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Essays on Economic Development

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Declaration

I, Nicolas Cerkez, confirm that the work presented in my thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Nicolas Cerkez
August 8, 2024

Abstract

This thesis consists of three chapters on political and economic attitudes in developing countries.

Chapter 1 establishes a causal relationship between individuals' beliefs for what type of political system should govern their country and extreme weather events, such as droughts, in sub-Saharan Africa. The main finding is that exposure to a drought reduces the support for democracy. The chapter then explores the extent to which this weakening of democratic norms relates to exposure to non-democratic systems of governance, finding that the impact of droughts on the support for democracy only exists for individuals exposed to non-democratic systems of governance.

Chapters 2 and 3 evaluate a partial population experiment tracking 15,000 households in Pakistan, where villages are randomly assigned to receive an intervention in the form of an unconditional cash or asset transfer.

Chapter 2 goes beyond the evaluation of economic impacts to study whether households perceive economic changes and whether these changes shift redistributive attitudes. The chapter documents that impacts on perceptions are much more muted than the economic impacts of the intervention. The wedge between economic reality and perceptions then also means that redistributive attitudes remain inelastic to exposure to these interventions.

Chapter 3 analyzes the aggregate impacts of the intervention in two ways. First, it studies how the transfers affect the supply of providers of various services at the village-level. Second, it analyzes how the transfers shape pro-market beliefs. At the village-level, it documents limited effects on the supply of providers. At the individual-level, it shows that both transfers alike positively impact the pro-market beliefs of beneficiaries and non-beneficiaries two years post intervention. These effects, however, fade out four years post intervention.

Impact Statement

This thesis examines how political and economic attitudes shift in response to shocks and policies in developing countries. By providing causal estimates and uncovering new mechanisms, the dissertation improves our understanding of the determinants of individuals' attitudes and informs academic and public debate.

Chapter 1 studies the relationship between climate change and the erosion of democratic norms, two of the most important and urgent global challenges. Existing research shows that democracies are more successful at dealing with climate change. The chapter thus documents the threat of a vicious cycle where climate change and eroding democratic norms negatively reinforce one another.

By highlighting that exposure to non-democratic systems of governance— measured by the exposure to development projects funded by technocrats (World Bank) and autocrats (China)—acts as a channel in explaining the main finding, the chapter highlights potential perils of development aid. In other words, there is a "catch 22" as combating droughts and associated climate change requires foreign funding but simultaneously this funding, interacted with droughts, erodes democracy.

Chapters 2 and 3 speak to the effects of randomized interventions in developing countries. Social assistance programs, such as cash or asset transfers, have become increasingly popular in the developing world. For example, unconditional cash transfer programs have been implemented in 119 low-income countries. There is a large literature documenting the economic benefits accruing to beneficiaries of these interventions. Much less is known, however, (a) about non-economic effects and (b) about aggregate impacts of such interventions.

Chapter 2 shows that even in small village economies, the experience or demonstration of welfare enhancing big push anti-poverty policies is unlikely to alter households' perceptions of economic outcomes or for them to become advocates for such interventions. This is relevant as it implies that welfare enhancing and cost effective interventions do not start a causal chain of demand for good policies.

The results in both chapters also have important implications for studying the general equilibrium effects of randomized interventions. For example, the attitudes affected by the intervention are shifted in the same direction for beneficiaries and non-beneficiaries. This suggests that attitudes are shaped by village-wide exposure to anti-poverty programs (and not individual-level benefits of a particular program). Furthermore, attitudes are only affected in the short-run. In the long-run, four years after the intervention occurred, all effects are null. This highlights the importance of studying dynamic effects when evaluating such interventions.

Overall, this thesis enriches academic debates on determinants of individuals' political and economic attitudes in developing countries while simultaneously contributing to policy-relevant debates.

Research Paper Declaration Form

- **For multi-authored work, please give a statement of contribution covering all authors:**
  All authors contributed to shaping the research question.
  Cerkez and Rasul took the lead in conducting the analysis and writing the manuscript. Khan provided inputs on various drafts of the manuscript.
- **In which chapter(s) of your thesis can this material be found?**
  Chapter 2
  Title: Big Push Pro-poor Policies and Economic Circumstances: Reality, Perceptions and Attitudes
  Authors: Nicolas Cerkez, Adnan Q.Khan, Imran Rasul, and Anam Shoaib
- **For multi-authored work, please give a statement of contribution covering all authors:**
  All authors contributed to shaping the research question.
  Cerkez took the lead in conducting the analysis and writing the manuscript, with inputs from Rasul and Khan.
- **In which chapter(s) of your thesis can this material be found?**
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  Title: The Aggregate Impacts of Big Push Pro-poor Policies
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Chapter 1

Extreme Weather Events and the Support for Democracy

1.1 Introduction

How do people form beliefs about the political governance system that they want to live in? Given that populist governments are gaining increasing traction globally, while support for democracy is falling and political polarization is rising (Guriev and Papaioannou, 2022), this question is once again at the forefront of the research frontier.

A prominent hypothesis, the "modernization theory," argues that economic development (in the form of higher incomes and more education), pushes a country towards democracy (e.g., Lipset, 1959; Huber et al., 1993) and that higher levels of development reduce the likelihood of democratic reversal (e.g., Lipset, 1959; Przeworski and Limongi, 1997). Other scholars argue that economic downturns contribute to democratization (e.g., Haggard and Kaufman, 1995; Acemoglu and Robinson, 2001).[1] Empirically, there is support for both views. For example, Barro (1999) argues that higher standards of living are associated with higher levels of democracy.[2] On the other side, Brückner and Ciccone (2011) provide empirical

[1]There are other theories of democratization, e.g., the "conditional modernization theory" (Treisman, 2020).

[2]Acemoglu et al. (2008) show that once one accounts for unobserved country-level characteris-

support that recessions can lead to "democratic windows of opportunity."[3] This literature evidently addresses the initial question from a "macroeconomic perspective," i.e., it focuses on regime changes. There is a much smaller literature taking a "microeconomic perspective," i.e., focusing on individual beliefs. This literature, analyzing the determinants of the support for democracy, argues that this support is acquired by experiences with democracy over time (e.g., Fuchs-Schündeln and Schündeln, 2015; Claassen, 2020b; Acemoglu et al., 2021; Tabellini and Magistretti, 2022).[4]

This paper extends this latter literature by establishing a relationship between individuals' beliefs for what type of political system should govern their country and climate change. Climate change is one of the most urgent policy challenges worldwide. Anthropogenic climate change has increased temperatures by 1.3 degrees Celsius from 1900 to 2010, affecting the frequency and severity of extreme weather events, such as droughts or floods (IPCC, 2021).

The theory of change motivating the idea of the paper is straightforward. There is extensive evidence documenting that weather shocks have large economic impacts on people's lives (e.g., Dell et al., 2014; Carleton and Hsiang, 2016). These changes in individuals' economic conditions may affect people's beliefs about the political governance system that they want in their country. Put differently, climate shocks affect exactly the economic circumstances that the literature has suggested lead to "democratization" (e.g., Lipset, 1959; Acemoglu and Robinson, 2001).

The paper empirically investigates this relationship in sub-Saharan Africa (SSA). SSA provides an interesting empirical setting for studying this relationship. The region is particularly vulnerable to climate change and is already experiencing large negative economic impacts as a consequence (e.g., IDA, 2021). In addition, the slowing rate at which democracy has been adopted in SSA since 2000 coupled

tics (country-level fixed effects) in these types of studies, there is no causal relationship between income and democracy.

[3]The robustness of this finding has been questioned by Barron et al. (2014).

[4]This is related to the notion of "democratic capital", introduced by Persson and Tabellini (2009), who argue that a nation's historical experience with democracy reduces the probability that it exits from democracy.

with the population's ambivalence towards democracy, raises the possibility that climate change influences the support for democracy.

To measure individuals' support for democracy, I use geolocalized data from five rounds of the Afrobarometer surveys in 16 SSA countries for the period 2002-2015. My main outcome is a dummy indicating whether individuals support democracies or are open to non-democratic systems. Across all countries and survey waves, 68.2% of respondents support democracy.

I proxy climate change by using a long-term measure of droughts: the Standardized Precipitation Evapotranspiration Index (SPEI) developed by Vicente-Serrano et al. (2010). The proxy is based on the scientific consensus that the frequency and intensity of natural disasters is amplified by anthropogenic climate change (IPCC, 2021). The SPEI index is a standardized and continuous drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. The index therefore captures both droughts and floods.[5]

To identify the effect of droughts on the support for democracy, I regress the support for democracy on the drought index, controlling for grid cell and month-by-year of the survey interview fixed effects and various household-level characteristics.

In the first part of the paper, I establish a robust relationship between extreme weather events and the support for democracy. My baseline finding is that a drought reduces the support for democracy by 2.56% to 5.28%. I further show that droughts reduce individuals' trust in government and institutions. The effects on the support for democracy and trust only persist in democracies. In autocracies, there are no effects of droughts on political beliefs.

[5]The key idea behind the SPEI index is that the impact of precipitation on agriculture depends not only on the level of precipitation, but also on the soil's ability to retain water. This ability is a function of a variety of other weather inputs, such as temperature, sunshine exposure, latitude, wind speed, and pressure. The SPEI index incorporates all of these inputs and outperforms other indices used to predict crop yields (Vicente-Serrano et al., 2012). The SPEI index is calculated using weather data at the grid cell level with monthly frequency from 1960 to 2015 and is expressed in units of standard deviations from the historical mean. In my sample, the mean (standard deviation) of the SPEI index is 0.475 (0.785), indicating that my sample period is drier than the historical period.

Democracy is a multi-dimensional concept, meaning different things to different people both across and within countries. The overwhelming majority of my sample (43.5%) associate democracy with personal freedom, followed by 10.2% who associate democracy with voting, and 9.90% who associate it with the idea of government by and for the people. Only 3.90% of respondents associate economic development with democracy.

I show that in response to droughts, respondents are more likely to want one man rule (i.e., to want a dictator) and one party rule (i.e., to abolish parliament and elections). Since elections, a parliament, and a leader/president with some constraints on their power are cornerstones of a democracy, the respondents' answers indicate that they want a consolidation of power in their country's politics in response to a drought. Given that droughts reduce democracy and given that 43.5% of the sample associate democracy with personal freedoms, it is not surprising that droughts also reduce the freedom of speech, the freedom to join any organization, and the freedom to vote. To interpret this result, I rely on the results just discussed on the desire for more consolidation of power within a country's politics. A logical continuation of this result is a loss in freedoms (e.g., if a country abolishes elections, there is no more freedom to vote). A way to interpret the "freedom findings" is therefore that individuals deliberately give up some freedom in exchange for a less democratic country if, for example, they believe that "less democracy" is better at dealing with climate change.

My findings hold for a variety of robustness checks. My estimations rely on three primary assumptions: (a) the exogeneity of the drought index, (b) homogeneous treatment effects (e.g., De Chaisemartin and d'Haultfoeuille, 2022b; Roth et al., 2023), and (c) no selected sample. The first assumption assumes that the weather is random conditional on geography and time fixed effects. The fact that the weather is random (within a place and time) has been a long-established result in the literature. The second assumption assumes that the treatment effect is constant across all 16 countries and five survey waves. I show that my results are robust to allowing for heterogeneous treatment effects. The assumption of no selected sample refers to the possibility that: (i) natural disasters can affect the roll out of the Afrobarometer surveys, (ii) conditional on the roll out of the surveys, the Afrobarometer interviews different "types" of individuals, and (iii) individuals exhibit adaptation behavior (e.g., they migrate) due to natural disasters and thus change the composition of the sample. I show that these considerations do not represent concerns in my analysis.

In the second part of the paper, I dive into the question of what channel is driving the main result. The theory of change proposed in the beginning of this paper emphasizes a channel via income based on prominent theoretical papers such as Lipset (1959) or Acemoglu and Robinson (2001). However, the list of possible channels driving the baseline result is more likely to be (almost) endless. For example, it is widely known that weather variations affect conflict, which may very well in turn affect respondent's support for democracy. My aim is therefore not to identify *the* channel driving the results but to parse out *a* channel driving the result.

I start by showing that my baseline finding from the first part of the paper is homogeneous across a wide range of dimensions: for example, the impact is the same for poor and rich individuals, for those exposed and not exposed to conflict, and for those with differing levels of education. The lack of heterogeneous effects of droughts on the support for democracy across these dimensions motivates me to focus on another channel altogether.

Specifically, I inquire whether the exposure to non-democratic systems is a channel driving the results. I proxy this exposure by exposure to development projects funded by the World Bank and China, the former being technocratic and the latter being autocratic.

The motivation for focusing on this channel comes from a growing, albeit very inconclusive, literature evaluating the relationship between the presence of foreign aid and political attitudes. For example, Bai et al. (2022) show that Chinese infrastructure aid significantly increases positive attitudes towards the government in the region where the aid was implemented. While Eichenauer et al.

(2021) and Blair et al. (2022) find no evidence that exposure to Chinese development projects in, respectively, Latin America and Africa increases attitudes towards China, Wellner et al. (2022) show that exposure to Chinese development projects can increase the support for China. Most closely related to this paper, Freytag et al. (2024) show that exposure to Chinese development aid in Latin America is associated with an increase in democratic values.

The hypothesis I test is whether the interaction of a drought and the exposure to a development project is the driving force that explains the observed reduction in the support for democracy. In other words, the mechanism underlying this channel is not (for example) an economic one, i.e., it is not an economic benefit (or lack thereof) of a development project that is relevant in mediating the impact of a drought. I argue that the exposure to these non-democratic systems per se is relevant in explaining the variation in the support for democracy when interacted with the exposure to a drought.

I find that respondents exposed to non-democratic systems of governance experience a decrease of 3.03% to 5.59% in their support for democracy after a drought. In contrast, the relationship between droughts and the support for democracy disappears for individuals not exposed to non-democratic systems of governance.

Because development projects are unlikely to be randomly allocated throughout SSA, likely targeting areas with particular characteristics (like poorer areas), one might worry that my results conflate other mechanisms. Examples include exposure to conflict or the income/wealth, health or education levels of the local population.

I implement three main tests to mitigate this concern. First, I test whether droughts impact the support for democracy for respondents who are not exposed to development projects at the time of the interview but who will be exposed to them in the future. I find no support for this. Areas that receive a project only after experiencing a drought do not exhibit any relationship between climate change and the support for democracy. In addition, if projects target certain types of areas, and certain characteristics of these areas drive the overall results, the drought index in these areas with these future development projects would display significant effects. Therefore, this test rules out local conditions as a potential mechanism.

Second, I rely on a doughnut design. The premise of this idea is that if the exposure to alternatives to democracy (i.e., the presence of official development assistance (ODA)) is orthogonal to some x, then this x cannot be a mechanism because the relationship between climate change and the support for democracy only exists for individuals exposed to alternatives to democracy. This simple insight rules out a whole range of possible mechanisms. To assess this empirically, I show that development projects correlate with various potential mechanisms, such as employment/income, in a radius of at most 10km around the development project. Thereafter, the presence of the development projects no longer correlates with local conditions. Replicating the main result while excluding individuals who live within a 10km radius of a development project therefore serves as a test whether I am conflating these potential mechanisms and exposure to non-democratic systems as mechanisms. I find no support for this, thus providing further evidence that local conditions do not act as potential confounders.

Third, I show that the results are not driven by development projects in particular sectors. This is further evidence that development projects do indeed act as proxies for exposure to non-democratic systems of governance and are not capturing a particular need of some people which may be driving the result.

Taken together, the evidence presented suggests that these development projects do indeed proxy exposure to non-democratic systems of governance and are not conflating other potential mechanisms.[6]

Finally, in the third part of the paper I provide suggestive evidence that the reduction in the support for democracy is associated with a reduction in riots and conflict events more broadly. While droughts significantly increase conflict in gen-

[6]A caveat relevant to the results in the second part of the paper is that the results presented could be either due to "cultural transmission" (exposure to some non-local population) or "propaganda" (money and media exposure). I leave the question which of the two channels is the underlying force driving my results open for future research.

eral, this effect becomes insignificant for individuals exposed to Chinese or World Bank projects. This is in line with the findings in Gehring et al. (2022) who show that Chinese and World Bank development projects reduce conflict occurrences and increase stability. For riots the relationship is even more extreme as individuals exposed to development projects are less likely to partake in riots in responses to droughts. This goes against the idea in Acemoglu and Robinson (2001) who argue that individuals may be more likely to protest to advance democracy ("threaten revolution") when the opportunity cost is low, which is likely the case during a drought (recession).

The paper contributes to various strands of the literature. Most closely, this paper relates to the "microeconomic literature" analyzing the determinants of the support for democracy (Fuchs-Schündeln and Schündeln, 2015; Claassen, 2020b; Acemoglu et al., 2021; Tabellini and Magistretti, 2022).[7] I contribute to this literature in two ways. First, I consider a new determinant for the support for democracy: climate change. Second, when thinking about the channel that drives this relationship, I provide evidence that channels other than the "obvious income mechanism" are (also) important in understanding the relationship between the climate and the support for democracy. This latter point is crucial in that it highlights (a) how only focusing on income as a mechanism can miss parts of the story and (b) how identifying the effects of weather shocks on the political variables via a 2SLS design can be misleading (Brückner and Ciccone, 2011).

More broadly, the paper contributes to a literature linking weather shocks to political outcomes. The most widely studied outcomes are voting outcomes (e.g., Malhotra and Kuo, 2008; Healy and Malhotra, 2009; Healy et al., 2010; Cole et al., 2012; Amirapu et al., 2022), though some papers do look at trust in government (e.g., Alfano and Aboyadana, 2020; Balcazar and Kennard, 2022) or even social capital/cultural persistence (e.g., Buggle and Durante, 2021; Giuliano and Nunn,

[7] Canonical theories of democratization in political science hinge on the support for democracy within the population (e.g., Lipset, 1959; Almond and Verba, 1963; Easton, 1965), thus emphasizing the importance of studying the "microeconomic perspective." For empirical evidence, see Claassen (2020a).

2021). I contribute to this literature by analyzing a new type of outcome: the support for democracy. This is important because (a) democratic norms around the world have been eroding and (b) electoral data in developing countries can be inaccurate and beliefs can signal future votes, providing useful information for policy makers. Furthermore, most of this literature, especially in developing countries, argues that the main mechanism is one through income or agricultural productivity (Cole et al., 2012; Amirapu et al., 2022). In contrast, my results highlight a different channel that does not operate via income or respondents' economic circumstances more broadly.

Finally, the paper relates to a large literature analyzing the drivers of people's political beliefs.[8] In particular, my paper builds on the strand in this literature looking at how exposure to foreign influences drives political outcomes (e.g.,. Meyersson et al., 2008). The emergence of China as an important global player has led to a growing literature studying the effects of Chinese foreign aid. Researchers have studied the impact of Chinese aid on (i) the behavior of traditional lenders such as the World Bank (Hernandez, 2017; Humphrey and Michaelowa, 2019; Zeitz, 2021; Watkins, 2022; Kern et al., 2024), (ii) economic and political outcomes (Isaksson and Kotsadam, 2018a,b; Bluhm et al., 2018; Dreher et al., 2019; Martorano et al., 2020; Dreher et al., 2021; Mueller, 2022), and (iii) political beliefs (Kleinberg and Fordham, 2010; Hanusch, 2012; Eichenauer et al., 2021; Bai et al., 2022; Blair et al., 2022; Wellner et al., 2022; Freytag et al., 2024). I contribute to this third literature in two ways. First, I show that political characteristics of aid donors, interacted with climate change, are important determinants of the beliefs about democracy in SSA, highlighting effects of foreign aid not studied yet. Second, by showing that climate change interacted with foreign aid reduces the support for democracy, I add a new negative externality to the list of potential concerns associated with the effects of foreign aid. Importantly, I provide evidence that this negative impact of climate change on the support for democracy

[8] For an overview looking at the burgeoning literature analyzing people's understanding of economic policies, see Stantcheva (2023). For an example related to climate change policies, see Dechezleprêtre et al. (2022).

for individuals exposed to development projects does not occur because of some economic effect of the project. Instead, the negative externality occurs because of the presence of this non-democratic system of governance itself. This is a type of externality the literature has not yet considered.

The rest of the paper is organized as follows. Section 1.2 describes the data. Section 1.3 establishes a robust relationship between extreme weather events and the support for democracy and presents all the robustness checks. Section 1.4 discusses the exposure to non-democratic systems of governance as the main mechanism. Section 1.5 demonstrates that the documented effects on beliefs in previous sections translate into effects on tangible outcomes, with a particular focus on conflict. Section 1.6 concludes and offers new avenues for future work.

1.2 Data

Afrobarometer data. To measure the support for democracy across SSA, I rely on the Afrobarometer surveys. These nationally representative surveys, conducted approximately every three years in a variety of African countries, contain a plethora of information regarding Africans' political preferences, social capital, economic conditions, as well as other topics. In each country-survey wave, interviews are conducted in the local language with a (random) sample of either 1,200 or 2,400 individuals.

This paper uses geocoded data from 16 SSA countries that were surveyed in all rounds from round 2 to round 6 (2002—2015), providing me with a sample of 129,002 individuals, representing 51.7% of the SSA population.[9] I match the locations of individuals to weather grid cells, which are described in more detail

[9] The countries are Botswana, Cape Verde, Ghana, Kenya, Lesotho, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda, Zambia, and Zimbabwe. The reason for restricting the sample to 16 countries is that they are the only ones surveyed in all five survey rounds.

below.[10,11]

The precise question respondents were presented with is "Which of these three statements is closest to your own opinion? A: Democracy is preferable to any other kind of government. B: In some circumstances, a non-democratic government can be preferable. C: For someone like me, it doesn't matter what kind of government we have." I use this question to code two different versions of the outcome used in this paper. First—coding 1—I create a dummy variable that equals 1 if participants answer "A" (i.e., they support democracy) and 0 if they answer "B" (i.e., they are open to non-democratic regimes). Second—coding 2—I create a dummy variable that equals 1 if participants answer "A" (i.e., they support democracy) and 0 if they answer "B" or "C" (i.e., they are open to non-democratic regimes or indifferent) or "don't know".[12]

The first row in Panel A of Table 1.1 displays the share of individuals who support democracy, showing that 85.9% of individuals support democracy across my full sample (Column 1) and that this share does not vary much across different regions in Africa (Columns 2—4). To delve into the geographical distribution of this

[10]At the time of writing (May 7, 2024), only survey rounds 1 through 6 have been geocoded. Since the wording of questions in survey round 1 differs substantially from that in other rounds, I exclude that round. Furthermore, in round 2, I lose 797 observations in Senegal as the date of those interviews is not known.

[11]Geocoded Afrobarometer surveys provide researchers with the location of an "Enumeration Area" (EA), i.e., the primary sampling unit (PSU). The precision of this PSU depends on the size of the EA, which varies between different population densities, but usually represents a village (or a several geographically close villages) or a neighborhood in an urban area. Each geocoded location is associated with a precision code ranging from 1 (most precise) to 8 (least precise). 98.46% of observations have precision codes between 1 and 4. As this is pretty much the complete sample (except for 1,986 observations), I keep the full sample in my main analysis. All results presented in this paper are robust to restricting the sample to precision codes 1 through 4. For more information on the process of geocoding the Afrobarometer data, see BenYishay et al. (2017).

[12]This question is unique in that it asks respondents directly about their belief whether democratic or non-democratic regimes are better. It does, to my knowledge, not exist in this form in any other survey. It is most closely related to the "democracy better" variable used in Tabellini and Magistretti (2022), which asks respondents to agree or disagree with the statement "Democracy may have problems but it's better than any other form of government." While similar in spirit, there is a subtle difference between the two questions. The Afrobarometer neutrally presents respondents with two alternatives, namely democratic or non-democratic regimes. It does not imply that one is better than the other. The "democracy better" variable suggests that democracy is flawed and then asks individuals to agree or disagree with this statement. This suggestion that democracy is flawed can influence respondents' answers. The Afrobarometer thus presents a unique opportunity to analyze respondents' answers to a simple straightforward question about their support for democracy.

support for democracy in more detail, Panels A and B of Figure 1.1 plots that same share at the state level for survey rounds 2 and 6 separately. While the overall support for democracy is quite high throughout, there is some variation that suggests, for example, that landlocked regions in southern and eastern Africa exhibit higher support for democracy than non-landlocked regions. All these shares are conditional on not picking option "C" (i.e., coding 1 of the outcome) or answering "don't know." As Panel A in Table 1.1 shows, 20.6% of respondents choose option "C" or answer "don't know." It follows that 68.2% of the sample support democracy unconditionally (as shown in the second row of Panel A in Table 1.1).

I conduct my main analysis relying on coding 1 of the outcome. I view this as the most conservative approach, as it relies on individuals who display strict preferences over which alternative is better. Indifferent individuals, or individuals who may not have views on political systems at all, are therefore excluded from the analysis. I show that the main result of the paper also holds for coding 2 of the outcome.

Democracy can, and likely does, mean different things to different people both across and within countries. Understanding what respondents perceive democracy to be is therefore important. To this end, Panel B of Table 1.1 displays the four answers to a question in the Afrobarometer asking individuals "what does democracy mean to you?". They are (i) personal freedoms (43.5%), (ii) government for and by the people (9.99%), (iii) voting (10.2%), and (iv) economic development (3.90%). Two facts are worth highlighting: (i) individuals seem to hold an overwhelmingly positive view of democracy and (ii) close to no one associates democracy with economic development. The second point suggests that in this context income/economic development may not serve as a mechanism when considering the relationship between droughts and the support for democracy, something I will return to multiple times throughout the paper.

Panels C and D in Table 1.1 and Table 1.2 provide further summary statistics for various political variables. First, Panel C of Table 1.1 displays variables relating to personal freedom, showing that 76.9%, 81.8%, and 84.3% of respondents perceive that they are free to, respectively, speak their mind, join any political organization, and vote. Second, Panel D of Table 1.1 provides the shares of respondents who do not support one-party rule, army rule, and one-man rule (i.e., abolishing parliament and elections). Table 1.2 displays three groups of variables relating to trust in government, the capabilities of the government, and trust in institutions. All these measures show that around half of the respondents trust the government, its institutions, and/or view it to be capable in providing various services. Finally, Panel D in Table 1.2 displays the share of respondents who believe their country to be a full democracy (23.2%), a democracy with minor problems (37.8%), a democracy with major problems (31.2%), or not a democracy (7.7%). For each group of the variables displayed in Panels C and D in Table 1.1 and Panels A, B, and C of Table 1.2, I also construct an index by (a) averaging the dummy variables in each category and (b) standardizing this measure. For the variables in Panel D of Table 1.2, I create a variable ranging from 1 to 4, where 1 indicates "not a democracy" and 4 indicates "full democracy."[13]

To validate the responses in the survey, Table 1.A3 presents OLS regressions of coding 1 of my main outcome variable on the above-mentioned household characteristics. The table shows that older respondents, respondents who completed at least high school, male respondents, black respondents, religious respondents, and respondents who are politically aligned with the party in power are more likely to support democracy. The respondent's employment status does not correlate significantly with the support for democracy and being white and having an occupation that is affected by climate change correlates negatively with the support for democracy.

Table 1.A4 provides further validation of the responses in the Afrobarometer. The table regresses various answers from the Afrobarometer on the polity score.[14] Column 1, relying on coding 1 of the support for democracy, shows that there is no

[13]The Afrobarometer also contains a battery of individual- and village-level characteristics that can be used as controls and which I've summarized in Tables 1.A1 and 1.A2, respectively.

[14]The polity2 measurement comes from the Polity5 project. This index, widely used in the literature (e.g., Burke and Leigh, 2010; Fuchs-Schündeln and Schündeln, 2015; Besley and Persson, 2019; Tabellini and Magistretti, 2022), ranges from -10 (autocracy) to $+10$ (democracy).

correlation between this outcome and the "true" level of democracy in a country. Column 2 uses the variable ranging from 1 to 4 measuring how democratic people think their country is (see Panel D in Table 1.2) to show that people who live in a more democratic country view their country as more democratic. The outcome in Column 3 (4) [5] {6} is an index created from variables in Panel A of Table 1.2 (Panel C of Table 1.2) [Panel D of Table 1.2] {Panel C of Table 1.1}, as described above. As can be seen, individuals living in more democratic countries display higher trust in government and institutions, view their government as more capable, and believe themselves to be more free. The directions of the significant correlations found in Columns 2—6 validate the Afrobarometer data. The fact that my main outcome in Column 1 is not correlated with the level of democracy in a country is not necessarily surprising. The outcome I am studying is distinct from other, more standard, political outcomes studied (such as the ones in Columns 2—6), i.e., my outcome measures an individual's belief about the "optimal system." There is a priori no reason to believe that such a belief is systematically correlated with the level of democracy in a country.

To show that my main outcome is distinct from other political beliefs, Figure 1.A1 looks at the raw correlations between the support for democracy and a set of other political beliefs found in the Afrobarometer surveys, using only data from the latest survey round. The correlations highlighted in yellow are the correlations of interest, i.e., the ones between the support for democracy and other political beliefs, while the correlations highlighted in orange represent the correlations amongst the other political beliefs. The figure clearly highlights that the correlations between political beliefs other than the support for democracy are much higher than the correlation between the support for democracy and these political beliefs. For example, the correlation between trust in the president and trust in parliament is 0.575, while the correlation between the support for democracy and these two beliefs is 0.073 and 0.054, respectively. If the support for democracy were capturing individuals' view of the government instead of their support for democracy, the correlations in the yellow part of the figure should be higher.

Weather data. As measuring climate change is inherently difficult, my focus here is on droughts. The rationale behind this is based on the scientific consensus that the frequency and intensity of natural disasters is amplified by anthropogenic climate change (IPCC, 2021).[15] A drought is a "temporal anomaly characterized by a deficit of water compared with long-term conditions" (Peng et al., 2020) that can be grouped into one of five types: meteorological (precipitation deficiency), agricultural (soil moisture deficiency), hydrological (runoff and/or groundwater deficiency), socioeconomic (social response to water supply and demand) and environmental or ecological (Mishra and Singh, 2010; Peng et al., 2020).

To identify droughts, or drought-like conditions, my main right-hand side variable is the SPEI index, developed by Vicente-Serrano et al. (2010).[16] The SPEI index is a standardized and continuous drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. More specifically, the impact of precipitation on agriculture not only depends on the level of precipitation, but also on potential evapotranspiration (PTE),[17] i.e., the soil's ability to retain water. PTE is a function of a variety of other weather inputs such as temperature, sunshine exposure, latitude, wind speed, and pressure. The SPEI index incorporates all of these components and has been found to outperform other indices in predicting crop yields (Vicente-Serrano et al., 2012).[18,19]

I rely on the daily ERA5 reanalysis dataset from the European Center for Medium-Range Weather Forecasts for the weather inputs to calculate the SPEI index, down-

[15] Examples of work looking at political outcomes include papers analyzing the effects of tornadoes (e.g., Healy et al., 2010), hurricanes (e.g., Malhotra and Kuo, 2008; Fitch-Fleischmann and Kresch, 2021), droughts (e.g., Tarquinio, 2022), earthquakes (e.g., Klomp, 2020; Pathak and Schündeln, 2022), or floods (e.g., Besley and Burgess, 2002; Cole et al., 2012; Kosec and Mo, 2017; Neugart and Rode, 2021).

[16] To ease the interpretation of my results, I multiply the final index by -1.

[17] PTE is the amount of evaporation that would occur if a sufficient water source were available.

[18] Two of these other indices are the Palmer Drought Severity Index (PDSI) (Palmer, 1965) and the Standardized Precipitation Index (SPI) (McKee et al., 1993). For more information on drought indices, see Mishra and Singh (2010). The details for the calculation of the SPEI index can be found in Vicente-Serrano et al. (2010) and can simply be executed in R using the package "SPEI".

[19] In terms of droughts, climate change has two implications: (i) a decrease in precipitation and (ii) an increase in temperature, which in turn causes an increase in the evapotranspiration rate. The SPEI is therefore "particularly suited to [detect, monitor, and explore] the consequences of global warming on drought conditions" (Vicente-Serrano et al., 2010, p. 1698)).

loading the data from 1960 until 2015 for a 0.25×0.25 degree (≈ 27×27km) grid spanning the world.[20]

The SPEI index is calculated for each grid cell-month and is expressed in units of standard deviations from the grid cell's historical mean. By construction it therefore has mean (standard deviation) 0 (1) in the historical sample, which in my case is 1960-2015. In my sample, the mean (standard deviation) of the SPEI index is 0.475 (0.785), indicating that my sample period (2002-2015) was both drier and exhibited less variability than the historical period.

Present drought conditions are not only a function of current weather conditions but also of past periods. The SPEI index can therefore be constructed over different timescales. This paper relies on the 12 months SPEI index which reflects long-run climatic conditions. The reasons for this choice are twofold. First, given my interest in the effects of climate change (i.e., a long-run event), it is imperative to focus on a SPEI index capturing long-run deviations from the historical mean. Second, individuals' recollection period is not infinite. As such, while I could compute the SPEI index for any other months, limiting the "recall period" is important. I choose 12 months in my main specification.

Notwithstanding its continuous nature, researchers have categorized the index. Values above 2.00 are classified as being "extremely wet", values between 1.50 and 1.99 are "very wet", values between 1.00 and 1.49 are "moderately wet", values between -0.99 and 0.99 are "near normal", values between -1.00 and -1.49 are "moderately dry", values between -1.50 and -1.99 are "severely dry", and values below -2.00 are "extremely dry". Throughout the paper, I sometimes define extreme weather event dummies. Extreme droughts or floods are classified as "extremely dry" or "extremely wet" in the SPEI categories. Droughts and floods add the categories "severely dry" and "very wet" to the "extreme" categories. (Extreme) disasters are defined to be (extreme) droughts and floods combined. Finally, for expositional simplicity, I call the 12 months SPEI index "drought index" in this

[20]See Auffhammer et al. (2013) for arguments why using reanalysis data is more suitable than simple gridded datasets such as UDEL or CRU.

paper.

Panels C and D of Figure 1.1 plot the distribution of the drought index for the grid cells in my data for survey rounds 2 and 6 separately, showing variation both across geography and time. As can be seen, large parts of western Africa, Kenya, Uganda, and Lesotho are the most dry areas in the sample. Other places like Namibia, Zimbabwe, and South Africa, for example, are wet areas, suffering from floods instead of droughts. Furthermore, over time, the graphs get "lighter" (in color), implying that the climate becomes drier.

There is a large literature documenting negative economic impacts of weather variations (e.g., Dell et al., 2014; Carleton and Hsiang, 2016). To this end, Table 1.A5 regresses five potentially climate-affected outcomes on the drought index used in this paper. In Columns 1—3, I rely on three proxies for income available from the Afrobarometer surveys: (positive) economic expectations, food availability, and cash availability.[21] As the table shows, a one standard deviation increase in the drought index (i) reduces individuals' economic expectations by 3.6 percentage points, (ii) reduces food availability by 0.070 points (on a 5 point scale), and (iii) reduces cash availability by 0.069 points (on a 5 point scale). Column 4 presents results relying on yet another proxy for income: the log of nightlights within the grid cell of the respondent.[22] Reassuringly, a drought reduces the luminosity of a grid cell. Finally, in Column 5, I rely on another outcome that is widely documented to be affected by droughts: conflict.[23] As expected, droughts increase

[21]The Afrobarometer does not have reliable income data, which is why I rely on proxies. The three questions are: (i) "looking ahead, do you expect the following to be better or worse: your living conditions in 12 months time?"—I convert the 5-scale answers provided by respondents into a dummy indicating a positive outlook; (ii) "over the last year, how often, if ever, have you or your family gone without enough food to eat?"—I flip the scale of the answers provided to a variable ranging from 1 to 5 with 1 indicating "always" and 5 indicating "never"; (iii) "over the past year, how often, if ever, have you or your family gone without cash income?"—I flip the scale of the answers provided to a variable ranging from 1 to 5 with 1 indicating "always" and 5 indicating "never".

[22]I download the widely used grid cell level nightlights data from 1992 to 2013 here (last accessed: May 8, 2024).

[23]I download the Armed Conflict Location & Event Data Project (ACLED) database for all years of my sample. I follow Harari and La Ferrara (2018) in defining dummy variables capturing conflict exposure. Specifically, I create two variables: (i) the dummy "battles" indicates having experienced a conflict classified as a battle of any kind (regardless whether control of geographies changes) and (ii) the variable "riots" captures riots and protests and indicates if (public) demonstrations against

the probability that a respondent's grid cell is exposed to a battle/conflict event. Overall, Table 1.A5 validates the drought index used in this paper.[24]

The drought index yields a level effect of drought conditions on the support for democracy. With climate change one might, however, also be interested in looking at the effects of higher moments or at nonlinearities.

The premise of this paper relies on the fact that individuals notice changes in the weather and update their beliefs accordingly. As such, these changes must be noticeable. This leaves the level effect and the effect of the variance (or standard deviation) of droughts, i.e., the fact that climate change doesn't just change the intensity of droughts but also affects the frequency and/or likelihood of their occurrence. As already mentioned, the mean (standard deviation) of the SPEI index is 0.475 (0.785) in my sample. This standard deviation is smaller than the one in the historical sample (which is 1). The individuals in my sample are therefore not exposed to more drought variability over time. This renders the context of my study more suited to study level effects.

Figure 1.A2 provides further intuition for this by plotting the 1, 12, 24, and 48-month SPEI index from January 1970 to December 2015 for Dakar, Senegal.[25] Shorter timescales of the index pick up a lot of short-run variation while longer time horizons vary much less. This is why I don't consider nonlinear effects—captured for example by the inclusion of drought or flood dummies—as my main point of interest. I want to capture the effect of long-run changes in drought conditions and these are not only represented by extreme drought dummies, but also by prolonged moderately dry periods, for example. The 12 months SPEI index allows me to capture all of these effects. Notwithstanding this, I show in robustness checks that my main result holds when measuring droughts using dummy

government institutions take place. In Table 1.A5 I use the "battle" dummy as the outcome. The "riot" dummy will be relevant only in section 1.5.

[24]As the outcomes in Columns 4 and 5 are at the grid cell (and yearly) level, I lag the drought index by one year (i.e., 12 months) to allow the impacts to be visible at this aggregation.

[25]The location is arbitrary. The point is to show inherent features of the different timescales of the SPEI index. These are similar for any location.

variables.

1.3 Main Results

Empirical strategy. To capture the reduced form effect of the drought index on the support for democracy, my main specification looks as follows

$$\text{Support for democracy}_{iegt} = \delta_g + \tau_t + \beta \text{Drought Index}_{gt} + \mathbf{x}_{iegt}\gamma + \epsilon_{iegt} \qquad (1.1)$$

where Support for democracy$_{iegct}$ denotes the outcome variable indicating whether individual i in enumeration area e in grid cell g in year-month t supports democracy or is open to non-democratic regimes. The right-hand side of the equation includes grid cell and month by year fixed effects, the drought index at the grid cell and month by year level, and allows for the inclusion of household level controls.[26] Standard errors are clustered at the grid cell level.[27]

The coefficient of interest in this TWFE regression, β, indicates the percentage point change in the outcome in response to a one standard deviation increase in the drought index. Recall from section 1.2 that values above 1.5 are considered severely dry and extremely dry and that the mean (standard deviation) of my drought index is 0.475 (0.785). Defining a drought as corresponding to severely and extremely dry conditions, the effect of a drought is therefore equivalent to a two standard deviation increase in the drought index.

Whether this regression succeeds in capturing the causal effect of the drought index on the support for democracy, hinges on at least three important assump-

[26]The controls I include in all regressions are the age of the respondent and dummy variables indicating (a) whether the respondent completed high school or more, (b) whether the respondent is male, (c) whether the respondent is white, (d) whether the respondent is religious, (e) whether the respondent is aligned with the political party in power, and (f) whether the respondent is employed (see Table 1.A1). Controls (d), (e), and (f) are potentially bad controls as they may themselves be affected by the drought index. In robustness tests I show that the results are robust to removing these potential bad controls (and also to removing all controls).

[27]The subscript i is redundant as I only know the enumeration area e an individual lives in. The subscript e clarifies that I merge the enumeration area e to the grid cell g. Specifically, geocoded Afrobarometer data contains the geographic center of each enumeration area e. I merge this information to the relevant grid cell g.

tions: (a) the exogeneity of the index, (b) homogeneous treatment effects (e.g., De Chaisemartin and d'Haultfoeuille, 2022b; Roth et al., 2023), and (c) no selected sample. The first assumption assumes that the weather is random conditional on geography and time fixed effects. Acemoglu et al. (2002) and Rodrik et al. (2004) argue that long-run climate averages can be associated with changes in institutional quality (hence rendering them endogenous), but that deviations from the long-run mean are not (hence rendering them exogenous). Recall that the drought index is a deviation from a long-run mean, making it exogenous. Given that my main specification relies on the (long-run) 12 months drought index (i.e., it is comparing the weather conditions in the last twelve months to the historical weather), I show, in robustness checks, that the main results also hold when relying on the (short-run) 3 months drought index. The second assumption assumes that the treatment effect is constant across all 16 countries and five survey waves. I show that my results are robust to allowing for heterogeneous treatment effects in robustness tests. The assumption of no selected sample refers to the possibility that: (i) natural disasters can affect the roll out of the Afrobarometer surveys, (ii) conditional on the roll out of the surveys, the Afrobarometer interviews different "types" of individuals, and (iii) individuals exhibit adaptation behavior (e.g., they migrate) due to natural disasters and thus change the composition of the sample. I show that these considerations do not represent concerns in my analysis in robustness checks.

Main results. Table 1.3 presents the main results, relying on coding 1 (2) of the main outcome in Columns 1 and 2 (3 and 4). Columns 1 and 3 estimate equation (1.1) and show that, depending on the coding of the outcome, a one standard deviation increase in the drought index decreases the support for democracy by 1.1 or 1.8 percentage points.[28] These effects are statistically significant at the 5%

[28]The precise interpretation is that a one standard deviation increase in the drought index decreases the probability that a respondent answers that they support democracy by 1.1 or 1.8 percentage points. For simplicity, I will refer to this simply as a decrease in the support for democracy throughout the paper.

and 1% level, respectively. The next part of the table translates these estimates into percentage effects for one drought. As mentioned above, one drought corresponds to an increase of 2 standard deviations in the drought index. Put differently, this means that one drought alone reduces the support for democracy by 2.56%—5.28%.

Columns 2 and 4 add one- and two-year (i.e., 12 months and 24 months) lags of the drought index to the regressions. While the contemporaneous effects in these regressions are unchanged, the effects fade out after one or two years, depending on the coding of the outcome. The contemporaneous and lagged effects in Column 4 are jointly significant.

Freedom. As discussed in section 1.2, democracy is a multi-dimensional concept, meaning different things to different people both across and within countries. To this end, Table 1.4 displays the effects of droughts on three variables relating to the erosion of democracy (see Panel D of Table 1.1) and three variables relating to personal freedoms (see Panel C of Table 1.1). Columns 1 and 3 of the table show that in response to droughts, respondents are more likely to want one-man rule (i.e., to want a dictator) and one-party rule (i.e., to abolish parliament and elections). Since elections, a parliament, and a leader/president with some constraints on their power are cornerstones of a democracy, the respondents answers indicate that they want a consolidation of power in their country's politics in response to a drought.[29]

Columns 4—6 show that droughts reduce the freedom of speech, the freedom to join any organization, and the freedom to vote by, respectively, 4.94%, 3.18%, and 2.85%. Given that droughts reduce democracy and given that 43.5% of the sample associates democracy with personal freedoms, this result is to be expected. To interpret this result, consider the results just discussed on the desire for more consolidation of power within a country's politics. A logical continuation of this

[29]Columns 1—3 reassuringly show that individuals are consistent in their answers. Table 1.3 showed that droughts reduce individuals' support for democracy. The answers in Columns 1—3 of Table 1.4 show that respondents understand what this decrease in democracy means.

result is a loss in freedoms (e.g., if a country abolishes elections, there is no more freedom to vote). A way to interpret the findings in Columns 4—6 is therefore that individuals deliberately give up some freedom in exchange for a less democratic country if, for example, they believe that "less democracy" is better at dealing with climate change.

Trust. Table 1.5 displays results for the effects of droughts on three further dimensions of democracy: trust in government (Columns 1—3), trust in institutions (Columns 4—6), and capabilities of the government (Columns 7—10). I observe that one drought significantly reduces trust in the president by 11.3%, trust in parliament by 9.71%, and trust in local government by 4.68%. Furthermore, one drought reduces individuals' trust in the police, the courts, and the army by 12.6%, 7.72%, and 5.36% respectively. Finally, in terms of citizens' views of the capabilities of the government, a drought reduces the share of individuals who believe the government can manage the economy and education services by 7.84% and 5.52%, respectively.[30]

Democracy vs. autocracy. Table 1.6 shows that the negative effects on the support for democracy and trust in government and institutions documented so far only persist in democracies. Specifically, the table explores heterogeneous effects by expanding equation (1.1) and adding an interaction term of the drought index and a variable indicating whether the respondent lives in an autocratic country and that variable itself.[31] Column 1 shows that the main effects from Column 1 in Table 1.3 are unique to democratic countries: in non-democratic systems (autocracies), droughts reduce the support for democracy insignificantly. The outcomes in Columns 2—4 are the democracy index, the trust in government index, and the trust in institutions index described in Tables 1.1 and 1.2 and the text in section

[30]The results on trust in government mirror the findings from the literature (Alfano and Aboyadana, 2020; Balcazar and Kennard, 2022). For example, Balcazar and Kennard (2022) find that temperatures above 3 degrees Celsius decrease trust in political leaders by 2-3 percentage points.

[31]The variable is created from the polity measurement (see Table 1.A4). Specifically, the dummy is equal to one if a country has a polity score of 0 or less.

1.2. Similarly to the heterogeneity analysis in Column 1, the negative effects on these indices only exist in democracies.

Two extensions. There are many extensions to the above that one can pursue. I here highlight two that seem of first-order importance. These are not the main subject of the paper and, therefore, should be analyzed in future research in more detail.

Country-level heterogeneities. My sample consists of 16 SSA countries. These countries of course vary in their levels of democracy, state capacity, or economic development. They also vary culturally. While the fixed effects in my analysis take these differences into account (to some degree at least), I here nonetheless estimate equation (1.1) at the country level. Table 1.A6 shows the results, presenting only the percentage effects of droughts or floods for each country where the effect is significant. The main results from Table 1.3 mask large country level variations. The table shows that the negative effects of droughts on the support for democracy are driven by Cape Verde, Tanzania, Senegal, Zambia, and Kenya, with effects of one drought implying reductions in the support for democracy of up to 18.1%. In other countries, i.e., Zimbabwe and South Africa, the drought index picks up the effects of a flood. Looking at Figure 1.1 confirms that these countries are confronted mainly with extremely wet conditions (i.e., floods). Finally, the remaining countries—Botswana, Ghana, Lesotho, Malawi, Mali, Mozambique, Namibia, Nigeria, and Uganda—display no effect of droughts on the support for democracy. There are many possible reasons for this. Note that most of the countries with null effects have large negative coefficients implying that droughts do reduce the support for democracy in these areas but that the effect is just not statistically significant.

Cumulative effects. Throughout a lifetime, an individual is unlikely to be affected only by one drought. Indeed, the median individual in my sample is affected by 7 droughts (min=0 and max=16).[32] Panel A of Figure 1.A3 estimates equation (1.1) but replaces "Drought Index$_{gt}$" with 16 dummy variables indicating whether the respondent has been exposed to 1, 2, 3, ..., 16 droughts, respectively.[33] As can be seen, all dummies have a negative effect on the support for democracy, with the effect clearly increasing with more drought exposure. For the first few droughts the effect is still non-significant but then becomes significant and remains so. For example, the cumulative effect of exposure to 7 droughts for the median individual in the sample is $-0.068(0.045)$, which translates into a 7.95% reduction in the outcome. For individuals exposed to 16 droughts, the effect is $-0.385(0.048)$, which translates into a 44.8% reduction in the outcome. Panel B of Figure 1.A3 repeats this procedure but relies only on extreme droughts, showing an even more extreme pattern.

1.3.1 Robustness of Main Results

Appendix 1.B presents details of various robustness tests of the main result from Table 1.3. First, I show that my results are robust to allowing for heterogeneous treatment effects. Second, I explore the possibility that the sample is selected. Specifically, this refers to the possibility that (i) natural disasters affect the roll out of the Afrobarometer surveys, (ii) conditional on the roll out of the surveys, the Afrobarometer interviews different "types" of individuals, and (iii) individuals exhibit adaptation behavior (e.g., they migrate) due to natural disasters and thus change the composition of the sample. I show that neither of these possibilities poses serious concerns in my setting. Third, I show that the results are robust to the inclusion of leads and therefore that there are no pre-trends in my empirical setting. Fourth, I consider two alternative ways of measuring droughts (drought dummies and the 3-month drought index) and show that the results survive this

[32] Since my weather data only goes back to 1960, and since my sample ends in 2015, I can only calculate the number of droughts individuals are exposed to for respondents 55 or younger. They make up 78% of the sample.

[33] The dummy indicating no drought exposure is the one excluded from the regression. The effect on the dummy indicating exposure to 16 droughts should be taken with a grain of salt as only 8 individuals are exposed to 16 droughts.

adjustment. Fifth, I show that the result is robust to the use of Conley standard errors (Conley, 1999). Sixth, I show that the main results are robust to the inclusion of different fixed effects. Seventh, I show that the results are unchanged when removing all controls. Eighth, I find that the results are robust when only controlling for age, gender, and education. Ninth, I show that the results survive when controlling for temperature and precipitation levels. Tenth, I show that the results remain unchanged when controlling for village controls.

1.4 Exposure to Non-Democratic Systems

The previous section has established that extreme weather events reduce the support for democracy and that this effect only persists in democracies. I have so far not looked at what mechanisms or channels drive this reduced form finding. This section dives into this question.

The list of possible channels driving the baseline result is (almost) endless. While the literature cited in the introduction emphasizes a channel via income (economic circumstances), this is by far not the only plausible mechanism. For example, it is widely known (and I show it in Table 1.A5) that weather variations affect conflict, which may very well in turn affect respondents support for democracy.[34] The aim of this section is therefore not to identify *the* channel driving the

[34]The empirical literature cited in the introduction often assumes that weather shocks only affect political outcomes via income. For example, Brückner and Ciccone (2011) write "under the assumption that rainfall shocks affect democratic change only through income, we can estimate the effect of transitory income shocks on democratic institutions using an instrumental variables approach." While they focus on democratic change (something I cannot do as I have a repeated cross-section), this is a very strong assumption that I do not think is empirically supported given the plethora of outcomes that the weather affects (Carleton and Hsiang, 2016).

results but to parse out *a* channel driving the result, amongst many others.[35,36]

This section inquires whether the exposure to non-democratic systems is a channel driving the results. I proxy this exposure by exposure to development projects funded by the World Bank and China, the former being technocratic and the latter being autocratic.

The motivation for focusing on this channel comes from a growing, albeit very inconclusive, literature evaluating the relationship between the presence of foreign aid and political attitudes. For example, Bai et al. (2022) show that Chinese infrastructure aid significantly increases positive attitudes towards the government in the region where the aid was implemented. While Eichenauer et al. (2021) and Blair et al. (2022) find no evidence that exposure to Chinese development projects in, respectively, Latin America and Africa increase attitudes towards China, Wellner et al. (2022) show that exposure to Chinese development projects can increase the support for China. Most closely related to this paper, Freytag et al. (2024) show that exposure to Chinese development aid in Latin America is associated with an increase in democratic values.[37]

[35]To explore how possible channels mediate the main effect, Table 1.A7 explores heterogeneous effects with respect to nine characteristics by expanding equation (1.1) and adding an interaction term of the drought index and a variable and said variable itself. The dimensions of heterogeneity are: (i) a variable indicating the number of years a country has been a democracy (to count the years as a democracy I count the number of years since 1990 that the polity measurement was larger than 0); (ii) a variable proxying the level of local state capacity (to create the local state capacity measure, I construct an index by adding all (except "urban") village characteristics from Table 1.A2 together. The resulting index ranges from 0 to 8); (iii) lagged log nightlights; (iv) lagged exposure to a conflict event; (v) economic expectations; (vi) a dummy indicating whether the respondent is employed; (vii) a dummy indicating whether the respondent has completed high school education or more; (viii) a dummy indicating whether the respondent is male; and (ix) a dummy indicating whether the respondent lives in an urban area. The main result worth highlighting is that there are no significant differential effects across any of these nine dimensions. The fact that dimensions that proxy income do not exhibit heterogeneous effects hints at the fact that income is not a mechanism driving the main result in this context. (I rely on lagged nightlights as current nightlights are affected by the drought index themselves. The result is unchanged when relying on the non-lagged measure. The same holds for the lagged conflict measure.) One possible explanation for this is the fact that only 3.9% of respondents associate economic development with democracy (see Table 1.1).

[36]Social capital is also a possible mechanism. For example, Buggle and Durante (2021) show that climate variability increases trust and cooperation and that communes—medieval cities characterized by inclusive political organization—are more widespread in regions with higher climate variability. Since my main finding is that droughts reduce the support for democracy, this mechanism is less likely to be at play here. See also Gorodnichenko and Roland (2021).

[37]There is also a literature that investigates the role of Chinese development aid and democratic

The hypothesis I test in this section is that the interaction of a drought and the exposure to a development project is the driving force that explains the observed reduction in the support for democracy in section 1.3. In other words, the mechanism underlying this channel is not (for example) an economic one, i.e., it is not an economic benefit (or lack thereof) of a development project that is relevant in mediating the impact of a drought. I argue that the exposure to these non-democratic systems per se is relevant in explaining the variation in the support for democracy when interacted with the exposure to a drought.

From a policy perspective, relying on ODA as a potential channel is interesting for at least two reasons. First, on average, ODA makes up 28.2% of the central government expenses for the countries in my sample, with a minimum of 1.22% in South Africa and a maximum of 88.2% in Malawi.[38] These numbers highlight the potential influence of exposure to alternative systems of governance. Second, the fight against climate change requires huge sums of money to flow to developing countries, with, for example, the World Bank being the "largest financier of climate action in developing countries delivering over $38.6 billion in [the] fiscal year 2023."[39] If ODA indeed does act as a driver of the results, this describes a "catch 22" as combating droughts and associated climate change requires foreign funding but simultaneously this funding, interacted with droughts, erodes democracy, thus highlighting a large negative externality.

1.4.1 Views of the World Bank and China

Supposing that development aid from the World Bank and China acts as a channel in explaining my result presumes that respondents hold some views about these entities. Table 1.A8 summarizes views respondents in the Afrobarometer hold on China and the World Bank.[40]

backsliding of countries (Bader, 2015; Li, 2017; Hess and Aidoo, 2019; Gamso, 2019).

[38]Source: World Bank Indicators in 2015. There is no data for Mozambique and Nigeria.

[39]Source: https://www.worldbank.org/en/topic/climatechange/overview#2 (Last accessed: May 9, 2024)

[40]All variables presented are only available as a cross-section. Panels A, B, and C rely on data from the sixth round of the Afrobarometer and Panel D relies on data from the second round of the

Panel A contains three pieces of information. First, around two-thirds of respondents think that Chinese aid is useful. Second, when asking individuals which country or international organization is the best model for their country, 27.9% name China, 34.7% list the US, and 5.5% state international organizations such as the World Bank or the United Nations. Third, when asked which country has the largest influence on their country, 31.4% name China while 24.0% list the US.

Panels B and C document further views respondents hold about China. Specifically, Panel B shows that 80.6% of respondents view China as having a lot of economic influence on their country and 73.4% view this as a positive influence. Panel C lists the most important factor explaining this positive image of China: over 50% of individuals name infrastructure projects and business investments as the primary reason.

Panel D presents answers to two questions about the United Nations and the World Bank from the Afrobarometer. On a scale from 0 to 10, individuals were asked whether these institutions are doing a good job. Respondents rate both institutions at roughly 6.7 out of 10.

Table 1.A9 regresses the support for democracy on some of these views to examine how they correlate. Column 1 (3) shows that individuals who believe China (the US) to be the best model for their country exhibit a lower (higher) support for democracy. Column 2 shows that similar to China, individuals who believe that the World Bank is doing a good job are less likely to support democracy. There is no correlation between people's view of the UN and their support for democracy (Column 4). Given my focus on development projects funded by China and the World Bank, the correlations from Table 1.A9 suggest that the mechanism proposed in this section may work in similar directions for both types of projects.[41]

While I don't know what the non-democratic regimes are that individuals see in China or the World Bank, I assume that these are autocratic and technocratic ones, respectively.

Afrobarometer.

[41] Columns 5 and 6 of Table 1.A9 are discussed later in this section.

1.4.2 Data

World Bank projects. Geocoded data on development projects approved by the World Bank from 1995-2014 are taken from AidData's Research Lab at William & Mary (Version 1.4.2).[42] I calculate the distance between each project location and individual (i.e., enumeration area) from the Afrobarometer and define exposure dummies indicating if the individual lives within 50km or 100km of a development project.[43]

Chinese projects. The data for development projects funded by China only are taken from AidData's Global Chinese Development Finance Dataset (Version 1.1.1). This data, introduced by Strange et al. (2017) and geocoded by Dreher et al. (2016), has widely been used in research (e.g., Dreher and Fuchs, 2015; Dreher et al., 2018; Mueller, 2022).[44] I again calculate the distance between each project location and individual from the Afrobarometer and define exposure dummies indicating if the individual lives within 50km or 100km of a development project.

Summary statistics. I create three groups of dummies. First, group G_{never} is an indicator for individuals that are never exposed to a project. Second, group G_{active} is an indicator for individuals that are interviewed after a project started to be implemented (i.e., they are exposed to a project at the time of the interview). Third, group G_{inactive} is an indicator for individuals that are interviewed before a project started to be implemented (i.e., they will be exposed in the future but are not exposed at the time of the interview).

Relying on the 50km (100km) radius for World Bank projects shows that 28.1%,

[42] I keep only projects in the sample that have precision codes 1 or 2. Furthermore, I assume that once a development project has been implemented it will "stay forever". The idea behind this is that if, for example, a road was built from 2002 to 2005, the road will not disappear in 2005. An individual interviewed in the Afrobarometer in 2009, for example, would therefore still be coded as being exposed to this road in my sample.

[43] I view 50km as the main distance because it is a reasonable commuting distance in Africa (Knutsen et al., 2017). I also report all result for 100km as a robustness test.

[44] I drop umbrella agreements (Dreher et al., 2021), only keep projects categorized as ODA (Isaksson and Kotsadam, 2018a), drop any co-financed projects, and only consider projects where the source of the project information comes from official sources.

65.0%, and 6.92% (18.7%, 75.8%, and 5.5%) of individuals are in groups G_{never}, G_{active}, and $G_{inactive}$, respectively. Similarly, relying on the 50km (100km) radius for Chinese projects shows that 67.6%, 22.4%, and 10.0% (49.2%, 35.8%, and 15.0%) of individuals are in groups G_{never}, G_{active}, and $G_{inactive}$, respectively.

1.4.3 The Development Projects

Figure 1.A4 displays the share of development projects by the World Bank (Panel A) and China (Panel B) by sector across time. While "government and civil society" rank high for both, the World Bank otherwise tends to focus more on "water supply and sanitation" projects while China stays in the "health" and "education" sectors.

Finally, Table 1.A10 regresses dummy variables indicating whether the respondent lives within 50km or 100km of a future development project on the drought index, thus assessing whether these projects are targeted towards drought areas.[45] The table shows no correlation for Chinese projects and a small negative correlation for World Bank projects.

For Chinese projects this implies that areas subject to disasters are not actively targeted.[46] For World Bank projects, the results suggest that drought occurrences do affect their (future) locations. More precisely, World Bank projects are less likely to be built in areas where droughts occurred in the past. As I posit that the presence of a World Bank project acts as a channel in explaining the effect of a drought on the support for democracy, this means that I will underestimate the effect of droughts on the support for democracy for individuals exposed to World Bank projects in the following subsection.

[45] To be clear, the outcome is the $G_{inactive}$ dummy indicating future exposure to projects. It is important to take this variable as the relevant question is whether droughts (or disasters more broadly) affect the location choice of future projects. How the location choice of past projects correlates with current droughts is irrelevant.

[46] This is contrary to the finding in Cervellati et al. (2022) who show that the location of Chinese projects is shaped by geo-climatological conditions.

1.4.4 The Exposure to Alternatives to Democracy

Empirical strategy. The empirical strategy to test whether development aid from the World Bank or China acts as a mechanism is a straightforward extension of the statistical model in (1.1)

$$\begin{aligned}\text{Support for democracy}_{iegct} &= \delta_{cy} + \tau_r + \beta_0 \text{Drought Index}_{gct} \\ &+ \beta_1(\text{Drought Index}_{gct} \times \text{G}^{x\text{km}}_{\text{active},iegct}) + \beta_2 \text{G}^{x\text{km}}_{\text{active},iegct} + \mathbf{x}_{iegct}\gamma + \epsilon_{iegct} \quad (1.2)\end{aligned}$$

where $\text{G}^{x\text{km}}_{\text{active},iegct}$ is a dummy variable indicating exposure to either a World Bank or a Chinese project and $x \in \{50\text{km}, 100\text{km}\}$. The remaining variables are defined as in equation (1.1).

A difference to equation (1.1) are the fixed effects. δ_{cy} are country by year fixed effects. These capture (i) the 16 countries' time-varying relations with China and the World Bank (e.g., diplomatic relations, trade, FDI) and (ii) changes in the political and economic landscape of the recipient country. τ_r are region fixed effects, controlling for time-invariant differences across regions. Jointly, these fixed effects control for factors that influence the allocation of aid by China and the World Bank.

In this specification, β_0 is the effect of the drought index on the support for democracy for individuals not exposed to a development project and β_1 represents the differential effect of the drought index on the support for democracy of exposed and not exposed individuals. $\beta_0 + \beta_1$ is thus the effect of the drought index on the support for democracy for individuals exposed to a development project funded by the World Bank or China.

Results. Table 1.A11 presents estimates of how exposure to Chinese projects affects views of democracy.[47,48] The table shows that exposure to both Chinese and World Bank projects negatively, but insignificantly, correlates with the democracy index, mirroring the findings in Gehring et al. (2022). The fact that these negative correlations are not significant does not imply that the exposure to non-democratic systems of governance does not act as a channel. In other words, this is not a "first stage" as the argument in this section is that the interaction of climate shocks and this exposure impact the support for democracy.

Table 1.7 displays the main results of section 1.4. Columns 1 and 2 (3 and 4) interact the drought index with exposure to Chinese (World Bank) projects within 50km and 100km, respectively. The top panel presents the estimated coefficients $\widehat{\beta_0}$ and $\widehat{\beta_1}$. The second panel then displays the sum of the estimates, $\widehat{\beta_0}+\widehat{\beta_1}$, as well as the p-value associated with said coefficients. Finally, the third panel translates the effects of $\widehat{\beta_0}$ and $\widehat{\beta_0}+\widehat{\beta_1}$ into percentage effects of one drought.

The drought index has no significant negative effect on the support for democracy for respondents not exposed to a development project. The differential effect of the index for exposed and not exposed individuals ranges from 1.6 to 2.5 percentage points. This difference is highly statistically significant. This then culminates in a significant effect of the drought index on the support for democracy of −1.3 to −2.4 percentage points for exposed individuals. In other words, respondents living in areas exposed to alternatives to democracy and exposed to one drought experience a reduction in the support for democracy of 3.03% to 5.59%.

[47] The outcome in this table is a democracy index, consisting of my main outcome (support for democracy) and the three variables summarized in Panel D of Table 1.1. I rely on all these outcomes since Gehring et al. (2022) show that they can all be affected by the exposure to development projects.

[48] To estimate causal effects of development projects on economic outcomes, the literature (e.g., Knutsen et al., 2017; Isaksson and Kotsadam, 2018a) here usually relies on a quasi-DiD design, which in my case translates to

$$\text{Democracy Index}_{iegct} = \delta_{cy} + \tau_r + \beta_1 \mathrm{G}^{x\text{km}}_{\text{inactive},iegct} + \beta_2 \mathrm{G}^{x\text{km}}_{\text{active},iegct} + \mathbf{x}_{iegct}\gamma + \epsilon_{iegct} \tag{1.3}$$

where $\mathrm{G}^{x\text{km}}_{\text{inactive},iegt}$ is a dummy variable indicating future exposure to either a World Bank or a Chinese project and $x \in \{50\text{km}, 100\text{km}\}$. Here, $\beta_2 - \beta_1$ provides a quasi-DiD effect of exposure to a development project on the democracy index (relative to individuals who are never exposed to a project).

1.4.5 Robustness

Development projects are unlikely to be randomly allocated throughout SSA, likely targeting areas with particular characteristics (like poorer areas). It is therefore possible that my results conflate other mechanisms. The aim here is to mitigate this concern.

Anticipation effects. To test for anticipation effects, I augment equation (1.3) to get

$$\begin{aligned}\text{Support for democracy}_{iegct} &= \delta_{cy} + \tau_r + \beta_0 \text{Drought Index}_{gct} \\ &+ \beta_1(\text{Drought Index}_{gct} \times \text{G}^{x\text{km}}_{\text{inactive},iegct}) + \beta_2(\text{Drought Index}_{gct} \times \text{G}^{x\text{km}}_{\text{active},iegct}) \\ &+ \beta_3 \text{G}^{x\text{km}}_{\text{inactive},iegct} + \beta_4 \text{G}^{x\text{km}}_{\text{active},iegct} + \mathbf{x}_{iegct}\gamma + \epsilon_{iegct} \qquad (1.4)\end{aligned}$$

β_1 in (1.4) indicates whether a drought has an effect on the support for democracy for individuals living in areas where a development project will be enacted in the future.

Table 1.8 presents the results. The interaction between the drought index and inactive development projects is insignificant. Areas that receive a project only after experiencing a drought do not exhibit any relationship between droughts and the support for democracy. If projects target certain types of areas, and certain characteristics of these areas drive the overall results, the drought index in these areas with these future development projects would display significant effects. Therefore, this test rules out local conditions as a potential mechanism.

Doughnuts. The premise of the doughnut idea is that if the exposure to alternatives to democracy (i.e., the presence of ODA) is orthogonal to some x, then this x cannot be a mechanism because the relationship between climate change and the support for democracy only exists for individuals exposed to alternatives to democracy. This relatively simple insight thus has the power to rule out a whole range of possible mechanisms.

To fix ideas, consider local employment, a proxy for income. Local development projects are not simply orthogonal to employment (e.g., Sautman and Yan, 2015; Guo et al., 2022). To show this, Table 1.9 regresses a dummy variable indicating whether the respondent is employed on a dummy variable indicating whether the respondent lives within a radius of, respectively, 10km, 20km (conditional on not living within 10km), and 30km (conditional on not living within 20km) of a development project funded by the World Bank or China. The idea behind this regression is simply that it is likely that development projects benefit respondents living close by a project and that at some point this economic benefit fades out. The table shows that individuals living within 10km of a development project benefit economically from it, while individuals living further away do not benefit from the project. As such, for individuals living beyond 10km of a development project, there is no correlation between employment and the presence of development projects.

Employment, or income, is a potential mechanism that may be confounding my results from the previous subsection. Because there is no relationship between the presence of development projects and employment beyond 10km of the project, replicating the results from Table 1.7 while excluding individuals who live within a 10km radius of a development project serves as a test whether I am conflating income and exposure to non-democratic systems as mechanisms above.

Table 1.10 does exactly that, i.e., it replicates Table 1.7 but drops individuals living within 10km of a development project from the sample. The results are unchanged. This suggests that the finding that droughts only affect the support for democracy for individuals exposed to development projects is unlikely to be driven by confounding factors such as income.[49]

Employment is not the only possible confounder that threatens the result in Table 1.7. The doughnut design can therefore be repeated with any other confounder one can think of. While not shown in the paper, I find that the presence of these

[49] Columns 5 and 6 of Table 1.A9 show that individuals' views on China and the World Bank also negatively correlate with the support for democracy if individuals living within 10km of a Chinese or World Bank project, respectively, are excluded from the sample.

development projects either does not correlate with potential confounders or, if so, affects them only within a 10km radius.[50] In other words, the regression in Table 1.10 simultaneously takes into account multiple confounders.

Sectors of ODA. Table 1.11 asks whether the results in Table 1.7 are driven by development projects in particular sectors. As can be seen, when defining (a) "government and civil society" and "other social infrastructure" as "infrastructure projects", (b) "health" and "education" as "health and education projects", (c) "water supply and sanitation" as "water supply and sanitation projects", and (d) "energy generation and supply" as "energy" projects, no sector in particular seems to be driving the results displayed above.[51] This is further evidence that the development projects here do indeed act as proxies for exposure to non-democratic systems of governance and are not targeting a particular need of people which may be driving the result.

Trust. Table 1.6 shows that the effects of droughts on the support for democracy as well as trust in government and institutions only exists in democracies. This leads to the plausible hypothesis that trust in government and institutions acts as mechanisms in explaining the reduction in the support for democracy. Table 1.12 provides evidence against this hypothesis. Specifically, the table shows that droughts reduce trust in government and institutions for individuals both exposed and not exposed to Chinese and World Bank development projects, which stands in contrast to the finding from Table 1.7 showing that the support for democracy is only reduced for individuals exposed to development projects.[52]

[50] As an example, the presence of development projects barely affects most village level characteristics from Table 1.A2, for example.

[51] For expositional simplicity I group exposure to Chinese or World Bank projects together into one exposure variable for this table. The results are unchanged if done separately for Chinese and World Bank projects.

[52] This result is related to the "backlash argument." The rise of populism around the world has, in popular writings, led to a widespread acceptance that individuals are upset and lash out against the political elites by voting for populists. Is it possible that this also holds in my context? In other words, can it be that this decrease in the support for democracy and the mechanism via exposure to "other actors" is purely a backlash against incumbent political elites? In regressions replicating the main results in Table 1.7, but adding an additional interaction indicating whether a respondent

Other radii. Table 1.A12 shows that the main result from Table 1.7 remains unchanged when changing the radius of exposure to 20km and 30km.[53]

Limitations. A caveat relevant to this section is that the results could still be either due to "cultural transmission" (exposure to some non-local population) or "propaganda" (money and media exposure). I leave the question which of the two channels is the underlying force driving my results open for future research.

1.5 Tangible Outcomes

This section tests whether the effects on the support for democracy translate into tangible effects, focusing on conflict events and demonstrations. Table 1.13 presents the results.

Columns 1 and 2 show that droughts reduce the probability of riots in a respondent's grid cell if the individual is exposed to a Chinese or World Bank project, respectively. There is no effect for individuals not exposed to development projects. This goes against the idea in Acemoglu and Robinson (2001) who argue that individuals may be more likely to protest to advance democracy ("threaten revolution") when the opportunity cost is low, which is likely the case during a drought (recession). It also goes against the finding in Iacoella et al. (2021), who show that the presence of Chinese development aid increases the occurrence of protests.

Columns 3 and 4 show that droughts increase the probability that people would demonstrate, but only if they are not exposed to development projects.[54] Similarly, in Columns 5 and 6, I show that droughts increase real conflict events in respondent's grid cells only for individuals not exposed to development projects. This is in line with the findings in Gehring et al. (2022) who show that development

lives in a high or low state capacity area, I show that droughts only affect the support for democracy for individuals exposed to development projects and this effect is significant and negative for individuals in high and low state capacity areas. In other words, the "backlash argument" does not apply.

[53]The results are unchanged for other radii as well.

[54]This is not a tangible outcome but a belief from the Afrobarometer.

projects reduce conflict occurrences/increase stability.[55]

Overall, the results suggest that there is positive relationship between the support for democracy and conflict (in the sense that when one decreases the other does as well). Columns 3—6 show that droughts do increase conflict for individuals not exposed to development projects. This is reassuring as there is a large literature documenting increases in conflict due to variations in the weather. However, this effect disappears for individuals exposed to development projects, possibly due to the decrease in the support for democracy.

1.6 Conclusion

To my knowledge, this paper is the first to analyze the relationship between individuals' support for democracy and climate change in detail. The main takeaway is that exposure to non-democratic systems of governance is a key channel when considering this relationship.

The paper opens the doors to many more research questions. Taken together, these avenues for future work lay out an exciting and policy relevant research agenda.

First, there is a need for more granular data on individuals' preferences on and beliefs about climate change and how they relate to a variety of political outcomes in developing countries. Specifically, the process of how individuals update their beliefs about climate change and politics is largely untouched in this paper.[56] Related to this is a need specific to this paper: given the decrease in the support for democracy, it is pertinent to understand what alternative systems of governance individuals have in mind.

Second, there is ample room for more theoretical contributions in political economy showing how individuals choose what political system they want to have

[55] Sardoschau and Jarotschkin (2024) show that Chinese development projects increase conflict incidents.

[56] There is some work on how individuals update beliefs about climate change in developed countries (e.g., Deryugina, 2013), but much more work is needed in developing countries. There is work on attitudes about climate change (e.g., Dechezleprêtre et al., 2022).

in their country. In this paper, I look at how extreme weather events in 16 SSA countries affect these beliefs, but more generally these could be a variety of conditions that individuals are exposed to. To date we lack theoretical models to help us understand how these beliefs are formed in detail. Detailed data collection processes on beliefs about climate change and political systems (point 1) can complement this theoretical undertaking.

Third, while I have analyzed the relationship between the support for democracy and conflict, the support for democracy may affect a range of other tangible outcomes as well. For example, voter turnout, voting outcomes, or, more extreme, participation in revolutions, are all actions by individuals that could be affected.

Finally, this paper has solely focused on developing countries. Climate change and the erosion of democratic norms are big policy issues in developed countries as well—it is therefore important to study this relationship in these countries as well.

1.7 Tables and Figures

Table 1.1. Summary Statistics of Political Variables (1)

	(1) Full Sample	(2) Eastern Africa	(3) Western Africa	(4) Southern Africa
A. Support for Democracy				
Respondent supports democracy (cond.)	0.859	0.872	0.872	0.818
	(0.348)	(0.334)	(0.334)	(0.386)
Respondent supports democracy (uncond.)	0.682	0.678	0.725	0.636
	(0.466)	(0.467)	(0.446)	(0.481)
Respondent indifferent to politics	0.206	0.222	0.168	0.223
	(0.404)	(0.416)	(0.374)	(0.416)
B. Meaning of Democracy				
Personal freedom	0.435	0.456	0.436	0.401
	(0.496)	(0.498)	(0.496)	(0.490)
Government for/by the people	0.099	0.072	0.135	0.094
	(0.299)	(0.258)	(0.342)	(0.292)
Voting	0.102	0.133	0.074	0.093
	(0.303)	(0.339)	(0.261)	(0.290)
Economic development	0.039	0.038	0.033	0.048
	(0.194)	(0.190)	(0.179)	(0.215)
C. Personal Freedom				
Freedom of speech	0.769	0.753	0.779	0.786
	(0.421)	(0.432)	(0.415)	(0.410)
Freedom to join organization	0.818	0.789	0.837	0.845
	(0.386)	(0.408)	(0.370)	(0.362)
Freedom to vote	0.843	0.831	0.849	0.857
	(0.363)	(0.375)	(0.358)	(0.350)
D. Erosion of Democracy				
Respondent doesn't support one party rule	0.741	0.701	0.833	0.699
	(0.438)	(0.458)	(0.373)	(0.459)
Respondent doesn't support army rule	0.798	0.837	0.769	0.765
	(0.402)	(0.370)	(0.422)	(0.424)
Respondent doesn't support one man rule	0.833	0.851	0.839	0.795
	(0.373)	(0.356)	(0.368)	(0.404)
Observations	128988	61208	37870	29910

Notes: The table displays mean sample characteristics and standard deviations (in parentheses) for a variety of political preferences. Panel A displays the share of individuals who indicate they support democracy vs. any other system of government (conditional on them not having answered that they are indifferent between democracy and other systems or on them having answered "don't know", and unconditionally) as well as the share of individuals who are indifferent to or don't know anything about politics. Panel B displays four meanings respondents associate with democracy: personal freedom, government by and for the people, voting, and economic development. Panel C displays summary statistics for three dimensions of personal freedom. Finally, Panel D displays summary statistics for three political variables relating to the erosion of democracy. Column 1 displays the characteristics across the full sample, while Columns 2—4 split the sample by regions in Africa. All summary statistics are calculated across all survey rounds.

Table 1.2. Summary Statistics of Political Variables (2)

	(1) Full Sample	(2) Eastern Africa	(3) Western Africa	(4) Southern Africa
A. Trust in Government				
Respondent trusts president	0.622	0.646	0.562	0.652
	(0.485)	(0.478)	(0.496)	(0.476)
Respondent trusts parliament	0.556	0.593	0.487	0.576
	(0.497)	(0.491)	(0.500)	(0.494)
Respondent trusts local government	0.513	0.545	0.472	0.506
	(0.500)	(0.498)	(0.499)	(0.500)
B. Capabilities of Government				
Gov. cap. of managing economy	0.485	0.480	0.420	0.574
	(0.500)	(0.500)	(0.494)	(0.494)
Gov. cap. of managing health	0.615	0.611	0.565	0.682
	(0.487)	(0.487)	(0.496)	(0.466)
Gov. cap. of managing education	0.652	0.667	0.560	0.736
	(0.476)	(0.471)	(0.496)	(0.441)
Gov. cap. of fighting corruption	0.433	0.423	0.409	0.478
	(0.495)	(0.494)	(0.492)	(0.500)
C. Trust in Institutions				
Respondent trusts police	0.539	0.530	0.510	0.588
	(0.498)	(0.499)	(0.500)	(0.492)
Respondent trusts courts	0.622	0.642	0.552	0.671
	(0.485)	(0.479)	(0.497)	(0.470)
Respondent trusts army	0.672	0.694	0.658	0.649
	(0.470)	(0.461)	(0.474)	(0.477)
D. Is your Country a Democracy?				
Not a democracy	0.077	0.097	0.072	0.052
	(0.267)	(0.296)	(0.258)	(0.221)
Democracy with major problems	0.312	0.316	0.334	0.279
	(0.463)	(0.465)	(0.472)	(0.448)
Democracy with minro problems	0.378	0.386	0.365	0.382
	(0.485)	(0.487)	(0.481)	(0.486)
Full democracy	0.232	0.201	0.229	0.288
	(0.422)	(0.401)	(0.420)	(0.453)
Observations	128705	61074	37750	29881

Notes: The table displays mean sample characteristics and standard deviations (in parentheses) for a variety of political preferences. Panel A displays the share of respondents who trust (a) the president, (b) parliament, and (c) local government. Panel B reports summary statistics for four variables indicating whether the respondent believes that the government is capable of (a) managing the economy, (b) managing health services, (c) managing education services, or (d) fighting corruption. Panel C displays the shares of individuals who trust (a) the police, (b) the courts, or (c) the army. Finally, Panel D presents the share of individuals who view their country as (a) not a democracy, (b) a democracy with major problems, (c) a democracy with minor problems, and (c) a full democracy. Column 1 displays the characteristics across the full sample, while Columns 2—4 split the sample by regions in Africa. All summary statistics are calculated across all survey rounds from the Afrobarometer surveys.

Table 1.3. Extreme Weather Events and the Support for Democracy

	Respondent supports democracy			
	Coding 1		Coding 2	
	(1)	(2)	(3)	(4)
Drought index	-0.011**	-0.011**	-0.018***	-0.017***
	(0.005)	(0.005)	(0.005)	(0.005)
Lagged drought index (1 year)		-0.001		-0.010**
		(0.004)		(0.005)
Lagged drought index (2 years)		-0.000		-0.008
		(0.004)		(0.005)
p-value of joint significance		[0.108]		[0.000]
Mean of outcome	0.859		0.682	
Effect of one drought (2 SDs)	-2.56%	-2.56%	-5.28%	-4.99%
Lagged effect of one drought (2 SDs)		-0.23%		-2.93%
Lagged effect of one drought (2 SDs)		-0.00%		-2.35%
Household controls	Yes	Yes	Yes	Yes
Cell fixed effects	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	63077	63077	76792	76792

Notes: The table displays OLS regressions of two codings of a dummy variable indicating support for democracy (vs. other systems of government) on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI) as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Table 1.4. Dimensions of Democracy

	Respondent doesn't support			Freedom		
	one party rule	army rule	one man rule	of speech	to join organization	to vote
	(1)	(2)	(3)	(4)	(5)	(6)
Drought index	-0.023***	0.002	-0.012**	-0.019***	-0.013***	-0.012**
	(0.006)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)
Mean of outcome	0.741	0.798	0.833	0.769	0.818	0.843
Effect of one drought (2 SDs)	-6.21%	0.50%	-2.88%	-4.94%	-3.18%	-2.85%
Household controls	Yes	Yes	Yes	Yes	Yes	Yes
Cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes
Observations	75489	74944	74479	75780	75475	75752

Notes: The table displays OLS regressions of dummy variables indicating (i) no support for one party rule, (ii) no support for army rule, (iii) no support for one man rule (i.e., abolishing parliament and elections), (iv) freedom of speech, (v) freedom to join political organizations, and (vi) freedom to vote of respondents on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI), as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Table 1.5. Further Dimensions of Democracy

	Respondent trusts						Government is capable of managing/fighting			
	the president	the parliament	the local government	the police	the courts	the army	the economy	health services	education services	corruption
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Drought index	-0.035***	-0.027***	-0.012**	-0.034***	-0.024***	-0.018***	-0.019***	-0.003	-0.018***	-0.006
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.008)	(0.006)	(0.005)	(0.008)
Mean of outcome	0.622	0.556	0.513	0.539	0.622	0.672	0.485	0.615	0.652	0.433
Effect of one drought (2 SDs)	-11.3%	-9.71%	-4.68%	-12.6%	-7.72%	-5.36%	-7.84%	-0.98%	-5.52%	-2.77%
Household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	74900	73869	71406	75639	73996	61706	73449	75392	75209	71830

Notes: The table displays OLS regressions of dummy variables indicating (i) trust in the president, (ii) trust in parliament, (iii) trust in the local government, (iv) trust in the police, (v) trust in the courts, (vi) trust in the local army, as well as the respondent's belief whether the government is capable (vii) of managing the economy, (viii) of managing health services, (ix) of managing education services, and (x) of fighting corruption on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI), as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Table 1.6. Democracies vs. Autocracies

	Respondent supports democracy	Democracy index	Trust in government	Trust in institutions
	(1)	(2)	(3)	(4)
Drought index	-0.012**	-0.040**	-0.067***	-0.071***
	(0.005)	(0.016)	(0.013)	(0.012)
Drought index x country is autocratic	0.009	0.061*	0.113**	0.042
	(0.013)	(0.037)	(0.045)	(0.036)
Coefficient of index + interaction	-0.003	0.021	0.046	-0.029
p-value: Coefficient of index + interaction	[0.787]	[0.532]	[0.274]	[0.383]
Mean of outcome		0.859		
Effect of one drought (2 SDs) (no interaction)	-2.79%	-9.31%	-15.60%	-16.53%
Effect of one drought (2 SDs) (interaction)	-0.70%	4.89%	10.7%	-6.75%
Household controls	Yes	Yes	Yes	Yes
Cell fixed effects	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	63077	76160	76143	76062

Notes: The table displays OLS regressions of a dummy variable indicating support for democracy (vs. other systems of government) (in Column 1), the democracy index (in Column 2), trust in government (in Column 3), and trust in institutions (in Column 4) on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI) as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. All columns add an interaction of the SPEI index with a variable indicating whether the respondent lives in an autocratic country and that variable itself. The outcomes in Columns 2—4 are described in Tables 1.1 and 1.2 and the main text. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Table 1.7. The Exposure to Alternatives to Democracy

	Respondent supports democracy			
	(1)	(2)	(3)	(4)
Drought index	-0.002	-0.001	0.003	0.010
	(0.005)	(0.005)	(0.006)	(0.007)
Drought index x Chinese project (50km)	-0.022***			
	(0.007)			
Drought index x Chinese project (100km)		-0.021***		
		(0.007)		
Drought index x World Bank project (50km)			-0.016**	
			(0.007)	
Drought index x World Bank project (100km)				-0.025***
				(0.007)
Coefficient of exposure to project	-0.024	-0.022	-0.013	-0.015
p-value: Coefficient of exposure to project	[0.002]	[0.001]	[0.010]	[0.004]
Mean of outcome	0.859			
Effect of one drought (2 SDs) (no project exposure)	-0.47%	-0.23%	0.070%	2.33%
Effect of one drought (2 SDs) (project exposure)	-5.59%	-5.12%	-3.03%	-3.49%
Household controls	Yes	Yes	Yes	Yes
Country by year effects	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	63216	63216	63216	63216

Notes: The table displays OLS regressions of a dummy variable indicating support for democracy (vs. other systems of government) on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI), an interaction of the SPEI index with a dummy variable indicating whether the respondent lives within a radius of 50km or 100km of a Chinese or World Bank project, said dummy itself, as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include country × year and region fixed effects and cluster standard errors at the grid cell level.

Table 1.8. Local Conditions do not act as Confounding Mechanisms

	Respondent supports democracy			
	(1)	(2)	(3)	(4)
Drought index	-0.001	0.001	0.006	0.011
	(0.004)	(0.005)	(0.006)	(0.007)
Drought index x inactive Chinese project (50km)	-0.010			
	(0.012)			
Drought index x active Chinese project (50km)	-0.023***			
	(0.008)			
Drought index x inactive Chinese project (100km)		-0.009		
		(0.011)		
Drought index x active Chinese project (100km)		-0.023***		
		(0.007)		
Drought index x inactive World Bank project (50km)			-0.024**	
			(0.010)	
Drought index x active World Bank project (50km)			-0.021***	
			(0.007)	
Drought index x inactive World Bank project (100km)				-0.013
				(0.010)
Drought index x active World Bank project (100km)				-0.026***
				(0.008)
Mean of outcome	0.859			
Household controls	Yes	Yes	Yes	Yes
Country by year effects	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	63216	63216	63216	63216

Notes: The table displays OLS regressions of a dummy variable indicating support for democracy (vs. other systems of government) on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI), an interaction of the SPEI index with a dummy variable indicating whether in a radius of, respectively, 50km or 100km from where the respondent resides a Chinese or World Bank project will exist in the future ("inactive project") or already exists ("active project"), said dummy itself, as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include country × year and region fixed effects and cluster standard errors at the grid cell level.

Table 1.9. Local Employment Correlates with Development Projects

	Respondent is employed					
	(1)	(2)	(3)	(4)	(5)	(6)
Chinese project: 10km	0.026** (0.012)					
Chinese project: 20km | not 10km		0.008 (0.014)				
Chinese project: 30km | not 20km			-0.001 (0.014)			
World Bank project: 10km				0.029*** (0.006)		
World Bank project: 20km | not 10km					-0.001 (0.008)	
World Bank project: 30km | not 20km						-0.002 (0.010)
Mean of outcome	0.345					
Household controls	No	No	No	No	No	No
Country by year effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes
Observations	128446	117480	112171	128446	86549	69452

Notes: The table displays OLS regressions of a dummy variable indicating whether the respondent is employed on a dummy variable indicating whether the respondent lives within a radius of, respectively, 10km, 20km, or 30km of a Chinese or World Bank project. Columns 2 and 5 (3 and 6) are conditional on not living within a radius of 10km (20km) of a project. Regressions include country × year and region fixed effects and cluster standard errors at the grid cell level.

Table 1.10. Excluding Income as a Mechanism

	Respondent supports democracy			
	(1)	(2)	(3)	(4)
Drought index	-0.002	-0.000	0.002	0.010
	(0.005)	(0.005)	(0.006)	(0.007)
Drought index x Chinese project (50km)	-0.027***			
	(0.009)			
Drought index x Chinese project (100km)		-0.023***		
		(0.008)		
Drought index x World Bank project (50km)			-0.019**	
			(0.008)	
Drought index x World Bank project (100km)				-0.029***
				(0.008)
Coefficient of exposure to project	-0.029	-0.023	-0.017	-0.019
p-value: Coefficient of exposure to project	[0.002]	[0.001]	[0.013]	[0.002]
Mean of outcome	0.859			
Effect of one drought (2 SDs) (no project exposure)	-0.47%	-0.00%	0.47%	2.33%
Effect of one drought (2 SDs) (project exposure)	-6.75%	-5.36%	-3.96%	-4.42%
Household controls	Yes	Yes	Yes	Yes
Country by year effects	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	58230	58230	44004	44004

Notes: The table replicates Table 1.7, but drops individuals living within 10km of a Chinese or World Bank project.

Table 1.11. Exposure to Different Sectors of Development Projects

	Respondent supports democracy			
	(1)	(2)	(3)	(4)
Drought index	0.000	0.000	0.004	-0.002
	(0.006)	(0.005)	(0.006)	(0.005)
Drought index x gov./infrastructure project	-0.014**			
	(0.007)			
Drought index x health/education project		-0.020***		
		(0.007)		
Drought index x sanitation/water project			-0.021***	
			(0.006)	
Drought index x energy project				-0.018**
				(0.008)
Coefficient of exposure to project	-0.013	-0.019	-0.017	-0.020
p-value: Coefficient of exposure to project	[0.018]	[0.004]	[0.002]	[0.004]
Mean of outcome	0.859			
Effect of one drought (2 SDs) (no project exposure)	0.00%	0.00%	0.93%	-0.47%
Effect of one drought (2 SDs) (project exposure)	-3.03%	-4.42%	-3.96%	-4.66%
Household controls	Yes	Yes	Yes	Yes
Country by year effects	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	63216	63216	63216	63216

Notes: The table displays OLS regressions of a dummy variable indicating support for democracy (vs. other systems of government) on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI), an interaction of the SPEI index with a dummy variable indicating whether the respondent lives within a radius of 75km of four types of development projects, said dummy itself, as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The sectors of development projects are: (i) “government and civil society” and “other social infrastructure”, (ii) “health” and “education”, (iii) “water supply and sanitation”, and (iv) “energy generation and supply”. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include country × year and region fixed effects and cluster standard errors at the grid cell level.

Table 1.12. Excluding Trust in Government and Institutions as a Mechanism

	Trust in government				Trust in institutions			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Drought index	-0.025**	-0.024**	-0.029**	-0.029**	-0.031***	-0.031***	-0.034***	-0.033**
	(0.010)	(0.010)	(0.012)	(0.013)	(0.010)	(0.011)	(0.012)	(0.013)
Drought index x Chinese project (50km)	-0.045**				-0.001			
	(0.020)				(0.020)			
Drought index x Chinese project (100km)		-0.029				-0.001		
		(0.018)				(0.017)		
Drought index x World Bank project (50km)			-0.001				0.006	
			(0.016)				(0.017)	
Drought index x World Bank project (100km)				-0.000				0.003
				(0.016)				(0.017)
Coefficient of exposure to project	-0.069	-0.053	-0.030	-0.030	-0.031	-0.032	-0.027	-0.030
p-value: Coefficient of exposure to project	[0.001]	[0.002]	[0.024]	[0.014]	[0.133]	[0.053]	[0.053]	[0.018]
Mean of outcome	0.000				0.000			
Effect of one drought (2 SDs) (no project exposure)	-5.82%	-5.59%	-6.75%	-6.75%	-7.22%	-7.22%	-7.92%	-7.68%
Effect of one drought (2 SDs) (project exposure)	-16.07%	-12.34%	-6.98%	-6.98%	-7.22%	-7.45%	-6.29%	-6.98%
Household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country by year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76216	76216	76216	76216	76137	76137	76137	76137

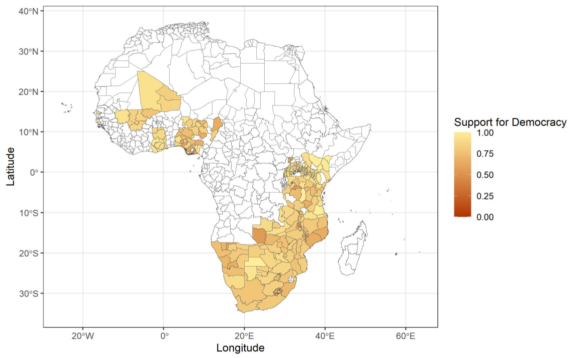
Notes: The table replicates Table 1.7, but changes the outcome to be trust in government (Columns 1—4) and trust in institutions (Columns 5—8).

Table 1.13. Conflict and the Support for Democracy

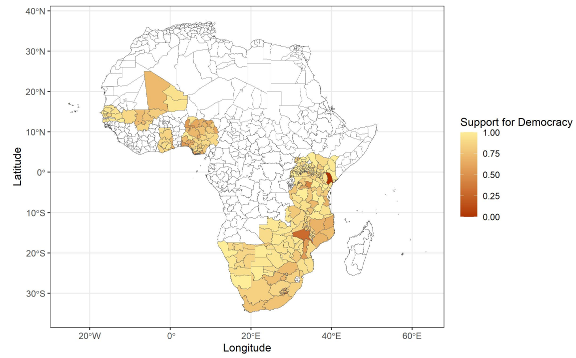
	Riots		Would attend demonstrations		Conflict event	
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged drought index	-0.010	-0.014			0.010*	0.015**
	(0.009)	(0.011)			(0.006)	(0.007)
Lagged drought index x Chinese project (50km)	-0.047**				-0.007	
	(0.020)				(0.012)	
Lagged drought index x World Bank project (50km)		-0.011				-0.011
		(0.014)				(0.010)
Drought index			0.010**	0.018***		
			(0.004)	(0.005)		
Drought index x Chinese project (50km)			-0.006			
			(0.008)			
Drought index x World Bank project (50km)				-0.016**		
				(0.006)		
Coefficient of exposure to project	-0.056	-0.025	0.005	0.002	0.004	0.004
p-value: Coefficient of exposure to project	[0.007]	[0.047]	[0.574]	[0.749]	[0.766]	[0.621]
Mean of outcome	0.182		0.118		0.073	
Effect of one drought (2 SDs) (no project exposure)	-2.33%	-3.26%	2.33%	4.19%	2.33%	3.49%
Effect of one drought (2 SDs) (project exposure)	-13.0%	-5.82%	1.16%	0.47%	0.93%	0.93%
Household controls	Yes	Yes	Yes	Yes	Yes	Yes
Country by year effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76900	76900	75560	75560	76900	76900

Notes: The table displays OLS regressions of dummy variables indicating (i) whether the respondent is exposed to a riot, (ii) whether a respondent would attend a demonstration or (iii) whether the respondent is exposed to a conflict event on the (lagged) 12 months Standardized Precipitation Evapotranspiration Index (SPEI), an interaction of the SPEI index with a dummy variable indicating whether the respondent lives within a radius of 50km of a Chinese or World Bank project, said dummy itself, as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include country × year and region fixed effects and cluster standard errors at the grid cell level.

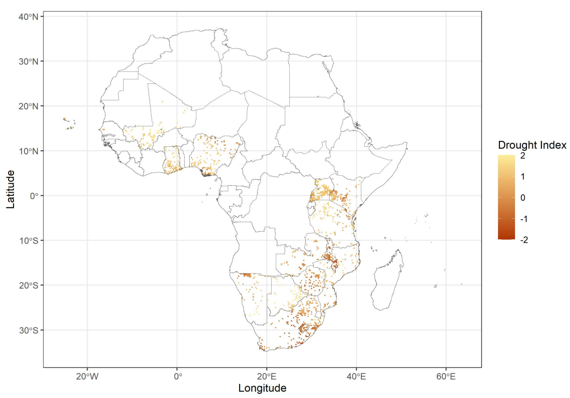
Figure 1.1. Distribution of the Support for Democracy and Drought Index



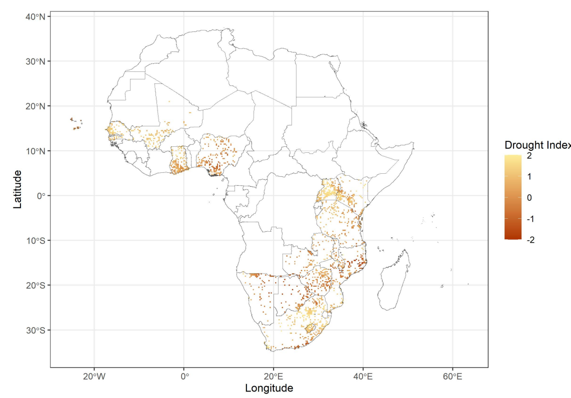
(a) Survey Round 2



(b) Survey Round 6



(c) Survey Round 2



(d) Survey Round 6

Notes: Panels A and B of the figure display the distribution of the support for democracy at the regional level in two survey rounds. Support for democracy is measured as a dummy variable indicating support for democracy vs. other systems of government at the individual level and is here aggregated to the regional level to preserve the anonymity of all respondents. Panels C and D of the figure displays the distribution of the drought index used in this paper, i.e., the 12 months Standardized Precipitation Evapotranspiration Index (SPEI), across all grid cells that appear in the data in each survey round. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations.

1.A Appendix Tables and Figures

Table 1.A1. Household Characteristics

	(1) Full Sample	(2) Eastern Africa	(3) Western Africa	(4) Southern Africa
Age	36.868	35.553	37.353	38.547
	(15.006)	(13.817)	(15.241)	(16.419)
High school education or more	0.270	0.242	0.244	0.352
	(0.444)	(0.428)	(0.429)	(0.478)
Male	0.498	0.498	0.499	0.497
	(0.500)	(0.500)	(0.500)	(0.500)
Race: black	0.946	0.992	0.904	0.915
	(0.226)	(0.091)	(0.295)	(0.279)
Race: white	0.012	0.002	0.004	0.035
	(0.107)	(0.043)	(0.067)	(0.185)
Religious	0.947	0.964	0.963	0.899
	(0.223)	(0.186)	(0.189)	(0.301)
Aligned with political party in power	0.518	0.519	0.394	0.645
	(0.500)	(0.500)	(0.489)	(0.479)
Employed (salaried)	0.345	0.333	0.376	0.329
	(0.475)	(0.471)	(0.484)	(0.470)
Occupation affected by climate change	0.710	0.747	0.737	0.600
	(0.454)	(0.435)	(0.440)	(0.490)
Observations	128988	61208	37870	29910

Notes: The table displays mean sample characteristics and standard deviations (in parentheses) for a variety of household characteristics. The variables displayed are the age of the respondent in years and dummy variables indicating (a) whether the respondent completed high school or more, (b) whether the respondent is male, (c) the race of the respondent (black or white), (d) whether the respondent is religious, (e) whether the respondent is aligned with the political party in power, (f) whether the respondent is employed, and (g) whether the respondent's occupation is affected by climate change. Column 1 displays the characteristics across the full sample, while Columns 2—4 split the sample by regions in Africa. All summary statistics are calculated across all survey rounds from the Afrobarometer surveys.

Table 1.A2. Village Characteristics

	(1) Full Sample	(2) Eastern Africa	(3) Western Africa	(4) Southern Africa
Post office	0.206	0.139	0.210	0.315
	(0.404)	(0.346)	(0.407)	(0.465)
School	0.835	0.857	0.866	0.758
	(0.371)	(0.350)	(0.341)	(0.428)
Police station	0.300	0.297	0.299	0.306
	(0.458)	(0.457)	(0.458)	(0.461)
Electricity	0.584	0.448	0.696	0.678
	(0.493)	(0.497)	(0.460)	(0.467)
Piped water	0.520	0.327	0.598	0.751
	(0.500)	(0.469)	(0.490)	(0.433)
Sewage	0.255	0.149	0.306	0.372
	(0.436)	(0.356)	(0.461)	(0.483)
Health clinic	0.534	0.528	0.573	0.495
	(0.499)	(0.499)	(0.495)	(0.500)
Market stalls	0.594	0.675	0.540	0.523
	(0.491)	(0.468)	(0.498)	(0.499)
Urban	0.372	0.276	0.460	0.436
	(0.483)	(0.447)	(0.498)	(0.496)
Observations	128988	61208	37870	29910

Notes: The table displays mean sample characteristics and standard deviations (in parentheses) for a variety of village characteristics. The variables displayed are dummy variables indicating whether the respondent's village (a) has a post office, (b) has a school, (c) has a police station, (d) has access to electricity, (e) has access to piped water, (f) has a sewage system, (g) has a health clinic, (h) has market stalls, and (i) is urban. Column 1 displays the characteristics across the full sample, while Columns 2—4 split the sample by regions in Africa. All summary statistics are calculated across all survey rounds from the Afrobarometer surveys.

Table 1.A3. Correlates of the Support for Democracy: Household Characteristics

	Respondent supports democracy								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Household Characteristics									
Age	0.000***								
	(0.000)								
High school education or more		0.017***							
		(0.003)							
Male			0.015***						
			(0.003)						
Race: black				0.026***					
				(0.010)					
Race: white					-0.053**				
					(0.022)				
Religious						0.022***			
						(0.007)			
Aligned with political party in power							0.024***		
							(0.004)		
Employed (salaried)								0.003	
								(0.003)	
Occupation affected by climate change									-0.008*
									(0.004)
Cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	101941	102750	102935	97120	97120	101724	70122	102596	50235

Notes: The table displays OLS regressions of a dummy variable indicating support for democracy (vs. other systems of government) on a variety of household characteristics, all of which are described in Table 1.A1 in detail. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Table 1.A4. Correlates Political Preferences and Polity Measurement

	Respondent supports democracy	How democratic is country?	Trust in government	Trust in institutions	Capabilities of government	Freedom
	(1)	(2)	(3)	(4)	(5)	(6)
Polity score	0.000	0.079***	0.067***	0.033***	0.053***	0.070***
	(0.003)	(0.009)	(0.009)	(0.008)	(0.009)	(0.010)
Mean of outcome	0.859	2.766	0.000	0.000	0.000	0.000
Cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102935	114925	126715	126759	127050	127575

Notes: The table displays OLS regressions of six outcomes on the polity measurement, a variable measuring the true level of democracy of a country and ranging from −10 (fully autocratic) to +10 (fully democratic). The outcomes are: (i) a dummy variable if the respondent supports democracy (vs. other systems of government), (ii) a variable ranging from 1 (not a democracy) to 4 (full democracy), indicating how much of a democracy respondents believe their country to be, and (iii) four indices measuring trust in government, trust in institutions, the capabilities of the government, and freedom. Each index is constructed in two steps. First, I average the components of the index, which are always dummy variables. Second, I standardize this average to get the final index. The trust in government index has three components: trust (a) in the president, (b) in parliament, and (c) in the local government. The institutions index has three components: trust (a) in the police, (b) in the courts, and (c) in the local army. The capabilities index has four components: the respondent's belief that the government is capable (a) of managing the economy, (b) of managing health services, (c) of managing education services, and (d) of fighting corruption. Finally, the freedom index has three components: perceived freedom of speech, freedom to join any political organization, and freedom to vote. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Table 1.A5. Validation of Drought Index

	Economic expectations	Food availability	Cash availability	Log(nightlights)	Conflict event
	(1)	(2)	(3)	(4)	(5)
Drought index	-0.036***	-0.070***	-0.069***		
	(0.008)	(0.015)	(0.016)		
Lagged drought index				-0.020**	0.012*
				(0.008)	(0.006)
Mean of outcome	0.621	3.934	3.011	1.388	0.073
Household controls	Yes	Yes	Yes	Yes	Yes
Cell fixed effects	Yes	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes
Observations	51927	76695	76529	58718	76828

Notes: The table displays OLS regressions of various outcomes on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI) as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. The outcomes are: (i) a dummy variable indicating the respondent's economic expectations, (ii) two variables ranging from 1 (always) to 5 (never) indicating how often the respondent's household has gone without food or cash in the past year, (iii) the log of nightlights in the respondent's grid cell, and (iv) a dummy indicating whether the respondent's grid cell has been exposed to a conflict event. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Table 1.A6. Effects by Country

Effect of Floods	Null Effect	Effect of Droughts
Zimbabwe (-8.79%)	Botswana	Cape Verde (-18.1%)
South Africa (-8.67%)	Ghana	Tanzania (-9.61%)
	Lesotho	Senegal (-8.12%)
	Malawi	Zambia (-8.01%)
	Mali	Kenya (-5.07%)
	Mozambique	
	Namibia	
	Nigeria	
	Uganda	

Notes: The table replicates the regression from Column 1 in Table 1.3 for each country in the sample individually and reports the percentage effect of a disaster for each country where the effect is significant.

Table 1.A7. Heterogeneous Effects

	Respondent supports democracy								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Drought index	-0.018*	-0.013*	-0.016*	-0.012**	-0.015**	-0.013**	-0.011**	-0.011**	-0.011**
	(0.011)	(0.007)	(0.009)	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)
Drought index x no. years democracy	0.000								
	(0.001)								
Drought index x local state capacity		0.000							
		(0.001)							
Drought index x lagged log(nightlights)			-0.001						
			(0.005)						
Drought index x lagged conflict event				0.006					
				(0.011)					
Drought index x econ. expectations					-0.004				
					(0.006)				
Drought index x employed						0.003			
						(0.004)			
Drought index x educated							-0.000		
							(0.005)		
Drought index x male								-0.001	
								(0.004)	
Drought index x urban									0.001
									(0.007)
Coefficient of index + interaction	-0.018	-0.013	-0.017	-0.005	-0.019	-0.009	-0.012	-0.012	-0.10
p-value: Coefficient of index + interaction	[0.086]	[0.059]	[0.004]	[0.641]	[0.004]	[0.099]	[0.038]	[0.025]	[0.136]
Mean of outcome					0.859				
Effect of one drought (2 SDs) (no interaction)	-4.19%	-3.03%	-3.73%	-2.79%	-3.49%	-3.03%	-2.56%	-2.56%	-2.56%
Effect of one drought (2 SDs) (interaction)	-4.19%	-3.03%	-3.96%	-1.16%	-4.42%	-2.10%	-2.79%	-2.79%	-2.33%
Household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63077	62471	48722	63077	43673	63077	63077	63077	62337

Notes: The table displays OLS regressions of a dummy variable indicating support for democracy (vs. other systems of government) on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI) as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. All columns add an interaction of the SPEI index with a variable and that variable itself. The variables added measure: (i) the number of years the respondent's country has been a democracy since 1990, (ii) local state capacity, (iii) lagged values of the log of nightlights in the respondent's grid cell, (iv) lagged values of a dummy indicating whether the respondent's grid cell has been exposed to a conflict event, (v) the respondent's economic expectations, (vi) the respondent's employment status, (vii) the respondent's education, (viii) the respondent's gender, and (ix) whether the respondent lives in an urban area. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Table 1.A8. Views on China and International Organizations

	(1) Full Sample	(2) Eastern Africa	(3) Western Africa	(4) Southern Africa
A. General Views				
Chinese aid is useful	0.622	0.630	0.668	0.555
	(0.485)	(0.483)	(0.471)	(0.497)
Best model for my country: China	0.279	0.295	0.278	0.255
	(0.449)	(0.456)	(0.448)	(0.436)
Best model for my country: US	0.347	0.333	0.412	0.288
	(0.476)	(0.471)	(0.492)	(0.453)
Best model for my country: UN/WB	0.055	0.052	0.061	0.052
	(0.228)	(0.221)	(0.240)	(0.223)
Most influence on my country: China	0.314	0.378	0.218	0.324
	(0.464)	(0.485)	(0.413)	(0.468)
Most influence on my country: US	0.240	0.256	0.252	0.196
	(0.427)	(0.437)	(0.434)	(0.397)
B. Chinas has [...] on my country				
a lot of economic influence	0.806	0.796	0.836	0.785
	(0.396)	(0.403)	(0.370)	(0.411)
a positive influence	0.734	0.747	0.769	0.669
	(0.442)	(0.435)	(0.421)	(0.471)
C. Factors explaining positive Chinese image				
Infrastructure and business investments	0.577	0.597	0.527	0.605
	(0.494)	(0.491)	(0.499)	(0.489)
D. International Organizations				
United Nations do a good job (0-10)	6.732	6.933	6.860	6.274
	(2.646)	(2.685)	(2.531)	(2.663)
World Bank does a good job (0-10)	6.726	6.971	6.938	5.971
	(2.630)	(2.663)	(2.489)	(2.622)
Observations (Panels A, B, C)	29948	15558	8400	5990
Observations (Panel D)	23486	10913	6582	5991

Notes: The table displays mean sample characteristics and standard deviations (in parentheses) for a variety of variables related to China and international organizations. The variables in the table indicate (a) whether China's overall economic development assistance is doing a good job of meeting the country's needs, (b) which country or international organization is the best model for the future development of the respondent's country, (c) which country has the most influence on the respondent's country, (d) whether China has a lot of economic influence on the respondent's country, (e) whether China has a positive economic and political influence on the respondent's country, (f) whether infrastructure and business investments are factors explaining the positive Chinese image, (g) whether the United Nations do their job well, and (h) whether the World Bank does its job well. Variables in Panels A, B, and C rely on data from the sixth round of the Afrobarometer surveys, while the two questions in Panel D are from the second round of the Afrobarometer surveys.

Table 1.A9. Views of China, the US, International Organizations, and the Support for Democracy

	Respondent supports democracy					
	(1)	(2)	(3)	(4)	(5)	(6)
Best model for my country: China	-0.015*				-0.022**	
	(0.008)				(0.009)	
World Bank does a good job (0-10)		-0.005**				-0.005**
		(0.002)				(0.003)
Best model for my country: US			0.017**			
			(0.008)			
United Nations do a good job (0-10)				0.000		
				(0.002)		
Mean of outcome	0.859					
Not living within 10km of project	No	No	No	No	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes	Yes
Country by year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13175	6551	13175	6913	11604	4995

Notes: The table displays OLS regressions of a dummy variable indicating support for democracy (vs. other systems of government) on, in Columns 1 and 3, a dummy variable indicating whether the respondent believes that, respectively, China or the US are the best model for the future development of the respondent's own country and, in Columns 2 and 4, variables indicating whether the United Nations or the World Bank are doing their job well, as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. Columns 5 and 6 recreate Columns 1 and 2 but drop individuals living within 10km of, respectively, a Chinese or World Bank project. Regressions include country × year and region fixed effects and cluster standard errors at the grid cell level.

Table 1.A10. Extreme Weather Events and the Exposure to Alternatives to Democracy

	Respondent will be exposed to:			
	Chinese project		World Bank project	
	50km	100km	50km	100km
	(1)	(2)	(3)	(4)
Drought index	-0.011	-0.001	-0.013*	-0.010*
	(0.009)	(0.008)	(0.007)	(0.006)
Mean of outcome	0.128	0.183	0.072	0.058
Household controls	Yes	Yes	Yes	Yes
Country by year fixed effects	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	76900	76900	76900	76900

Notes: The table displays OLS regressions of dummy variables indicating whether a respondent lives within 50km or 100km of a location where a Chinese or World Bank project will be built in the future on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI), as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include country $\times$ year and region fixed effects and cluster standard errors at the grid cell level.

Table 1.A11. Democracy and the Exposure to Alternatives to Democracy

	Democracy index			
	(1)	(2)	(3)	(4)
Inactive Chinese project (50km)	0.015			
	(0.025)			
Active Chinese project (50km)	-0.020			
	(0.022)			
Inactive Chinese project (100km)		0.021		
		(0.026)		
Active Chinese project (100km)		-0.002		
		(0.022)		
Inactive World Bank project (50km)			0.031	
			(0.027)	
Active World Bank project (50km)			0.013	
			(0.024)	
Inactive World Bank project (100km)				0.062*
				(0.033)
Active World Bank project (100km)				0.050*
				(0.028)
DiD coefficient	-0.035	-0.023	-0.018	-0.012
p-value: DiD coefficient	[0.176]	[0.301]	[0.457]	[0.668]
Mean of outcome	0.000			
Household controls	Yes	Yes	Yes	Yes
Country by year fixed effects	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	76523	76523	76523	76523

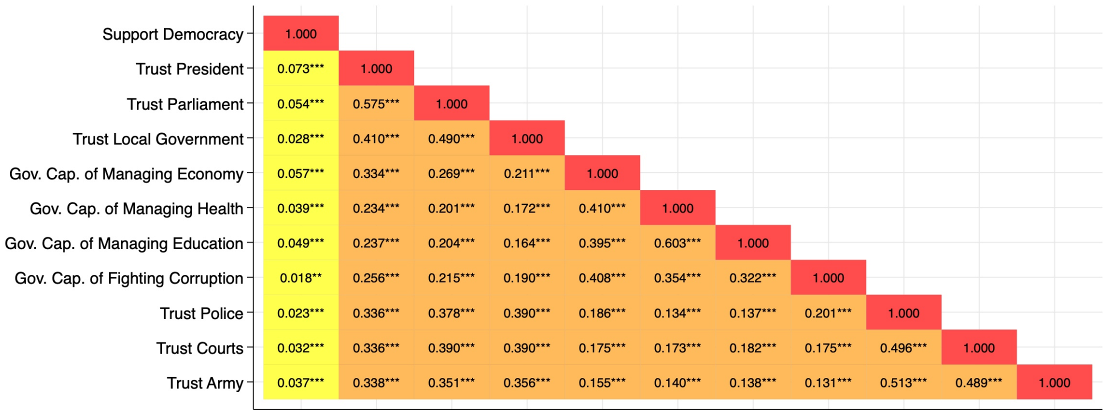
Notes: The table displays OLS regressions of a democracy index on dummy variables indicating whether in a radius of, respectively, 50km or 100km from where the respondent resides a Chinese or World Bank project will exist in the future ("inactive project") or already exists ("active project"), as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The democracy index is constructed in two steps. First, I average the components of the index, which are dummy variables. Second, I standardize this average to get the final index. The index consists of four variables: (i) no support for one party rule, (ii) no support for army rule, (iii) no support for one man rule (i.e., abolishing parliament and elections), and (iv) support for democracy (vs. other systems of government). Regressions include country × year and region fixed effects and cluster standard errors at the grid cell level.

Table 1.A12. Robustness of Results to Different Radii

	Respondent supports democracy			
	(1)	(2)	(3)	(4)
Drought index	-0.004	-0.004	0.001	0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Drought index x Chinese project (20km)	-0.021**			
	(0.009)			
Drought index x Chinese project (30km)		-0.019**		
		(0.008)		
Drought index x World Bank project (20km)			-0.019***	
			(0.007)	
Drought index x World Bank project (30km)				-0.017**
				(0.007)
Coefficient of exposure to project	-0.025	-0.023	-0.018	-0.015
p-value: Coefficient of exposure to project	[0.009]	[0.010]	[0.006]	[0.011]
Mean of outcome	0.859			
Effect of one drought (2 SDs) (no project exposure)	-0.93%	-0.93%	0.23%	0.23%
Effect of one drought (2 SDs) (project exposure)	-5.82%	-5.36%	-4.19%	-3.49%
Household controls	Yes	Yes	Yes	Yes
Country by year effects	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	63216	63216	63216	63216

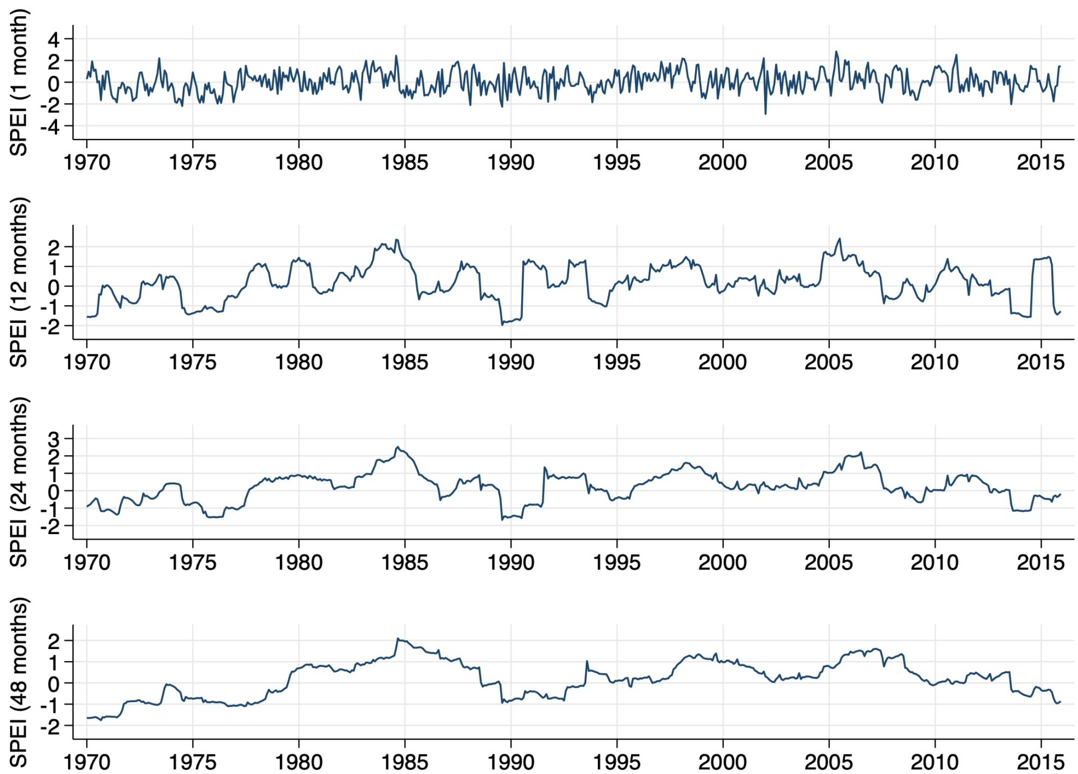
Notes: The table replicates Table 1.7, but changes the radius of exposure to Chinese and World Bank development projects to 20km and 30km (instead of 50km and 100km).

Figure 1.A1. Raw Correlations Between Political Preferences



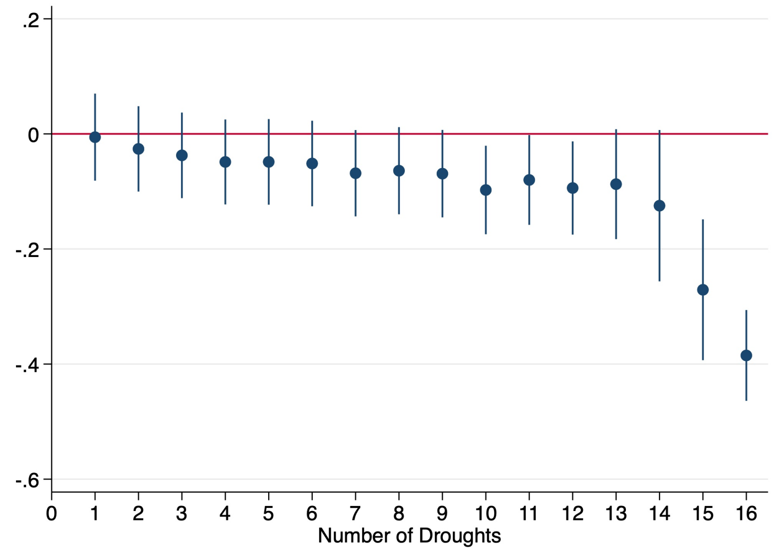
Notes: The figure displays correlations between my main outcome variable (the support for democracy vs. other systems of government) with other political variables using data from the latest survey round only. These are: (i) the respondent's trust in the president, the parliament, and the local government, (ii) the respondent's belief in the government's capabilities of managing the economy, managing health services, managing education services, and fighting corruption, and (iii) the respondent's trust in the police, the courts, and the army.

Figure 1.A2. Four Timescales of the Drought Index

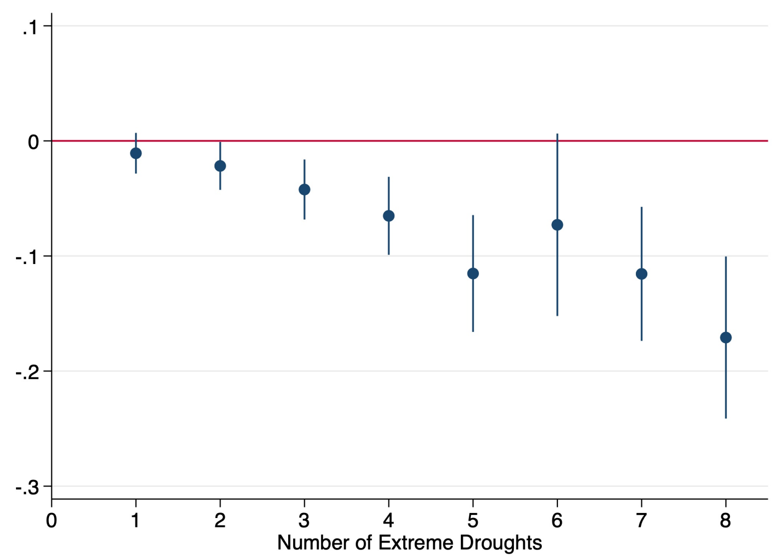


Notes: The figure displays four different drought indices—the 1, 12, 24, and 48 months SPEI index—in Dakar (Senegal) from January 1970 until December 2015.

Figure 1.A3. Cumulative Effects of Droughts on the Support for Democracy



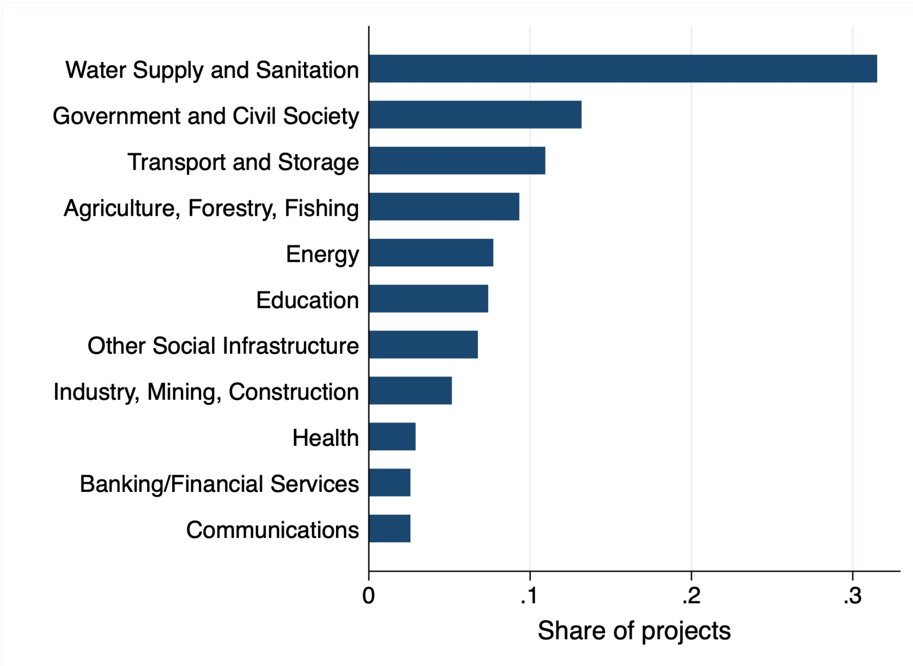
(a) Cumulative Exposure to Droughts



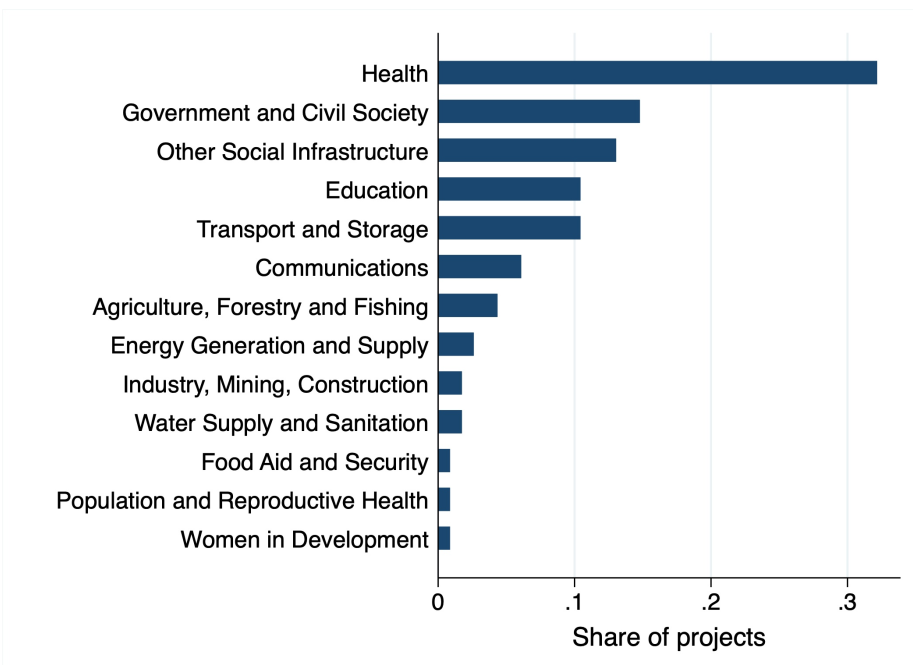
(b) Cumulative Exposure to Extreme Droughts

Notes: The figure displays the coefficients from OLS regressions of a dummy variable indicating support for democracy (vs. other systems of government) on dummy variables indicating how many drought years (Panel A) or extreme drought years (Panel B) the respondent has been exposed to throughout their lifetime, as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Figure 1.A4. Development Projects funded by the World Bank and China by Sector



(a) World Bank Development Projects



(b) Chinese Development Projects

Notes: The figure displays the share of development projects funded by the World Bank (Panel A) and China (Panel B) by sector across time.

1.B Robustness Tests

Heterogeneous treatment effects. The recent literature on heterogeneous treatment effects, summarized by De Chaisemartin and d'Haultfoeuille (2022b) and Roth et al. (2023), shows that the assumption underlying simple TWFE regression is one of homogeneous treatment effects, i.e., β in equation (1.1) is assumed to be constant across geography and time.[57,58]

To my knowledge, the only paper that allows for continuous treatments at every period in the sample is de Chaisemartin et al. (2022). Intuitively, the procedure they propose is as follows (in the case of multiple time periods). First, one estimates the treatment effects they propose (relying on their "did_multiplegt" package) for each consecutive pair of time periods. In my case, given my five survey waves, this yields four estimates (i.e., one for survey waves two to three, a second for survey waves three to four, etc.). Each treatment effect essentially compares switchers (i.e., individuals who changes their treatment from one period to the other) to stayers (i.e., individuals who did not change their treatment from one period to the other) conditional on them having had the same treatment status in the initial period (sections 4.3 and 5.3). Second, one calculates weights to take a weighted average and calculate the overall treatment effect (see Point 1 in Theorem 8 in section 5.3 for the weights).

While there are multiple differences between my set-up and theirs, two are especially relevant. First, there are no stayers in my sample as the values of the drought index always change for everyone (i.e., the weather is never the same at two time periods). Second, there are (almost) no individuals (or grid cells) with the

[57] More specifically, the TWFE regressions, under a parallel trend assumption, estimate a weighted sum of treatment effects across geography and time, with some negative weights. Due to these negative weights, the overall treatment effect might, for example, be negative even if the treatment effect is positive for every unit × period.

[58] Three types of estimators have been proposed to address this issue. The first type applies to designs with binary and absorbing treatments (Borusyak et al., 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). The second type extends this and applies to binary or discrete treatments (De Chaisemartin and d'Haultfoeuille, 2020; De Chaisemartin and d'Haultfoeuille, 2022a). The third type of estimators allows for continuously distributed treatments, but imposes that all units start with no treatment (De Chaisemartin and d'Haultfoeuille, 2022a). Neither directly applies to my setting as the drought index is continuously distributed at every period in my sample.

same value of the drought index at the initial treatment period (i.e., the first time period of the two). The first issue can be resolved by specifying a number such that individuals whose treatment changes by less than said number between two subsequent periods act as "quasi-stayers." The second issue cannot be addressed and, if I run their procedure, significantly reduces the sample size in my case. Specifically, each estimator in the first step of the procedure is estimated with a sample size of roughly 800-1000 observations. Given that my original sample contains 129,002 observations, relying on at best 5,000 of these to conduct a robustness test is suboptimal. It follows that unfortunately even this procedure is not applicable in my setting.[59]

To at least improve on the homogeneous treatment effects assumption from my main results, I therefore rely on Wooldridge (2021). Wooldridge (2021) proposes a simple two-step procedure to deal with heterogeneous treatment effects. Step 1 of the procedure consists of running the TWFE regression at the desired "level of heterogeneity." In my case, I estimate equation (1.1) at the country level, yielding 16 βs. In terms of econometric assumptions, this assumes homogeneous treatment effects within each country (and over time). While this may still not be fully realistic, it is a step in the right direction since assuming that treatment effects are constant within a country is a much milder assumption than the assumption that they are constant across all 16 countries. Step 2 of the procedure aggregates these 16 βs by taking a simple average. I bootstrap standard errors.

Table 1.B1 displays the final results of the procedure. The table shows that the main results from Table 1.3 are unchanged and therefore robust.

Sample selection. Sample selection presents a serious concern for the analysis presented in this paper. The assumption of no selected sample refers to the possibility that: (i) natural disasters can affect the roll out of the Afrobarometer surveys, (ii) conditional on the roll out of the surveys, the Afrobarometer interviews

[59] If I nonetheless run their procedure, relying on a variety of different threshold values and bootstrapping standard errors, the resulting estimates are always positive and larger in magnitude than my estimates from Table 1.3.

different "types" of individuals, and (iii) individuals exhibit adaptation behavior (e.g., they migrate) due to natural disasters and thus change the composition of the sample.

Timing of survey. Table 1.B2 regresses the number of days needed to conduct all interviews within a region or subregion (Columns 1—4) or the number of people interviewed within a region or subregion (Columns 5—8) on dummy variables indicating whether the region or subregion was hit by a (extreme) disaster (and a full set of unit and time fixed effects). With the exception of one coefficient, the table suggests that (extreme) disasters do not affect the outcomes, thus suggesting that neither droughts nor floods affect the timing of the survey.[60]

Balancedness of interviewees. Table 1.B3 compares household and village characteristics between respondents interviewed before and after a (extreme) disaster hit a region where the interview process took more than one month. The table shows that the characteristics are largely balanced, thus suggesting that, conditional on the roll out of the survey, the Afrobarometer's targeting of individuals is not affected by natural disasters.

Similarly, Table 1.B4 regresses the household characteristics on the continuous measure of the drought index and finds no correlation (except on employment where one expects an effect).

Adaptation behavior. There is ample evidence that individuals adapt to climate change. The most concerning adaptation behavior in my case is migration in response to climate change (e.g., Burzyński et al., 2022; Castells-Quintana et al., 2022; Conte, 2022). There are two types of migration: across country migration and within country migration. To address the former, Table 1.B5 reproduces Column 1 of Table 1.3 but, one by one, drops the four countries in my sample with the largest number of emigrants. The results remain unchanged. I unfortunately do

[60]The results remain unchanged when regressing these outcomes on my continuous drought index.

not have data within country migration flows and therefore have to assume that individuals do not endogenously migrate away from drought hit regions within countries.

Leads. Table 1.B6 adds a 12 month lead of the drought index, showing that the main results in Table 1.3 are unchanged and ruling out pre-trends.

Other drought measurements. Table 1.B7 considers two other ways of measuring droughts. Both confirm the main result. First, Columns 1 and 2 utilize a drought dummy and show that the main results from Table 1.3 are unchanged. Second, Column 3 relies on the 3 months drought index and three of its lags. As can be seen, the second lag has a significant negative effect, similar in magnitude as the main effect in Column 1 of Table 1.3. This suggests that the impact of a drought shock on respondents' support for democracy is lagged by roughly half a year.

Six other robustness checks. Table 1.B8 presents six further robustness checks. First, in Column 1, I follow Conley (1999) and use a spatial correction to calculate standard errors with a threshold of 300km. Second, Column 2 adds strata fixed effects (instead of grid cell fixed effects). In the Afrobarometer, every region (state) in each country has two strata: one for urban households and one for rural households. Third, Column 3 removes all controls. Fourth, Column 4 includes only age, gender, and education as controls. Fifth, Column 5 goes back to the original specification from equation (1.1), but adds weather controls (temperature and precipitation and their squares, measured in degrees Celsius and mm, respectively). Finally, Column 6 also relies on the main specification from equation (1.1), but adds village controls (see Table 1.A2). My main specification is robust to all these alternative specifications.

Table 1.B1. Heterogeneous Treatment Effects (Wooldridge, 2021)

	Respondent supports democracy			
	Coding 1		Coding 2	
	(1)	(2)	(3)	(4)
Drought index	-0.019***	-0.015**	-0.018***	-0.019***
	(0.005)	(0.006)	(0.006)	(0.007)
Lagged drought index (1 year)		-0.006		-0.009
		(0.005)		(0.006)
Lagged drought index (2 years)		-0.001		-0.003
		(0.005)		(0.006)
Mean of outcome	0.859		0.682	
Effect of one drought (2 SDs)	-4.42%	-3.49%	-5.28%	-5.57%
Lagged effect of one drought (2 SDs)		-1.40%		-2.64%
Lagged effect of one drought (2 SDs)		-0.23%		-0.88%
Household controls	Yes	Yes	Yes	Yes
Cell fixed effects	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	63077	63077	76792	76792

Notes: The table displays robustness checks to the main results in Table 1.3, following the procedure described in Wooldridge (2021). The coefficients displayed stems from OLS regressions of two codings of a dummy variable indicating support for democracy (vs. other systems of government) on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI), as well as a variety of household characteristics, all of which are described in Table 1.A1 in detail. The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level. Step 1 of the procedure consists of country level regressions in the same spirit as the ones run in Table 1.3. Step 2 of the procedure aggregates these individual effects by taking a simple average. The standard errors are bootstrapped in step 2.

Table 1.B2. Sample Selection: Roll Out of Survey

	Nr. days needed for interviews				Nr. people interviewed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Region hit by disaster	0.162				-2.015			
	(0.650)				(6.204)			
Subregion hit by disaster		0.068				2.283		
		(0.189)				(3.056)		
Region hit by extreme disaster			4.067**				15.104	
			(2.046)				(17.043)	
Subregion hit by extreme disaster				-0.934				4.608
				(0.954)				(10.265)
Mean of outcome	8.78	4.46	8.78	4.46	155	64.7	155	64.7
Region level	Yes	No	Yes	No	Yes	No	Yes	No
Subregion level	No	Yes	No	Yes	No	Yes	No	Yes
Region/Subregion fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey round fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at region x survey level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	129002	129002	129002	129002	129002	129002	129002	129002

Notes: The table displays OLS regressions of a variable indicating the number of days needed to conduct all interviews within a (sub)region (Columns 1—4) or the number of people interviewed within a (sub)region (Columns 5—8) on a dummy variable indicating whether that region/subregion was hit by a disaster (i.e., a flood or drought) or an extreme disaster (i.e., an extreme flood or extreme drought). Regressions include (sub)region and survey wave fixed effects and cluster standard errors at the (sub)region × survey wave level.

Table 1.B3. Sample Selection: Balance of Household and Village Characteristics

	Age	Educated	Male	Black	White	Religious	Politically aligned	Employed	Occ Affected
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Interviewed after disaster	-1.371*	0.024	0.016*	-0.010	-0.001	-0.016	-0.022	0.009	-0.003
	(0.757)	(0.040)	(0.009)	(0.033)	(0.014)	(0.015)	(0.029)	(0.033)	(0.032)
Region x survey wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at region x survey level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3442	3491	3503	3196	3196	3423	2149	3482	1788

	Post office	School	Police station	Electricity	Piped water	Sewage	Health clinic	Market stalls	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Interviewed after disaster	-0.002	-0.039	-0.023	-0.050	0.034	-0.070	-0.037	-0.003	0.002
	(0.042)	(0.039)	(0.044)	(0.057)	(0.052)	(0.041)	(0.055)	(0.058)	(0.061)
Region x survey wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at region x survey level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3412	3483	3416	3479	3477	3453	3414	3478	3336

	Age	Educated	Male	Black	White	Religious	Politically aligned	Employed	Occ Affected
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Interviewed after extreme disaster	-1.140	-0.011	0.009	0.054	0.025	0.017	-0.075	-0.093	-0.025
	(1.330)	(0.056)	(0.016)	(0.033)	(0.040)	(0.023)	(0.052)	(0.049)	(0.073)
Region x survey wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at region x survey level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1108	1111	1114	1114	1114	1086	657	1111	726

	Post office	School	Police station	Electricity	Piped water	Sewage	Health clinic	Market stalls	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Interviewed after extreme disaster	0.158	0.094	0.089*	-0.024	-0.279	-0.075	0.149*	0.154*	-0.249
	(0.151)	(0.088)	(0.040)	(0.183)	(0.170)	(0.063)	(0.067)	(0.073)	(0.200)
Region x survey wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at region x survey level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1114	1114	1114	1114	1114	1114	1109	1114	1114

Notes: This table compares household and village characteristics between respondents interviewed before and after a disaster (i.e., a flood or drought) or an extreme disaster (i.e., an extreme flood or extreme drought) hit a region where the interviewing process took more than one month. The coefficients come from a regression of the household or village characteristic in question on a dummy indicating whether the respondent was interviewed after the disaster or extreme disaster hit the region. Regressions include region × survey wave fixed effects and cluster standard errors at the region × survey wave level.

Table 1.B4. Sample Selection: Further Balance of Household Characteristics

	Age	Educated	Male	Black	White	Religious	Politically aligned	Employed	Occ Affected
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SPEI (12 months)	-0.152	0.001	0.000	0.007*	-0.001	-0.000	-0.001	0.020***	0.016**
	(0.117)	(0.004)	(0.001)	(0.004)	(0.001)	(0.002)	(0.007)	(0.004)	(0.008)
Cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	127300	128684	128985	121307	121307	127367	86163	128443	65143

Notes: This table regresses a variety of household controls on the 12 months Standardized Precipitation Evapotranspiration Index (SPEI). The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Regressions include grid cell and month by year fixed effects and cluster standard errors at the grid cell level.

Table 1.B5. Sample Selection: International Migration

	Respondent supports democracy			
	(1)	(2)	(3)	(4)
Drought index	-0.012**	-0.015***	-0.014***	-0.015***
	(0.005)	(0.005)	(0.005)	(0.006)
Mean of outcome	0.859			
Effect of one drought (2 SDs)	-2.79%	-3.49%	-3.26%	-3.49%
Household controls	Yes	Yes	Yes	Yes
Uganda dropped	Yes	Yes	Yes	Yes
Zimbabwe dropped	No	Yes	Yes	Yes
Tanzania dropped	No	No	Yes	Yes
Senegal dropped	No	No	No	Yes
Cell fixed effects	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	58094	54477	49348	46788

Notes: The table replicates Column 1 of Table 1.3 but, one by one, drops the countries in my sample with the highest number of emigrants (Uganda, Zimbabwe, Tanzania, Senegal).

Table 1.B6. Robustness of Main Results to Inclusion of Leads

	Respondent supports democracy			
	Coding 1		Coding 2	
	(1)	(2)	(3)	(4)
Lead of drought index (1 year)	0.004	0.004	-0.002	-0.002
	(0.004)	(0.004)	(0.005)	(0.005)
Drought index	-0.015***	-0.016***	-0.027***	-0.026***
	(0.005)	(0.005)	(0.005)	(0.005)
Lagged drought index (1 year)		0.001		-0.009*
		(0.004)		(0.005)
Lagged drought index (2 years)		0.003		-0.005
		(0.004)		(0.005)
Mean of outcome	0.859		0.682	
Lead effect of one drought (2 SDs)	0.93%	0.93%	-0.59%	-0.59%
Effect of one drought (2 SDs)	-3.49%	-3.73%	-7.92%	-7.62%
Lagged effect of one drought (2 SDs)		0.23%		-2.64%
Lagged effect of one drought (2 SDs)		0.70%		-1.47%
Household controls	Yes	Yes	Yes	Yes
Cell fixed effects	Yes	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes	Yes
Observations	57402	57402	69604	69604

Notes: The table replicates Table 1.3, adding a one year lead of the drought index to every regression.

Table 1.B7. Robustness to Different Drought Measures

	Respondent supports democracy		
	(1)	(2)	(3)
Drought dummy	-0.093**	-0.091**	
	(0.047)	(0.045)	
Lagged drought dummy (1 year)		0.035	
		(0.029)	
Lagged drought dummy (2 years)		0.004	
		(0.016)	
3 Months drought index			-0.000
			(0.004)
Lagged drought index (3-6 months)			0.007
			(0.005)
Lagged drought index (6-9 months)			-0.010**
			(0.005)
Lagged drought index (9-12 months)			0.001
			(0.004)
Mean of outcome		0.859	
Household controls	Yes	Yes	Yes
Cell fixed effects	Yes	Yes	Yes
Month by year fixed effects	Yes	Yes	Yes
SEs clustered at cell level	Yes	Yes	Yes
Observations	63077	63077	63077

Notes: The table replicates Column 1 of Table 1.3, but changes the variable used to measure drought occurrences. Columns 1 and 2 rely on a dummy variable indicating a drought, constructed from the 12 months Standardized Precipitation Evapotranspiration Index (SPEI). The SPEI index is a standardized drought index, where negative values indicate wet weather conditions and positive values indicate drought-like conditions. A drought corresponds to a shock of approximately two standard deviations. Column 3 uses the 3 months version of the SPEI index (instead of the usual 12 months SPEI index used in the paper).

Table 1.B8. Further Robustness Tests

	Respondent supports democracy					
	(1)	(2)	(3)	(4)	(5)	(6)
Drought index	-0.010**	-0.009**	-0.010**	-0.010**	-0.009*	-0.015***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Mean of outcome	0.859					
Effect of one drought (2 SDs)	-2.33%	-2.10%	-2.33%	-2.33%	-2.10%	-3.49%
Selected household controls	No	No	No	Yes	No	No
Household controls	Yes	Yes	No	No	Yes	Yes
Village controls	No	No	No	No	No	Yes
Weather controls	No	No	No	No	Yes	No
Cell fixed effects	Yes	No	Yes	Yes	Yes	Yes
Strata fixed effects	No	Yes	No	No	No	No
Month by year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
SEs clustered at cell level	No	Yes	Yes	Yes	Yes	Yes
Conley SEs	Yes	No	No	No	No	No
Observations	63219	62473	102935	101768	62319	58649

Notes: The table replicates Column 1 of Table 1.3 but, in Column 1, follows Conley (1999) and uses a spatial correction with a threshold of 300km, in Column 2, includes strata fixed effects, in Column 3, removes all controls, in Column 4, includes only age, gender, and education as controls, in Column 5, controls for weather controls (temperature and precipitation and their squares, measured in degrees Celsius and mm, respectively), and, in Column 6, adds a variety of village controls, all of which are described in Table 1.A2.

Chapter 2

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

Nicolas Cerkez, Adnan Q.Khan, Imran Rasul, and Anam Shoaib

2.1 Introduction

The last few decades have witnessed a steady rise in programs providing direct transfers to the poor (Banerjee et al., 2024). Among the most successful forms such interventions have taken are big push in-kind or cash transfers. Unconditional cash transfer programs have been implemented in 119 low-income countries and in-kind livestock asset transfers are being implemented as part of poverty graduation interventions in over 50 programs worldwide (CGAP, 2016; Handa et al., 2018). 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., 2015b; Haushofer and Shapiro, 2016; Bandiera et al., 2017; Blattman et al., 2020; Balboni et al., 2022; Egger et al., 2022).[1]

[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,

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

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

For both big push interventions considered, eligibility was determined by households lying below a poverty threshold and identified as poor. In a first treatment arm, poor households in a village were offered productive assets in-kind. They

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 and Musgrave, 1959), or endorsement effects (Benhassine et al., 2015).

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

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

Our field experiment follows a two-stage randomization design. In the first, we randomly assign villages to T1, T2 or control. At a second stage, within treated villages, we randomly assign the actual offer of treatment among eligible households. Half of those eligible are actually offered treatment. Among the poor in treated villages, we thus distinguish between the treated poor (TP) and the not treated poor (NTP). This design allows us to evaluate the causal impacts of the interventions on beneficiaries (TP), impacts on those overtaken in economic standing (NTP) and wider spillovers to those never eligible (NP).

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

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

On the impacts of the interventions on economic circumstances, we first document large and persistent gains on noticeable economic outcomes for the TP—those margins most noticeable to others in the village. For example, using the within-village randomization we document gains to the TP in terms of livestock ownership, the value of livestock owned, and consumption of own produced milk, relative to the NTP in the same village. The magnitude of the effects are of economic significance. For example, for the TP in T1, livestock ownership increases by 20pp, a 35% increase over the baseline mean for the poor in controls, the value of livestock owned increases by between 10-15% across periods, and by the four-year endline, the consumption of own produced milk increases by around 25%.

As treated and not treated poor households are balanced on observables at baseline, the magnitudes of these gains imply that many of the NTP are overtaken by their treated poor neighbors. These changes in relative standing can shape the perceptions and attitudes of the NTP if they have concerns for their relative standing or exhibit last place aversion (Duesenberry, 1949; Luttmer, 2005; Card et al., 2012; Kuziemko et al., 2014).

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

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

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

Our long-run partial population experiment design reveals four core insights. First, perceptions are shifted by big push economic interventions targeting the poor, but these impacts are far more muted than measurable changes in economic standing and village inequality. Most impacts on perceptions fade four years post-intervention, despite far more persistent changes in economic circumstances. For example, the TP—direct beneficiaries of the interventions—have little change in perception of their current economic standing, while non-beneficiaries report significant falls in their standing at midline. This is in line with findings from higher income settings that individual well-being can fall when individuals observe changes in wealth/income in people around them (Luttmer, 2005; Card et al., 2012; Perez-Truglia, 2020; Cullen and Perez-Truglia, 2022). At the same time, there are very muted impacts on households perceptions of changes in village inequality as a whole.

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

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

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

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

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

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

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

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

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

Section 2.2 describes our context, interventions and research design. Section 2.3 examines impacts on noticeable economic outcomes and village inequality. Section 2.4 details how perceptions and economic attitudes are shifted by the interventions. Section 2.5 discusses impacts on voting, differential impacts of cash and asset transfers, external validity and directions for future work. The Appendix presents additional results and checks.

2.2 Context, Interventions and Design

2.2.1 Context

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

2.2.2 Interventions

The interventions we study take two forms. The first offered households productive assets in-kind. To determine the menu of assets to offer, in each village we initially conducted an assessment of assets likely to generate high returns. These typically included livestock, assets to start a retail business (e.g., grocery shop, fruit stall), crop farming, and other forms of self-employment (e.g., tailoring). Figure 2.B1 shows a stylized representation of an asset menu. Households were free to choose any combination of assets off the menu up to a total value of PKR50K (500USD in 2012 prices). In conjunction with in-kind asset transfers, households were offered training providing skills to run a micro-enterprise, as well as skills specific to the chosen asset(s). The value of training was fixed at PKR12K. Hence the total value of transfers and training offered was PKR62K (around 620USD). We refer to this as treatment T1.[2]

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

Both treatments were implemented in collaboration with quasi-government agencies: the Pakistan Poverty Alleviation Fund (PPAF) and their government field partners, FDO and NRSP. Each intervention is thus best perceived as a government delivered program.[3]

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

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

The interventions are big push, representing high-valued resource transfers to the poor. The value of transfers corresponds to the equivalent of eight months of food consumption at baseline. Such resource injections are large enough to shift forward levels of economic well-being of the poor, do so in noticeable ways to others in these small village economies, and they have the potential to reduce village consumption and asset inequality.[4]

Eligibility. To establish eligibility, we first conducted a census of 35,522 households in our villages. Each was assigned a 0-100 poverty score based on characteristics proxying household's permanent income, that we collected in the census. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The interquartile range of poverty scores is 19 to 37, with the highest decile of households having a score above 46. The poverty score construction is similar to that used to target welfare programs to the rural poor in Pakistan, including the prominent Benazir Income Support Programme. This is the most widespread social protection program in Pakistan, reaching nearly five million households in 2012. Households are thus familiar with the kind of poverty score construction used to determine eligibility. Not treated poor households were given no promise of future treatment. Not poor households were aware they were never going to be eligible.[5]

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

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

2.2.3 Research Design

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

Sampling. We sample 6237 eligible poor households in treated and control villages (so around 75% of all poor households): 3052 reside in controls, 1598 are in T1 villages (of which 854 are treated), and 1587 are in T2 villages (942 are treated). We use our census to draw a random sample of non poor households from across all deciles of poverty scores. We denote non poor households as NP. We survey 9435 non poor households in total (so around 33% of all non poor households): 3130 reside in controls, 3306 in T1 villages, and 2999 in T2 villages.

Take-Up. In both treatment arms, there is near 100% take-up of the offer of transfers. In T1, 50% of eligibles chose some combination of livestock, 22% chose assets to set-up a small-scale retail business, and 15% chose assets related to petty trade. In T2, over 91% of households chose the unconditional cash transfer over any form of in-kind asset transfer. Hence the majority of households in T2 reveal prefer cash over assets.[6]

Timeline. We conducted our household census from May to July 2012, and our baseline household survey from February to June 2013. Interventions were rolled

[6]Given the scale of cash transfers offered, two other design features are relevant. First, after their initial choice, households were given a two-week window to finalize their choice, in case they preferred an alternative bundle after having discussed further with family and neighbors. Nearly all households stuck with their initial choice of cash transfers in T2. Second, the cash transfer is best interpreted as a labeled 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.

out January—March 2014. In this paper we focus on the one, two and four-year follow-up surveys that were fielded May to July 2015, September/October 2016, and February/March 2018 respectively. Noticeable economic outcomes are measured at the one, two- and four-year follow ups. Perceptions and economic attitudes are measured at the two-year midline and four-year endline.

Balance. Table 2.1 shows samples are balanced on village characteristics measured from the census, across treatment arms. Table 2.B1 shows balance when pooling the two treatment arms. On most dimensions the samples are well balanced (whether we pool or split treatment arms).

Panel A of Table 2.1 shows that villages are small, with 400 households in each. The average distance between treated and control villages is 13km, 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 2.2 shows balance on household characteristics, splitting for the across and within village randomization. Table 2.B2 shows the same test of household balance pooling the two treatment arms. On most dimensions the samples are again well balanced on household characteristics (whether we pool or split the treatment arms).

Panel A shows characteristics measured in the census: poor households have a poverty score of 13, while NP households have a score of 34 (there is far more variation in the poverty scores of the NP because they are drawn from across all deciles of poverty). Poor households are larger. Heads of household are nearly always male, aged around 41: in poor households the majority have no formal education, but even among the NP, over 40% have no formal education. 90% of household heads are engaged in some form of income generating labor activity.

Panel B shows livestock ownership and consumption at baseline (that are not available for NTP households as they were not surveyed at baseline). Around 55% of poor households in controls own livestock, rising to 64% in non poor households. Monthly food expenditure per adult equivalent is around $80 for the poor, and 20% higher among the non poor.

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

Attrition. Table 2.B3 shows that households are more likely to attrit from treated villages irrespective of the intervention type. Poor households are 4pp to 6pp more likely to attrit from treated than control villages (of whom 5 to 7 percent attrit by endline). These magnitudes are small, in line with comparable studies, and mostly occur in the first year post intervention. In each treatment arm, we cannot reject the null that attrition is the same across all groups between midline and endline (when perceptions and attitudes are measured). At the four-year endline, we cannot reject the null that attrition in each treatment arm is the same for all groups.

2.3 Economic Circumstances

2.3.1 Empirical Method

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

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

$$y_{hvt} = \alpha + \sum_{j=1,2} \sum_{t=1,2,4} \beta_{jt} \left(T_{jv} \times W_t \times P_h \right) + \tau_t + \lambda_s + u_{hvt} \tag{2.1}$$

where P_h is a dummy indicating poor households, W_t for $t \in \{1, 2, 4\}$ indicates survey waves, τ_t and λ_s are survey wave and strata fixed effects, and standard errors are clustered by village. The NTP are the omitted group from the regression.

2.3.2 Noticeable Impacts

Table 2.3 shows the results. For the TP relative to the NTP, there are large and sustained treatment effects of each intervention on livestock ownership, the value of livestock owned and consuming own produced milk. The magnitude of impacts are of economic significance: for the TP in T1, livestock ownership increases by 20pp, a 35% increase over the baseline mean for the poor in controls, the value of livestock owned increases by between 10-15% across all periods and interventions, and by the four-year endline, the consumption of own produced milk increases by around 25%.

Two other points are of note. First, gains to the TP relative to the NTP accrue within a year post-intervention, and stabilize thereafter until endline. The treated poor thus experience a pattern of immediate changes in economic circumstances following the transfer of assets or cash, with gains persisting, but not accumulating further.

Second, both big push interventions have similar impacts: at the foot of table we report p-values of the equality of treatment effects by survey wave. With the exception of livestock ownership—that increases significantly more for those offered in-kind asset transfers in T1—all other treatment effects do not differ by intervention and period. Hence for the purpose of studying economic preferences, we pool treatments for the remainder of the analysis. We showed earlier in Tables 2.B1 and 2.B2 that the samples are balanced on village and household characteristics between controls and pooled treated villages and households.

Table 2.4 repeats the exercise pooling treatments, allowing gains to be estimated more precisely in each wave. We find that across all margins, TP households have significant impacts relative to the NTP. The TP have a 16% increase in livestock ownership (corresponding to a 29% increase over baseline), the value of livestock owned increases by around 14%, they are 4pp more likely to have an iron roof one year post-intervention (an 11% increase over baseline), are around 20% more likely to have improved diets as measured through the consumption of own produced milk, and have gains in food consumption of around 3% over baseline (the short run fall in consumption might reflect the switch from market purchased dairy products to home production).

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

$$y_{hvt}^{g} = \alpha^{g} + \sum_{t=1,2,4} \beta_{t}^{g} (T_{v} \times W_{t}) + \tau_{t}^{g} + \lambda_{s}^{g} + u_{hvt}^{g} \tag{2.2}$$

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

Table 2.B4 presents the spillover results: we see little evidence that economic outcomes shift for not treated poor or not poor households relative to counterfactuals in controls. The point estimates on many of the estimates are close to zero, suggesting weak within village spillovers on these specific outcomes.[7]

Given that treated and not treated poor households are balanced on observables at baseline and the lack of spillovers onto others, the magnitudes of the gains to the TP imply that many of the NTP are overtaken by their TP neighbors along these margins. These changes in relative standing will be noticeable given that half of all eligibles in treated villages are actually treated. Changes in relative economic standing can shape some attitudes of the TP and NTP if they have concerns for their relative standing or last place aversion (Duesenberry, 1949; Luttmer, 2005; Card et al., 2012; Kuziemko et al., 2014).

[7] Consistent with this, in their meta-analysis of asset transfer interventions across six countries, Banerjee et al. (2015b) 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).

2.3.3 Village Inequality

Our results so far replicate findings from the literature that big push interventions impact levels of economic outcomes (Banerjee et al., 2015b; Haushofer and Shapiro, 2016; Bandiera et al., 2017; Blattman et al., 2020; Balboni et al., 2022; Egger et al., 2022). As a consequence, the NTP are overtaken in economic standing on a number of important margins. What has been less discussed in the literature is that such interventions can also impact overall levels of village inequality. This is especially the case in our context because villages are small and half the eligible poor, or 10% of all households (40 households per village), are actually treated. To examine the possibility, we estimate the following between village treatment effect on measures of consumption inequality, I_{vt}, for village v in survey wave t:

$$I_{vt} = \alpha + \sum_{t=1,2,4} \beta_t (T_v \times W_t) + \tau_t + \lambda_s + u_{vt} \tag{2.3}$$

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

Table 2.5 presents the results for three measures of inequality. In line with the dynamic impacts on consumption of the treated poor, reductions in inequality in food expenditure take a few years to materialize, but there are statistically significant reductions in consumption inequality at two- and four-years post intervention. The magnitudes 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

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

four-year endline.

2.4 Perceptions and Attitudes

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

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

$$y^g_{hvt} = \alpha^g + \sum_{t=2,4} \beta^g_t (T_v \times W_t) + \tau^g_t + \lambda^g_s + \lambda^g_e + u^g_{hvt} \tag{2.4}$$

where y^g_{hvt} is the perception of household head h in village v for period t. These outcomes relate to how they perceive their own economic standing in their village, what has happened to inequality in their village, and how they perceive the rich and poor more generally. We continue to pool interventions, and all other variables are as defined earlier. Given the nature of questions asked about perceptions, we include a full set of dummies for enumerators, λ_e. We cluster standard errors by village.[9]

Standard identifying assumptions for the treatment effects on each group are that there is random assignment, and that there are no spillovers onto controls. The effects on the perceptions of the NTP and NP capture their exposure to the pro-poor interventions, that can operate through them: (i) observing intervention impacts on the TP and village outcomes as a whole; (ii) any changes in their own economic circumstances occurring through spillovers or general equilibrium ef-

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

fects; (iii) any emotional connection with beneficiaries. As we come back to in our concluding discussion, all these channels are likely relevant given the close proximity of poor and non poor households and the likely complex set of family and economic network ties between them.

Exploiting the within-village randomization, we estimate treatment effects on the perceptions of TP relative to the NTP in treated villages from the following specification for household h in village v for period t:

$$y_{hvt} = \alpha + \sum_{t=2,4} \beta_t (T_v \times W_t \times P_h) + \tau_t + \lambda_s + \lambda_e + u_{hvt} \tag{2.5}$$

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

Throughout we report p-values on treatment effects at midline and endline, and also account for multiple hypothesis testing (MHT) by also presenting sharpened two-stage q-values (Benjamini et al., 2006; Anderson, 2008). These q-values conservatively account for the fact that for each outcome we test eight hypotheses, six related to the between village estimates ($\widehat{\beta_2}^g, \widehat{\beta_4}^g$) across group g at midline and endline, and two related to the within-village estimates ($\widehat{\beta_2}, \widehat{\beta_4}$).

2.4.1 Perceptions of Current and Future Standing

Current Standing. Motivated by an existing literature using non-experimental data to document households are imperfectly informed about their own relative standing (Benabou and Ok, 2001; Alesina and Angeletos, 2005; Hoy and Mager, 2021; Hvidberg et al., 2023), we start by examining how households' perceived own current economic standing is impacted by the interventions. This is perhaps the most closely linked perception to the reality of changed economic circumstances for the TP. We consider their perceived current standing by asking, *On a ladder with 10 steps, where do you currently stand?*. The results are in Table 2.6

where Panel A shows midline and endline impacts for TP, NTP and NP households as estimated from the between village specification (2.4). Panel B shows midline and endline impacts on the TP using the within village specification (2.5). Focusing first on the results for the TP in Column 1a, we see they report no change in their perceived own standing at midline or endline, despite measurable and persistent economic gains from the intervention to them. The 95% confidence interval at midline rules out a change larger than .096, or a 3% change over the baseline level.

In contrast, the NTP and NP report significant falls in their perceived own standing at midline, with both results being robust to MHT. This is in line with findings from higher income settings that individual well-being can fall when individuals observe changes in wealth/income in people around them (Luttmer, 2005; Card et al., 2012; Perez-Truglia, 2020; Cullen and Perez-Truglia, 2022). The results highlight the potential for pro-poor interventions to generate negative psychological spillovers to non-beneficiaries, although households appear to adapt to this by endline. Panel B highlights that within-village, the TP diverge significantly from the NTP in their own standing, a divergence in perceptions that is sustained until endline. This finding is robust to MHT, and to reiterate, this specification accounts for any village-level unobservables that are common drivers of perceptions of the TP and NTP in treated villages.[10]

Future Standing. Motivated by the literature emphasizing that perceived prospects for upward mobility (POUM) can shape redistributive demands (Piketty, 1995; Benabou and Ok, 2001; Fong, 2001; Alesina and La Ferrara, 2005; Alesina et al., 2018), we next consider whether exposure to big push interventions affects household perceptions of their future economic standing. We did so by asking household heads: *On a ladder with 10 steps, what is the best life you can achieve?*. We

[10]Haushofer et al. (2015) are among the few other experimental studies in a low-income setting to study how exogenous changes in the wealth of neighbors impacts psychological wellbeing. They also find increases in neighbors' wealth decrease life satisfaction (but with positive effects on the life satisfaction of beneficiaries), and also find evidence of adaptation, in that the negative spillover decreases over time.

estimate whether views of future standing across groups are impacted by the pro-poor interventions. The results are in the remaining Columns of Table 2.6. As Column 2a shows, the interventions have no impact on beneficiaries perceived social mobility. This is not true for the other groups. For the overtaken NTP in treated villages, by endline they have significantly higher expectations for their future than the poor in controls ($p = .037$, $q = .421$). For the NP the results differ again: they have significant declines in their future expected standing at midline, although these recover significantly by endline.

2.4.2 Perceptions of Village Inequality

We next ask whether households perceive the changes in village level inequality caused by the big push interventions. To examine this we asked household heads whether: (i) inequality in their village has decreased in the last three years; (ii) the share of households in the village that do not have enough to eat has fallen. The results are in Table 2.7.[11]

Panel A shows a near complete set of null impacts across both perceptions of inequality for the TP, NTP and NP. These null impacts are again quite precise. For example, on whether village inequality has decreased, the endline impact for TP households is −.011, where the 95% confidence interval rules out an impact larger than .053, or 16% of the view held by the TP in controls. On the more noticeable margin of others not having enough food to eat, we find generally negative point estimates but these are not significant except for the NP at midline. The endline impact for TP households is −.005, and the 95% confidence interval rules out an impact larger than .005, or 6% of the view held by the TP in controls.

Panel B confirms that within villages, perceptions of village inequality do not

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

significantly differ between the TP and NTP.

The measurable and persistent changes in village consumption inequality documented earlier thus largely do not translate into perceived changes among households of how inequality has changed in their village, irrespective of whether they are poor or non poor, irrespective of whether they are beneficiaries of these big push pro-poor interventions, and irrespective of the time frame considered. Our results build on work—mostly from high-income settings—documenting that individuals misperceive levels of economic inequality (Hauser and Norton, 2017; Gimpelson and Treisman, 2018)—to demonstrate that such misperceptions persist even in the face of large shifts in local economic circumstances.

2.4.3 Perceptions of the Rich

We have so far mapped the economic impacts of the interventions—through changes in the level, relative standing and inequality of outcomes across households—to perceptions of these changes. We now move to consider perceptions towards groups of households more widely: this goes beyond social preferences towards others, but rather the deservedness of the rich, and the causes of their status. More precisely, we first examine whether exposure to the interventions impacts how households perceive the rich. To do so we asked whether *the rich rightfully deserve their income*, where the outcome is whether the household head agreed/strongly agreed with the statement. Around a third of poor and non poor households in controls perceive the rich to be deserving. The result in Columns 1a to 1c of Table 2.8 shows that at midline all households in treated villages are significantly more likely hold this view. Relative to counterfactual households in controls, the TP are 7.5pp more likely to move towards this notion of the deserving rich (a 23% increase over controls), with the corresponding impact for the NTP being 5.7pp and for the NP we find a 7.2pp increase in this notion of the deserving rich.

Why are the Rich Rich? We probe the issue further in the remaining Columns of Table 2.8 by examining positive and negative opinions of how the rich in the village achieved their economic status. The positive view is elicited by asking respondents whether they believe the reason for the rich being rich are *education*, *intelligence or hard work*. The negative view is elicited by asking whether they believe the reason relates to ill-gotten gains through *illegal activities*. While we generally see little impact on positive perceptions towards the rich, in contrast, negative views towards the rich decline across groups—by endline the TP are 3.6pp less likely to think the rich are rich because of crime, relative to 11% of the poor holding this view in controls. The NTP share this change in belief: their likelihood to report a negative view of the rich falls 3.0pp by endline. Panel B confirms that within villages, views of the rich do not diverge significantly between the TP and NTP.

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

2.4.4 Perceptions of the Poor

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

Focusing first on perceptions of the character of the poor, we asked households whether they thought the poor: (i) *lack the ability to manage money or other assets*; (ii) *waste their money on inappropriate items*; (iii) *do not actively seek to improve their lives*; (iv) *are not motivated because of outside support from government/NGOs*. The non poor were only surveyed on these questions at endline. Table 2.9 shows the results where the outcome is whether the household head agreed or strongly agreed with each statement about the poor. To begin with we note that 30-40% of respondents in controls at midline agree/strongly agree with each statement, irrespective of whether they are themselves poor. The strongest agreement is for the view that the poor are not motivated because of outside support from government/NGOs. However, we find little evidence that perceptions of the character of the poor are shifted by the big push pro-poor interventions.

Why are the Poor Poor? Considering perceptions of the causes of poverty, we divide these causes as structural features of the economy leading to poverty, versus the view of poverty as destiny/fate. On structural causes, we asked households whether they thought the poor were poor because: (i) *they are exploited by rich people*; (ii) *society fails to help and protect the most vulnerable*; (iii) *the distribution of land between poor and rich people is uneven/unequal*; (iv) *they lack opportunities due to the fact that they come from poor families*. Table 2.10 shows the results. In each case the outcome is whether the household head agreed or strongly agreed with the statement. We see that 70-80% of respondents in controls at midline agree/strongly agree with each statement about the structural causes of poverty, irrespective of whether they are themselves poor. The belief in structural causes of poverty is thus far more prevalent among all households than negative views of the character of the poor.

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

On poverty as destiny/fate, we asked households whether they thought the poor were poor because: (i) *they are unlucky*; (ii) *they have encountered misfortunes*; (iii) *they have bad fate/destiny*. Table 2.11 shows the results. The perception that poverty is one's destiny is generally less prevalent among controls than the view that poverty is down to structural causes. The interventions do little to shift perceptions of poverty as destiny/fate among the TP or NTP. However, among the NP, by endline we find significant increases in agreement with the view that the poor are poor because of being unlucky or having bad fate/destiny.[12]

2.4.5 Attitudes Towards Redistribution

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

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

We next take these predictions to data. While there are many potential ways to measure redistributive preferences, we anchor our results by following the influential work of Kuziemko et al. (2015) and construct an index of redistributive preferences based on four questions. The first is a blanket statement of views on

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

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

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

The results are in Table 2.12. Using either the between village specification reported in Panel A or the within village specification reported in Panel B, we find little shift in the redistributive attitudes of any group in either time period. For example, among the TP at midline we can rule out an increase in the redistributive attitudes index greater than .105 or 3% of its baseline level in controls. The

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

null impacts on the index overall are despite the fact that we have shown earlier that one component of the index—related to perceptions towards the rich—does shift at midline in a direction that makes them hold less redistributive attitudes. To understand whether this shift towards less redistributive attitudes is offset by other components of the index, the remaining Columns of Table 2.12 show results for the other three components of the index.

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

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

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

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

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

Revisiting these results through the lens of theory, we note that MR has the basic prediction that the redistributive preferences of the TP should weaken as they economically gain from receipt of the asset/cash transfers. This is exactly in line with their response at midline. However, our partial population experiment reveals similar shifts occur among the NTP and NP, in contradiction to the MR model, and more in line with community-wide attitudinal shifts shaped by exposure to the interventions rather than beneficiary status *per se*. Moreover, the long run impacts we estimate establish that attitudinal shifts do not persist, again counter to the MR model.

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

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

2.4.6 Ideal Income Distribution

To gauge redistributive preferences from another societal perspective, we asked households about their ideal income distribution. Panel A of Figure 2.1 shows the choices presented to households, alongside a description of each. The choices vary the position of the modal household, ranging from Distribution A—where a mass of the population remains poor, through to the top heavy Distribution E. Panel B shows the ideal distributions reported in controls at midline, splitting reports by the poor and non poor. Preferences across distributions are similar across groups. The most favored distribution is D (chosen by 35%): where the modal household resides in the middle classes, and there are few households in the tails of the distribution. Bottom-heavy Distributions A and B are the least preferred (chosen by fewer than 10%).[16]

We estimate between village treatment effects on each distribution being reported as the ideal one. Panels C and D summarize 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.

2.5 Discussion

Big push pro-poor interventions hold immense promise for pulling the world's poorest out of poverty. In this paper we move beyond the existing evidence base of economic impacts of such interventions, to study their impacts on perceptions of changed economic circumstances in their village, and related attitudes towards redistribution. We do so using a partial population experiment that combines layers of between and within village randomization, tracking over 15,000 rural households that are either the treated poor, not treated poor or not poor, for four years.

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

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

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

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

We conclude by discussing three issues. First, we consider whether big push

[17] We show big push interventions can drive perceptions and attitudes even when those experiences occur late in life—our household heads are aged in their early 40s at baseline. However, we do not find evidence that such shifts in perceptions and attitudes persist. This complements work emphasizing how experiences in formative years are more likely to determine long run attitudes and behaviors (Malmendier, 2021; Margalit, 2019; Giuliano and Spilimbergo, 2022).

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

2.5.1 Voting

Between baseline and midline high stakes local elections were held across our study region. We thus probe the possibility of lasting impacts of the big push interventions occurring through political processes—rather than stated perceptions or attitudes—using self-reported data on turnout in these elections. Of course such self-reports are 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 2.13. We find all groups become significantly more likely to report voting in local elections: the TP are 5.8pp more likely, and the NTP are 5.1pp more likely—both impacts are significant at the 1% level and robust to MHT. However, the largest increase is seen among the NP, who are 9.2pp more likely to self-report having voted.[18]

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

[18] As a benchmark, Giné 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 et al., 2012), Uruguay (Manacorda et al., 2011), and Mexico (De La O, 2013).

or right-leaning. Our classification suggests that in controls, around 14% of poor household heads are left leaning, 69% are centrist and 16% are right leaning.[19]

The remaining Columns in Table 2.13 show heterogeneous impacts on voting by political affinity expressed at baseline. Household heads of all political affinities significantly increase their likelihood to vote. Among the TP, the largest effects are among left- and right-leaning households, although the impacts are not significantly different across political preferences. Among the NP, the largest point estimate is for right-leaning households, that increase their voting by 11.4pp, but again these are not different from the impacts on left-leaning households ($p =$ .208). Overall, while the evidence suggests interventions increase political participation across the board, this does not differ by political affinities expressed at baseline.

2.5.2 Asset Transfers versus Revealed Preferred Cash Transfers

We exploit the treatment arms to examine whether in-kind asset transfers and reveal preferred unconditional cash transfers have similar impacts on perceptions and attitudes. These results are summarized in Figures 2.B2 to 2.B4. Each panel shows the estimated treatment effect $(\widehat{\beta_{2j}}^g, \widehat{\beta_{4j}}^g)$ for group g and treatment arm j from the between village estimates, and $(\widehat{\beta_{2j}}, \widehat{\beta_{4j}})$ from the within-village estimates, and we indicate whenever impacts differ across treatment arms. Treatment T1 refers to when the poor are offered a menu of in-kind asset transfers. Treatment T2 refers to when households are additionally offered the equivalent valued cash

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

transfers, and the majority reveal prefer cash over in-kind transfers.

On most dimensions, we find little differential impact on perceptions and attitudes, for any group and in either time period, between when the poor are assisted with asset or cash transfers. More precisely, Figure 2.B2 focuses on perceptions of own standing and perceptions of inequality, so outcomes considered in Tables 2.6 and 2.7. For the four perceptions considered, we see the between and within village estimates are largely the same across treatment arms, and this is the case for each group of households, and across both midline and endline estimates. The most notable difference is for the perception of future economic standing, where at midline this is higher for the TP and the NTP if the poor receive assets rather than cash ($p = .065, .029$, respectively).

Figure 2.B3 summarizes perception of the rich and poor, so outcomes considered in Tables 2.8 to 2.11. Shifts in the 14 perceptions and views considered largely do not differ depending on whether the poor are provided asset transfers, or reveal prefer cash over in-kind transfers. The views on which the metric of transfers matters most are: (i) that the rich are rich for positive reasons such as education/hard work, where this shift at endline is greater among the TP and NTP if the poor are provided asset transfers ($p = .012, .064$, respectively); (ii) that the poor are poor because they do not actively seek to improve their lives, where the shift at midline is greater among the TP and NTP if the poor are provided asset transfers ($p = .099, .045$, respectively).

Finally, Figure 2.B4 summarizes the results for attitudes towards redistribution and voting, the five outcomes in Tables 2.12 and 2.13. Nearly all of these margins have impacts that do not statistically differ depending on the form of big push assistance to the poor.

2.5.3 Future Agenda and External Validity

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

Our results also suggest a far broader agenda for future work. As highlighted throughout, there is the need to develop theory to microfound the link between whether and how large noticeable changes in economic circumstances translate into perceptions of those changes. In our context, the fact that beneficiary and non-beneficiary households reside next to each other and are likely tied through social networks or networks of economic exchange might play an important role in how reality maps into perceptions. We highlight three other areas for future work based on dimensions of our data that are likely critical for thinking through the external validity of our findings to other settings and interventions.

Setting. Villages in our field experiment are close-knit and ethnically homogeneous. This makes them an almost ideal setting in which to study the link between changes in economic circumstances and perceptions of those changes: there are large and persistent real world shifts in noticeable economic gains, changes in relative economic standing, and reductions in village inequality. However, in more geographically dispersed settings, economic impacts on beneficiaries might not be so noticeable. Alternatively, in more diverse or ethnically fragmented settings, perceptions of targeting biases, or actual targeting biases of local delivery agents across groups, might be first order (Londoño-Vélez, 2022; Bandiera et al., 2023). It thus remains an open question to understand whether in such settings, pro-poor interventions are more likely to lead to polarization or conflict in perceptions and attitudes than we find in our study setting.

Financing Interventions. Our results suggest the link between pro-poor policy interventions, economic reality, and perceptions, does not depend on whether households are themselves beneficiaries—rather our partial population experiment reveals that perceptions are largely driven by common village-wide exposure to such pro-poor policies. However, the big push interventions studied are financed and delivered by a quasi-governmental NGO—they are not financed through general taxation, nor through informal local taxation. The perceptions and attitudes of the rich (non-eligibles) might be impacted very differently by pro-poor interventions when they are implicitly financing them or when they come at the expense of some other policy or local public good they favor. It remains an open question to understand how perceptions across households might be shifted when within-village redistributive institutions, such as local taxation schemes, are used to target resources to the poor, and whether such financed pro-poor interventions are more likely to lead to polarization or conflict in perceptions and attitudes than we find in our setting.

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

2.6 Tables and Figures

Table 2.1. 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 2.2. Balance on Household Characteristics

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

	Control		T1: Asset Transfer			T2: Revealed Preferred Unconditional Cash Transfer			Treated Poor			Not Treated Poor			Non Poor		
	(1) P	(2) NP	(3) TP	(4) NTP	(5) NP	(6) TP	(7) NTP	(8) NP	C = T1	C = T2	T1 = T2	C = T1	C = T2	T1 = T2	C = T1	C = T2	T1 = T2
Panel A. Household Characteristics (census)																	
Poverty score (1-100)	13.1	34.2	13.6	13.6	34.3	13.4	13.6	33.8	[.050]	[.221]	[.610]	[.133]	[.929]	[.258]	[.946]	[.815]	[.772]
	(3.91)	(12.6)	(3.54)	(3.72)	(11.9)	(3.84)	(3.71)	(12.0)									
Household size	7.63	5.07	7.60	7.60	4.93	7.58	7.60	5.07	[.802]	[.489]	[.752]	[.820]	[.407]	[.347]	[.837]	[.839]	[.726]
	(2.32)	(2.53)	(2.09)	(2.05)	(2.42)	(2.16)	(2.05)	(2.45)									
Female headed household	.018	.026	.010	.018	.024	.020	.018	.027	[.106]	[.705]	[.075]	[.859]	[.645]	[.487]	[.664]	[.948]	[.565]
Age of household head	41.4	42.5	41.6	40.9	41.9	41.5	40.9	42.0	[.924]	[.861]	[.935]	[.781]	[.496]	[.737]	[.818]	[.566]	[.762]
	(12.2)	(15.8)	(12.3)	(12.0)	(15.6)	(12.4)	(12.0)	(15.6)									
Household head has no formal education	.549	.433	.529	.538	.412	.586	.538	.418	[.174]	[.848]	[.121]	[.280]	[.537]	[.556]	[.569]	[.789]	[.744]
Household head is currently working	.931	.893	.934	.927	.908	.936	.927	.891	[.761]	[.432]	[.741]	[.453]	[.208]	[.552]	[.404]	[.851]	[.294]
Panel B. Household Welfare (baseline)																	
Own any livestock	.542	.638	.572		.607	.556		.605	[.450]	[.757]	[.650]				[.518]	[.285]	[.757]
Monthly food expenditure (AE, US$ PPP)	82.1	98.7	82.7		100	84.6		99.5	[.304]	[.085]	[.608]				[.516]	[.748]	[.651]
	(35.8)	(45.4)	(35.1)		(45.1)	(37.1)		(42.9)									
Non food expenditure (pc, US$ PPP)	18.1	28.0	18.2		29.7	19.8		30.5	[.641]	[.076]	[.215]				[.454]	[.194]	[.604]
	(13.4)	(24.3)	(15.2)		(28.9)	(15.2)		(29.2)									
Panel C. Attitudes (census)																	
Government is effective	.271	.256	.265	.238	.257	.275	.238	.295	[.919]	[.836]	[.921]	[.784]	[.926]	[.763]	[.888]	[.468]	[.718]
NGOs are effective	.274	.276	.231	.248	.248	.280	.248	.319	[.710]	[.707]	[.426]	[.712]	[.420]	[.285]	[.657]	[.544]	[.302]
Private sector is effective	.196	.183	.154	.181	.196	.182	.181	.216	[.686]	[.985]	[.633]	[.854]	[.710]	[.611]	[.830]	[.566]	[.843]
Government represents people like me	.196	.213	.163	.198	.225	.131	.199	.182	[.349]	[.059]	[.449]	[.812]	[.324]	[.621]	[.992]	[.385]	[.610]
People can affect government policies	.310	.269	.288	.331	.294	.253	.331	.282	[.666]	[.291]	[.524]	[.992]	[.326]	[.389]	[.739]	[.876]	[.827]

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

Table 2.3. Noticeable Economic Impacts

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

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

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

Table 2.4. Noticeable Economic Impacts, Pooled Specification

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

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

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

Table 2.5. 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 2.6. Perception of Current and Future Standing

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

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

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions in Panel A compare Treated Poor (Columns 1a, 2a), Not Treated Poor (Columns 1b, 2b), and Not Poor (Columns 1c, 2c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a). All regressions include treatment dummies (pooling T1 and T2), district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village-survey wave level, and 95% confidence intervals are reported in brackets. For the first outcome, respondents were shown a picture of a ladder and were told, "The top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you." We then asked "On which step of the ladder would you say you personally feel you stand at this time?" The second outcome is based on a similar ladder of life wording as the first, except respondents are then asked to name the highest rung of the ladder they could achieve in future. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention.

Table 2.7. Perceptions of Village Inequality

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

	Inequality decreased in the last three years			Share in village that do not have enough to eat		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP
A. Between Village Estimates (Treated vs Control)						
Two year impact	.037	.011	.002	-.013	-.012	-.024**
	(.031)	(.033)	(.027)	(.009)	(.009)	(.011)
	[.236]	[.737]	[.934]	[.187]	[.186]	[.031]
	{1.00}	{1.00}	{1.00}	{.775}	{.775}	{.330}
Four year impact	-.011	-.008	-.011	-.005	-.002	-.004
	(.032)	(.032)	(.028)	(.004)	(.005)	(.006)
	[.744]	[.813]	[.700]	[.318]	[.619]	[.533]
	{1.00}	{1.00}	{1.00}	{.803}	{.995}	{.995}
Two Year = Four Year	[.378]	[.749]	[.711]	[.473]	[.405]	[.165]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)						
Two year impact	.018			-.001		
	(.017)			(.004)		
	[.329]			[.902]		
	{1.00}			{1.00}		
Four year impact	-.012			-.002		
	(.020)			(.002)		
	[.549]			[.254]		
	{1.00}			{.801}		
Two Year = Four Year	[.243]			[.764]		
Mean Outcome, Controls	34.0%		38.8%	9.05%		10.8%
Observations: Panel A	8126	9382	17004	8126	9382	17004
Observations: Panel B	8262			8262		

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

Table 2.8. Perceptions of the Rich

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

	The rich rightfully deserve their income			Reason rich: education, intelligence, hard work			Reason rich: illegal activities		
	(1a) TP	**(1b) NTP**	**(1c) NP**	**(2a) TP**	**(2b) NTP**	**(2c) NP**	**(3a) TP**	**(3b) NTP**	**(3c) NP**
A. Between Village Estimates (Treated vs Control)									
Two year impact	.075***	.057*	.072***	-.005	.011	-.021	-.014	-.015	-.022**
	(.032)	(.030)	(.027)	(.022)	(.019)	(.015)	(.015)	(.015)	(.010)
	[.021]	[.062]	[.010]	[.838]	[.557]	[.170]	[.351]	[.323]	[.031]
	{.087}	{.142}	{.087}	{1.00}	{1.00}	{1.00}	{.541}	{.541}	{.153}
Four year impact	-.017	.005	-.001	.028	.036*	.012	-.036**	-.030*	-.001
	(.030)	(.031)	(.025)	(.022)	(.019)	(.019)	(.016)	(.015)	(.011)
	[.563]	[.876]	[.976]	[.220]	[.060]	[.533]	[.033]	[.058]	[.932]
	{.603}	{.954}	{.954}	{1.00}	{.924}	{1.00}	{.153}	{.153}	{1.00}
Two Year = Four Year	[.060]	[.327]	[.061]	[.268]	[.377]	[.168]	[.419]	[.533]	[.166]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)									
Two year impact	.017			-.010			.002		
	(.023)			(.017)			(.009)		
	[.472]			[.563]			[.849]		
	{.601}			{1.00}			{1.00}		
Four year impact	-.024			-.002			-.005		
	(.016)			(.015)			(.012)		
	[.145]			[.914]			[.663]		
	{.222}			{1.00}			{1.00}		
Two Year = Four Year	[.208]			[.707]			[.611]		
Mean Outcome, Controls	32.3%		31.0%	30.0%		33.5%	11.2%		11.0%
Observations: Panel A	8126	9382	17004	8126	9382	17004	8126	9382	17004
Observations: Panel B	8262			8262			8262		

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

Table 2.9. Perceptions of the Character of the Poor

Strongly agree or agree with statements
OLS estimates, standard errors clustered by village in parantheses
p-values in brackets, FDR adjusted q-values in braces

	They lack the ability to manage money or other assets			They waste their money on inappropriate items			They do not actively seek to improve their lives			They are not motivated because of outside support from government/NGOs		
	(1a) TP	**(1b) NTP**	**(1c) NP**	**(2a) TP**	**(2b) NTP**	**(2c) NP**	**(3a) TP**	**(3b) NTP**	**(3c) NP**	**(4a) TP**	**(4b) NTP**	**(4c) NP**
A. Between Village Estimates (Treated vs Control)												
Two year impact	.030	.059*		.008	.036		.018	.033		.007	.014	
	(.030)	(.034)		(.030)	(.032)		(.036)	(.034)		(.039)	(.040)	
	[.321]	[.088]		[.804]	[.254]		[.608]	[.325]		[.854]	[.725]	
	{1.00}	{1.00}		{1.00}	{1.00}		{1.00}	{1.00}		{1.00}	{1.00}	
Four year impact	-.021	-.004	-.004	-.003	.006	-.011	.006	.015	-.001	.008	-.004	.008
	(.026)	(.027)	(.019)	(.030)	(.032)	(.024)	(.032)	(.030)	(.021)	(.030)	(.029)	(.020)
	[.423]	[.891]	[.831]	[.919]	[.850]	[.657]	[.863]	[.629]	[.950]	[.805]	[.902]	[.700]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Two Year = Four Year	[.289]	[.247]		[.839]	[.585]		[.830]	[.743]		[.995]	[.768]	
B. Within Village Estimates (Treated Poor vs Not Treated Poor)												
Two year impact	-.021			-.019			-.006			.002		
	(.015)			(.017)			(.016)			(.018)		
	[.174]			[.257]			[.719]			[.926]		
	{1.00}			{1.00}			{1.00}			{1.00}		
Four year impact	-.007			.001			-.000			.020		
	(.016)			(.015)			(.020)			(.018)		
	[.644]			[.963]			[.990]			[.252]		
	{1.00}			{1.00}			{1.00}			{1.00}		
Two Year = Four Year	[.616]			[.456]			[.842]			[.486]		
Mean Outcome, Controls	.330		.256	.357		.348	.362		.333	.400		.413
Observations: Panel A	7505	8502	8039	7537	8551	8089	7527	8530	8065	7271	8195	7757
Observations: Panel B	7499			7544			7527			7204		

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

Table 2.10. Poverty as Driven by Structural Causes

Strongly agree or agree with statements

OLS estimates, standard errors clustered by village in parantheses

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

	They are exploited by rich people			Society fails to help and protect the most vulnerable			The distribution of land between poor and rich people is uneven /unequal			They lack opportunities due to the fact that they come from poor families		
	(1a) TP	**(1b) NTP**	**(1c) NP**	**(2a) TP**	**(2b) NTP**	**(2c) NP**	**(3a) TP**	**(3b) NTP**	**(3c) NP**	**(4a) TP**	**(4b) NTP**	**(4c) NP**
A. Between Village Estimates (Treated vs Control)												
Two year impact	-.052*	-.062**		-.075**	-.093***		-.067**	-.062**		-.057**	-.101***	
	(.028)	(.024)		(.030)	(.031)		(.028)	(.030)		(.026)	(.026)	
	[.068]	[.011]		[.014]	[.004]		[.017]	[.041]		[.029]	[.000]	
	{.257}	{.084}		{.044}	{.029}		{.136}	{.141}		{.085}	{.001}	
Four year impact	-.000	-.017	-.026	-.026	-.023	-.027	-.011	-.017	-.007	-.013	-.035	-.012
	(.025)	(.025)	(.023)	(.025)	(.025)	(.020)	(.025)	(.026)	(.022)	(.022)	(.023)	(.017)
	[.995]	[.499]	[.265]	[.310]	[.361]	[.165]	[.659]	[.513]	[.739]	[.553]	[.142]	[.484]
	{1.00}	{1.00}	{.792}	{.565}	{.565}	{.380}	{1.00}	{1.00}	{1.00}	{.331}	{.166}	{.331}
Two Year = Four Year	[.252]	[.308]		[.324]	[.159]		[.238]	[.375]		[.282]	[.105]	
B. Within Village Estimates (Treated Poor vs Not Treated Poor)												
Two year impact	.003			.015			-.006			.036**		
	(.017)			(.019)			(.018)			(.017)		
	[.848]			[.435]			[.730]			[.039]		
	{1.00}			{.569}			{1.00}			{.085}		
Four year impact	.008			-.005			.008			.014		
	(.015)			(.015)			(.012)			(.016)		
	[.582]			[.743]			[.514]			[.372]		
	{1.00}			{.738}			{1.00}			{.331}		
Two Year = Four Year	[.828]			[.397]			[.544]			[.393]		
Mean Outcome, Controls	.795		.767	.796		.751	.807		.762	.803		.756
Observations: Panel A	7522	8530	8065	7403	8353	7842	7375	8302	7816	7440	8411	7937
Observations: Panel B	7526			7332			7285			7399		

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

Table 2.11. Poverty as Destiny or Fate

Strongly agree or agree with statements
OLS estimates, standard errors clustered by village in parantheses
p-values in brackets, FDR adjusted q-values in braces

	They are unlucky			They have encountered misfortunes			They have bad fate/destiny		
	(1a) TP	(1b) NTP	(1c) NP	(2a) TP	(2b) NTP	(2c) NP	(3a) TP	(3b) NTP	(3c) NP
A. Between Village Estimates (Treated vs Control)									
Two year impact	-.036	-.012		-.054	-.048		-.040	-.038	
	(.036)	(.037)		(.034)	(.036)		(.035)	(.032)	
	[.318]	[.741]		[.116]	[.186]		[.257]	[.248]	
	{.956}	{.956}		{1.00}	{1.00}		{.540}	{.540}	
Four year impact	.006	.031	.045*	.012	.016	.023	.027	.015	.052**
	(.028)	(.027)	(.025)	(.028)	(.027)	(.023)	(.026)	(.026)	(.022)
	[.827]	[.267]	[.080]	[.680]	[.555]	[.315]	[.292]	[.574]	[.022]
	{.956}	{.956}	{.956}	{1.00}	{1.00}	{1.00}	{.540}	{.692}	{.183}
Two Year = Four Year	[.452]	[.458]		[.239]	[.243]		[.214]	[.334]	
B. Within Village Estimates (Treated Poor vs Not Treated Poor)									
Two year impact	-.018			-.002			.001		
	(.019)			(.024)			(.020)		
	[.349]			[.924]			[.942]		
	{.956}			{1.00}			{.692}		
Four year impact	-.019			.002			.018		
	(.017)			(.018)			(.014)		
	[.275]			[.934]			[.206]		
	{.956}			{1.00}			{.540}		
Two Year = Four Year	[.975]			[.908]			[.533]		
Mean Outcome, Controls	.484		.417	.489		.395	.391		.285
Observations: Panel A	7518	8532	8040	7426	8399	7926	7526	8535	8006
Observations: Panel B	7530			7373			7537		

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

Table 2.12. Redistributive Attitudes

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

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

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

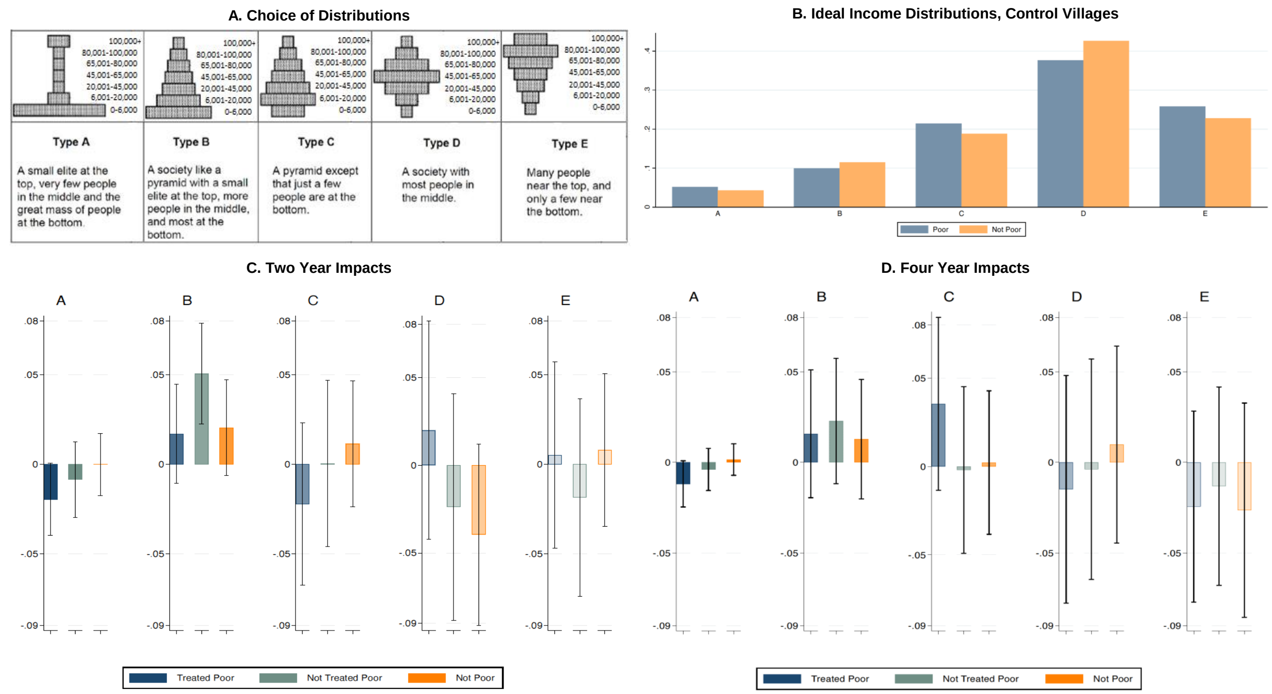
Table 2.13. Voting

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

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

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

Figure 2.1. Ideal Income Distributions



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

2.A Appendix

Luck versus Merit. Redistributive attitudes might depend on whether luck or effort are viewed as responsible for individual success (Piketty, 1995; Benabou and Ok, 2001; Fong, 2001; Alesina and Angeletos, 2005).[20] To consider this, we follow the approach of Almås et al. (2020) in asking household heads questions related to a redistributive task, where we vary whether income differences between individuals arise because of luck or merit. We inform respondents that *two people have randomly been allocated PKR 5'000 and PKR 15'000. The recipients have been told about the allocation.* We then ask, *should the government forcefully reallocate the money?* We then repeat the exercise but initially inform respondents, *two people have been allocated PKR 5'000 and PKR 15'000 based on test scores (where a higher test score implies higher reward).* The contrast in wording is designed to change the circumstances under which this inequality has been created: luck or merit, and to capture distributional preferences without the confounding influence of material self-interest. The results are in Table 2.B5. We see little evidence that behavior in the redistributive task of any group, at either midline or endline, is impacted by the intervention irrespective of whether inequalities are initially framed as being driven by luck or merit.

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

We can examine the issue in our context given both treatments were imple-

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

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

mented in collaboration with quasi-government agencies, and so the interventions are best perceived as government delivered programs. Table 2.B6 shows the results, where we estimate treatment effects on the index of redistributive attitudes by baseline views on the effectiveness of government. Recall that around a quarter of household heads believe government is effective (Table 2.2). Irrespective of households' pre-intervention beliefs over the effectiveness of government, we replicate the broad findings on redistributive attitudes documented earlier. In no case do we find significant differences in intervention responses based on beliefs on government effectiveness. This holds across TP, NTP and NP households, at midline and endline.[22]

[22]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.

2.B Appendix Tables and Figures

Table 2.B1. Balance on Village Characteristics

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

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

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

Table 2.B2. Balance on Household Characteristics

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

	Control		Treated			Treated Poor	Not Treated Poor	Non Poor
	(1) P	**(2) NP**	**(3) TP**	**(4) NTP**	**(5) NP**	**C = T**	**C = T**	**C = T**
Panel A. Household Characteristics (census)								
Poverty score (1-100)	13.1	34.2	13.5	13.3	34.1	[.055]	[.340]	[.944]
	(3.91)	(12.6)	(3.70)	(3.84)	(11.9)			
Household size	7.63	5.07	7.59	7.56	4.99	[.578]	[.733]	[.950]
	(2.32)	(2.53)	(2.12)	(2.14)	(2.43)			
Female headed household	.018	.026	.015	.019	.026	[.602]	[.834]	[.823]
Age of household head	41.4	42.5	41.5	40.9	42.0	[.873]	[.594]	[.657]
	(12.2)	(15.8)	(12.4)	(12.1)	(15.6)			
Household head has no formal education	.549	.433	.559	.541	.414	[.531]	[.305]	[.611]
Household head is currently working	.931	.893	.935	.920	.901	[.517]	[.174]	[.668]
Panel B. Household Welfare (baseline)								
Own any livestock	.542	.638	.563		.606	[.551]		[.337]
Monthly food expenditure (AE, US$ PPP)	82.1	98.7	83.7		99.8	[.135]		[.581]
	(35.8)	(45.4)	(36.1)		(44.0)			
Non food expenditure (pc, US$ PPP)	18.1	28.0	19.0		30.1	[.179]		[.253]
	(13.4)	(24.3)	(15.2)		(29.0)			
Panel C. Attitudes (census)								
Government is effective	.271	.256	.270	.256	.274	[.849]	[.903]	[.663]
NGOs are effective	.274	.276	.256	.299	.280	[.985]	[.773]	[.991]
Private sector is effective	.196	.183	.168	.204	.205	[.810]	[.913]	[.680]
Government represents people like me	.196	.213	.147	.181	.206	[.112]	[.498]	[.713]
People can affect government policies	.310	.269	.270	.301	.289	[.399]	[.569]	[.760]

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

Table 2.B3. Attrition

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

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

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions utilize the sample of treated poor and not treated poor households within treated villages using date from baseline, the one-, two and four-year follow ups. All regressions include treatment dummies (for T1 and T2 separately), district (strata) and survey wave fixed effects. Standard errors are clustered by village. The dependent variable is a dummy variable indicating attrition. Household controls include a dummy for whether the household head has any formal education, the age of the household head, household size, and the household poverty score. At the foot of each Column we report p-values on tests of equality of treatment effects between T1 and T2 at one, two and four years post intervention.

Table 2.B4. Spillovers onto Not Treated Poor and Not Poor Households, Pooled Specification

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

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

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

Table 2.B5. Luck versus Merit

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

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

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

Table 2.B6. Belief in Government Effectiveness

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

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

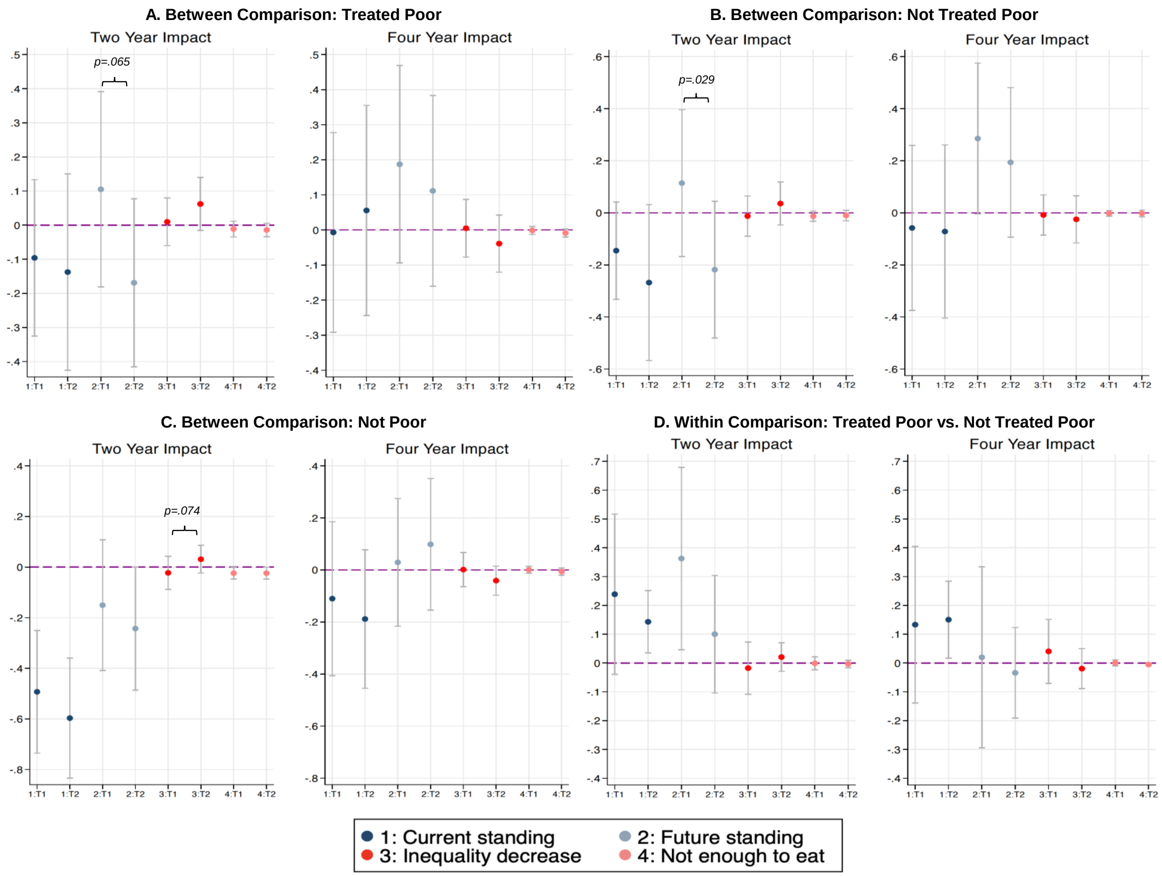
Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. Households with a score of 0-18 are deemed to be ultra-poor and hence eligible for the interventions. The regressions compare Treated Poor (Column 1a), Not Treated Poor (Column 1b), and Not Poor (Column 1c)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

Figure 2.B1. 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		

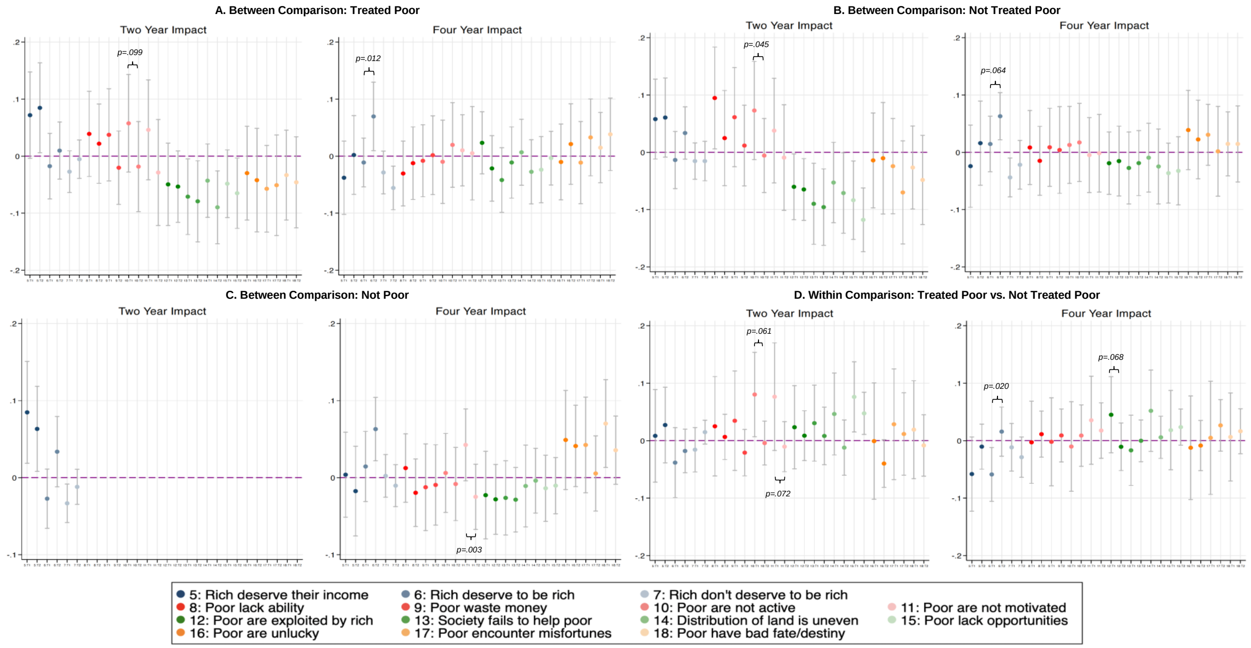
Notes: The figure presents a stylized example of an asset list that households were shown in both treatment arms. Households were allowed to choose any combination of assets they desired, up to a total value of PKR50K.

Figure 2.B2. Perceptions, Asset versus Cash Transfers



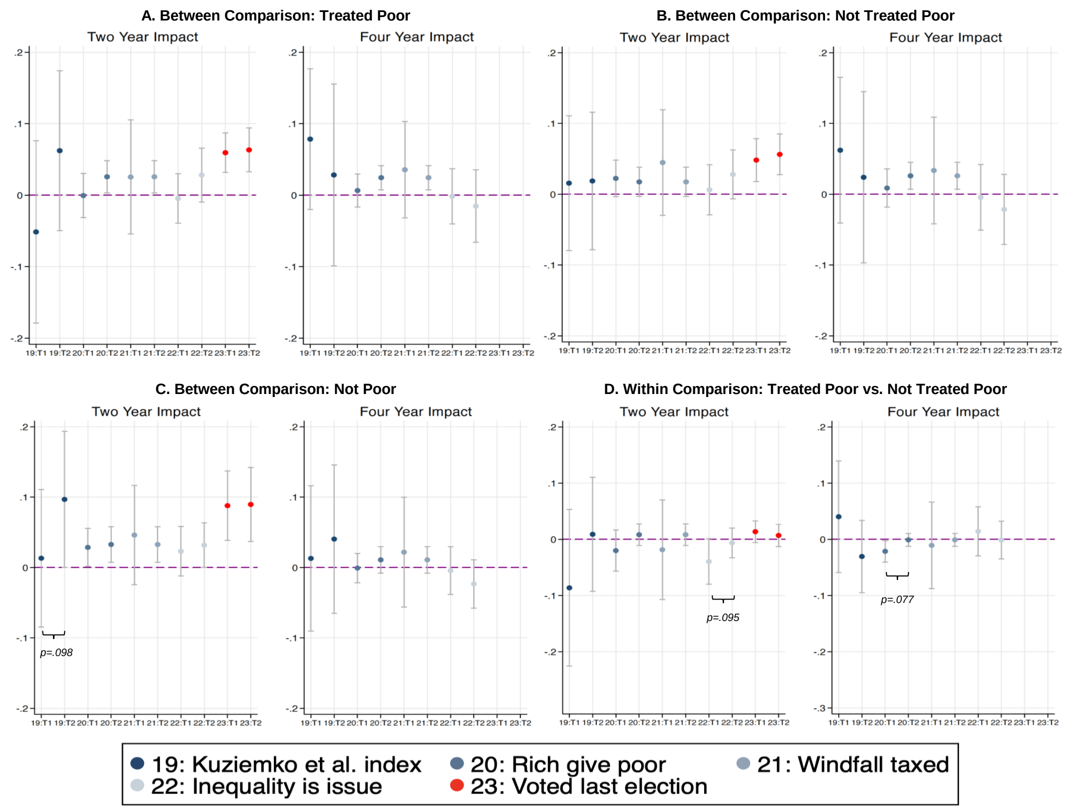
Notes: Panel A (B) [C] {D} displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households] {within estimates for the treated poor and not treated poor households}. For each specification we report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

Figure 2.B3. Perceptions of the Rich and Poor, Asset versus Cash Transfers



Notes: Panel A (B) [C] {D} displays the checks for the between estimates for treated poor households (between estimates for not treated poor households) [between estimates for the not poor households] {within estimates for the treated poor and not treated poor households}. For each specification we report the treatment effects for T1 and T2. The outcomes are the three perceptions of the rich reported in Table 8, the four perceptions of the poor reported in Table 9, views on the four structural causes of poverty reported in Table 10, and views on the three views on poverty as destiny or fate reported in Table 11 (that are not all available for not poor households at midline). Wherever treatment effects differ across arms, we report the p-value on the null of equality of treatment effects.

Figure 2.B4. Redistributive Attitudes and Voting, Asset versus Cash Transfers



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

Chapter 3

The Aggregate Impacts of Big Push Pro-poor Policies

Nicolas Cerkez, Adnan Q.Khan, Imran Rasul, and Anam Shoaib

3.1 Introduction

Social assistance programs, such as cash or assets transfers, have become increasingly popular in the developing world (Banerjee et al., 2024). Big push interventions — a specific type of these programs—have been shown to have positive and long-lasting impacts on the lives of the poor (Banerjee et al., 2015b; Haushofer and Shapiro, 2016; Bandiera et al., 2017; Blattman et al., 2020; Balboni et al., 2022; Egger et al., 2022).[1] This evidence base documents how individuals or firms—beneficiaries of the intervention—respond to interventions.

To gain a full understanding of the economic impacts of such big-push interventions, studying aggregate outcomes is important. This, however, is rarely done.[2] Interventions have been shown to positively influence the consumption be-

[1]See Crosta et al. (2024) for a recent review of large one-off unconditional cash transfers.

[2]How large shocks (e.g., cash infusions) affect aggregate outcomes has long been of interest to economists (Keynes, 1936). There is a growing body of evidence deriving (positive) fiscal multipliers in response to policy shocks in rich countries (Nakamura and Steinsson, 2014; Serrato and Wingender, 2016; Auerbach et al., 2019; Chodorow-Reich, 2019). The focus of this paper is on a developing country, where the evidence is scarce.

havior of non-beneficiaries in treatment villages (Angelucci and De Giorgi, 2009; Egger et al., 2022). The channels through which the effect operates are disputed: Angelucci and De Giorgi (2009) find that ineligible households dissave and receive more loans, while Egger et al. (2022) argue that ineligible households benefit from higher wages as firm revenues in treatment areas increase, which translates into higher wage bills and profits. Similarly, interventions have been shown to affect local prices (Cunha et al., 2019; Egger et al., 2022; Filmer et al., 2023); however, the magnitudes and severities of these effects differ significantly. Filmer et al. (2023), for example, find a large price increase in food items in response to a cash transfer in the Philippines and show that this is associated with increased stunting amongst children of non-beneficiary households. Cunha et al. (2019) show that price effects differ for in-kind versus cash transfers in Mexico: in-kind transfers reduce local prices, while cash transfers display no significant effects (though the coefficient is positive). The price effects in Egger et al. (2022) are positive but economically minimal. Finally, studies analyzing the effects of transfers on the aggregate level of economic activity find multipliers in the range of 1.5 to 2.6 (Sadoulet et al., 2001; Thome et al., 2013; Corbi et al., 2019; Egger et al., 2022).

This paper contributes to our understanding of the aggregate implications of transfers in two ways. First, we study how cash and asset transfers affect the supply of providers of various services at the village level. We specifically focus on informal vets, informal dhodis (i.e., intermediaries who take milk from villages to urban markets), and informal and formal money lenders. The decision to focus on these providers is primarily informed by the transfers we study (described in more detail below): cash and assets, mainly livestock, may affect the demand for vets, dhodis, and money lenders within villages. Second, we analyze how the transfers shape the pro-market beliefs of beneficiaries and non-beneficiaries in treatment villages. For beneficiaries the transfers can lead to changes in occupational choice by enabling them to combine their labor with capital, and hence engage to a greater extent day-to-day in market transactions. The pro-market beliefs of non-beneficiaries can also shift if there is a demonstration effect of the greater market engagement of the beneficiaries, or through any changes in their own economic circumstances occurring through spillovers or general equilibrium effects.

We study a randomized controlled trial with two treatment arms tracking 15,000 households in 88 villages in rural Punjab, Pakistan. For both treatments, eligibility was determined based on a poverty score identifying households as poor. The first treatment arm, T1, provided recipients with an in-kind asset transfer. Recipients were shown a menu of productive assets and were allowed to pick any combination of assets up to PKR50,000 (500USD in 2012 prices). Additionally, households were offered a training of value PKR12,000. The total value of the first treatment was therefore PKR62,000 (620USD). The second treatment arm, T2, was identical to the first, but with one modification: recipients could choose to receive a one-off unconditional cash transfer (UCT) of 620USD. These transfers represent a big push in that they correspond to approximately the equivalent of eight months of food consumption at baseline.

The take-up rate is near 100% for both treatments. In T1, 50% of households chose livestock assets, 22% chose assets to set-up a small-scale retail business, and 15% chose assets related to petty trade. In T2, over 91% of recipients chose the UCT. Hence, T2 is a revealed preference cash transfer and our experiment compares an asset (T1) to a cash (T2) transfer.

The randomization is conducted in two stages. First, we randomly select 29 T1 villages, 29 T2 villages, and 30 control villages. Second, within treatment villages, we randomly assign the actual offer of treatment among eligible households. About half the eligible households in treatment villages are actually offered treatment. We denote eligible recipient households as treated poor (TP), eligible non-recipient households as not treated poor (NTP), and non-eligible non-recipient households as not poor (NP). Put differently, our experiment is a partial population experiment that allows us to estimate causal (spillover) effects on the TP, the NTP, and the NP. We track households two (midline) and four (endline) years post intervention.

We randomly sample 6237 eligible (i.e., TP and NTP) households in treatment and control villages: 1598 reside in T1 villages (of which 854 are treated), 1587 in T2 villages (of which 942 are treated), and 3052 in control villages. We draw a random sample of 9435 non-eligible (i.e., NP) households from all deciles of baseline poverty scores: 3306 reside in T1 villages, 2999 in T2 villages, and 3130 in control villages.

To elicit village-level outcomes, we rely on focus group discussions. These are discussions that were held in each village at baseline, midline, and endline with a group of elders and influential people within a village. On average, the groups consisted of eight participants. For each provider, we, where possible, focus on three categories of outcomes that are indicative of (i) the extensive margin, (ii) the reliability and quality, and (iii) the intensive margin. To measure the extensive margin, we check whether a provider is available in the village and, if so, how many are available. For the intensive margin, we ask focus groups to tell us (i) whether the provider charges a fee for their services, (ii) what the fee is that they charge (we record the minimum and maximum fee), (iii) the share of the village that uses the provider's services, and, for informal money lenders only, (iv) what the interest rate is that lenders charge on loans.

For individual-level outcomes, we exploit both the within- and between-village variation stemming from our research design. Specifically, the between village variation implies that we can compare TP, NTP, and NP households in treatment and control villages, while the within variation allows us to compare TP and NTP households within treatment villages. We measure pro-market beliefs following Di Tella et al. (2007) and create an index ranging from 0 to 4 capturing beliefs over individualism, meritocracy, materialism, and generalized trust. This last component is included because trust in others is a foundation for anonymous market exchange.

We present four findings for our village-level regressions. First, for informal vets, we find that the intervention affects the extensive and intensive margin, but not the quality and reliability of vets. Both asset and cash transfers increase the probability that a vet is available in a village at midline (but not at endline), though the effect is only significant for cash transfers (+16pp). Building on this, the number of vets available in the village increases both at midline and endline, with the effect again only being significant for cash transfers (+1 vet). On the intensive margin, we show that, at midline, both treatments decrease the maximum fee vets charge for their services by PKR651 and PKR598 in T1 and T2 villages, respectively. Given a baseline average maximum fee of PKR1149, this represents a decrease of more than 50% in both treatment arms. At endline, this decrease becomes insignificant for both treatments.

Second, we find limited impacts of the intervention on the supply of dhodis along all three dimensions we study. While there is some evidence that cash transfers minimally shift the extensive margin (the number of dhodis available in a village increases somewhat significantly by 1.14 dhodis at midline for cash transfers), the intensive margin is completely unaffected. The reliability and quality of dhodis is insignificantly but negatively affected by the transfers.

Third, we present similarly limited impacts of cash and asset transfers on the supply of informal money lenders. Only asset transfers significantly increase the probability that an informal money lender is available in a village at midline (+21pp), though this effect fades by endline. There is no significant effect for cash transfers. The number of informal money lenders present in villages is also not affected. The intensive margin and the quality and reliability of informal money lenders are only minimally affected.

Fourth, on formal money lenders, we show that neither treatment affects the probability that a mobile banking outlet or microfinance institution is available within a village. For mobile banking outlets, the satisfaction with the quality of services and prices decreases, but mostly insignificantly. For microfinance institutions, the same decrease is visible. However, at midline it is significant for both types of transfers. At endline, the negative effects are once again insignificant. We do not have data on the intensive margin for formal money lenders.

These results suggest limited effects on the supply of providers of various services, with the exception of informal vets (at midline). Interestingly, the effects on informal vets are only significant in T2 villages. This may simply be because cash increases the willingness to pay for services. Put differently, both transfers increase the demand for services, but since cash also increases the willingness to pay for services, more vets "appear" in T2 villages.

The muted impacts of our intervention on informal and formal money lenders speak to the role of informal (e.g., Aleem, 1990; Ghosh et al., 2000; Banerjee, 2001; Banerjee and Duflo, 2007; Karlan and Zinman, 2009; Banerjee and Duflo, 2010; Karlan et al., 2019) and formal (e.g., Banerjee, 2002; Giné and Karlan, 2009; Banerjee, 2013; Banerjee et al., 2015a; Meager, 2022) financial institutions in the lives of the poor.[3] In particular, evidence shows that less than six percent of all funds borrowed by the poor come from formal institutions (Banerjee and Duflo, 2007). This suggests a large role for informal money lenders, something confirmed in our context by Aleem (1990) and Khan (2005). Informal financial markets are, amongst other things, characterized by the presence of an excess of lenders (Aleem, 1990) and exorbitant interest rates (Banerjee, 2001; Banerjee and Duflo, 2010). The decision to focus on whether our intervention affects the extensive and intensive margin of informal money lenders was therefore (partially at least) informed by these stylized facts. The fact that neither of these dimensions is affected by our intervention can be due to multiple reasons. For example, borrowers and lenders tend to borrow from and lend to the same person multiple times (Aleem, 1990). This "path dependency" may disincentivize new lenders from entering the market, even if the demand for loans increases. Concurrently, the reasons for high interest rates are largely due to asymmetric information: lenders face large fixed costs in an imperfect screening process and borrowers lack information about the loan terms available from (informal and formal) lenders. Neither a cash nor an asset transfer tackles these issues directly. Providing the poor with access to formal financial institutions has mainly been done via access to microfinance. Broadly, microfinance distinguishes itself from informal financial markets with three main features: re-

[3] The landscape of informal financial markets in Pakistan is described in Khan (2005) in detail. For a broad overview of access to finance in developing countries, see Karlan and Morduch (2010).

peat lending, regular repayment schedules, and group lending (Banerjee, 2002). Our cash transfers do not affect the behavior of these institutions but, in the short run, do affect the satisfaction with the quality and prices of microfinance institutions. While the reasons for this are also manifold, one possibility is increased underlying demand and take-up in treatment villages.

Our results on the pro-market beliefs index reveal that all groups (TP, NTP, and NP) hold significantly more pro-market beliefs at midline, notwithstanding their treatment status. The results fade by endline. For recipients of asset transfers, materialism and generalized trust are the drivers of the overall impact seen in the index. For recipients of cash transfers, it is meritocracy, materialism, and generalized trust.

These findings relate to a large literature debating how exposure to markets affects social and political outcomes (De Montesquieu, 1748; Marx and Engels, 1848; Polanyi, 1957; McCloskey, 2006; Sandel, 2012). Evidence shows that individuals exposed to markets view markets more favorably (Di Tella et al., 2007), favor privatization (Di Tella et al., 2012), and are more right-leaning on various policy issues (Margalit and Shayo, 2021). Our findings build on this evidence base to show that in the context of pro-poor interventions in developing countries, pro-market beliefs are shaped by village wide exposure to policies (instead of individual-level exposure). Furthermore, we show that these effects are short lived.

Overall, the results presented in this paper suggest that neither the supply of providers of services in rural villages in Pakistan nor the pro-market beliefs of beneficiaries and non-beneficiaries in treatment villages are affected by the transfers studied in the long-run (i.e., at endline). This is notwithstanding the fact that in the short-run the supply of informal vets is shifted along the extensive and intensive margin and that beneficiaries and non-beneficiaries are more pro-market. These short-run results themselves imply two things. First, for vets, higher competition ("more vets") can lead to lower prices. In villages where agriculture and livestock rearing occupy a large share of the population, this has the potential to significantly impact people's life. The results therefore suggest that prices of various service providers in the village may also be affected by big push interventions, and that the price effects here may be opposite to the often feared price inflation. Second, the fact that individuals are more believing in markets suggests that they are also more willing to engage in markets. If this leads to more efficient market outcomes because more players are actively engaged in markets, this can (positively) impact aggregate outcomes of big push interventions. When thinking about general equilibrium effects of social assistance programs, it is therefore pertinent to account for both the short-run and the long-run effects documented here. This also suggests that focusing on long-run impacts of interventions to quantify dynamic effects should be a first-order concern in future research.

The rest of the paper is organized as follows. Section 3.2 describes the intervention and research design. Section 3.3 presents village-level impacts on supply side providers and section 3.4 documents how individual-level pro-market beliefs change. Finally, section 3.5 concludes.

3.2 Context, Interventions and Design

As this study is based on the same intervention evaluated in Cerkez et al. (2024), this section, with minor adjustments when necessary for the focus of this paper, follows said paper when describing the context, interventions, and research design.

3.2.1 Context

The evaluation takes places in 88 villages in four districts in Punjab, Pakistan: Bahawalpur, Bahawalnagar, Lodhran, and Muzaffargarh. Households are predominantly Muslim and are engaged in cropping/farming (38%), unskilled laboring (19%), and livestock rearing (12%).

3.2.2 Interventions

We study two types of interventions. In the first, we offered households productive assets in-kind. To determine the menu of assets to offer, we conducted an assessment of assets likely to generate high returns in each village. These typically included livestock, assets to start a retail business (e.g., grocery shop, fruit stall), crop farming, and other forms of self-employment (e.g., tailoring). Figure 3.A1 displays a stylized representation of an asset menu. Households were allowed to choose any combination of assets of the menu up to a total value of PKR50,000 (500USD in 2012 prices). In conjunction with in-kind asset transfers, households were offered training providing skills to run a micro-enterprise, as well as skills specific to the chosen asset(s). The value of the training was fixed at PKR12,000. Hence, the total value of transfers and training offered was PKR62,000 ($\approx$ 620USD). We refer to this as treatment T1.[4]

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 PKR62,000. To mimic the timing of transfers and training in T1, the delivery of cash transfers was staggered as an up-front payment of PKR50,000, followed by PKR12,000 a month later. We refer to this as treatment T2.

The treatments were implemented in collaboration with quasi-government agencies: the Pakistan Poverty Alleviation Fund (PPAF) and their government field partners, FDO and NRSP.[5]

The interventions are big push, representing high-valued resource transfers to

[4]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 PKR50,000 they self-finance the excess.

[5]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.

the poor. The value of transfers corresponds to the equivalent of eight months of food consumption at baseline.[6]

Eligibility To determine the eligibility of households for the intervention, we conducted a census of 35,522 households in our 88 villages. We assigned to each household a poverty score ranging from 0-100 based on characteristics proxying the household's permanent income. Households with a score of 0-18 are deemed to be poor and hence eligible for the interventions. The interquartile range of poverty scores is 19 to 37, with the highest decile of households having scores above 46. The poverty score construction is similar to that used to target welfare programs to the rural poor in Pakistan, including the prominent Benazir Income Support Programme. This is the most widespread social protection program in Pakistan, reaching nearly five million households in 2012. Households are thus familiar with the kind of poverty score construction used to determine eligibility. Not treated poor households were given no promise of future treatment. Not poor households were aware they were never going to be eligible.[7]

3.2.3 Research Design

Randomization We follow a two-stage randomization design. In the first stage, we randomly assign villages to T1, T2 or control. Randomization is stratified by district. In the second stage, within treated villages, we randomly assign the actual offer of treatment among eligible households. Half of those eligible are actually of-

[6]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. (2015b) 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.

[7]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.

fered treatment. Among the poor in treated villages, we thus distinguish between the treated poor (TP) and the not treated poor (NTP).

Our design differs from the one in Egger et al. (2022), who, in a first step, randomize groups of villages into high- or low saturation groups and, in a second step, randomly select two-thirds or one-third of the villages, respectively, to be treated (they treat all eligible individuals within treatment villages). This allows them to identify spillovers both within and across villages. In contrast, our design only allows us to identify within village spillovers. Indeed, our identification assumption is that there are no spillover effects onto control villages. However, Egger et al. (2022) do find spillovers to other villages, suggesting that our design may be misspecified. However, Egger et al.'s (2022) study takes place in a densely populated area ("households are located within 2km of seven other villages"), whereas the distance to the nearest control village in our study is about 12km (see Table 1 in Cerkez et al. (2024)). While we cannot identify across village spillovers with our research design, misspecification should therefore not be a concern.

Sampling We sample 6237 eligible poor households in treated and control villages (approximately 75% of all poor households): 3052 reside in controls, 1598 are in T1 villages (of which 854 are treated), and 1587 are in T2 villages (of which 942 are treated). We use our census to draw a random sample of non poor households from across all deciles of poverty scores. We refer to non poor households as NP. We survey 9435 non poor households in total (about 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 eligible households 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.[8]

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 two- and four-year follow-up surveys that were conducted in September/October 2016 and February/March 2018. We refer to the two-year and four-year follow-up as the midline and endline, respectively.

Focus Groups To elicit village-level outcomes, we relied on focus groups discussions. These are discussions that were held in each village at baseline, midline, and endline with a group of elders and influential people within a village. Table 3.A1 shows the average number of participants in focus groups at baseline, midline, and endline in control, T1, and T2 villages. As can be seen, the average groups consisted of 7 to 9 participants.

Balance

Village Characteristics Table 3.1 shows that the sample is balanced on village characteristics across both treatment arms. Panel A of the table shows that villages are small, with approximately 400 households in each. The travel times to the nearest livestock market and police stations, in minutes, are just above and below 60 minutes, respectively. Finally, a self-constructed measure of market access ranging from 1 to 10 has an average score of about 5.5.[9]

[8] Given the scale of cash transfers offered, two other design features are relevant. First, after their initial choice, households were given a two week window to finalize their choice, in case they preferred an alternative bundle after having discussed further with family and neighbors. Nearly all households stuck with their initial choice of cash transfers in T2. Second, the cash transfer is best interpreted as a labeled 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.

[9] Market access is defined in three steps. First, we sum the travel time (in minutes) to 12 different places and markets. These are: (i) the local bus station, (ii) the intercity bus terminal, (iii) the local transport stop (e.g., rickshaw), (iv) the railway station, (v) the vet center/vet, (vi) the private

Panel B focuses on 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 for the intervention).

Finally, Panel C displays the most owned livestock types in our villages. The most popular livestock type are cows, with almost 40% of households in villages owning at least one. Buffaloes and goats are owned by 28% and 21%, respectively. Bulls and sheep are less popular, being owned by less than 10% of the villages.

Supply Side Providers Table 3.2 shows that the sample is largely balanced on supply side providers across control and treatment villages. Panel A shows that informal vets are active in around 65% of villages. Conditional on a vet being available, the average number of vets in a village is approximately 2. Control villages significantly have more vets than T1 and T2 villages.

Focusing on dhodis in Panel B, we see that around 63% of control and T2 villages and 83% of T1 villages have dhodis operating in them. The share in T1 villages is significantly higher than in control and T2 villages. Conditional on a dhodi being available, approximately 2.5 dhodis are active control and T2 villages and 1.8 dhodis are available in T1 villages. Control and T2 villages have significantly more dhodis than T1 villages.

Panel C focuses on informal and formal money lenders. Around 25% of villages have informal money lenders and, conditional on having informal money lenders, the average number of these informal money lenders is just below 2. Around 13% of villages have a mobile banking outlet.

Panel D shows that 40% of villages have informal job helpers operating in them. Conditional on job helpers being present in a village, the average number of job helpers in villages is 1.75.

company milk collection point, (vii) the police station, (viii) the post office, (ix) the bank branch, (x) the grain market, (xi) the livestock market, and (xii) the mobile banking outlet. Second, we look at the deciles of that sum. Third, we flip the measure to get a market access measurement ranging from 1-10, where 10 implies "more market access."

Attrition We have a balanced panel of 88 villages at baseline, midline, and endline. There is therefore no attrition.[10]

3.3 Impacts on Supply Side Providers

3.3.1 Empirical Specification

To explore the impacts of the intervention on supply side providers at the village level, we rely on the following specification

$$y_{vt} = \alpha + \sum_{j=1,2} \sum_{t=2,4} \beta_{jt} \left(T_{jv} \times W_t \right) + \tau_t + \lambda_s + u_{vt} \tag{3.1}$$

where y_{vt} is a village-level v outcome (described below) at time $t \in \{2, 4\}$ for the midline and endline, respectively. T_{jv} is a dummy variable indicating treatment status $j \in \{1, 2\}$ for village v, W_t, for $t \in \{2, 4\}$, is a dummy variable indicating, respectively, the midline or endline, and τ_t and λ_s are survey wave and strata (district) fixed effects. We report robust standard errors.

When estimating equation (3.1), we make two modification to the stated equation. First, when possible, we control for the baseline value of the outcome (y_{v0}). Second, we control for various village-level characteristics. Due to the small sample size, we rely on a (post-double selection) lasso procedure to select relevant controls (e.g., Belloni et al., 2011, 2012).

Standard identifying assumptions for the treatment effects are that there is random assignment, and that there are no spillovers onto control villages (see section 3.2.3).

3.3.2 Informal Vets

Outcomes We rely on three categories of outcomes for informal vets. Outcomes are indicative of (i) the extensive margin, (ii) the reliability and quality, and (iii)

[10] For the results in section 3.4, which rely on individual-level data, we refer the reader to Table A3 in Cerkez et al. (2024). We there show that 5 to 7 percent of the sample attrit by endline. These are small magnitudes.

the intensive margin. For this third group of outcomes, focus group participants answered questions for each vet in the village individually. We take their vet-level answers and aggregate them to the village-level.

Within the extensive margin, we construct three outcomes based on the question *suppose a farmer's bull/cow was sick, would there be someone in the village who could help with the animal's care/treatment?*. The first is a dummy indicating whether a vet is available in the village, the second is a variable counting how many vets are available in the village ("including 0s"), and the third is a variable counting how many vets are available in the village, conditional on vets being available ("excluding 0s").

The two questions in the second category are: (i) *on a scale of 1 to 10, with 1 being extremely unreliable and 10 being extremely reliable, how would you rate the reliability of services that these vets provide?* and (ii) *on a scale of 1 to 10, with 1 being extremely low quality and 10 being extremely high quality, how would you rate the quality of services that these vets provide?*.

Within the intensive margin, we use the following three questions: (i) *does this person charge a fee for helping?*, (ii) *what is the amount of the fee charged?*—we record the minimum and maximum for each vet, and (iii)*what percentage of the village households use his/her services?*.

Results Table 3.3 presents the results. We document three findings. First, on the extensive margin, while there is no movement in T1 villages, there are impacts in T2 villages.[11] As seen in Column 1, two years post intervention, the probability that a vet is active increases by 16 percentage points. This effect, however, fades out four years post intervention. Furthermore, the number of vets increases two and four years post intervention in Column 2. In Column 3, when looking at the number of vets in a village conditional on vets being available, the increase is only significant four years post interventions. These treatment effects in Columns 2 and 3 differ significantly from the T1 treatment effects at midline.

[11] While the effects in T1 villages are not significant, the effects go in the same directions as the ones in T2.

Second, there are no visible treatment effects on the reliability and quality scores of vets in either T1 or T2 village at midline or endline.

Third, the intensive margin is affected. While both the minimum and maximum fees vets charge for helping decrease (Columns 7 and 8), the treatment effects are only significant for the maximum fees. Specifically, two years post intervention, the maximum fees vets charge decrease by PKR651 and PKR598 in T1 and T2 villages, respectively. Given a baseline average maximum fee of PKR1149, this represents a decrease of more than 50% in both treatment arms. The other outcomes—the share of vets who charge a fee and the share of the village who use their services—are not affected by the intervention.

Overall, Table 3.3 serves as evidence that the interventions studied affect the supply of informal vets both on the extensive and intensive margin.

3.3.3 Informal Dhodis

Outcomes We rely on three categories of outcomes for informal dhodis. Outcomes are indicative of (i) the extensive margin, (ii) the reliability and quality, and (iii) the intensive margin. For this third group of outcomes, focus group participants answered questions for each dhodi in the village individually. We take their dhodi-level answers and aggregate them to the village-level.

Within the extensive margin, we construct three outcomes based on the question *suppose a farmer's bull/cow gave excess milk, would there be someone in the village who could help with the transport of milk to other consumers/producers?*. The first is a dummy indicating whether a dhodi is available in the village, the second is a variable counting how many dhodis are available in the village ("including 0s"), and the third is a variable counting how many dhodis are available in the village, conditional on dhodis being available ("excluding 0s").

The two questions in the second category are: (i) *on a scale of 1 to 10, with 1 being extremely unreliable and 10 being extremely reliable, how would you rate the reliability of services that these dhodis provide?* and (ii) *on a scale of 1 to 10, with 1 being extremely low quality and 10 being extremely high quality, how would you rate* *the quality of services that these dhodis provide?*.

Within the intensive margin, we use the following two questions: (i) *does this person charge a fee for helping?* and (ii) *what percentage of the village households use his/her services?*.

Results The results are displayed in Table 3.4. There are again three main findings. First, the impacts on the extensive margin are minimal. T2 villages see an increase of 1.14 dhodis in response to the intervention at midline (conditional on dhodis being active in the village). The effect fades out at endline. Otherwise, there are no impacts on the extensive margin.

Second, the reliability and quality scores mostly insignificantly decrease. The only significant effect exists at midline for T1 villages: the reliability score is reduced by .760 points.

Third, the intensive margin is not significantly affected by the intervention.

Overall, the table suggests limited impacts on the supply of dhodis due to our intervention.

3.3.4 Informal Money Lenders

Outcomes We rely on three categories of outcomes for informal money lenders. Outcomes are indicative of (i) the extensive margin, (ii) the reliability and quality, and (iii) the intensive margin. For this third group of outcomes, focus group participants answered questions for each money lender in the village individually. We take their money lender-level answers and aggregate them to the village-level.

Within the extensive margin, we construct three outcomes based on the question *is there someone in this village that is prepared to help others when they need money/financial assistance?*. The first is a dummy indicating whether a money lender is available in the village, the second is a variable counting how many money lenders are available in the village ("including 0s"), and the third is a variable counting how many money lenders are available in the village, conditional on money lenders being available ("excluding 0s").

The two questions in the second category are: (i) *on a scale of 1 to 10, with 1 being extremely unreliable and 10 being extremely reliable, how would you rate the reliability of services that these money lenders provide?* and (ii) *on a scale of 1 to 10, with 1 being extremely low quality and 10 being extremely high quality, how would you rate the quality of services that these money lenders provide?*.

Within the intensive margin, we use the following four questions: (i) *does this person charge a fee for helping?*, (ii) *if the person provides loans, does he/she charge interest?*, (iii) *what is the monthly interest rate this person charges?*—this question is only asked for money lenders where the focus group answered yes to the previous question, and (iv) *what percentage of the village households use his/her services?*.

Results Table 3.5 presents three findings. First, on the extensive margin, we see that at midline in T1 villages, the probability that an informal money lender is available increases by 20.6 percentage points. This effect, however, vanishes at endline. The number of informal money lenders available in the village (Columns 2 and 3) is not affected.

Second, while the reliability and quality scores decrease insignificantly for T1 and T2 villages two years post intervention, they increase (still insignificantly) four years post intervention. The only significant effect is visible in T1 villages at endline: treatment increases the quality score of informal money lenders by 1.21 points.

Third, the share of money lenders who charge a fee for helping significantly increases at midline and significantly decreases at endline in T1 villages. Specifically, the share of informal money lenders who charge a fee increases and then decreases by approximately 54 percentage points. The other outcomes studied are not affected.

Overall, Table 3.5 indicates that while the extensive margin is affected by our intervention, the intensive margin is not shifted by much.

3.3.5 Formal Money Lenders

Outcomes We focus on two types of formal money lenders: mobile banking outlets and microfinance institutions. For each, we ask three questions: (i) *do [mobile banking outlet/microfinance institution] facilities exist within the village?*, (ii) *on a scale from 1 to 5, how satisfied are you with the quality of services provided by the [mobile banking outlet/microfinance institution]?*, and (iii) *on a scale from 1 to 5, how satisfied are you with the price you pay for the services provided by the [mobile banking outlet/microfinance institution]?*. Questions 2 and 3 are answered on a scale from 1 to 5, where 1 implies highly satisfied and 5 implies highly unsatisfied. We flip the respondents' answers so that higher values indicate higher levels of satisfaction.

Results Columns 1 and 4 of Table 3.6 show that the probability that a mobile banking outlet or microfinance institution is available within a village is not affected by our treatments. For mobile banking outlets, the satisfaction with the quality of services and prices decreases, but mostly insignificantly (with one exception) (Columns 2 and 3). For microfinance institutions, the same decrease is visible. However, at midline it is significant in T1 and T2 villages alike. At endline, the negative effects are once again insignificant. The magnitude of the significant effects indicate a reduction in the satisfaction of half a point (Columns 5 and 6).

3.3.6 Placebo Tests

The decision to focus on vets, dhodis, and money lenders as service providers was, as stated in the introduction, informed by the type of the intervention. There are services, or more broadly markets, that we do not expect to be affected by this intervention. To this end, Table 3.A2 shows that neither the availability, quality, nor prices of various animals in livestock markets near the villages are affected. This exercise can be repeated for a variety of other markets.[12]

[12] For example, we find that the availability of banks, police stations, or post offices is not affected by the intervention either. These results are available upon request.

3.4 Impacts on Pro-market Beliefs

Pro-market beliefs can be impacted by the kinds of big push interventions 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 in day-to-day 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. Observing how village-level outcomes, such as the ones examined in section 3.3, are affected can also affect pro-market beliefs.

To measure pro-market beliefs, we follow Di Tella et al. (2007) and create an index ranging from 0 to 4 capturing beliefs over individualism, meritocracy, materialism, and generalized trust. This last component is included because trust in others is a foundation for anonymized market exchange. In particular, we sum positive answers to the following four 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?*; and (iv) *in general, in our country, would you say that one can trust other people?*.

3.4.1 Empirical Specification

To examine the impacts of the intervention on pro-market beliefs, we exploit both the between and within village randomization. For the between village randomization, we estimate treatment effects on the pro-market beliefs of the TP, NTP, and NP using the following specification

$$y^g_{hvt} = \alpha^g + \sum_{j=1,2} \sum_{t=2,4} \beta^g_{jt} \left(T_{jv} \times W_t \right) + \tau^g_t + \lambda^g_s + \delta^g_e + u^g_{hvt} \tag{3.2}$$

where everything is as in equation (3.1), except: (i) g indicates the group, i.e., $g \in \{\text{TP, NTP, NP}\}$, (ii) y^g_{hvt} is the pro-market belief of household head h in village v in survey wave t, (iii) δ^g_e are enumerator fixed effects, and (iv) standard errors are clustered at the village level.[13]

The identifying assumptions for the treatment effects on each group are that there is random assignment, and that there are no spillovers onto controls. The effects on the beliefs of the NTP and NP capture their exposure to the pro-poor interventions. These effects can operate through: (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; and/or (iii) any emotional connection with beneficiaries.

For the within village randomization, we estimate treatment effects on the beliefs of TP relative to the NTP within treatment villages relying on the following specification

$$y_{hvt} = \alpha + \sum_{j=1,2} \sum_{t=2,4} \beta_{jt} \left(T_{jv} \times W_t \times P_h \right) + \tau_t + \lambda_s + \delta_e + u_{hvt} \tag{3.3}$$

where P_h is a dummy indicating a poor (i.e., NTP or TP) household and otherwise everything is as described above. A key advantage of this within specification is that it removes village-level unobservables that are common drivers of beliefs of the TP and NTP.

Throughout we also report p-values on treatment effects at midline and endline, and also account for multiple hypothesis testing (MHT) by also presenting sharpened two-stage q-values (Benjamini et al., 2006; Anderson, 2008). These q-values conservatively account for the fact that, for each outcome, we test 16 hypotheses: twelve related to the between village estimates and four related to the within village estimates.

[13]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).

3.4.2 Results

Panel A of Columns 1* in Table 3.7 show how the pro-market index overall is impacted. We find that all groups hold significantly more pro-market beliefs at midline. The impact on the TP is .174 for those residing in T1 villages and .219 for those residing in T2 villages. Both impacts are statistically significant. The magnitudes of impact on the pro-market beliefs of the NTP and NP are similar. However, for all three groups, we see a significant decline in these beliefs by endline, so that pro-market beliefs no longer differ from controls four years post-intervention. Panel B shows that the TP hold lower pro-market beliefs than NTP in T1 villages only.

Columns 2* to 5* display impacts on each component of the pro-market beliefs index individually. Panel A shows that, for individuals in T1 villages, materialism and generalized trust are the drivers of the overall impact seen in the index. For individuals in T2 villages, it is meritocracy, materialism, and generalized trust.[14]

3.5 Conclusion

The study contributes to our understanding of the general equilibrium effects of big push pro-poor interventions in developing countries. Leveraging rich data from Pakistan, we show that in the long-run, neither the supply of village-level providers of services nor the pro-market beliefs of beneficiaries and non-beneficiaries are shifted. This is notwithstanding the fact that in the short-run we do demonstrate significant effects of the interventions on some providers and beliefs. The results underline the importance of thinking about short- and long-run dynamics when evaluating aggregate impacts of randomized evaluations.

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

3.6 Tables and Figures

Table 3.1. 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	[.500]	[.496]	[.236]
	(180)	(271)	(199)			
Travel time to nearest livestock market (mins)	67.0	64.0	74.3	[.681]	[.457]	[.307]
	(32.4)	(40.1)	(44.3)			
Travel time to nearest police station (mins)	50.9	53.4	54.0	[.916]	[.829]	[.840]
	(35.1)	(33.4)	(39.0)			
Market access (1-10)	5.60	5.41	5.72	[.965]	[.760]	[.843]
	(2.62)	(2.81)	(3.30)			
Panel B. Poverty						
Average poverty score (0-100) of households	29.2	30.6	29.0	[.240]	[.997]	[.160]
	(4.77)	(3.79)	(4.31)			
Standard deviation of poverty score of households	13.7	13.6	13.2	[.872]	[.352]	[.345]
	(2.43)	(2.43)	(2.24)			
Share of poor households	.248	.202	.240	[.037]	[.563]	[.103]
	(.091)	(.073)	(.087)			
Panel C. Share of households who own:						
cows	.388	.390	.398	[.663]	[.667]	[.915]
	(.156)	(.149)	(.138)			
buffalos	.289	.320	.283	[.418]	[.778]	[.245]
	(.142)	(.171)	(.153)			
goats	.214	.239	.218	[.435]	[.971]	[.364]
	(.120)	(.103)	(.118)			
bulls	.062	.075	.067	[.419]	[.616]	[.713]
	(.050)	(.065)	(.053)			
sheep	.017	.023	.019	[.250]	[.734]	[.473]
	(.017)	(.029)	(.021)			

Notes: Columns 1, 2, and 3 show sample means and standard deviations (in parentheses for continuous variables) for each village characteristic. 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. The share of poor households within a village is derived from a household poverty score, which 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 poor and hence eligible for the interventions. Market access is defined in three steps. First, we sum the travel time (in minutes) to 12 different places and markets (such as bus/train stations or grain or livestock markets). Second, we look at the deciles of that sum. Third, we flip the measure to get a market access measurement from 1-10, where 10 implies "more market access."

Table 3.2. Supply Side Providers

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
Panel A. Vets						
Share with informal vets available	.633	.690	.690	[.610]	[.771]	[.867]
Avg. number of informal vets | vets available	2.47	1.80	1.55	[.047]	[.004]	[.207]
	(1.02)	(.834)	(.836)			
Panel B. Dhodis						
Share with informal dhodis available	.633	.828	.621	[.099]	[.857]	[.030]
Avg. number of informal dhodis | dhodis available	2.53	1.88	2.83	[.089]	[.460]	[.018]
	(1.43)	(.992)	(1.76)			
Panel C. Credit Markets						
Share with informal money lender available	.233	.241	.345	[.908]	[.283]	[.462]
Avg. number of informal money lenders | lender available	1.71	1.71	1.80	[.814]	[.866]	[.751]
	(.951)	(.488)	(1.03)			
Share with mobile banking outlet available	.133	.103	.172	[.760]	[.636]	[.516]
Panel D. Job Helpers						
Share with informal job helpers available	.400	.414	.379	[.751]	[.993]	[.715]
Avg. number of informal job helpers | helper available	2.00	1.58	1.73	[.238]	[.278]	[.955]
	(.603)	(.996)	(1.19)			

Notes: Columns 1, 2, and 3 show sample means and standard deviations (in parentheses for continuous variables) for each village characteristic. 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.

Table 3.3. Informal Vets

OLS estimates, robust standard errors in parentheses

	Informal vets available?	No. of informal vets	No. of informal vets | vets available	Average reliability score (1-10) | vets available	Average quality score (1-10) | vets available	Share who charge a fee for helping?	Average minimum fee for helping (2012 PKR)	Average maximum fee for helping (2012 PKR)	Share of village who use their services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T1: Asset Transfer, two year impact	.130	.052	-.462	-.064	-.001	.059	-29.9	-651***	.026
	(.087)	(.352)	(.356)	(.467)	(.500)	(.111)	(.49.2)	(236)	(.062)
T1: Asset Transfer, four year impact	-.037	.576	.544	.036	-.268	.052	-4.91	21.0	.014
	(.058)	(.391)	(.544)	(.471)	(.472)	(.108)	(43.4)	(176)	(.084)
T2: RP UCT, two year impact	.160**	.776**	.608	-.021	.027	-.054	24.6	-598**	.019
	(.080)	(.388)	(.449)	(.484)	(.422)	(.111)	(50.7)	(238)	(.062)
T2: RP UCT, four year impact	-.007	1.16***	1.20*	-.256	-.124	.074	-4.81	-200	-.091
	(.046)	(.430)	(.613)	(.466)	(.384)	(.102)	(30.0)	(176)	(.072)
District and Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Controls (LASSO Selection)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Outcome, Controls at Baseline	.633	1.57	2.47	N/A	N/A	.886	261	1149	.300
Mean Outcome, Controls at Midline				7.53	7.50				
T1 (midline) = T2 (midline) [p-value]	[.612]	[.040]	[.012]	[.851]	[.945]	[.353]	[.268]	[.641]	[.918]
T1 (endline) = T2 (endline) [p-value]	[.600]	[.135]	[.168]	[.545]	[.750]	[.821]	[.998]	[.124]	[.147]
T1 (midline) = T1 (endline) [p-value]	[.107]	[.302]	[.092]	[.879]	[.681]	[.963]	[.699]	[.023]	[.910]
T2 (midline) = T2 (endline) [p-value]	[.073]	[.495]	[.385]	[.680]	[.790]	[.387]	[.603]	[.173]	[.246]
Observations (Villages)	176	176	112	163	163	112	87	87	112

Notes: *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All regressions include treatment dummies, district (strata) and survey wave fixed effects, and select village-level controls via LASSO selection. Robust standard errors are reported. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention. Columns 1 to 3 are based on the question "suppose a farmer's bull/cow was sick, would there be someone in the village who could help with the animal's care/treatment?" - in Column 1 we construct a dummy indicating whether a vet is available in the village; in Column 2 we construct a variable counting how many vets are available in the village ("including 0s"); and in Column 3 we construct a variable counting how many vets are available in the village conditional on vets being available ("excluding 0s"). The outcome in Column 4 is based on the question "on a scale of 1 to 10, with 1 being extremely unreliable and 10 being extremely reliable, how would you rate the reliability of services that these vets provide?". The outcome in Column 5 is based on the question "on a scale of 1 to 10, with 1 being extremely low quality and 10 being extremely high quality, how would you rate the quality of services that these vets provide?". Focus group participants answered the questions in Columns 6 to 9 for each vet in the village individually. We take their vet-level answers and aggregate them to the village-level. The outcome in Column 6 is based on the question "does this person charge a fee for helping?". The outcomes in Columns 7 and 8 are based on the question "what is the amount of the fee charged?" - we record the minimum and maximum for each vet. Finally, the outcome in Column 9 is based on the question "what percentage of the village households use his/her services?".

Table 3.4. Informal Dhodis

OLS estimates, robust standard errors in parentheses

	Informal dhodis available?	No. of informal dhodis	No. of informal dhodis | dhodis available	Average reliability score of dhodis (1-10) | dhodis available	Average quality score of dhodis (1-10) | dhodis available	Share who charge a fee for helping?	Share of village who use their services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
T1: Asset Transfer, two year impact	.043	.356	.270	-.760*	-.248	.041	-.013
	(.060)	(.472)	(.598)	(.447)	(.456)	(.040)	(.053)
T1: Asset Transfer, four year impact	-.024	.202	.366	.188	-.140	-.005	-.014
	(.055)	(.525)	(.553)	(.498)	(.451)	(.007)	(.072)
T2: RP UCT, two year impact	-.040	.860	1.14*	-.671	-.494	-.001	-.045
	(.074)	(.534)	(.679)	(.464)	(.480)	(.005)	(.038)
T2: RP UCT, four year impact	-.039	.016	.433	.028	-.510	-.003	-.082
	(.059)	(.506)	(.647)	(.472)	(.453)	(.005)	(.065)
District and Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Controls (LASSO Selection)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Outcome, Controls at Baseline	.633	1.60	2.53	N/A	N/A	.053	.209
Mean Outcome, Controls at Midline				8.09	7.71		
T1 (midline) = T2 (midline) [p-value]	[.212]	[.326]	[.153]	[.857]	[.600]	[.309]	[.557]
T1 (endline) = T2 (endline) [p-value]	[.813]	[.722]	[.897]	[.731]	[.441]	[.620]	[.233]
T1 (midline) = T1 (endline) [p-value]	[.402]	[.828]	[.906]	[.157]	[.867]	[.309]	[.986]
T2 (midline) = T2 (endline) [p-value]	[.990]	[.247]	[.442]	[.287]	[.980]	[.834]	[.620]
Observations (Villages)	176	176	114	165	165	114	114

Notes: *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All regressions include treatment dummies, district (strata) and survey wave fixed effects, and select village-level controls via LASSO selection. Robust standard errors are reported. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention. Dhodis are intermediaries who take milk from villages to urban markets. Columns 1 to 3 are based on the question "suppose a farmer's bull/cow gave excess milk, would there be someone in the village who could help with the transport of milk to other consumers/producers?" - in Column 1 we construct a dummy indicating whether a dhodi is available in the village; in Column 2 we construct a variable counting how many dhodis are available in the village ("including 0s"); and in Column 3 we construct a variable counting how many dhodis are available in the village conditional on dhodis being available ("excluding 0s"). The outcome in Column 4 is based on the question "on a scale of 1 to 10, with 1 being extremely unreliable and 10 being extremely reliable, how would you rate the reliability of services that these dhodis provide?". The outcome in Column 5 is based on the question "on a scale of 1 to 10, with 1 being extremely low quality and 10 being extremely high quality, how would you rate the quality of services that these dhodis provide?". Focus group participants answered the questions in Columns 6 and 7 for each dhodi in the village individually. We take their dhodi-level answers and aggregate them to the village-level. The outcome in Column 6 is based on the question "does this person charge a fee for helping?". Finally, the outcome in Column 7 is based on the question "what percentage of the village households use his/her services?".

Table 3.5. Informal Money Lenders

OLS estimates, robust standard errors in parentheses

	Informal money lenders available?	No. of informal money lenders	No. of informal money lenders | lender available	Average reliability score of lenders (1-10) | lender available	Average quality score of lenders (1-10) | lender available	Share who charge a fee for helping	Share who charge interest on loans	What is the monthly interest rate on loans?	Share of village who use their services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T1: Asset Transfer, two year impact	.206*	.382	.266	-.860	-.960	.544**	.142	-.028	-.036
	(.112)	(.523)	(1.04)	(.763)	(.873)	(.212)	(.190)	(.148)	(.037)
T1: Asset Transfer, four year impact	-.071	.213	.539	.973	1.21*	-.546**	.165	.020	.013
	(.117)	(.484)	(.641)	(.715)	(.680)	(.238)	(.254)	(.070)	(.059)
T2: RP UCT, two year impact	-.031	-.190	-.462	-1.33	-.766	.049	.117	-.123	-.026
	(.104)	(.552)	(1.00)	(.832)	(.780)	(.181)	(.183)	(.210)	(.043)
T2: RP UCT, four year impact	.003	.296	1.11	-.652	.222	-.189	.153	-.080	.058
	(.115)	(.451)	(.760)	(.712)	(.667)	(.242)	(.136)	(.086)	(.054)
District and Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Controls (LASSO Selection)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Outcome, Controls at Baseline	.233	.400	1.71	N/A	N/A	.476	.762	.175	.333
Mean Outcome, Controls at Midline				7.22	6.91				
T1 (midline) = T2 (midline) [p-value]	[.033]	[.322]	[.485]	[.592]	[.819]	[.023]	[.866]	[.346]	[.834]
T1 (endline) = T2 (endline) [p-value]	[.532]	[.870]	[.502]	[.001]	[.046]	[.118]	[.960]	[.256]	[.484]
T1 (midline) = T1 (endline) [p-value]	[.087]	[.810]	[.813]	[.083]	[.052]	[.001]	[.935]	[.785]	[.424]
T2 (midline) = T2 (endline) [p-value]	[.829]	[.497]	[.198]	[.541]	[.338]	[.420]	[.866]	[.855]	[.228]
Observations (Villages)	176	176	34	86	86	34	34	24	34

Notes: *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All regressions include treatment dummies, district (strata) and survey wave fixed effects, and select village-level controls via LASSO selection. Robust standard errors are reported. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention. Columns 1 to 3 are based on the question "is there someone in this village that is prepared to help others when they need money/financial assistance?" - in Column 1 we construct a dummy indicating whether a money lender is available in the village; in Column 2 we construct a variable counting how many money lenders are available in the village ("including 0s"); and in Column 3 we construct a variable counting how many money lenders are available in the village conditional on money lenders being available ("excluding 0s"). The outcome in Column 4 is based on the question "on a scale of 1 to 10, with 1 being extremely unreliable and 10 being extremely reliable, how would you rate the reliability of services that these money lenders provide?". The outcome in Column 5 is based on the question "on a scale of 1 to 10, with 1 being extremely low quality and 10 being extremely high quality, how would you rate the quality of services that these money lenders provide?". Focus group participants answered the questions in Columns 6 to 9 for each money lender in the village individually. We take their money lender-level answers and aggregate them to the village-level. The outcome in Column 6 is based on the question "does this person charge a fee for helping?". The outcome in Column 7 is based on the question "if the person provides loans, does he/she charge interest?". The outcome in Column 8 is based on the question "what is the monthly interest rate this person charges?" - this question is only asked for money lenders where the focus group answered "yes" to the question in Column 7. Finally, the outcome in Column 9 is based on the question "what percentage of the village households use his/her services?".

Table 3.6. Formal Money Lenders

OLS estimates, robust standard errors in parentheses

	Mobile Banking Outlet			Microfinance Institution		
	Mobile banking outlet available in village?	Satisfaction with services (1-5)	Satisfaction with price (1-5)	MFI available in village?	Satisfaction with services (1-5)	Satisfaction with price (1-5)
	(1)	(2)	(3)	(4)	(5)	(6)
T1: Asset Transfer, two year impact	.031	-.438*	-.349	-.001	-.572***	-.673***
	(.106)	(.239)	(.314)	(.003)	(.210)	(.233)
T1: Asset Transfer, four year impact	-.071	.096	.273	-.034	-.074	-.138
	(.111)	(.277)	(.295)	(.034)	(.213)	(.227)
T2: RP UCT, two year impact	.102	-.278	-.324	.001	-.443**	-.067
	(.105)	(.259)	(.278)	(.003)	(.194)	(.190)
T2: RP UCT, four year impact	.000	-.041	.291	-.032	-.117	-.153
	(.106)	(.287)	(.275)	(.032)	(.203)	(.194)
District and Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Village Controls (LASSO Selection)	Yes	Yes	Yes	Yes	Yes	Yes
Mean Outcome, Controls at Baseline	.133	N/A	N/A	N/A	N/A	N/A
Mean Outcome, Controls at Midline		3.67	3.61	0	3.70	3.67
T1 (midline) = T2 (midline) [p-value]	[.500]	[.539]	[.919]	[.504]	[.562]	[.005]
T1 (endline) = T2 (endline) [p-value]	[.497]	[.613]	[.944]	[.504]	[.817]	[.945]
T1 (midline) = T1 (endline) [p-value]	[.503]	[.152]	[.156]	[.310]	[.094]	[.101]
T2 (midline) = T2 (endline) [p-value]	[.492]	[.534]	[.112]	[.310]	[.245]	[.753]
Observations (Villages)	176	97	97	176	175	175

Notes: *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All regressions include treatment dummies, district (strata) and survey wave fixed effects, and select village-level controls via LASSO selection. Robust standard errors are reported. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention. Columns 1 and 4 are based on the question "do [mobile banking outlet/microfinance institution] facilities exist within the village?". The outcomes in Columns 2 and 5 are based on the question "on a scale from 1 to 5, how satisfied are you with the quality of services provided by the [mobile banking outlet/microfinance institution]?" - focus groups answered from 1=highly satisfied to 5 = highly unsatisfied. We flip their answers so that higher values imply higher levels of satisfaction. Finally, the outcomes in Columns 3 and 6 are based on the question "on a scale from 1 to 5, how satisfied are you with the price you pay for the services provided by the [mobile banking outlet/microfinance institution]?" - focus groups answered from 1=highly satisfied to 5 = highly unsatisfied. We flip their answers so that higher values imply higher levels of satisfaction.

Table 3.7. Pro-Market Beliefs

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

	Pro Market Beliefs Index			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	Treated Poor	Not Treated Poor	Not Poor
	(1a)	(1b)	(1c)	(2a)	(2b)	(2b)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)	(5a)	(5b)	(5c)
A. Between Village Estimates (Treated vs Control)															
T1: Asset Transfer, two year impact	.174**	.140*	.143**	.030	-.017	-.018	.036	.019	.022	.067***	.059**	.072**	.041	.079**	.067***
	(.071)	(.079)	(.069)	(.038)	(.034)	(.035)	(.028)	(.034)	(.027)	(.025)	(.026)	(.030)	(.037)	(.034)	(.025)
	[.017]	[.080]	[.041]	[.434]	[.610]	[.615]	[.205]	[.579]	[.418]	[.009]	[.027]	[.019]	[.273]	[.022]	[.009]
	{.043}	{.130}	{.082}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{.045}	{.045}	{.045}	{.280}	{.047}	{.031}
T1: Asset Transfer, four year impact	-.120	-.092	-.005	-.032	-.003	.034	-.013	.007	.009	-.001	-.026	-.014	-.074**	-.070**	-.034
	(.080)	(.071)	(.061)	(.037)	(.034)	(.038)	(.038)	(.038)	(.036)	(.030)	(.027)	(.031)	(.031)	(.031)	(.035)
	[.136]	[.197]	[.932]	[.390]	[.926]	[.372]	[.732]	[.860]	[.804]	[.962]	[.343]	[.651]	[.018]	[.026]	[.330]
	{.204}	{.224}	{.538}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{.727}	{.263}	{.460}	{.046}	{.047}	{.316}
T2: RP UCT, two year impact	.219***	.249***	.205***	-.001	.012	-.036	.082***	.070**	.091***	.053*	.062**	.066**	.085***	.105***	.084***
	(.061)	(.069)	(.065)	(.033)	(.032)	(.031)	(.030)	(.032)	(.024)	(.028)	(.024)	(.027)	(.029)	(.028)	(.027)
	[.001]	[.000]	[.002]	[.975]	[.709]	[.242]	[.008]	[.032]	[.000]	[.063]	[.011]	[.016]	[.004]	[.000]	[.002]
	{.008}	{.001}	{.010}	{1.00}	{1.00}	{1.00}	{.064}	{.176}	{.001}	{.060}	{.045}	{.045}	{.020}	{.001}	{.016}
T2: RP UCT, four year impact	.060	.095	.051	.005	.029	.051	.011	.014	-.015	.052**	.058**	.014	-.009	-.005	.001
	(.071)	(.066)	(.061)	(.035)	(.031)	(.037)	(.035)	(.037)	(.029)	(.021)	(.023)	(.025)	(.030)	(.030)	(.030)
	[.406]	[.152]	[.409]	[.882]	[.349]	[.163]	[.750]	[.705]	[.597]	[.014]	[.013]	[.579]	[.767]	[.855]	[.974]
	{.326}	{.204}	{.326}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{.045}	{.045}	{.454}	{.664}	{.664}	{.743}
T1 (midline) = T2 (midline) [p-value]	[.548]	[.211]	[.370]	[.461]	[.438]	[.605]	[.163]	[.197]	[.003]	[.612]	[.898]	[.840]	[.276]	[.461]	[.515]
T1 (endline) = T2 (endline) [p-value]	[.017]	[.002]	[.301]	[.362]	[.369]	[.574]	[.557]	[.858]	[.474]	[.065]	[.004]	[.264]	[.052]	[.042]	[.264]
T1 (midline) = T1 (endline) [p-value]	[.010]	[.052]	[.095]	[.299]	[.816]	[.376]	[.332]	[.829]	[.775]	[.093]	[.053]	[.052]	[.008]	[.001]	[.031]
T2 (midline) = T2 (endline) [p-value]	[.157]	[.195]	[.098]	[.911]	[.728]	[.108]	[.199]	[.359]	[.012]	[.988]	[.899]	[.177]	[.022]	[.014]	[.067]
B. Within Village Estimates (Treated Poor vs Not Treated Poor)															
T1: Asset Transfer, two year impact	-.066			.015			-.018			-.011			-.052		
	(.072)			(.039)			(.031)			(.026)			(.038)		
	[.363]			[.696]			[.565]			[.675]			[.171]		
	{.326}			{1.00}			{1.00}			{.460}			{.180}		
T1: Asset Transfer, four year impact	-.209***			-.063			-.012			-.075**			-.059*		
	(.075)			(.038)			(.047)			(.030)			(.032)		
	[.007]			[.100]			[.802]			[.014]			[.075]		
	{.024}			{1.00}			{1.00}			{.045}			{.093}		
T2: RP UCT, two year impact	-.030			-.011			.007			-.019			-.007		
	(.030)			(.016)			(.015)			(.019)			(.020)		
	[.318]			[.503]			[.622]			[.332]			[.703]		
	{.326}			{1.00}			{1.00}			{.263}			{.664}		
T2: RP UCT, four year impact	-.040			-.022			-.008			-.015			.005		
	(.041)			(.022)			(.017)			(.016)			(.022)		
	[.340]			[.318]			[.631]			[.356]			[.810]		
	{.326}			{1.00}			{1.00}			{.263}			{.664}		
T1 (midline) = T2 (midline) [p-value]	[.602]			[.536]			[.465]			[.788]			[.254]		
T1 (endline) = T2 (endline) [p-value]	[.027]			[.306]			[.932]			[.031]			[.057]		
T1 (midline) = T1 (endline) [p-value]	[.189]			[.168]			[.919]			[.123]			[.888]		
T2 (midline) = T2 (endline) [p-value]	[.829]			[.703]			[.444]			[.871]			[.622]		
Mean Outcome, Controls at Midline	2.40		2.40	51.7%		54.8%	66.4%		67.5%	78.5%		73.0%	42.9%		45.1%
Observations (Households): Panel A	8126	9382	17004	8126	9382	17004	8126	9382	17004	8126	9382	17004	8126	9382	17004
Observations (Households): Panel B	8262			8262			8262			8262			8262		

Notes: *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. Households with a poverty 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, 5a), Not Treated Poor (Columns 1b, 2b, 3b, 4b, 5b), and Not Poor (Columns 1c, 2c, 3c, 4c, 5c) households in treatment and control villages. The regressions in Panel B compare Treated Poor and Not Treated Poor households within treated villages (Columns 1a, 2a, 3a, 4a, 5a). All regressions include treatment dummies, district (strata), survey wave, and enumerator fixed effects. Standard errors are clustered at the village level. We also report p-values in brackets and FDR adjusted q-values in braces. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention. 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.

3.A Appendix Tables and Figures

Table 3.A1. Respondents of Focus Groups

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. Basline						
Number of participants in focus group	7.20	7.00	6.90	[.954]	[.245]	[.177]
	(2.04)	(2.38)	(1.92)			
Panel B. Midline						
Number of participants in focus group	9.40	9.86	9.93	[.170]	[.106]	[.792]
	(1.43)	(1.16)	(1.46)			
Panel C. Endline						
Number of participants in focus group	9.10	9.34	9.45	[.546]	[.343]	[.826]
	(1.71)	(1.63)	(1.33)			

Notes: Columns 1, 2, and 3 show sample means and standard deviations for the number of participants in focus groups at baseline, midline, and endline. The p-values on the tests of equality are derived from OLS regressions of the corresponding number of focus group participants on a treatment dummy variable, and district fixed effects. Robust standard errors are estimated.

Table 3.A2. Livestock Markets

OLS estimates, robust standard errors in parentheses

Livestock type:	**Bulls/Bullocks**			**Cows**			**Sheep**		
	Available from nearest livestock market?	**Average quality of animals (1-10)**	**Average price of animal (2012 PKR)**	**Available from nearest livestock market?**	**Average quality of animals (1-10)**	**Average price of animal (2012 PKR)**	**Available from nearest livestock market?**	**Average quality of animals (1-10)**	**Average price of animal (2012 PKR)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
T1: Asset Transfer, two year impact	.081	-.200	-5338	.072	.273	-1597	.001	.185	-339
	(.102)	(.580)	(4940)	(.091)	(.510)	(3927)	(.047)	(.502)	(915)
T1: Asset Transfer, four year impact	.010	.239	-11452	.043	.022	-538	-.033	.504	-2672
	(.018)	(.408)	(7778)	(.036)	(.378)	(4986)	(.057)	(.418)	(2783)
T2: RP UCT, two year impact	.029	-.173	-1825	.087	-.331	-1540	.031	-.161	778
	(.103)	(.602)	(5136)	(.087)	(.524)	(3752)	(.034)	(.627)	(1209)
T2: RP UCT, four year impact	-.007	.369	-8664	.024	.426	2556	.031	.343	-2695
	(.019)	(.428)	(7309)	(.034)	(.380)	(4626)	(.035)	(.445)	(2694)
District and Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Controls (LASSO Selection)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Outcome, Controls at Baseline	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Mean Outcome, Controls at Midline	.759	6.70	60515	.800	6.48	55124	.967	6.61	9345
T1 (midline) = T2 (midline) [p-value]	[.609]	[.960]	[.404]	[.852]	[.234]	[.986]	[.355]	[.486]	[.381]
T1 (endline) = T2 (endline) [p-value]	[.371]	[.778]	[.612]	[.352]	[.341]	[.565]	[.148]	[.726]	[.983]
T1 (midline) = T1 (endline) [p-value]	[.495]	[.534]	[.503]	[.768]	[.691]	[.866]	[.643]	[.624]	[.421]
T2 (midline) = T2 (endline) [p-value]	[.732]	[.464]	[.442]	[.504]	[.243]	[.493]	[1.00]	[.464]	[.254]
Observations (Villages)	175	148	157	176	155	162	176	163	171

Notes: *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All regressions include treatment dummies, district (strata) and survey wave fixed effects, and select village-level controls via LASSO selection. Robust standard errors are reported. At the foot of each Column we report p-values on tests of equality of treatment effects at two and four years post intervention. Columns 1 to 3 (4 to 6) [7 to 9] inquire about the availability, quality, and price of bulls (cows) [sheep] in the nearest livestock market from the village. For each animal, the precise questions are: (i) "is this item available to buy from a livestock market in/near the village?", (ii) "on a scale of 1 to 10, rate the quality of the animal for each season of the year" - we average the answers to each season to create an average yearly measure, and (iii) "list the price at which the animal can be bought for every season of the year they are available" - we average the answers to each season to create an average yearly measure.

Figure 3.A1. Stylized Example of an Asset Menu

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

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

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