline, while it does so for the NP by 9.9pp – like the TP, both groups converge back to the economic preferences of counterfactual household heads in controls by endline. The within village randomization reveals this convergence is not quite identical between the TP and NTP. Within treated villages, there is a gradual divergence in the likelihood of belonging to be right-types between the TP and NTP: by endline the TP are 3.1pp more likely than the NTP to be right-type, an impact that is statistically significant ($p = .018$).

Our study reveals three core insights. First, economic preferences/beliefs can be shifted by big push pro-poor economic interventions. Second, beneficiaries and non-beneficiaries all shift their preferences/beliefs by midline. This is despite the very different intervention impacts on the economic outcomes across groups, that cause many of the NTP to be overtaken by the TP. The evidence suggests shifts in preferences/beliefs do not depend on whether an individual is an actual intervention beneficiary or not – rather they are driven by common village-wide exposure to such pro-poor policies. *A fortiori*, such policies do polarize preferences/beliefs – in nearly all cases impacts on the poor and non poor are of the same sign. Third, there is little to suggest persistent (four-year) changes in preferences/beliefs among TP, NTP and NP households relative to controls. Again this is despite persistent gains in economic outcomes to the TP from the intervention, and significant long run reductions in consumption inequality in villages. Hence there is no virtuous cycle created feeding back from shifting economic preferences to impacts on economic outcomes.

Among the economic preferences we study, theory provides the clearest guidance on how redistributive preferences might be impacted by pro-poor interventions. The workhorse framework in which to understand such preferences is Meltzer and Richards [1981] (MR). This 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 exactly in line with their response at midline. However, our partial population experiment reveals similar preference shifts occur among the NTP and NP, in contradiction of the MR model, and more in line with community-wide shifts in preferences shaped by exposure to the interventions rather than beneficiary status *per se*. Moreover, the long run impacts we estimate establish that shifts in economic preference does not persist, again counter to the MR model.[4]

To better understand why exposure to big push pro-poor interventions might have little long lasting impact on preferences/beliefs, we revisit the finding that the economic gains from the intervention accrue in the first two years of the intervention and stabilize thereafter. This means it is natural to consider whether economic preferences respond to *changes* in economic outcomes and inequality rather than their *levels*. We explore this using variation in treatment effects on economic preferences and outcomes across each treated village. We find a positive correlation across villages in locations where there is a continued shift to right-type preference clusters across all groups between midline and endline, and where consumption impacts continue growing between midline and endline. In other words, in villages where economic impacts continue growing over time, there is suggestive evidence of continued shifts in economic preferences towards right-types.

The remainder of our analysis delves into greater detail to understand exactly which dimension of economic preferences causes households to shift towards being right-types at midline.

When unpacking the effects of redistributive preferences, we find the shift to right-types is largely driven by households becoming more likely to view the rich as deserving. The shift is not driven by changes in demands for windfalls to be taxed, perceptions of village inequality, or viewing inequality as less of a societal concern. We also consider how views of the poor and the causes of poverty are shifted by exposure to the big push interventions. Views of the poor being poor because of individual traits, or due to bad luck or destiny, do not shift in response to the interventions. However, all households become significantly less likely to hold the view that the causes of poverty are structural – say due to the poor being exploited by the rich, not offered assistance by the state, due to unequal land holdings, or a lack of opportunities.

The right-shift of households economic preferences is also driven by them also becoming significantly more pro-market. More precisely, they hold significantly stronger beliefs in meritocracy,

[4] A large literature has extended the MR framework to explain redistributive preferences, including allowing individual views to be driven by fairness concerns [Alesina and Angeletos 2005], expectations over upward social mobility [Piketty 1995, Benabou and Ok 2001], whether luck or effort are viewed as responsible for individual success [Benabou and Ok 2001, Fong 2001], belief in government effectiveness [Sapienza and Zingales 2013, Alesina *et al.* 2018], or imperfect information about their own relative standing [Hoy and Mager 2021, Hvidberg *et al.* 2023]. None of these extensions are well suited to explain our results that preferences shift irrespective of a household's beneficiary status, and these impacts do not persist. However, in the Appendix we examine evidence related to these extensions of the MR framework in more detail.

materialism, and generalized trust. This shift in beliefs reinforces the weaker redistributive preferences held, in that household heads become more likely to view market mechanisms – not governments – as the means by which to allocate resources.

The right-shift in economic preferences is also driven by them becoming significantly more trusting of their neighbors. All groups hold a stronger belief that in their village the rule of law operates, that crime is down relative to three years ago, and of feeling safe.

Hence at midline, trust of neighbors moves in the same direction as pro-market beliefs. There is no evidence that increasing one crowds out the other. One reason these preferences can shift together is that they both relate to motivations to exert productive effort. Specifically, some components of the pro-market beliefs index can be seen as encouraging productive effort and activity. Similarly, some components of trust in neighbors index can also be seen as encouraging productive effort because individuals perceive their returns to effort are less likely to be expropriated.

At a final stage of analysis, we consider whether such interventions have persistent impacts through increased engagement of households with political processes. We probe this using self-reported data on past voting – between baseline and midline high stakes local elections were held in our study region. We find all groups become significantly more likely to report voting in these elections: the TP are 5.8pp more likely to vote, and the NTP are 5.1pp more likely – both impacts significant at the 1% level. However, the largest point estimate increase is among the NP (9.2pp).

To examine whether vote shares for political parties might be swayed by the interventions, we exploit the fact that at baseline, we asked TP and NP households their views over political parties. Although imperfect in the Pakistani context, we use respondent's expressed affinity to party platforms 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 then while the evidence suggests effective pro-poor interventions increase political participation, this does not favor political views of any particular kind.

Our work contributes to long-standing debates over what shapes economic preferences in two fundamental ways.

First, we extend much of the earlier work that has concentrated on redistributive preferences. This includes lab experiments on distributional preferences [Fisman *et al.* 2007, Fisman *et al.* 2021], non-experimental studies on how these preferences 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 preferences are shaped by providing respondents information about the extent of inequalities, or about their position in the income distribution [Ciani *et al.* 2021, Stantcheva 2022].

We build on these branches of literature by examining how economic preferences are shaped by real world big push interventions, using a large scale and long term field experiment that reveals whether and how economic preferences differentially shift among beneficiaries of pro-poor interventions, those whose relative economic standing falls because of the interventions, and wealthier never eligible households. We show preference shifts 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 such pro-poor policies. By tracing dynamic impacts over a four year horizon, we reveal a divergence between short and long run shifts in economic preferences. We provide suggestive evidence that such dynamics are consistent with preferences being driven by *changes* in economic outcomes and inequality in village economies, rather than their level.

Second, we advance the literature by considering linkages between preferences/beliefs across domains, utilizing cluster analysis that has recently been used to understand risk, time and social preferences within households [Chowdhury *et al.* 2022]. Studying interlinked economic preferences is natural given redistributive preferences, pro-market beliefs and trust in neighbors are intertwined. Beliefs in the market mechanism to allocate resources naturally influences views of how the state should intervene to redistribute resources. Moreover, a long-standing concern expressed across social sciences is that greater engagement in anonymized market exchange risks crowding out social capital and trust in others [Margalit and Shayo 2020, Heβ *et al.* 2021]. We document that in our context, in the face of big push pro-poor interventions, such concerns do not hold up – experimentally induced changes in pro-market beliefs 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. Our evidence suggests these preferences shift together because they both capture motivations to exert productive effort, and these rise in the presence of pro-poor interventions in village economies.

The paper is organized as follows. Section 2 describes our context, interventions and research design. Section 3 examines impacts on noticeable economic outcomes and village inequality. Section 4 uses cluster analysis to shed light on whether the interventions impact preference types of households. Section 5 details which dimensions of economic preference/belief are shifted by the interventions. Section 6 concludes by discussing impacts on voting, external validity and directions for future work. The Appendix presents robustness checks and additional results.

2 Context, Interventions and Design

2.1 Context

Our evaluation covers 88 villages in four districts in southern Punjab: Bahawalpur, Bahawalnagar, Lodhran and Muzaffargarh. Villages are located in remote semi-arid regions, far from market/state institutions. Households are almost all Muslim, and pre-intervention, heads of household engage primarily in cropping/farming (38%), as unskilled laborers (19%), or in livestock rearing (12%).

2.2 Interventions

The interventions we study take two forms. The first offered households productive assets in-kind. To determine the menu of assets to offer, in each village we initially conducted an assessment of assets most likely to provide high returns. The menu of assets typically covered livestock, enabling households to start a retail business (e.g. grocery shop, fruit stall), crop farming, and other forms of self-employment (e.g. tailoring). Figure A2 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).[5]

In conjunction with in-kind asset transfers, households were offered training providing skills to run a small-scale enterprise, as well as skills specific to the chosen asset(s). The value of training was fixed at PKR12K. Hence the total value of transfers and training offered was PKR62K (around 620USD). We refer to this as treatment T1.

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 field partners, FDO and NRSP. Each intervention is best perceived as a government delivered program.[6]

The interventions are big push, representing high-valued resource transfers to the poor. The value of transfers corresponds to the equivalent of eight months of food consumption at baseline. Such resource injections are large enough to shift forward levels of economic well-being of the poor, do so in noticeable ways to others in these small village economies, and they have the possibility to reduce village consumption and asset inequality.[7]

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

[6] 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.

[7] 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.

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 ultra-poor and hence eligible for the interventions. The interquartile range of poverty scores is 19 to 37, with the highest decile of households having a score above 46.[8]

The poverty score construction is similar to that used to target welfare programs to the rural poor in Pakistan, including the prominent Benazir Income Support Programme. This was launched in 2008 and is the most widespread social protection program, reaching nearly five million households in 2012. Households are thus familiar with the kind of poverty score construction used to determine eligibility. Not treated poor households were given no promise of future treatment. Not poor households were aware they were never going to be eligible.

2.3 Research Design

Randomization We follow a two-stage randomization design. In the first, we randomly assign villages to T1, T2 or control. Randomization is stratified by district. At a second stage, within treated villages, we randomly assign the actual offer of treatment among eligible households. Half of those eligible are actually offered treatment. Among the poor in treated villages, we thus distinguish between the treated poor (TP) and the not treated poor (NTP).

Sampling We sample 6237 eligible poor households in treated and control villages (so around 75% of all poor households): 3052 reside in control villages, 1598 are in T1 villages (of which 854 are treated), and 1587 are in T2 villages (of which 942 are treated). We use our census to draw a random sample of non poor (hence never eligible) households from across all deciles of poverty scores. We denote non poor households as NP. We survey 9435 non poor households in total (so around 33% of all non poor households): 3130 reside in controls, 3306 in T1 villages, and 2999 in T2 villages.

Take-Up In both treatment arms, there is near 100% take-up of the offer of transfers. In T1, 50% of eligibles chose some combination of livestock, 22% chose assets to set-up a small-scale retail business, and 15% chose assets related to petty trade. In T2, over 91% of households chose the unconditional cash transfer over any form of in-kind asset transfer. Hence the majority of households in T2 reveal prefer cash over assets.[9]

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

[9] 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

Timeline We conducted our household census from May to July 2012, and our baseline household survey from February to June 2013. Interventions were rolled out January-March 2014. In this paper we focus on the one, two and four-year follow-up surveys that were fielded May to July 2015, September/October 2016, and February/March 2018 respectively. Economic outcomes are measured at the one, two- and four-year follow ups. Economic preferences are measured for all households at the two-year midline and four-year endline.

Balance Table 1 shows samples are balanced on village characteristics measured from the census, across treatment arms. Panel A shows that villages are small, with 400 households in each. The average distance between treated and control villages is 13kms, with travel times to market and state infrastructures such as livestock markets or police stations being around an hour.

Panel B focuses on village poverty. The average household poverty score is 29, with the standard deviation of scores across households being just under half the mean. Around 23% of households are classified as poor (and therefore eligible). Of those, around 45% are actually treated (creating the division between the TP and NTP in treated villages).

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

Table 2 shows balance on household characteristics, splitting for the across and within village randomization. On most dimensions the samples are well balanced.

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.

Finally, given the intervention is delivered by a quasi-government agency, Panel C shows house-

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.

hold views of government, NGOs and the private sector. Pre-intervention, only a quarter of households think government is effective, with similar beliefs in NGOs, and slightly lower beliefs for 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 A1 shows attrition by survey wave, separately for the TP, NTP and NP. 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 over the first to fourth years post-intervention). 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 economic preferences 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 briefly discuss intervention impacts on a narrow subset of economic outcomes (y_{hvt}): whether the household owns livestock, the value of livestock owned conditional on ownership, whether the household has an iron roof (that is only measured at one year post-intervention but is a durable and irreversible investment), whether the household often consumes home produced milk, and monthly food expenditure. We do not claim these are the most important dimensions of impact for well-being, but they are more relevant for the current study because they can drive changes in economic preferences because they are more noticeable outcomes in these small village economies, leaving little scope for misperceptions of intervention gains to persist [Alesina *et al.* 2021].

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

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

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-survey wave (vt).

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%.

Three other points are of note. First, given that treated and not treated poor households are balanced on observables at baseline, the magnitudes of these gains imply that many of the NTP are overtaken by their 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 the preferences/beliefs 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].

Second, 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.

Third, both big push interventions have similar impacts: at the foot of table we report p-values of the equality of treatment effects by survey wave. With the exception of livestock ownership – that increases significantly more for those offered in-kind asset transfers in T1 – all other treatment effects do not differ by intervention and period. Hence for the purpose of studying economic preferences, we pool treatments for the remainder of the analysis.

Table 4 repeats the exercise pooling treatments, allowing gains to be estimated more precisely in each wave. We find that across all margins of noticeable outcomes, TP households have significant impacts relative to the NTP. The TP have a 16% increase in livestock ownership (corresponding to a 29% increase over the baseline for the poor in controls), the value of livestock owned increases by around 14%, they are 4pp more likely to have an iron roof one year post-intervention (an 11% increase over baseline), are around 20% more likely to have improved diets as measured through the consumption of own produced milk, and have gains in food consumption of around 3% over baseline (the short run fall in consumption might reflect the switch from market purchased dairy products to home production).

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

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

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

Table A2 presents the results: we see little evidence that outcomes shift for not treated poor or not poor households relative to controls. The point estimates on many of the estimates are close to zero, suggesting weak within village spillovers on these specific outcomes.[10]

3.3 Village Inequality

Beyond noticeable gains to the TP, the interventions can also impact overall levels of village inequality. This is because villages in our study context are relatively small and half the eligible poor, or 10% of all households (40 households per village), are actually treated. To examine the possibility, we estimate the following between village treatment effect on measures of consumption inequality, I_{vt}, for village v in survey wave t:

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

where our consumption inequality measure is based on the value of adult-equivalent food expenditure, we pool treatments, and robust standard errors are reported.[11]

Table 5 presents the results for three measures of consumption 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.

[10] 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).

[11] 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.

4 Economic Preferences

4.1 Definitions

Given this backdrop of economic gains to the TP, changes in relative standing of the NTP and reduction in village-wide economic inequality in treated villages, we now consider how the big push pro-poor interventions translate into shifts in economic preferences along three linked dimensions: (i) redistributive preferences; (ii) pro-market beliefs; (iii) trust in neighbors.

Building on Kuziemko *et al.* [2015], we construct an index of redistributive preferences based on four questions. The first is a blanket statement of views on redistribution: *do you think the rich in your village should give a part of their income to the poor in some form?*. Second, on perceptions of the rich, we asked whether they agreed *the rich rightfully deserve their income*. The third question 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?* Finally in terms of concerns for societal inequality we asked, *do you think inequality is one of the larger socioeconomic issues of Pakistan?* We sum the number of affirmative answers to these four questions (reversing the reply to the second 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, the rich do not rightfully deserve their income, that windfall gains should be redistributed to the poor, and/or because inequality is a major societal concern.

On pro-market beliefs, we follow Di Tella *et al.* [2007] and create a 0-4 index capturing beliefs over individualism, meritocracy, materialism, and generalized trust, summing 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?*

On trust in neighbors we construct a 0-4 index 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?*

4.2 Descriptives

We focus on the economic preferences of heads of household (that in 98% of cases are men). As these are measured at midline and endline, in Figure 1 we present midline descriptives for controls.

The poor hold relatively pro-redistributive preferences, with an average score of 3.14. As Panel A shows, 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. The poor hold less strong pro-market beliefs, but again there is variation with 7% of households having a score of one or zero, 23% having a score of two, 47% having a score of three and 24% scoring four. The trust in neighbors index lies between these two, with the average score being 2.85.

Correlations across the three preferences are weak: redistributive preferences have a correlation of .033 with pro-market beliefs and .036 with trust in neighbors. We note a slight positive correlation of .035 between pro-market beliefs and trust in neighbors, somewhat counter to the idea that market interactions erode moral sentiments.

Panel B of Figure 1 shows how economic preferences vary across poverty deciles. Poor households make up the first four deciles. Across all dimensions, preferences do not display a sharp gradient across poverty deciles. Correlations between dimensions also remain relatively low across poverty deciles, although there is a tendency for pro-market beliefs and trust in neighbors to become more positively correlated for better off households.[12]

The fact that among control households, preferences and beliefs do not correlate strongly with economic standing is in line with existing cross country evidence.[13] For each dimension of economic preference, we note the variation within villages is around three times the variation between villages. In Table A3 we explore correlates of each dimension to better understand what might drive variation in initial preferences/beliefs.[14]

4.3 Household Types

We start by identifying clusters of preferences/beliefs across household heads to classify them into types. Following Chowdhury *et al.* [2022] we do so using cluster analysis. The intuition behind this is to identify groups of household that are similar to each other in preferences/beliefs – their

[12] There is a positive time trend among controls in each preference dimension, of similar magnitude for poor and non-poor households. These time effects from midline to endline correspond to around a 4% increase in the redistributive preferences index, a 10% increase in pro-market beliefs, and a 7% (13%) increase in trust in neighbors among poor (non poor) heads of household. Our study period is one in which Pakistan experienced steady growth in income per capita.

[13] 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.

[14] In Table A3 we first consider how redistributive preferences correlate to household characteristics. Given respondents might be influenced by enumerator styles, we include a set of enumerator dummies. Redistributive preferences do not correlate with household characteristics (Column 1). In Column 2 we control for village fixed effects (and so drop enumerator fixed effects). We do not find any evidence that the within village variation in redistributive preferences is explained by these household characteristics. Column 3 replaces the village fixed effects with village characteristics: we again see no evidence that these features of the village economy correlate to redistributive preferences. Columns 4 to 6 repeat the analysis for pro-market beliefs. Household characteristics do little to explain these beliefs in controls, although larger households and those in larger villages express more pro-market beliefs. Finally, Columns 7 to 9 repeat the analysis for trust in neighbors. This is also higher among larger households. Those in smaller or more unequal villages have less trust in neighbors.

type – but differ considerably from other types. To establish types we use a k-median clusters algorithm. Given the evidence in Figure 1, we apply the algorithm pooling poor and non poor households. Our aim is to establish whether households change type over time, hence we run the algorithm separately at midline and endline. Finally, for expositional ease, in each period we fix the number of clusters at two. We then verify whether this is (close to) the optimal number of clusters such that the Calinski-Harabasz F-statistic is minimized.[15]

The resulting types are shown in Table 6. We refer to the two distinct clusters as left- and right-type households. Right-types hold less redistributive preferences, more pro-market beliefs, and trust neighbors more. Differences in each dimension of economic preference are statistically significant except on the dimension of redistributive preferences at midline, where left- and right-types do not differ.

Repeating the algorithm at endline and fixing the number of clusters to two, the resulting left- and right-type classification is shown in the remaining Columns of Table 6. At endline, right-type household heads hold less redistributive preferences, more pro-market beliefs, and are more trusting of neighbors. Differences in each dimension are statistically significant.[16]

4.4 Estimation

Exploiting the between village randomization, we estimate treatment effects on the preference type of the TP, NTP and NP using the following specification for heads of household in group $g \in \{TP, NTP, NP\}$:

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

where $y^g_{hvt} \in$ {left, right} is the preference type of household head h. We estimate whether they belong to the right-type cluster (with the left-type being the omitted category). We continue to pool interventions, and all other variables are as defined earlier. Given the nature of questions asked about preferences/beliefs, we include a full set of dummies for enumerators, λ_e. We cluster standard errors by village-survey wave (vt). Throughout we also report 95% confidence intervals on treatment effects at midline and endline to make precise the magnitude of impacts that could be detected given the precision of estimates.[17]

[15] The algorithm operates as follows. First, k points are selected from the data as medians. Then, every data point is associated with the closest median, i.e. assigned to the respective cluster. For this configuration, the total distance of the data to their respective median is calculated. Then the k-medians are iteratively replaced by non-medians if that change minimizes the total distance of the data to the medians of the clusters.

[16] At midline the algorithm delivers that the optimal number of clusters is two: the Calinski-Harabasz F-statistic is 4809 when two clusters are allowed for, is 4420 when three are allowed for, and continues falling as more clusters are allowed for. At endline, the F-statistic is 4266 when two clusters are allowed for, rises slightly to 4402 when three are allowed for, but falls to 3876 when four clusters are allowed for, and continues to fall as up to eight clusters are allowed for. The k-medians clusters algorithm is more robust to outliers than the k-means approach.

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

Standard identifying assumptions for the treatment effects on each group are that there is random assignment, and there are no spillovers onto controls. The effects on the NTP and NP capture their exposure to the pro-poor interventions, that can operate through them: (i) observing intervention impacts on the TP and village outcomes as a whole; (ii) any changes in their own economic circumstances occurring through spillovers or general equilibrium effects (say through labor, asset or credit markets); (iii) any emotional connection with beneficiaries, that in these tight-knit villages can also shape preferences and beliefs of non treated households.

Exploiting the within-village randomization, we estimate treatment effects on the preference types of the 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-survey wave.

4.5 Results

Panel A of Table 7 of shows midline and endline impacts on being a right-type for TP, NTP and NP households respectively, as estimated from the between village specification (4). Focusing first on the TP in Column 1a we find: (i) at midline, they are significantly more likely to be in the right-type cluster of preferences: so to be less redistributive, hold stronger pro-market beliefs, and have higher trust in neighbors. The magnitude of the impact is 8.9pp and statistically significant ($p = .000$); (ii) this shift is not sustained over time: by endline TP are no more likely to belong to the right-type cluster than poor households in controls. The magnitude of this effect is .022 and represents a statistically significant decline from midline ($p = .053$).

The remaining Columns in Panel A repeat the analysis for NTP and NP households. We see that both groups of household also increase their likelihood to be in the right-type cluster at midline: for the NTP the magnitude of the treatment effect is 8.5pp ($p = .000$) and for the NP the effect is 9.9pp ($p = .000$). For both groups we observe significant declines in their likelihood to remain in the right-type cluster between midline and endline. These null effects at endline are precisely estimated: for the NTP the effect is $-.013$ and for the NP it is .006. For both groups the 95% confidence intervals rule out an increase around half the impact at midline.

Panel B shows midline and endline impacts on the TP using the within village specification (5). We see that there is a gradual divergence in the likelihood of belonging to the right-type cluster between the TP and NTP: by endline the TP are 3.1pp more likely to belong to the right-cluster that NTP households in the same village ($p = .018$).

These results reveal three core insights. First, economic preferences/beliefs can be shifted by big push economic interventions targeting the poor. This is despite such preferences/beliefs not correlating strongly with economic standing in controls (Figure 1).

Second, the within-village estimates in Panel A confirm that all groups shift preferences/beliefs by midline. This is despite the very different intervention impacts on economic outcomes across groups. Overall, the evidence suggests shifts in preferences/beliefs in response to pro-poor interventions do not depend on whether an individual is an actual beneficiary of the intervention or not – rather they are driven by common village wide exposure to the pro-poor interventions. *A fortiori*, such policies do polarize preferences/beliefs – in nearly all cases impacts on the poor and non poor are of the same sign.

Third, there is little to suggest persistent (four-year) changes in preferences/beliefs among TP, NTP and NP households relative to controls. Again this is despite persistent gains in economic outcomes to the TP from the intervention, and significant long run reductions in consumption inequality in villages. Hence there is no virtuous cycle created feeding back from shifting economic preferences to impacts on economic outcomes.

Robustness Checks In the Appendix we present a sequence of robustness checks on our core finding. We first show the findings are robust to accounting for multiple hypothesis testing given the eight coefficients of interest presented in Table 7. We then examine alternative specifications, allow for attrition, consider alternative approaches to identifying preference clusters, and allowing for alternative numbers of types. Finally, we show that in-kind asset transfers and reveal preferred unconditional cash transfers have similar impacts on preference types. These robustness checks are presented in Table A4 and summarized in Figure A3.

4.6 Interpretation

Meltzer and Richards [1981] Among the economic preferences that go into determining left- and right-type clusters, theory provides far more guidance on how redistributive preferences might be impacted by pro-poor interventions, relative to how such policies might shift pro-market beliefs and trust in neighbors, despite the three being interlinked. The workhorse framework for understanding redistributive preferences is Meltzer and Richards [1981] (MR). Their model assumes self-interested individuals and has the basic predictions that: (i) pre-intervention, the poor (relative to the mean income group) should be more in favor of redistribution; (ii) the redistributive preferences of the treated poor should weaken as they benefit from pro-poor interventions.

In line with much cross-country evidence [Hoy and Mager 2021], our descriptive evidence does not favor prediction (i). However, the midline impacts on the TP are supportive of (ii): they are significantly more likely to hold less redistributive preferences and switch to the right-type preference cluster. However, our partial population experiment reveals similar preference shifts occur among the NTP and NP. This is in contradiction of the MR model, and is more in line with community-wide shifts in economic preferences being shaped by exposure to the interventions rather than beneficiary status *per se*. Moreover, the long run impacts we estimate establish that shifts in economic preference does not persist, again counter to the MR model.

A large literature has extended the MR framework to help explain redistributive preferences of the rich and poor [Alesina and Giuliano 2011]. These extensions include allowing individual views to be driven by fairness concerns, expectations over upward social mobility, whether luck or effort are viewed as responsible for individual success, belief in the effectiveness of government, or imperfect information about their own relative standing. None of these are well suited to explain our two core results: that preferences shift irrespective of a household's beneficiary status, and while economic preferences shift two-years post intervention, these impacts do not persist.[18]

Nevertheless, given the prominence of these hypotheses in the literature, we use our data to probe them further, presenting results in the Appendix. In short, we find little evidence that redistributive preferences are shaped by changing views over whether inequalities are driven by luck versus merit (Table A5), or beliefs over the effectiveness of government (Table A6). Some of the results for the NTP can be partly reconciled by their changing aspirations over social mobility (Table A7). Finally, we find non-beneficiary households do perceive falls in their absolute economic standing, but households do not perceive impacts on their relative standing (Table A8).

Changes We therefore consider an alternative explanation for the documented dynamic impacts on economic preferences/beliefs across groups. To do we return to the earlier evidence that noticeable economic gains to the TP accrue within a year post-intervention, and stabilize thereafter until the four-year endline (Tables 3 and 4). The treated poor thus experience a pattern of immediate changes in economic circumstances following the transfer of assets/cash, with gains persisting, but not accumulating further. A natural explanation for the short lived treatment effects on the economic preferences is that they respond to changes in the economic environment, rather than the level of economic outcomes or inequality.

To explore this possibility a little we proceed as follows. First, we estimate treatment effects on preference types of treated poor households h separately for each treated village v. We thus estimate specification (4) comparing treated poor households from one treated village v at a time, relative to poor households in control villages in the same district. This generates endline and midline impacts on the treated poor belonging to the right-type preference-cluster, for each treated village v, $(\widehat{\beta}^r_{v2}, \widehat{\beta}^r_{v4})$, thus indicating how preference-types change within a village over time: $\widehat{\beta}^r_{v4} - \widehat{\beta}^r_{v2}$. This reveals that the null impact at endline documented earlier masks considerable heterogeneity across treated villages: in around half of them, there is a continued shift of treated poor households towards the right-type preference cluster between midline and endline (so $\widehat{\beta}^r_{v4} -$

[18] This list is not exhaustive. Other explanations for determinants of redistributive preferences include a history of personal misfortune, institutions/indoctrination, intergenerational transmission, family networks/insurance and culture. Our data does not allow us to explore these factors in so much detail.

$\widehat{\beta}^{r}_{v2} > 0$) and in the others households shift back to the left-type cluster (so $\widehat{\beta}^{p}_{v4} - \widehat{\beta}^{p}_{v2} < 0$).

To see how these shifts relate to changes in noticeable economic outcomes among the TP, we repeat the exercise using the log of monthly food expenditure (in adult equivalence) as the outcome, based on (2). This generates estimates of how these consumption impacts vary over time within a village: $\widehat{\beta}^{c}_{v4} - \widehat{\beta}^{c}_{v2}$.

Panel A of Figure 2 then plots $\widehat{\beta}^{r}_{v4} - \widehat{\beta}^{r}_{v2}$ against $\widehat{\beta}^{c}_{v4} - \widehat{\beta}^{c}_{v2}$ for each treated village v. There is a positive relationship between the two: weighting observations by village size, the line of best fit is shown on Panel A and the correlation between the two changes in .267. In short, villages that have increases consumption impacts among the treated poor from midline to endline also observe greater continued shifts towards the right-type preference cluster among the treated poor from midline to endline. This result offers suggestive evidence that the economic preferences of treated poor households respond to changes in noticeable economic outcomes.

Panel B repeats the exercise based on the within-village specifications (5) and (1) for treated poor households, by treated village v (where we now use the not treated poor in the same village as counterfactuals). We again find a positive relationship between these changes, with the weighted regression implying a correlation between the two changes of .396.

Finally, the between village approach can be repeated for not treated poor and not poor households. Given the lack of spillovers to these groups in terms of noticeable economic outcomes (Table A2), we explore whether their changes in preference cluster relate to changes in food consumption among the treated poor in the same village v. We continue to find positive correlations between these changes, although they are weaker as expected: for the not treated poor the correlation is .061 and for the not poor it is .040. However the fact the correlations remain positive is reassuring and remains consistent with the idea that preferences of non beneficiaries also respond to changes not levels of economic outcomes.

5 Dimensions of Economic Preferences

The remainder of our analysis delves into greater detail to understand which dimensions of economic preferences are shifted by the interventions. We unpack both what drives the between village impacts for TP, NTP and NP households at midline, and what drives the more gradual divergence in preferences between TP and NTP households by endline.

5.1 Redistributive Preferences

We first asked, *should the rich give part of their income to the poor?* Panel A of Table 8 of shows midline and endline impacts on this preference for TP, NTP and NP households as estimated from the between village specification (4). Panel B shows midline and endline impacts on the TP using the within village specification (5). Although the vast majority agree with this statement in controls, we find: (i) at midline, the NTP and NP nudge forward in being more likely to hold this view. The magnitude of impacts is 2.0pp for the NTP and 3.0pp for the NP ($p = .036$, $.013$ respectively); (ii) at endline, the TP nudge forward on this view by 1.6pp ($p = .090$), while the NTP and NP no longer differ from controls; (iii) Panel B confirms that within villages, we observe no differential responses between the TP and NTP in either period.

The second preference is framed in terms of redistributive responses towards the poor when others receive a substantial windfall. We asked, *one year ago, a person's monthly income increased to PKR 250'000 as a result of luck. Should (s)he be taxed by the government to raise funds for the poor?* If the respondent replied they should be taxed, we asked a follow up question on the how much they should be taxed to derive an implied desired average tax rate on windfalls. The remaining Columns of Table 8 show both sets of results. At midline the TP and NP are significantly more likely to believe large windfalls should be taxed to redistribute towards the poor, but these changes are not sustained at endline. These null effects are precise: for example, among the TP at endline, on the belief that windfalls should be taxed, the 95% confidence interval rules out an increase greater than 9.5pp (where 65% of the poor hold such a belief in control villages).

Throughout, we find no evidence that any group changes their desired average tax rates for recipients of large windfalls – and again, these null impacts are precise.

The within-village estimates in Panel B all confirm there are no statistically significant divergences in redistributive preferences between the TP and the NTP.

Overall, in the long run, these redistributive preferences are inelastic to exposure to these big push pro-poor interventions. The effective experience or demonstration of pro-poor policies even in these small village economies – a context with low levels of asymmetric information between the poor and non poor, and non-eligibles have emotional connections with beneficiaries – does not in itself generate demand for more/less redistribution.

5.2 Ideal Income Distribution

To gauge redistributive preferences from another perspective, we asked households about their ideal income distribution. Panel A of Figure 3 shows the choices visually 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 most top heavy Distribution E. Panel B shows the ideal distributions reported in the control group 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 around 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%).[19]

[19] 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.

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

5.3 Perceptions of Village Inequality

We next examine perceptions of village inequality. We asked household heads whether: (i) inequality in their village has decreased in the last three years; (ii) the share of households in the village that do not have enough to eat has fallen. The results are in Table 9. Panel A shows a near complete set of null impacts across both dimensions for the TP, NTP and NP. These null impacts are again 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 .052, or 15% 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 effects are not significant. The endline impact for TP households is $-.005$, and the 95% confidence interval rules out an impact larger than .004, or 4% of the view held by the TP in controls.

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

Overall, despite actual measurable and persistent changes in economic standing and village consumption inequality, these do not translate into perceived changes among households of how they view inequality to have changed in their village, irrespective of whether they are poor or non poor, irrespective of whether they are beneficiaries of these big push pro-poor interventions, and irrespective of the time frame considered. This wedge between reality and perceptions can be another reason why redistributive preferences remain inelastic to the interventions [Alesina *et al.* 2012, Alesina *et al.* 2018].

The final set of results extend concerns for inequality to the country as a whole where we ask respondents whether they view inequality as a major concern in Pakistan. 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 ($p = .085$, .065 respectively).

5.4 Perceptions of the Rich and the Causes of Economic Status

The next dimension of redistributive preferences we consider are perceptions of the rich, and of what causes households to hold such economic status. Existing evidence shows individuals are more likely to support redistribution if they believe economic status is due to luck or circumstances beyond the control of individuals [Fong 2001, Alesina and Angeletos 2005, Alesina and La Ferrara

2005, Almas *et al.* 2020]. We documented earlier (Table A5) that the interventions do not change redistributive preferences irrespective of whether inequalities are driven by luck rather than merit. We shed further light on the issue by comparing changes in perceptions of the rich between the TP and NTP. The results are in Table 10.

We start by asking whether *the rich rightfully deserve their income.* We see that two-years post intervention *all* households in treated villages are significantly more likely to agree with this statement than controls. The TP are 7.5pp more likely to agree with this notion of the deserving rich (a 23% increase over controls), the NTP are 5.7pp more likely to agree, and the NP become 7.2pp more likely to agree.

The remaining Columns examine positive and negative views of why the rich 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 *crime or special interests* (a euphemism for corruption). We generally see little impact on positive perceptions of the rich. In contrast, negative views of the rich decline across groups – by endline the TP are 3.6pp less likely to think the rich are rich because of crime or special interests. The NTP share this change in belief: their likelihood to report a negative view of the rich falls 3.0pp by endline. Panel B confirms that views of the rich do not diverge significantly between the TP and NTP.

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 partial population experiment however reveals similar changes in beliefs among the NTP and NP, again suggesting community-wide shifts in perceptions of the rich in response to exposure to pro-poor interventions rather than them being shifting through self-serving biases.

5.5 Perceptions of the Poor and the Causes of Poverty

A natural counterpart is whether and how perceptions of the poor and the causes of poverty are shifted by the pro-poor interventions [Andersen *et al.* 2023]. Focusing first on perceptions of the poor, we asked households whether they thought the poor: (i) *lack the ability to manage money or other assets*; (ii) *waste their money on inappropriate items*; (iii) *do not actively seek to improve their lives*; (iv) *are not motivated because of outside support from government/NGOs.* The non poor were only surveyed on these questions at endline.[20]

Table 11 shows the results where the outcome is whether the household head agreed or strongly

[20] Andersen *et al.* [2023] use a housing lottery in Ethiopia to study how an increase in wealth affects support for redistribution, and beliefs about the causes of poverty. They find attitudes toward redistribution and inequality acceptance are insensitive to economic circumstances, but lottery winners become more likely to attribute poverty to character traits rather than luck, in line with a self-serving bias.

agreed with each statement about the poor. To begin with we note that 30-40% of respondents in controls at midline agree/strongly agree with each statement, irrespective of whether they are themselves poor or non poor. The strongest agreement is for the view that the poor are not motivated because of outside support from government/NGOs. However, we find little evidence that views of the poor are shifted by the big push pro-poor interventions.

We next consider views on the causes of poverty. We divide these into structural causes, and those more closely related to seeing poverty as destiny or 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.* On poverty as destiny, we asked households whether they thought the poor were poor because: (i) *they are unlucky*; (ii) *they have encountered misfortunes*; (iii) *they have bad fate/destiny.*

Table 12 and 13 show results for views on structural causes of poverty, and poverty as destiny respectively. In each case the outcome is whether the household head agreed or strongly agreed with each statement. Focusing first on structural causes, we see that 70-80% of respondents in controls at midline agree/strongly agree with each statement, irrespective of whether they are themselves poor or non poor. The belief in structural causes of poverty is thus far more prevalent – among all households – than views based on the traits of the poor. As Panel A of Table 12 shows, at midline, the big push interventions cause significant falls in the view among households that the causes of poverty are structural. This holds across all four causes and magnitudes of impacts vary between 5pp and 9pp. However, by endline these treatment effects fade, and Panel B shows no divergence in views between the treated poor and not treated poor on structural causes of poverty.

Table 13 shows the view of poverty as destiny is generally less prevalent among controls than the view of poverty as being due to structural causes. The interventions do little to shift views of poverty as destiny among the treated poor and not treated poor. However, among the non poor, we find significant increases in agreement with the views that the poor are poor because of being unlucky or having bad fate/destiny. That this is not shared among the not treated poor suggests this is not merely reflecting the within-village randomization of asset/cash transfers.

5.6 Pro-Market Beliefs

Pro-market beliefs can be impacted by the kinds of big push intervention we study. For beneficiaries, the interventions lead to changes in occupational choice by enabling them to combine their labor with capital, and hence they engage to a greater extent day-to-day in market transactions. The pro-market beliefs of the NTP and NP can also shift if there is a demonstration effect of the greater market engagement of the TP, or through any changes in their own economic circumstances occurring through spillovers or general equilibrium effects .

As described earlier, we measure pro-market beliefs using the same index components as Di Tella *et al.* [2007], capturing beliefs related to individualism, meritocracy, materialism and generalized trust. Table 14 shows how the pro-market index overall is impacted, and Table A9 shows how each component shifts.

In Table 14 we find that all groups hold significantly more pro-market beliefs at midline. The impact on the TP is .198, an effect significant at the 1% level and from baseline level of 2.4 among controls. The magnitudes of impact on the pro-market beliefs of the NTP and NP are similar. However, for each group, we see a significant decline in these beliefs by endline (p = .008, .022 and .050 respectively), so by endline, pro-market beliefs no longer differ to controls.

Examining how each component of the pro-market beliefs index shifts, Table A9 shows that across groups, the impacts on the aggregate index are driven by greater beliefs in meritocracy, materialism, and generalized trust. The treated poor 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 – where the treated poor are 6.4pp more likely to report trusting other people in Pakistan than controls, relative to a baseline of 42.9pp.[21]

5.7 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]. While we earlier described a weak positive correlation between pro-market beliefs and trust in neighbors, we can use our experimental variation to estimate whether these beliefs shift in similar or different directions as a result of big push pro-poor interventions. Treatment effects on the index of trust in neighbors we used for the cluster analysis are shown in the remaining Columns of Table 14. Table A9 shows how each component of this index shifts.

All groups have significant increases in their index of trust in neighbors at midline. The impact on the TP is .179, an effect significant at the 1% level. The magnitude of impact is similar across the TP, NTP and NP. However, impacts on trust in neighbors fade by endline. For the NTP and NP, these declines over time are statistically significant (p = .058, .003 respectively). Panel B shows that as a result, by endline there are significant differences in trust in neighbors between the treated poor and not treated poor: the within-village estimate at endline is .072 and significant at the 1% level.

Table A10 shows that for all groups, treatment effects on the index are driven by greater perceptions that the rule of law operates, that crime is down relative to three years ago, and

[21] Margalit and Shayo [2020] 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 find treated subjects shift right on policy, driven by growing familiarity with, and trust of, markets.

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 in belief relative to controls). Finally, the treated poor are 3.5pp more likely to report feeling safe in their village. This is a dimension of beliefs where intervention impacts are sustained at endline: four-years post intervention, the treated poor remain 2.3pp more likely than the poor in controls to report feeling safe in their village.[22]

The within-village estimates shown in Panel B of Table A9 highlight a long run divergence in beliefs between the treated and not treated poor on one dimension: the treated poor are 4.1pp more likely to report crime being down relative to three years ago ($p = .034$).

These changes move 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 beliefs can shift together is that they both relate to motivations to exert productive effort. 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 their returns to effort are less likely to be expropriated.[23]

5.8 Taking Stock

Tying the findings together, we can link back the results in this Section to our core finding that the big push interventions shift all households towards the right-type preference cluster at midline (Table 7). To the extent that these are driven by changes in redistributive preferences, the detailed results suggest the key driver is in terms of whether they view the *rich to rightfully deserve their income*. Other dimensions of the redistributive index used for the cluster analysis related to taxing windfall gains or societal concerns over inequality do not drive preference shifts to the right-type cluster. The impacts on the statement on whether *the rich should give a part of their income to the poor*, if anything goes the other way, nudging households towards the left-type preference cluster. The shifts towards stronger pro-market beliefs and higher trust in neighbors also drive the shift towards right-type preference clusters for household heads. Both reflect that the big push pro-poor interventions encourage productive effort – through stronger beliefs in meritocracy and reduced perceptions that the returns to effort will be expropriated by others.

It is also useful to tie these findings back to those the dynamic patterns of noticeable economic gains to the TP. As shown earlier, these accrue within a year post-intervention, and stabilize

[22] 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.

[23] Our findings thus build on nascent experimental evidence on this issue. Heβ *et al.* [2021] show that randomly allocated community development projects in villages in Gambia lead to a modest transformation of villages from gift economies to more formal economies.

thereafter until the four-year endline. The TP thus experience a pattern of immediate changes in economic circumstances following the transfer of assets/cash, with gains persisting, but not accumulating further. The fact that many dimensions of economic preference are shifted two-years post intervention does not itself then lead to further economic gains to households. In other words, the increased views among the TP of the rich as being deserving, increased pro-market beliefs, or higher trust in neighbors, do not lead to a virtuous cycle feeding back to improve their economic outcomes. This suggests anti-poverty approaches that aim solely to shift economic preferences of the poor might be less likely by themselves to trigger sustained economic gains.

6 Discussion

6.1 Voting

One route through which big push pro-poor interventions can have persistent impacts is through changes in engagement with political processes. Between baseline and midline high stakes local elections were held across our study region. We thus probe this possibility using self-reported data on turnout in these elections. Of course this is likely upwards biased, but if this bias does not differ between treated and control villages, the estimated treatment effects remain informative. The results are in Table 15.

We find all groups become significantly more likely to report voting in local elections: the TP are 5.8pp more likely, and the NTP are 5.1pp more likely – both impacts significant at the 1% level. However, the largest increase is seen among the NP, who are 9.2pp more likely to self-report having voted.[24]

As non-eligibles are likely to outnumber those eligible for any pro-poor intervention, the median voter will typically be from a non-eligible household. Hence it is important to consider the possibility that across groups, votes for political parties might be swayed by the interventions – even if stated redistributive preferences themselves are largely inelastic in the long run. To probe this, we exploit the fact that at baseline, for TP and NP households, we asked them their affinity with platforms of political parties in Pakistan. Although imperfect in this context, we can still classify parties on a left-centre-right spectrum and use each respondent's affinity with party platforms to classify household heads as left-leaning, centrist or right-leaning. Our classification suggests that in controls, around 14% of poor household heads are left leaning, 69% are centrist and 16% are right leaning.[25]

[24] 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].

[25] 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

The remaining Columns in Table 15 show heterogeneous impacts on voting by political affinity expressed at baseline. Household heads of all political affinities significantly increase their likelihood to vote. Among the TP, the largest effects are among left- and right-leaning households, although the impacts are not significantly different across political preferences. Among the NP, the largest point estimate is for right-leaning households, that increase their voting by 11.4pp, but again these are not different from the impacts on left-leaning households ($p = .208$). Overall, while the evidence suggests interventions increase political participation across the board, it does not suggest this favors political views of any particular kind.

6.2 External Validity and Future Agenda

We conclude by discussing study features that are key to the external validity of our findings. All represent important directions in which to extend our work in future.

Setting Villages in our field experiment are close-knit and ethnically homogeneous. This makes them an almost ideal setting in which to extend the existing literature on redistributive preferences based on lab or survey experiments, to study how a wider set of economic preferences shift in response to large real world shifts in noticeable economic gains, changes in relative economic standing, and reductions in village inequality. However, in more geographically dispersed settings, economic impacts on beneficiaries might not be so noticeable, and misperceptions of intervention gains could be more likely to persist. Whether this weakens the finding that common exposure to interventions matters for shifting economic preferences remains open for future work to explore. In more diverse 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 remains an open question to understand whether in such settings pro-poor interventions are more likely to lead to polarization or conflict on some dimensions of economic preference than we find in our study setting.

Financing Interventions Our results suggest the link between pro-poor policy interventions and economic preferences does not depend on whether households are themselves beneficiaries – rather our partial population experiment reveals that preferences are driven by common village-wide exposure to such pro-poor policies. We show such experiences can drive economic preferences

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.

even when those experiences occur later in life – this complements work emphasizing how experiences in formative years can determine long run economic preferences and behaviors [Malmendier 2021, Margalit 2019, Giuliano and Spilimbergo 2023]. 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 economic preferences of the rich (non eligibles) might be impacted differently by pro-poor interventions when they are implicitly financing them or when they come at the expense of some other policy or public good they favor.

The Design of Social Protection Systems We have examined the impacts of one-off big push policies in the form of asset or cash transfers. However, social protection systems are designed not only to redistribute resources but also to provide social insurance. As such, a very rich policy space exists including small and frequent transfers, conditional cash transfers, universal transfers (such as UBI), indirect transfers (such as minimum wages), or insurance against shocks to earnings, health, crop failure etc. [Banerjee *et al.* 2022]. While a large literature exists to understand the economic impacts of transfers in-kind versus in cash, as well as political economy arguments in favor of one form of transfer over another, much less is known about how the design of social protection more broadly impacts economic preferences and beliefs of the poor and non poor. There remains huge scope for a future research agenda to be developed along these lines.

A Appendix

A.1 Robustness Checks

We present a sequence of checks on our core finding. We first establish the robustness of our core results when accounting for multiple hypothesis testing (MHT) using sharpened two-stage q-values [Benjamini *et al.* 2006]. Table A4 shows our core results and additionally adds q-values on the eight coefficients of interests once we account for MHT: all the results remain robust to this.

We next check for robustness to attrition. We noted earlier that the bulk of attrition occurs in the first year post intervention and stabilizes thereafter. In each treatment arm, we cannot reject the null that attrition is the same across all groups between midline and endline (when economic preferences are measured). We next estimate Lee Bounds on each treatment effect. We estimate these at midline and endline survey wave separately, and have to drop enumerator dummies when doing so (as those do not exist for attriters). The upper and lower bounds for these modified specifications are shown in Table A4: for the four treatment effects for the treated poor the results are unchanged. For the not treated poor, the lower bound at endline is negative and now statistically different from zero, and for the not poor, the upper bound at endline is positive and statistically different from zero.

The most succinct way to summarize the remaining checks is in Figure A3: each panel shows the estimated treatment effect $(\widehat{\beta}_2^g, \widehat{\beta}_4^g)$ for group g from the between village estimates, and $(\widehat{\beta}_2, \widehat{\beta}_4)$ from the within-village estimates. The baseline estimate in Table 7 is denoted Check 0.

The first check we consider is to drop the enumerator fixed effects from (4) and (5). This is check 1: we see the eight coefficients of interest across panels are generally slightly more imprecisely estimated, although their levels of statistical significance remain unchanged. Check 2 adds the following household characteristics to the baseline between and within village specifications: the households poverty score, household size, age and gender of the household head, whether they have any formal education and whether they are working (as recorded in the pre-intervention census). We find the eight treatment effects remain unchanged in terms of magnitude and precision.

The next series of checks examine robustness to alternative approaches to deriving preference clusters. To begin with we consider a k-means algorithm (rather than k-medians), but still impose a requirement of there being two clusters. Check 3 shows the resulting coefficients of interest, that are very similar in point estimates and precision as our baseline results. We next consider using the k-medians algorithm but allowing for three clusters at endline (so that the Calinski-Harabasz F-statistic is minimized). The optimal number of clusters at midline is two using this statistic. Hence at midline we still identify the same left- and right-type preference clusters, while at endline we identify left-, centre- and right-type clusters. We then estimate (4) and (5) where the outcome is equal to one if the household head belongs to the right-type cluster, and is zero otherwise. The resulting estimates are shown in Check 4, that are again very similar to our baseline estimates. Check 5 repeats this approach using the k-means algorithm and the optimal number of clusters (that is three at midline and two at endline). The resulting estimates are again very similar to the baseline estimates from Table 7.

The final set of checks explore differences across treatment arms T1 and T2. In Check 6 we estimate (4) and (5) using data only from the asset transfer treatment arm T1 and controls. Check 7 uses data only from the T2 treatment arm and controls, where the vast majority of treated households reveal prefer unconditional cash transfers over any combination of assets from the menu presented to them. We see the estimates are largely the same between T1 and T2: for seven out of eight of them their magnitude and significance remains unchanged. The only exception is for the within-village estimate at endline, where we find the divergence in likelihood of being in the right-type preference cluster for TP relative to NTP household heads is more pronounced when cash transfers are provided. In short, impacts on economic preferences of big push pro-poor interventions are mostly similar irrespective of whether the interventions provides transfers in-kind or in cash. Finally, we estimate treatment effects of T1 and T2 from the same between and within village specifications – the coefficients on T1 (T2) are shown in Checks 8 and 9 respectively. We again see a very similar pattern of results as when the sample is limited to controls and one treatment arm at a time, although the within village estimates are noisier (but still statistically significant at endline).

A.2 Other Determinants of Redistributive Preferences

Our data collection exercise was designed to shed light on some of the extensions proposed to the MR framework to better understand determinants of redistributive preferences.

Luck versus Merit One view put forward is that redistributive preferences 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].[26] 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. The wording of the redistributive task is designed to capture distributional preferences without the confounding influence of material self-interest. The results are in Table A5. We see little evidence that behavior in the redistributive task of any group, at either midline or endline, is impacted by the intervention irrespective of whether inequalities are initially framed as being driven by luck or merit.

Effectiveness of Government Redistributive preferences might be easier to shift among those who hold greater belief in the effectiveness of government [Sapienza and Zingales 2013, Kuziemko *et al.* 2015, Alesina *et al.* 2018]. While much of the evidence related to this is taken from cross country data, findings from information experiments remain mixed – but this channel might be especially relevant in low state capacity context like Pakistan [Acemoglu *et al.* 2020].[27]

We can examine the issue in our context given both treatments were implemented in collaboration with quasi-government agencies, and so the interventions are best perceived as government delivered programs. Table A6 shows the results, where we estimate treatment effects on redistributive preferences by households baseline views on the effectiveness of government. Recall that around a quarter of household heads believe government is effective (Table 2). Irrespective of households pre-intervention beliefs over the effectiveness of government, we generally replicate the broad findings on redistributive preferences documented earlier. In no case do we find significant differences in intervention responses based on beliefs on government effectiveness. This holds

[26] 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].

[27] 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.

across TP, NTP and NP households, at midline and endline.[28]

This highlights the potential for a vicious cycle to develop, whereby low state capacity leads to a widespread belief that government is ineffective, and so preferences for redistribution remain inelastic even with the demonstration of an effective government-sponsored pro-poor intervention – that can ultimately feed back to further weaken state capacity.

Social Mobility An important modification of the MR framework is to consider perceived social mobility as determining redistributive demands [Piketty 1995, Benabou and Ok 2001, Fong 2001, Alesina and La Ferrara 2005, Alesina *et al.* 2018, Fong and Poutvaara 2019]. More precisely, the more perceived social mobility (or prospects for upward mobility (POUM)), then under some conditions, demand for redistribution will be lower than in the standard MR model [Benabou and Ok 2001]. We examine this by asking households about their aspirations for their future life as follows: *On a ladder with 10 steps, what is the best life you can achieve?* We then estimate whether aspirations across groups are impacted by the pro-poor interventions.

The results are in Table A7. As Column 1a shows, the interventions have no impact on beneficiaries perceived social mobility. In contrast, for the NTP, by endline they perceive their best life to be significantly higher than for poor households in controls, with these aspirations increasing between midline and endline ($p = .068$). This could partly explain the relative weak response in redistributive preferences among the NTP to the interventions. At the same time, the same is not true for the NP: they have significant declines in their aspirations at midline, although these recover significantly by endline ($p = .086$).[29]

Imperfect Information About Own Standing We consider the idea that inelastic redistributive preferences stem from households imperfect information or beliefs about their own relative standing [Benabou and Ok 2001, Alesina and Angeletos 2005, Hoy and Mager 2021, Hvidberg *et al.* 2023]. Cross-country surveys typically find a majority think they are around the middle of the income distribution regardless of whether they are rich or poor [Hoy and Mager 2021]. Misperceptions are important because preferences for redistribution are more correlated with perceived than actual position in the income distribution [Hauser and Norton 2017, Alesina *et al.* 2018, Gimpelson and Treisman 2018].

We examine how households own perceived standing is impacted by the interventions. We first consider absolute standing by asking, *On a ladder with 10 steps, where do you currently stand?* The results are in Table A8. The beneficiaries of the interventions – the TP – have no change in

[28] 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, or that people can affect government policies, as well as in beliefs of whether NGOs are effective.

[29] Treating these outcomes as ordinal rather than cardinal suggests using an ordered logit specification. When doing so it is however not possible to condition out enumerator fixed effects. Notwithstanding this concern, the ordered logit specification suggests rather similar impacts on the TP, the NTP have a significant rise at endline, and the impacts on the NTP become weaker and the decline at midline is no longer significant at the 10% level.

their perceived own standing, despite their measurable and persistent gains from the intervention. In contrast, both the NTP and NP report significant falls in their own standing at midline. As a result, Panel B highlights that within-village, the TP diverge significantly from the NTP in their own perceived standing. This is in line with findings from higher income settings that individual well-being can fall when individuals observe changes in wealth/income in people around them [Luttmer 2005, Card *et al.* 2012, Perez-Truglia 2020, Cullen and Perez-Truglia 2022]. The results highlight the potential for pro-poor interventions to generate negative psychological spillovers to non-beneficiaries, although households appear to adapt to this by endline.[30]

To assess whether households perceive changes in their relative ranking, we showed respondents the five figures of income distributions. They were asked where they see themselves in the distribution 'today' and 'three years ago.' We then construct a dummy indicating if the individual perceived their rank in the distribution to have risen. These results are shown in the remaining Columns of Table A8. Here we find null impacts throughout. Hence while non-beneficiary households do perceive changes in their absolute standing as a result of the pro-poor interventions, no group of households recognize changes in their relative standing. This matches some of the earlier results on perceptions of village inequality, again emphasizing that perceptions are hard to shift even in the presence of big push interventions in small village economies.

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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. Beliefs (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-survey wave

	(1) Own Livestock	(2) Value Livestock | Own Livestock	(3) Iron Roof	(4) Often Consume Own Produced Milk	(5) Monthly Food Expenditure (AE)
Treatment 1: Asset Transfer					
One year impact	.211***	.133*	.034	.082**	-.015
	(.028)	(.079)	(.029)	(.032)	(.028)
Two year impact	.231***	.157***		.113***	.022
	(.023)	(.059)		(.027)	(.019)
Four year impact	.190***	.107**		.087***	.032
	(.025)	(.051)		(.029)	(.022)
Treatment 2: Revealed Preferred Unconditional Cash Transfer					
One year impact	.102**	.153*	.048	.038	-.036
	(.043)	(.090)	(.046)	(.036)	(.030)
Two year impact	.138***	.138**		.086***	.028*
	(.021)	(.062)		(.024)	(.017)
Four year impact	.131***	.139**		.053**	.042*
	(.025)	(.065)		(.024)	(.024)
Mean (poor, controls at baseline)	.563	2836	.360	.328	83.7
p-values:					
T1=T2 (one year)	[.039]	[.868]	[.837]	[.395]	[.690]
T1=T2 (two year)	[.004]	[.835]		[.510]	[.812]
T1=T2 (four year)	[.095]	[.742]		[.426]	[.809]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of observations	10784	6601	2340	10785	10700

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

Table 4: Noticeable Economic Impacts, Pooled Specification

Within Village Estimates Treated Poor vs Not Treated Poor

Standard errors clustered by village-survey wave

	(1) Own Livestock	(2) Value Livestock | Own Livestock	(3) Iron Roof	(4) Often Consume Own Produced Milk	(5) Monthly Food Expenditure (AE)
One year impact	.160***	.142**	.040**	.061***	-.025*
	(.024)	(.059)	(.016)	(.023)	(.014)
Two year impact	.184***	.148***		.099***	.025**
	(.015)	(.040)		(.016)	(.012)
Four year impact	.160***	.123***		.069***	.037***
	(.017)	(.034)		(.015)	(.013)
Mean (poor, controls at baseline)	.563	2836	.360	.328	83.7
p-values:					
One year = Two year	[.396]	[.940]		[.168]	[.008]
Two year = Four year	[.298]	[.647]		[.176]	[.531]
One year = Four year	[.998]	[.785]		[.766]	[.002]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of observations	10784	6601	2340	10785	10700

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

Table 5: Village Consumption Inequality

Between Village Estimates Treated vs Controls

OLS estimates, robust standard errors

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

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

Table 6: Preference Cluster Types

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

	Two Year Midline			Four Year Endline		
	Left	**Right**	**Left = Right**	**Left**	**Right**	**Left = Right**
Redistributive preferences	3.19	3.16	[.615]	3.61	3.06	[.000]
	(.837)	(.827)		(.619)	(.724)	
Pro-market beliefs	1.96	2.80	[.000]	2.48	2.78	[.000]
	(.960)	(.820)		(.918)	(.868)	
Trust in neighbors	1.91	3.30	[.000]	2.28	3.57	[.000]
	(.753)	(.542)		(.729)	(.503)	
Observations	5143	9780		6034	7670	

Notes: We conduct a k-median cluster analysis separately for the midline and endline survey waves, picking two random cluster centers to start the analysis and using a Euclidian distance measure of (dis)similarity. Columns marked left and right show sample means and standard deviations (in parentheses) for each cluster for the three dimensions of economic preferences: redistributive preferences, pro market beliefs, and trust in neighbors. The p-values on the tests of equality are derived from OLS regressions of the corresponding economic preference index on a dummy variable indicating cluster membership and district (strata) fixed effects. Standard errors are clustered by village.

Table 7: Preference Cluster Types

OLS estimates, standard errors clustered by village-survey wave
95% confidence interval in brackets

	Belongs to Right-type: less redistribution, more pro-market, higher trust in neighbors		
	Treated Poor	**Not Treated Poor**	**Not Poor**
	(1a)	**(1b)**	**(1c)**
A. Between Village Estimates (Treated vs Control)			
Two year impact	.089***	.085***	.099***
	(.023)	(.021)	(.021)
	[.045,.135]	[.044,.127]	[.058,.141]
Four year impact	.022	-.013	.006
	(.027)	(.029)	(.025)
	[-.031,.074]	[-.071,.044]	[-.043,.055]
Two Year = Four Year	[.053]	[.005]	[.005]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)			
Two year impact	.002		
	(.011)		
	[-.021,.025]		
Four year impact	.031**		
	(.013)		
	[.005,.056]		
Two Year = Four Year	[.087]		
Mean Outcome, Controls	.611		.580
Observations: Panel A	7800	8988	16278
Observations: Panel B	7910		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Column 1a), Not Treated Poor (Column 1b), and Not Poor (Column 1c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Column 1a). All regressions include treatment dummies (pooling T1 and T2), district (strata), 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 outcome is a dummy indicating if the household head is assigned to the right-type preference cluster. 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: Redistributive Preferences

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	Should the rich give part of their income to the poor?			A 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?			How much should they be taxed (implied ATR)?		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)
A. Between Village Estimates (Treated vs Control)									
Two year impact	.012	.020**	.030**	.060*	.039	.071**	.004	-.000	.003
	(.012)	(.009)	(.012)	(.033)	(.034)	(.029)	(.004)	(.004)	(.001)
	[-.010,.035]	[.001,.038]	[.006,.054]	[-.004,.125]	[-.028,.106]	[.014,.128]	[-.005,.012]	[-.008,.007]	[-.006,.012]
Four year impact	.016*	.016	.005	.028	.034	.029	-.005	-.003	.001
	(.009)	(.009)	(.008)	(.034)	(.035)	(.034)	(.005)	(.003)	(.004)
	[-.001,.034]	[-.003,.035]	[-.011,.022]	[-.039,.095]	[-.034,.102]	[-.039,.097]	[-.012,.002]	[-.009,.004]	[-.007,.010]
Two Year = Four Year	[.763]	[.787]	[.098]	[.490]	[.913]	[.358]	[.141]	[.620]	[.811]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)									
Two year impact	-.006			.010			.004		
	(.007)			(.017)			(.004)		
	[-.020,.009]			[-.025,.044]			[-.004,.011]		
Four year impact	.002			-.017			-.001		
	(.005)			(.012)			(.002)		
	[-.009,.012]			[-.041,.008]			[-.006,.003]		
Two Year = Four Year	[.431]			[.237]			[.256]		
Mean in Controls	95.2%		93.8%	64.7%		66.9%	6.8%		7.33%
Observations: Panel A	8126	9382	17004	7800	8988	16279	5516	6296	11802
Observations: Panel B	8269			7910			5968		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Columns 1a, 2a, 3a), Not Treated Poor (Columns 1b, 2b, 3b), and Not Poor (Columns 1c, 2c, 3c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered 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 9: Perceptions of Inequality

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	Inequality decreased in the last three years			Share in village that do not have enough to eat			Inequality is a serious problem in Pakistan?		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)
A. Between Village Estimates (Treated vs Control)									
Two year impact	.037	.011	.002	-.013	-.012	-.024**	.013	.017	.027*
	(.031)	(.033)	(.027)	(.009)	(.009)	(.011)	(.015)	(.014)	(.015)
	[-.023,.098]	[-.053,.076]	[-.051,.056]	[-.032,.006]	[-.029,.005]	[-.046,-.003]	[-.017,.043]	[-.012,.045]	[-.003,.058]
Four year impact	-.011	-.008	-.011	-.005	-.002	-.004	-.012	-.021	-.010
	(.032)	(.032)	(.028)	(.004)	(.005)	(.006)	(.017)	(.018)	(.015)
	[-.073,.052]	[-.071,.056]	[-.067,.045]	[-.014,.004]	[-.011,.007]	[-.015,.008]	[-.046,.022]	[-.056,.014]	[-.039,.019]
Two Year = Four Year	[.263]	[.674]	[.723]	[.402]	[.319]	[.093]	[.249]	[.085]	[.065]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)									
Two year impact	.018			-.001			-.009		
	(.017)			(.004)			(.011)		
	[-.016,.051]			[-.009,.008]			[-.030,.012]		
Four year impact	-.012			-.002			.003		
	(.020)			(.001)			(.010)		
	[-.051,.027]			[-.005,.001]			[-.016,.021]		
Two Year = Four Year	[.256]			[.763]			[.403]		
Mean Outcome, Controls	34.0%		38.8%	9.05%		10.8%	85.5%		86.1%
Observations: Panel A	8126	9382	17004	8126	9382	17004	8126	9382	17004
Observations: Panel B	8262			8262			8262		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Columns 1a, 2a, 3a), Not Treated Poor (Columns 1b, 2b, 3b), and Not Poor (Columns 1c, 2c, 3c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave level, and 95% confidence intervals are reported in brackets. The outcomes are three variables measuring individuals' perceptions of inequality. The first is “"Do you think that the difference in income between the few people at the top and most people at the bottom has [...] in the last three years?" where respondents were presented them with five possible answers (has decreased a lot; has decreased a little; has remained the same; has increased a little; has increased a lot). We convert this into a dummy equal to one if the respondent answers "decreased a little" or "decreased a lot." The second outcome asks “Think of the people in your village who do not have enough to eat or sometimes may have to skip meals. Out of every 100 people, how many do you think are in that situation in your village?”. The third asks "Do you think inequality is one of the larger socio-economic issues of Pakistan?” 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 the Rich and Causes of Economic Status

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	The rich rightfully deserve their income			Reason rich: education, intelligence, hard work			Reason rich: crime, special interests		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)
A. Between Village Estimates (Treated vs Control)									
Two year impact	.075***	.057**	.072***	-.005	.011	-.021	-.014	-.015	-.022**
	(.032)	(.028)	(.027)	(.021)	(.019)	(.014)	(.016)	(.016)	(.010)
	[.013,.138]	[.001,.113]	[.018,.126]	[-.047..038]	[-.025,.048]	[-.049,.008]	[-.046,.018]	[-.035,.030]	[-.042,-.002]
Four year impact	-.017	.005	-.001	.028	.036*	.012	-.036**	-.030*	-.001
	(.029)	(.029)	(.024)	(.022)	(.018)	(.019)	(.017)	(.016)	(.011)
	[-.074,.040]	[-.053,.063]	[-.048,.047]	[-.016,.071]	[-.000,.073]	[-.026,.049]	[-.069,-.002]	[-.061,.002]	[-.023,.021]
Two Year = Four Year	[.024]	[.201]	[.041]	[.262]	[.328]	[.160]	[.311]	[.450]	[.143]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)									
Two year impact	.017			-.010			.002		
	(.021)			(.016)			(.009)		
	[-.025,.058]			[-.043,.023]			[-.017,.021]		
Four year impact	-.024			-.002			-.005		
	(.015)			(.014)			(.012)		
	[-.054,.006]			[-.030,.027]			[-.028,.018]		
Two Year = Four Year	[.144]			[.708]			[.637]		
Mean Outcome, Controls	32.3%		31.0%	30.0%		33.5%	11.2%		11.0%
Observations: Panel A	8126	9382	17004	8126	9382	17004	8126	9382	17004
Observations: Panel B	8262			8262			8262		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Columns 1a, 2a, 3a), Not Treated Poor (Columns 1b, 2b, 3b), and Not Poor (Columns 1c, 2c, 3c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered 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 11: Perceptions of the Poor

Strongly agree or agree with statements

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	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		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)
A. Between Village Estimates (Treated vs Control)												
Two year impact	.030	.059*		.008	.036		.018	.033		.007	.014	
	(.029)	(.034)		(.030)	(.032)		(.035)	(.034)		(.039)	(.040)	
	[-.028,.088]	[-.008,.127]		[-.051,.066]	[-.026,.099]		[-.051,.088]	[-.034,.101]		[-.070,.084]	[-.064,.092]	
Four year impact	-.021	-.004	-.004	-.003	.006	-.011	.006	.015	-.001	.008	-.004	.008
	(.026)	(.028)	(.019)	(.029)	(.032)	(.024)	(.031)	(.031)	(.021)	(.031)	(.030)	(.020)
	[-.072,.030]	[-.058,.051]	[-.043..034]	[.797]	[-.058,.070]	[-.060,.038]	[-.056,.066]	[-.046,.075]	[-.043,.041]	[-.053,.068]	[-.063,.056]	[-.032,.048]
Two Year = Four Year	[.178]	[.135]			[.484]		[.783]	[.669]		[.994]	[.713]	
B. Within Village Estimates (Treated Poor vs Not Treated Poor)												
Two year impact	-.021			-.019			-.006			.002		
	(.014)			(.016)			(.016)			(.017)		
	[-.049,.008]			[-.050,.012]			[-.037,.025]			[-.032,.036]		
Four year impact	-.007			.001			-.000			.020		
	(.014)			(.014)			(.019)			(.017)		
	[-.036,.021]			[-.028,.029]			[-.038,.037]			[-.012..053]		
Two Year = Four Year	[.538]			[.370]			[.820]			[.459]		
Mean Outcome, Controls	.330		.256	.357		.348	.362		.333	.400		.413
Observations: Panel A	7505	8502	8039	7537	8551	8089	7527	8530	8065	7271	8195	7757
Observations: Panel B	7499			7544			7527			7204		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Columns 1a, 2a, 3a, 4a), Not Treated Poor (Columns 1b, 2b, 3b, 4b), and Not Poor (Columns 1c, 2c, 3c, 4c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a, 4a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered 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 12: Poverty as Driven by Structural Causes

Strongly agree or agree with statements

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	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		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)
A. Between Village Estimates (Treated vs Control)												
Two year impact	-.052*	-.062**		-.075**	-.093***		-.067**	-.062**		-.057**	-.101***	
	(.027)	(.025)		(.030)	(.031)		(.028)	(.031)		(.026)	(.026)	
	[-.105,.002]	[-.111,-.014]		[-.134,-.016]	[-.154,-.031]		[-.122,-.012]	[-.123,-.002]		[-.109,-.005]	[-.153,-.049]	
Four year impact	-.000	-.017	-.026	-.026	-.023	-.027	-.011	-.017	-.007	-.013	-.035	-.012
	(.024)	(.025)	(.023)	(.026)	(.025)	(.020)	(.025)	(.028)	(.022)	(.023)	(.024)	(.017)
	[-.048,.048]	[-.067,.033]	[-.071,.020]	[-.077,.025]	[-.072,.026]	[-.066..011]	[-.061,.039]	[-.072,.037]	[-.051,.036]	[-.058,.032]	[-.081,.012]	[-.046,.022]
Two Year = Four Year	[.159]	[.189]		[.203]	[.070]		[.124]	[.248]		[.186]	[.051]	
B. Within Village Estimates (Treated Poor vs Not Treated Poor)												
Two year impact	.003			.015			-.006			.036		
	(.016)			(.018)			(.017)			(.017)		
	[-.028,.035]			[-.021,.051]			[-.041,.028]			[.002,.069]		
Four year impact	.008			-.005			.008			.014		
	(.014)			(.014)			(.012)			(.015)		
	[-.019,.035]			[-.033,.023]			[-.015,.031]			[-.016,.045]		
Two Year = Four Year	[.820]			[.394]			[.520]			[.357]		
Mean Outcome, Controls	.795		.767	.796		.751	.807		.762	.803		.756
Observations: Panel A	7522	8530	8065	7403	8353	7842	7375	8302	7816	7440	8411	7937
Observations: Panel B	7526			7332			7285			7399		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Columns 1a, 2a, 3a, 4a), Not Treated Poor (Columns 1b, 2b, 3b, 4b), and Not Poor (Columns 1c, 2c, 3c, 4c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a, 4a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered 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 13: Poverty as Destiny

Strongly agree or agree with statements

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	They are unlucky			They have encountered misfortunes			They have bad fate/destiny		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)
A. Between Village Estimates (Treated vs Control)									
Two year impact	-.036	-.012		-.054	-.048		-.040	-.038	
	(.036)	(.038)		(.034)	(.036)		(.035)	(.033)	
	[-.107,.035]	[-.087,.063]		[-.120,.013]	[-.119,.023]		[-.108,.029]	[-.103,.028]	
Four year impact	.006	.031	.045*	.012	.016	.023	.027	.015	.052**
	(.028)	(.029)	(.025)	(.028)	(.027)	(.023)	(.026)	(.027)	(.022)
	[-.048,.061]	[-.025,.087]	[-.005,.095]	[-.044,.067]	[-.038,.069]	[-.023,.069]	[-.025,.079]	[-.039,.068]	[.008,.097]
Two Year = Four Year	[.339]	[.341]		[.132]	[.136]		[.109]	[.196]	
B. Within Village Estimates (Treated Poor vs Not Treated Poor)									
Two year impact	-.018			-.002			.001		
	(.019)			(.023)			(.019)		
	[-.055,.020]			[-.048,.044]			[-.037,.040]		
Four year impact	-.019			.002			.018		
	(.016)			(.018)			(.015)		
	[-.049,.012]			[-.034,.037]			[-.011..047]		
Two Year = Four Year	[.973]			[.897]			[.501]		
Mean Outcome, Controls	.484		.417	.489		.395	.391		.285
Observations: Panel A	7518	8532	8040	7426	8399	7926	7526	8535	8006
Observations: Panel B	7530			7373			7537		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Columns 1a, 2a, 3a), Not Treated Poor (Columns 1b, 2b, 3b), and Not Poor (Columns 1c, 2c, 3c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered 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 14: Pro-Market Beliefs and Trust in Neighbors

OLS estimates, standard errors clustered by village-survey wave
95% confidence interval in brackets

	Pro Market Beliefs Index			Trust in Neighbors		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
A. Between Village Estimates (Treated vs Control)						
Two year impact	.198***	.196***	.174***	.179***	.152***	.199***
	(.054)	(.060)	(.057)	(.056)	(.055)	(.045)
	[.092,.304]	[-.078,.314]	[.062,.286]	[.069,.290]	[.044,.260]	[.110,.287]
Four year impact	-.027	.002	.023	.070	-.002	.016
	(.065)	(.062)	(.054)	(.062)	(.064)	(.041)
	[-.154,.100]	[-.121,.124]	[-.084,.131]	[-.052,.192]	[-.129,.124]	[-.066,.097]
Two Year = Four Year	[.008]	[.022]	[.050]	[.187]	[.058]	[.003]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)						
Two year impact	.012			.030		
	(.027)			(.025)		
	[-.042,.066]			[-.021,.080]		
Four year impact	-.022			.072***		
	(.028)			(.026)		
	[-.077,.033]			[.022,.123]		
Two Year = Four Year	[.395]			[.234]		
Mean Outcome, Controls	2.40		2.40	2.75		2.67
Observations: Panel A	8126	9382	17004	8126	9382	17003
Observations: Panel B	8262			8262		

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

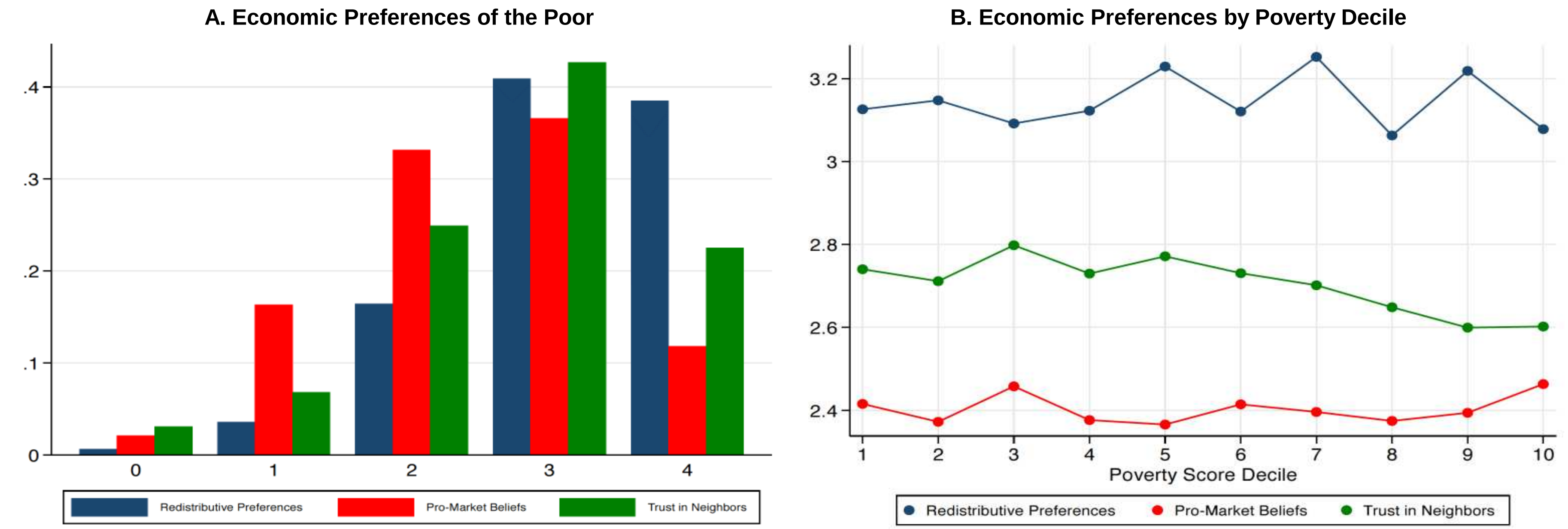
Table 15: Voting

OLS estimates, standard errors clustered by village-survey wave
95% confidence interval in brackets

	Voted in Past Local Election				
	Treated Poor	**Not Treated Poor**	**Not Poor**	**Treated Poor**	**Not Poor**
	(1a)	**(1b)**	**(1c)**	**(2a)**	**(2c)**
A. Between Village Estimates (Treated vs Control)					
Two year impact	.058***	.051***	.092***		
	(.011)	(.011)	(.025)		
	[.036,.081]	[.030,.073]	[.043,.141]		
Two year impact | left leaning				.097***	.072***
				(.026)	(.025)
				[.045,.149]	[.022,.122]
Two year impact | centrist				.065***	.075***
				(.019)	(.027)
				[.027,.103]	[.020,.129]
Two year impact | right leaning				.091**	.114***
				(.038)	(.024)
				[.016,.166]	[.067,.162]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)					
Two year impact	.012				
	(.008)				
	[-.004,.029]				
Mean Outcome, Controls	89.1%		84.6%	89.1%	84.6%
p-values:					
Left leaning = Centrist				[.224]	[.912]
Left leaning = Right leaning				[.891]	[.208]
Centrist = Right leaning				[.529]	[.113]
Observations: Panel A	4043	4677	8489	1589	5341
Observations: Panel B	4144				

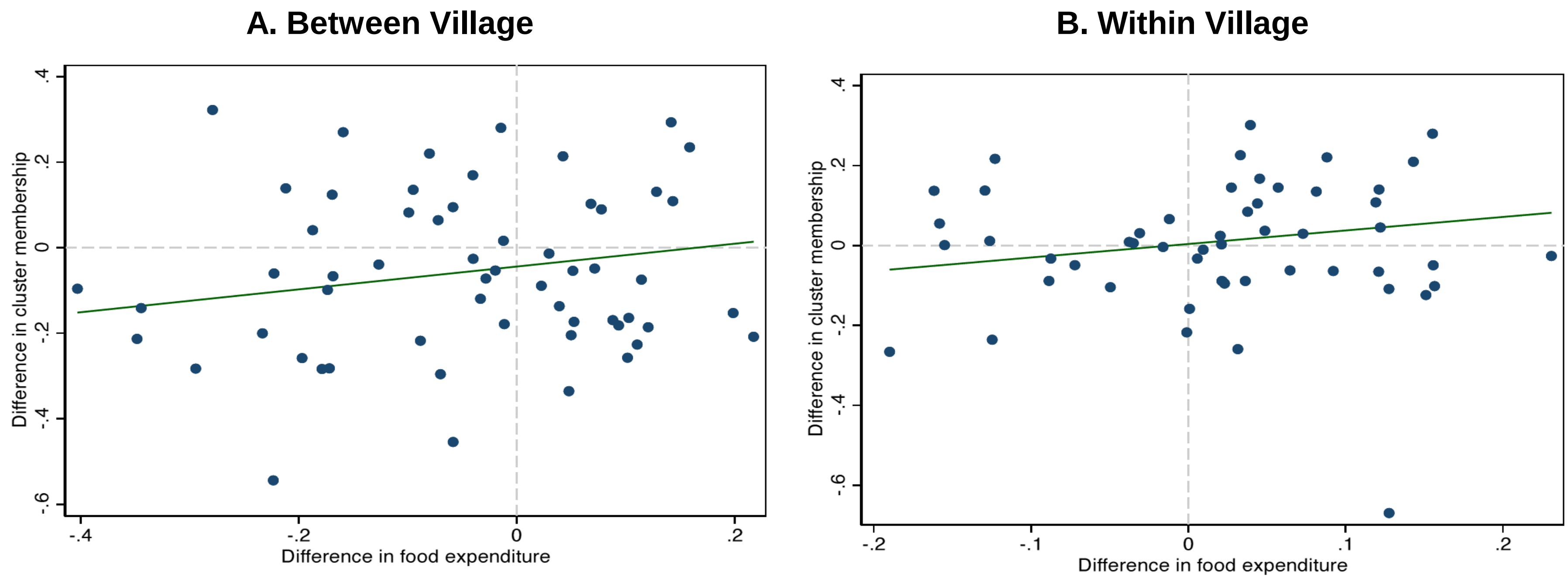
Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Column 1b), and Not Poor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



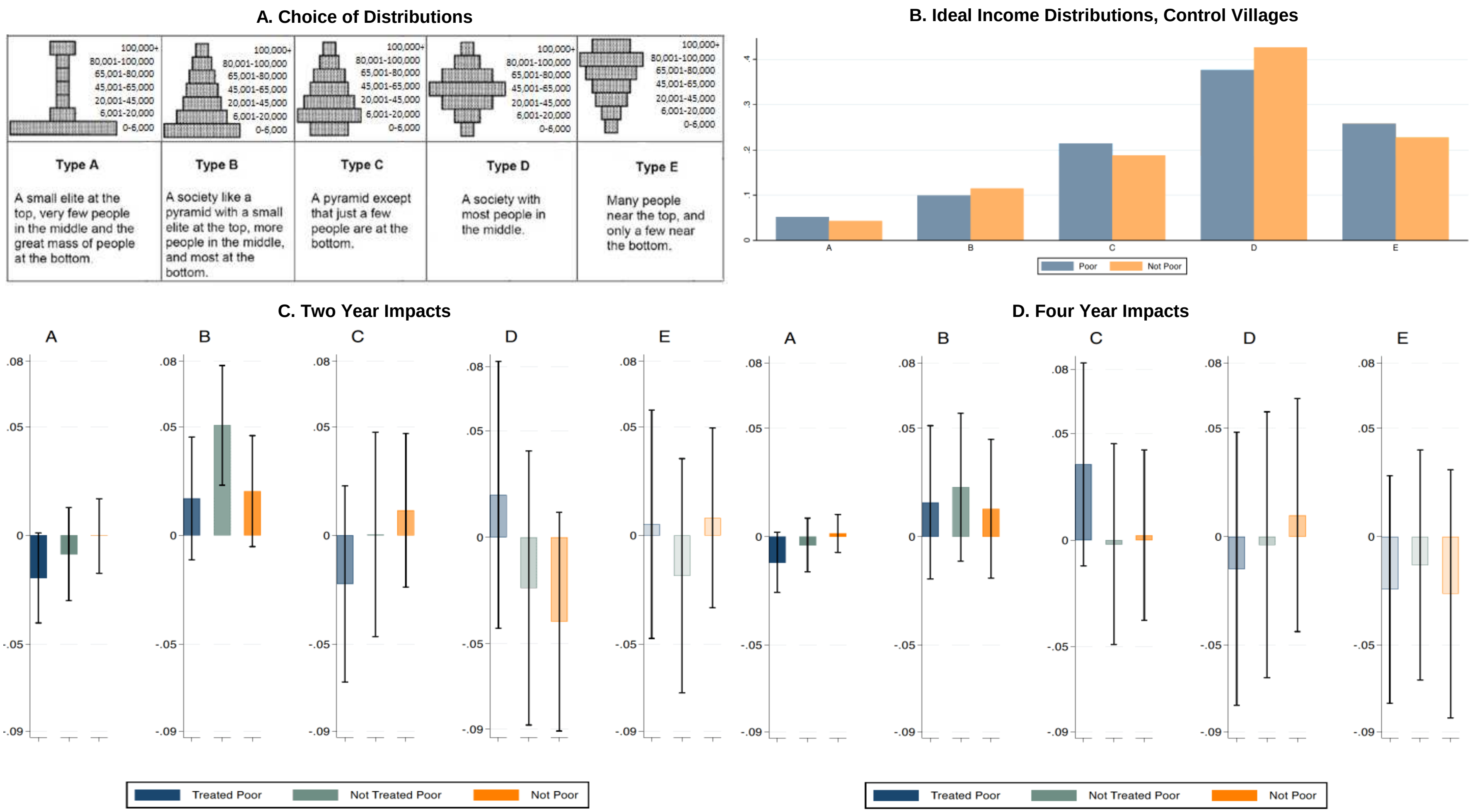
Notes: Panel A shows the distribution of economic preferences among the poor in control villages at midline. We do so for each of the three economic preferences indices. Panel B shows how the average economic preference for each index varies across deciles of the household poverty score. Poor households eligible for the interventions are in the first four deciles. Not poor households make up the last six deciles.

Figure 2: Changes in Preference Clusters of the Treated Poor



Notes: The Figure shows the relation between estimated changes in treatment effects between midline and endline on the likelihood to belong to the right-type preference cluster among the treated poor, and estimated changes in treatment effects between midline and endline on log food expenditure per adult equivalent among the treated poor. In Panel A each point shows the change in both treatment effects for treated poor households in each treated village relative to poor households in control villages in the same district. Panel B undertakes a similar exercise based on the comparison of treated and not treated poor households within treated villages only. On each Panel, the line of best fit is shown (where observations are weighted by the size of treated villages).

Figure 3: 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 at the village-survey wave level and we report 95% confidence intervals.

Table A1: Attrition

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

	Treated Poor	Not Treated Poor	Not Poor
	(1)	(2)	(3)
Treatment 1: Asset Transfer			
One year	.048***	.066***	.081***
	(.008)	(.008)	(.010)
Two year	.040***	.002	.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	.061***	-.007	.090***
	(.009)	(.014)	(.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 at the village-survey wave level. 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 A2: Noticeable Impacts on Not Treated Poor and Not Poor Households, Pooled Specification

Between Village Estimates: Treatment vs Control

Standard errors clustered by village-survey wave

	Not Treated Poor					Not Poor			
	(1) Own Livestock	(2) Value Livestock | Own Livestock	(3) Iron Roof	(4) Often Consume Own Produced Milk	(5) Monthly Food Expenditure (AE)	(8) Own Livestock	(9) Value Livestock | Own Livestock	(10) Often Consume Own Produced Milk	(11) Monthly Food Expenditure (AE)
One year impact	-.020	.003	.065	-.006	-.012			.003	-.057
	(.039)	(.149)	(.051)	(.047)	(.049)			(.041)	(.036)
Two year impact	-.028	-.044		-.049	.022	-.056*	-.014	-.036	.070***
	(.033)	(.094)		(.043)	(.025)	(.031)	(.061)	(.028)	(.018)
Four year impact	-.007	-.110		-.026	-.038	-.030	-.064	-.005	-.025
	(.036)	(.094)		(.043)	(.035)	(.033)	(.058)	(.031)	(.024)
Mean (P, controls at baseline)	.563	2836	.360	.328	83.7	.638	4213	.421	98.7
p-values:									
One year = Two year	[.874]	[.785]		[.500]	[.528]	[.077]		[.428]	[.002]
Two year = Four year	[.662]	[.617]		[.698]	[.158]	[.572]	[.552]	[.473]	[.002]
One year = Four year	[.808]	[.522]		[.759]	[.673]	[.362]		[.862]	[.474]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	12325	6704	2666	12326	12220	17021	9317	22141	21744

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

Table A3: Correlates of Economic Preferences

Controls at midline (poor and non poor)
Standard errors in parentheses clustered by village

	Redistribution			Pro-Market Beliefs			Trust in Neighbors		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Poverty score (1-100)	.001	.001	.001	.002	.002*	.002	-.001	-.001	-.001
	(.001)	(.001)	(.002)	(.002)	(.001)	(.002)	(.001)	(.001)	(.001)
Household size	-.001	-.001	-.001	.009	.011*	.010*	.014*	.015**	.016**
	(.005)	(.005)	(.005)	(.006)	(.006)	(.006)	(.007)	(.006)	(.007)
Age of household head	-.001	-.001	-.001	-.000	-.001	-.000	.001	.001	.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Household head has no formal education	-.029	-.007	-.024	-.012	-.015	-.020	-.012	-.014	-.005
	(.033)	(.027)	(.033)	(.040)	(.031)	(.039)	(.036)	(.031)	(.037)
Household head is currently working	-.036	-.037	-.035	.077	.057	.071	.039	.051	.036
	(.040)	(.045)	(.040)	(.055)	(.050)	(.056)	(.046)	(.051)	(.047)
Village size			-.000			.000*			-.000**
			(.000)			(.000)			(.000)
Village inequality (sd of poverty scores)			.002			-.041			-.072***
			(.020)			(.027)			(.018)
Share of poor households in the village			-.752			1.26			-.234
			(.725)			(.932)			(.439)
Strata fixed effects	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Enumerator fixed effects	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Village fixed effects	No	Yes	No	No	Yes	No	No	Yes	No
Observations	5014	5014	5014	5014	5014	5014	5014	5014	5014

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 pool poor and non poor households in controls at midline. The regressions display OLS estimates, where the outcome variables are the three economic preferences indices: redistributive preferences (Columns 1 to 3), pro-market beliefs (Columns 4 to 6), and trust in neighbors (Columns 7 to 9). 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. Standard errors are clustered at the village level.

Table A4: Multiple Hypothesis Testing and Lee Bounds

OLS estimates, standard errors clustered by village-survey wave
95% confidence interval in brackets
q-values adjusted for multiple hypothesis testing in italics
Lee bounds in curly brackets

	Belongs to Right-type: less redistribution, more pro-market, higher trust in neighbors		
	Treated Poor	**Not Treated Poor**	**Not Poor**
	(1a)	**(1b)**	**(1c)**
A. Between Village Estimates (Treated vs Control)			
Two year impact	.089***	.085***	.099***
	(.023)	(.021)	(.021)
	[.045,.135]	[.044,.127]	[.058,.141]
	[.001]	*[.001]*	*[.001]*
	{.097***, .100***}	{.066***, .082***}	{.095***, .129***}
Four year impact	.022	-.013	.006
	(.027)	(.029)	(.025)
	[-.031,.074]	[-.071,.044]	[-.043,.055]
	[.494]	*[.764]*	*[.764]*
	{.020, .028}	{-.028*, .002}	{.008, .029*}
B. Within Village Estimates (Treated Poor vs Not Treated Poor)			
Two year impact	.002		
	(.011)		
	[-.021,.025]		
	[.764]		
	{.009, .021}		
Four year impact	.031**		
	(.013)		
	[.005,.056]		
	[.024]		
	{.018, .036**}		
Mean Outcome, Controls	.611		.580
Observations: Panel A	7800	8988	16278
Observations: Panel B	7910		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Column 1a), Not Treated Poor (Column 1b), and Not Poor (Column 1c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Column 1a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave level, 95% confidence intervals are reported in brackets, q-values adjusted for multiple hypothesis testing in italics, and Lee bounds to control for attrition in curly brackets. q-values are sharpened two-stage q-values [Benjamini et al. 2006]. The Lee bounds are estimated separately for each survey wave and do not include enumerator fixed effects. The outcome is a dummy indicating if the household head is assigned to the right-type preference cluster.

Table A5: Luck versus Merit

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	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?					
	Treated Poor	**Not Treated Poor**	**Not Poor**	**Treated Poor**	**Not Treated Poor**	**Not Poor**
	(1a)	**(1b)**	**(1c)**	**(2a)**	**(2b)**	**(2c)**
A. Between Village Estimates (Treated vs Control)						
Two year impact	-.079	-.036	-.057	-.064	-.052	-.010
	(.087)	(.087)	(.067)	(.109)	(.138)	(.096)
	[-.250,.093]	[-.208,.137]	[-.189,.075]	[-.280,.152]	[-.325,.221]	[-.200,.180]
Four year impact	.007	.014	-.016	.014	.024	.006
	(.029)	(.036)	(.030)	(.026)	(.034)	(.026)
	[-.050,.064]	[-.057,.086]	[-.074,.043]	[-.039,.066]	[-.043,.090]	[-.045,.056]
Two Year = Four Year	[.332]	[.587]	[.571]	[.485]	[.594]	[.874]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)						
Two year impact	-.034			-.001		
	(.034)			(.062)		
	[-.101,.033]			[-.123,.121]		
Four year impact	-.006			-.008		
	(.015)			(.013)		
	[-.037,.024]			[-.034,.017]		
Two Year = Four Year	[.461]			[.909]		
Mean Outcome, Controls	41.8%		37.8%	48.2%		40.7%
Observations: Panel A	4793	5725	10328	4536	5298	9479
Observations: Panel B	5118			4652		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Columns 1b, 2b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave level, and 95% confidence intervals are reported in brackets. In the "luck" scenario, the exact wording of the vignette is as follows: "Two people in your village, A & B, have been allocated PKR 5,000 and PKR 15,000 respectively based on a coin toss. The recipients know that they have been allocated PKR 5,000 and 15,000 respectively." In the "merit" scenario, the exact wording of the vignette is, "The initial allocation was based on the recipients score in a school test instead of a coin toss. The higher scorer was given the higher award and lower scorer was given the smaller award." In both cases, we report the answer to the question "Should the government forcefully reallocate the money?" At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention.

Table A6: Belief in Government

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	Should the rich give part of their income to the poor?			A 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?		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
A. Between Village Estimates (Treated vs Control)						
Two year impact | Government Ineffective	.013	.019*	.030**	.057	.028	.065**
	(.014)	(.011)	(.012)	(.035)	(.035)	(.031)
	[-.014,.041]	[-.003,.041]	[.006,.054]	[-.013,.127]	[-.041,.097]	[.033,.126]
Two year impact | Government Effective	.011	.022**	.033**	.067	.070	.087***
	(.013)	(.011)	(.014)	(.042)	(.044)	(.032)
	[-.015,.037]	[.000,.044]	[.004,.061]	[-.018,.151]	[-.017,.156]	[.024,.150]
Four year impact | Government Ineffective	.023**	.020*	.007	.028	.027	.032
	(.009)	(.011)	(.009)	(.039)	(.036)	(.038)
	[.004,.041]	[-.002,.041]	[-.010,.024]	[-.049,.105]	[-.045,.099]	[-.044,.107]
Four year impact | Government Effective	.000	.006	.002	.026	.053	.021
	(.013)	(.009)	(.009)	(.038)	(.041)	(.038)
	[-.025,.025]	[-.013,.024]	[-.017,.020]	[-.049,.101]	[-.028,.133]	[-.053,.094]
Two Year = Four Year | Government Ineffective	[.893]	[.787]	[.758]	[.819]	[.285]	[.435]
Two Year = Four Year | Government Effective	[.106]	[.199]	[.474]	[.951]	[.472]	[.765]
Mean in Controls | Government Ineffective	94.8%		93.9%	63.4%		66.9%
Mean in Controls | Government Effective	96.1%		93.7%	68.2%		67.1%
Observations	8126	9382	17004	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 in Panel A compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Columns 1b, 2b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave level, and 95% confidence intervals are reported in brackets. 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 A7: Social Mobility

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	On a ladder with 10 steps, what is the best life you can achieve?		
	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)
A. Between Village Estimates (Treated vs Control)			
Two year impact	-.035	-.055	-.193*
	(.117)	(.125)	(.113)
	[-.265,.196]	[-.301,.191]	[-.415,.030]
Four year impact	.171	.242**	.064
	(.120)	(.117)	(.103)
	[-.050,.392]	[.012,.473]	[-.140,.268]
Two Year = Four Year	[.194]	[.068]	[.086]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)			
Two year impact	.068		
	(.066)		
	[-.063,.200]		
Four year impact	-.024		
	(.055)		
	[-.133,.085]		
Two Year = Four Year	[.304]		
Mean Outcome, Controls	7.08		7.21
Observations: Panel A	8126	9382	17001
Observations: Panel B	8262		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Column 1a), Not Treated Poor (Column 1b), and Not Poor (Column 1c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Column 1a). All regressions include treatment dummies (pooling T1 and T2), district (strata), 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: Perception of Own Standing

OLS estimates, standard errors clustered by village-survey wave
95% confidence interval in brackets

	On a ladder with 10 steps, where do you currently stand?			Own rank in the income distribution has risen		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(2a)	(2b)	(2c)	(2a)	(2b)	(2c)
A. Between Village Estimates (Treated vs Control)						
Two year impact	-.119	-.206**	-.539***	.036	.011	.007
	(.107)	(.100)	(.101)	(.034)	(.036)	(.033)
	[-.330,.093]	[-.403,-.008]	[-.739,-.339]	[-.032,.103]	[-.061,.083]	[-.058,.072]
Four year impact	.050	-.048	-.126	.039	.056	.033
	(.124)	(.138)	(.117)	(.037)	(.039)	{.036}
	[-.196,.295]	[-.321,.225]	[-.358,.105]	[-.034,.113]	[-.020,.133]	[-.037,.104]
Two Year = Four Year	[.284]	[.327]	[.007]	[.942]	[.370]	[.581]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)						
Two year impact	.121***			.034*		
	(.043)			(.018)		
	[.036,.206]			[-.001,.070]		
Four year impact	.135***			-.007		
	(.049)			(.015)		
	[.037,.233]			[-.037,.023]		
Two Year = Four Year	[.833]			[.082]		
Mean Outcome, Controls	2.78		3.34			
Observations: Panel A	8126	9382	17001	8126	9382	17004
Observations: Panel B	8262			8262		

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Column 1a), Not Treated Poor (Column 1b), and Not Poor (Column 1c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Column 1a). All regressions include treatment dummies (pooling T1 and T2), district (strata), 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?" For the second, respondents were shown five figures of income distributions (see Panel A of Figure 3). They were asked where they see themselves in the distribution “today” and “three years ago.” Based on their answers, we construct a dummy variable indicating if the individuals perceived their rank in the distribution to have increased. 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: Pro-Market Beliefs Index Components

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	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?		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2b)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)
A. Between Village Estimates (Treated vs Control)												
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)
	[-.042,.070]	[-.054,.050]	[-.081,.028]	[.012,.108]	[-.006,.094]	[.011,.100]	[.017,.103]	[.020,.103]	[.023,.115]	[.014,.115]	[.043,.142]	[.032,.119]
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)
	[-.056,.063]	[-.040,.065]	[-.023,.107]	[-.059,.058]	[-.050,.072]	[-.056,.052]	[-.014,.066]	[-.025,.056]	[-.047,.046]	[-.090,.010]	[-.088,.012]	[-.070,.037]
Two Year = Four Year	[.503]	[.678]	[.108]	[.110]	[.392]	[.108]	[.267]	[.119]	[.035]	[.004]	[.000]	[.007]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)												
Two year impact	.020			.016			-.009			-.016		
	(.016)			(.014)			(.013)			(.016)		
	[-.013,.052]			[-.012,.045]			[-.034,.017]			[-.047,.016]		
Four year impact	-.022			-.011			.003			.008		
	(.016)			(.013)			(.013)			(.015)		
	[-.055,.010]			[-.036,.014]			[-.022,.028]			[-.022,.038]		
Two Year = Four Year	[.065]			[.157]			[.515]			[.274]		
Mean Outcome, Controls	51.7%		54.8%	66.4%		67.5%	78.5%		73.0%	42.9%		45.1%
Observations: Panel A	8126	9382	17004	8126	9382	17004	8126	9382	17004	8126	9382	17004
Observations: Panel B	8262			8262			8262			8262		

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

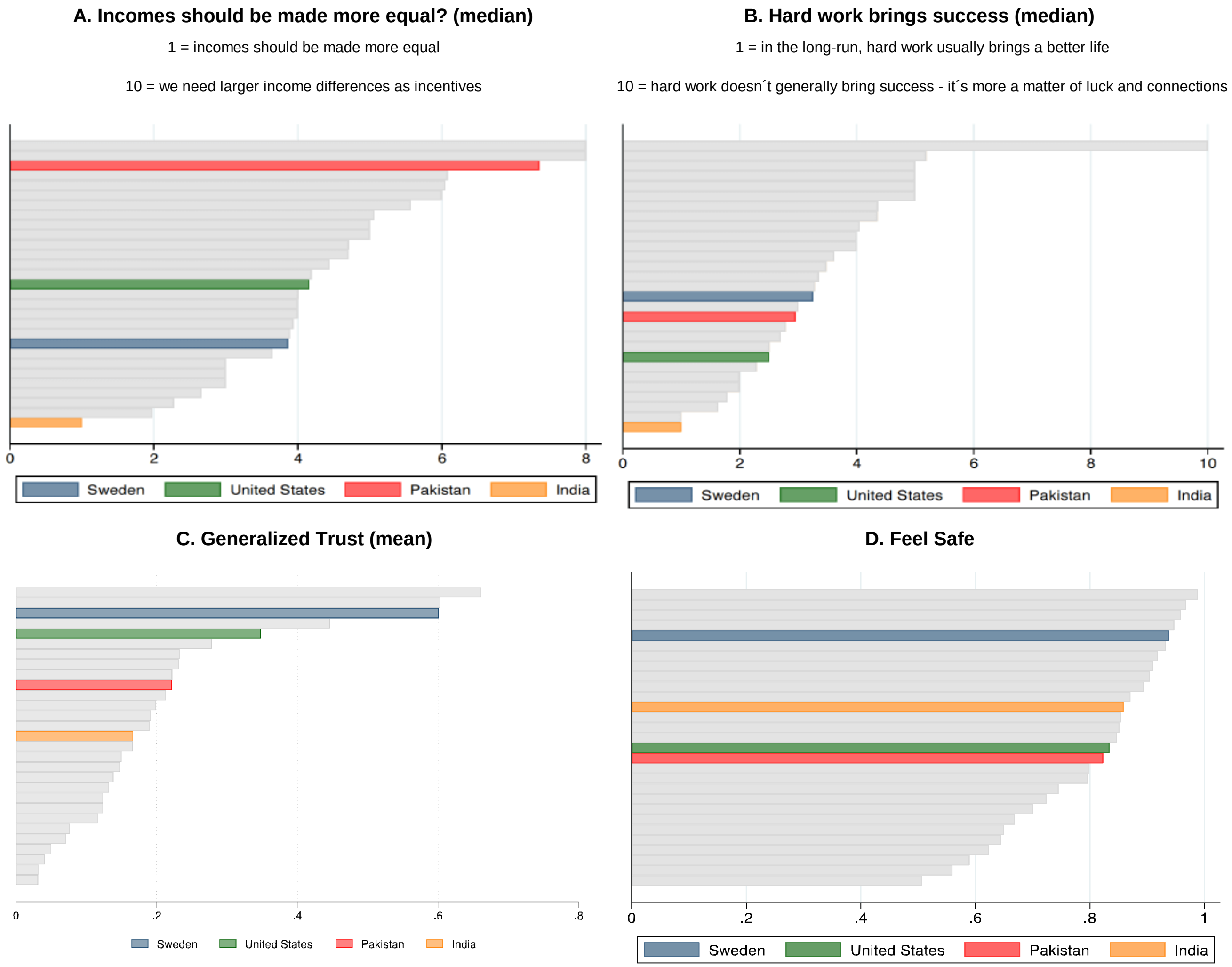
Table A10: Trust in Neighbors Index Components

OLS estimates, standard errors clustered by village-survey wave, 95% confidence interval in brackets

	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?		
	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2b)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)
A. Between Village Estimates (Treated vs Control)												
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)
	[-.014,.094]	[-.042,.074]	[-.038,.044]	[.013,.075]	[.007,.063]	[.028,.082]	[.007,.114]	[.008,.119]	[.041,.164]	[.015,.056]	[.017,.058]	[.021,.056]
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)
	[-.050,.072]	[-.066,.064]	[-.042,.053]	[-.046,.028]	[-.051,.021]	[-.032,.013]	[-.005,.094]	[-.051,.052]	[-.040,.081]	[-.001,.048]	[-.013,.040]	[-.015,.015]
Two Year = Four Year	[.467]	[.686]	[.947]	[.026]	[.028]	[.000]	[.664]	[.091]	[.055]	[.457]	[.162]	[.001]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)												
Two year impact	.026			.007			-.004			.001		
	(.016)			(.011)			(.013)			(.007)		
	[-.006,.058]			[-.015,.028]			[-.030,.022]			[-.013,.015]		
Four year impact	.013			.004			.041***			.013		
	(.014)			(.011)			(.012)			(.009)		
	[-.014,.041]			[-.018,.027]			[.017,.066]			[-.004,.030]		
Two Year = Four Year	[.564]			[.886]			[.010]			[.271]		
Mean Outcome, Controls	38.2%		38.7%	86.4%		83.9%	58.6%		51.7%	91.6%		92.3%
Observations: Panel A	8126	9382	17003	8126	9382	17003	8126	9382	17003	8126	9382	17003
Observations: Panel B	8262			8262			8262			8262		

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

Figure A1: Study Context, World Values Survey Data 2010-4



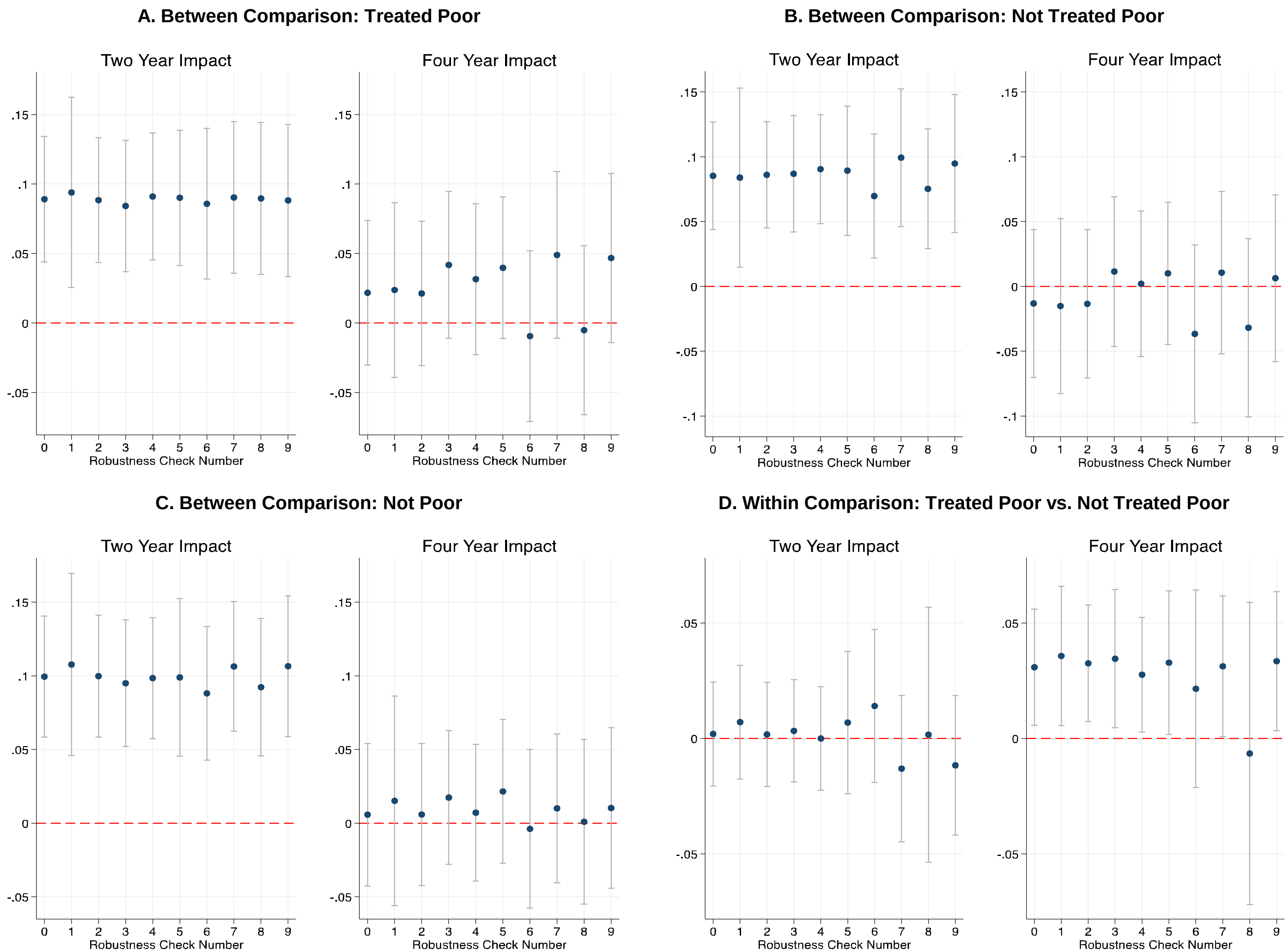
Notes: Each Panel shows responses from 29 countries from the World Values Survey 2010-14. Four are highlighted: Pakistan, India, Sweden and the United States. In Panel A on income inequality, the exact wording of the question is, "Now I'd like you to tell me your views on income inequality. How would you place your views on this scale? 1 means you agree completely with the statement ‘incomes should be made more equal’; 10 means you agree completely with the statement ‘we need larger income differences as incentives’; and if your views fall somewhere in between, you can choose any number in between." In Panel B on whether hard work brings success, the exact wording of the question is, “Now I'd like you to tell me your views on the importance of hard work for success. How would you place your views on this scale? 1 means you agree completely with the statement ‘in the long run hard work usually brings a better life’; 10 means you agree completely with the statement ‘hard work doesn´t generally bring success - it´s more a matter of luck and connections’; and if your views fall somewhere in between, you can choose any number in between." In Panel C on generalized trust, the exact wording of the question is, "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?" Respondents can answer either "most people can be trusted" or "need to be very careful." We display the share of individuals who answered "most people can be trusted." In Panel D on feeling safe, the exact wording of the question is, "Could you tell me how secure do you feel these days in your neighborhood?" Respondents can answer "very secure", "quite secure", "not very secure", and "not at all secure". We show the share of individuals who answered they feel "very secure" and "quite secure". Each figure plots responses for the following 29 countries: Azerbaijan, Argentina, Brazil, Chile, China, Colombia, Germany, Ghana, Haiti, India, Jordan, Mexico, Netherlands, Nigeria, Pakistan, Philippines, Poland, Romania, Russia, Rwanda, Slovenia, South Africa, Spain, Sweden, Trinidad and Tobago, Turkey, Ukraine, United States, and Uzbekistan.

Figure A2: 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 A3: Robustness Checks



Notes: Panel A (B) [C] {D} displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households] {within estimates for the treated poor and not treated poor households}. The baseline estimate from Table 7 is reported as Check 0. The other checks do the following: Check 1 removes enumerator fixed effects. Check 2 adds household controls from the census. Check 3 reports estimates when the cluster analysis is done using a k-means algorithm and imposing two clusters at midline and endline. Check 4 reports the results when the cluster analysis is done using k-medians but relying on the optimal number of clusters (that are found to be two at midline and three at endline). Check 5 reports results when the cluster analysis is done using the k-means algorithm but relying on the optimal number of clusters (that are found to be three at midline and two at endline). Check 6 estimates impacts of T1 alone. Check 7 estimates effects of T2 alone. Checks 8 and 9 separate the treatments into T1 and T2 again but include them jointly in the same regression specifications.