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Does Relative Deprivation Condition the Effects of Social Protection Programs on Political Attitudes?
Experimental Evidence from Pakistan*

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Abstract

Why might citizens fail to reward incumbents for providing private goods? While extant literature on the political consequences of targeted government welfare programs has focused on attribution challenges and low levels of civic education and engagement, we offer an additional explanation related to perceived inequality, or the feeling that one is poor relative to other citizens. Leveraging a regression discontinuity design and a survey experiment, we identify the causal effects of a large cash transfer program in Pakistan when relative deprivation is and is not pronounced. When relative deprivation is not salient, we find that cash transfers have little effect on attitudes toward government. However, when relative deprivation is salient, those receiving cash transfers experience increased support for their political leaders and institutions, while those denied transfers become more politically disgruntled. This has important implications for our understanding of the political ramifications of rising inequality and social protection programs.

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While income inequality between countries has declined worldwide over the last three decades, there has been a simultaneous rise in income inequality within countries—particularly in the developing world (Ravallion 2014). This trend stands to powerfully affect the political landscape of the developing world given the influence that inequality (Solt 2008; 2010) or perceived inequality (Healy, Kosec, and Mo 2017; Gimpelson and Treisman 2018) has over public opinion and political engagement. At the same time, governments are increasingly addressing poverty through social protection programs—including cash transfer programs that carefully target the poor via proxy means tests (i.e., poverty scores). Social protection programs, too, have vast potential to change public opinion and political engagement, as these programs reallocate wealth within a society from the wealthiest citizens to the poorest. However, little is known about how this trend toward greater income inequality may affect the relationship between social protection and political attitudes.

On the one hand, one’s perception of their relative economic standing may be irrelevant for the subsequent formation of political attitudes. The value of social protection to an individual may purely be a function of their absolute poverty level—for example, as when making the common assumption of decreasing marginal utility of consumption—but not a function of their relative poverty level (or their perception of it). Indeed, classic economic voting theory focuses on absolute rather than relative welfare, and holds that citizens reward the government for good economic outcomes and punish it for bad ones (Lewis-Beck and Nadeau 2011; Nadeau, Bélanger, and Didier 2013; Lewis-Beck, Nadeau, and Foucault 2013). Conversely, literature from sociology, psychology, and behavioral decision-making suggests that reference points (Kahneman and Tversky 1979; Levy 2003; Bendor 2010; Mo 2018) and policy framing (Chong and Druckman 2007) matter, and that perceived inequality may influence attitudes toward both the state (Healy, Kosec, and Mo 2017) and non-state actors (Fair et al. 2018). Namely, several strands of social science research have focused on the concept of reference groups, where a process of social comparisons with others impact both people’s affect and behavior. Equity theory (Adams 1965), relative deprivation theory (Crosby 1976; Walker and Smith 2001), and social comparison theory (Festinger 1954; Suls and Wheeler 2000) all argue that people’s evaluations of their absolute levels of income or achievements are acutely affected by comparisons with others. If perceptions of relative economic position impact the relationship between social protection and political attitudes, then social protection may not have an unambiguously positive impact on support for government.
Indeed, extant evaluations of whether citizens reward political incumbents for the provision of social protection programs find mixed impacts. We contend that citizens' perceptions of relative poverty are a significant factor in their selective rewarding of government for the provision of social protection. Specifically, when relative deprivation is not salient, receipt of goods has minimal long-term effects on attitudes toward government. However, when it is made salient, those who receive targeted government social protection feel greater support for government than those who do not receive this assistance for two reasons. First, when people feel relatively poor, beneficiaries of social protection may feel more appreciative of this aid. Second, non-beneficiaries who feel relatively deprived may feel like they are being economically left behind by their political institutions and actors. Moreover, lacking access to economic assistance that others have access to while feeling others in the country are generally better off may make the sting of one's relative poverty more acute.

We test this hypothesis using a quasi-experimental regression discontinuity design to identify the causal effects of a large unconditional cash transfer program in Pakistan—the Benazir Income Support Program (BISP)—on support for government. While it is challenging to study the effects of social protection programs since program targeting may be politically motivated, the BISP is awarded according to whether or not an individual’s family has a wealth score below a given cutoff (i.e., sufficiently poor). We show that there is no evidence of manipulation of scores at the cutoff, which provides additional reassurance that we can learn about the causal impacts of the program by comparing individuals whose families are just above and just below the cutoff. We then observe how the impacts of the BISP vary with perceptions of one’s relative economic position following an original, face-to-face survey experiment we conducted in Pakistan for which we randomly primed half of our respondents to feel relatively poor and the other half to feel that their incomes was fairly typical (e.g., in the middle of the income distribution) in Pakistan. Gimpelson and Treisman (2018) provide motivation for an approach that focuses on perceptions of inequality like we do as opposed to actual inequality. They find that individuals generally do not know how high inequality is or where they fit in the income distribution. As a result, “most theories about political effects of inequality need to be reframed as theories about effects of perceived inequality” (p. 27).

Among those primed to feel relatively poor, we find that the impact of the BISP is an order of magnitude larger than it is for those made to feel that their income is typical. This is largely
driven by those who did not get the BISP and yet were primed to feel poor experiencing severely lowered support for government. Being primed to feel poor has no negative effects on support for government among those who received the BISP. Further, we find no statistically significant effect of the BISP on those primed to feel that their incomes were fairly average and typical in their society. These findings provide rich insights into how government provision of public and private goods influences attitudes toward government. They also have important implications for our understanding of the political ramifications of inequality. For the case of cash transfers, the findings show that, absent perceptions of inequality, cash transfers do very little in fostering greater long-term support for government relative to non-beneficiaries.

Our study relates to a large literature on the impacts of inequality on political outcomes. The canonical model of Meltzer and Richard (1981) shows that the larger the gap between median and mean incomes, the greater the likelihood of fiscal transfers (e.g., via a cash transfer program) from rich to poor under majority-rule voting. A number of studies find links between inequality and public investment in goods that predominately benefit the poor—including social protection (Lupu and Pontusson 2011; Kosec 2014). Some studies further link inequality directly with opposition to status quo political institutions and political violence (Alesina and Perotti 1994; Cramer 2005). This literature points to endogeneity challenges when studying how inequality affect the relationship between any redistributive social welfare policy on political attitudes, as inequality may make the likelihood of social protection programs more likely, and the levels and/or salience of inequality impacts political attitudes. Thus, our reliance on an experiment to generate exogenous variation in perceived inequality is an important methodological contribution that enables us to make causal inferences in an examination of whether perceived inequality affects the relationship between targeted government programs and political attitudes.

The paper is organized as follows. First, we explore the rich set of studies exploring the effects of targeted cash transfer programs and other social protection programs on political attitudes. Second, we provide background information on Pakistan’s Benazir Income Support Program (BISP)—one of the world’s largest unconditional cash transfer programs to date, and the social protection program whose effects we consider in this paper. Next, we describe our dataset, which combines (1) administrative data from the Pakistani government containing wealth scores and information on BISP receipt at the individual-level; and (2) an original face-to-face survey experiment we
conducted in Pakistan in 2013. We also provide details on the government support measures we employ in this section. We then discuss our estimation strategy, which involves overlaying a regression discontinuity design on top of a priming experiment in order to generate two source of plausibly exogenous variation: (1) variation in whether or not an individual received cash transfers through the BISP; and (2) variation in whether or not an individual feels relatively poor. Finally, we present our empirical findings and discuss their implications for political science research and public policy.

Citizen Reactions to Social Protection Programs

The evidence on whether citizens reward policymakers for providing public and private goods that raise their welfare is mixed. To shed further light on this question, we carried out a systematic review of studies published in top outlets in the last 20 years examining the effects of cash transfer or social protection programs on political attitudes. Appendix Table A.3 documents key features of each study. Online Appendix D details the criteria we followed to conduct the systematic literature review. A subset of studies demonstrates that receipt of targeted social protection programs translates into increased voter turnout and increased support for the incumbent delivering the program (e.g., Diaz-Cayeros, Estévez, and Magaloni (2009); Manacorda, Miguel, and Vigorito (2011); Pop-Eleches and Pop-Eleches (2012); Chen (2013); Labonne (2013); Zucco (2013); Linos (2013); De La O (2013); Layton and Smith (2015); Marschall, Aydogan, and Bulut (2016); Conover, Zarate, Camacho, and Baez (2018)). Indeed, even campaign promises to provide such programs can elicit voter support (Elinder, Jordahl, and Poutvaara 2008). In contrast, work by Green (2006) and Imai, King, and Velasco Rivera (2019) in Mexico challenges the conclusion that targeted government welfare programs translate to political support. Reexamining the effects of Progresa, a major conditional cash transfer program in Mexico, they find no impacts of the cash transfer program Progresa on voter turnout or the vote share of the ruling party. Ellis and Faricy (2011) similarly find that U.S. public opinion is unresponsive to the total amount of federal social welfare spending. And Corrêa and Cheibub (2016), reviewing several cash transfer programs in Latin America, actually find net anti-incumbent effects, driven by increases in support for the opposition among non-beneficiaries that outweigh increased support for the incumbent among beneficiaries. Camacho (2014) similarly identifies mixed impacts of cash transfers in Peru on trust in institutions—including reduced trust.
among non-beneficiaries—and finds no impacts on membership in social organizations.

Extant literature has focused heavily on attribution challenges contributing to citizens’ failure to reward policymakers for social protection provision. If citizens do not know who deserves credit, they will not properly assign it. León (2011) shows that voters are best able to appropriately give credit for government efforts in regions with very high and very low levels of decentralization; in such settings, one level of government clearly predominates, and it is clear who deserves credit or blame. In contrast, voters find attribution challenging in regions with a more intertwined distribution of powers (Hobolt, Tilley, and Wittrock 2013; Buntaine, Jablonski, Nielson, and Pickering 2018). Attributing blame and credit to government has been shown to be affected by the political party in power, as citizens have biased assessments of government performance based upon whether the party in power is the party with which they identify (Malhotra and Kuo 2008). Voters may also exhibit recency bias—or the tendency to credit government for recent provision of goods and services more than provision further in the past (Cole, Healy, and Werker 2012; Galiani et al. 2017). This helps explain increased political spending preceding elections—especially in developing countries (Breder and Drazen 2005; Shi and Svensson 2006; Drazen and Eslava 2010) and other settings with low fiscal transparency or a high degree of political polarization (Alt and Lassen 2006).

Other literature explaining citizens’ inconsistency in rewarding policymakers for provision of social protection has focused on the role of low levels of civic education and civic engagement. The seminal work of Downs (1960) shows that voters are ignorant of the costs and benefits of many actual and potential government policies, leading governments to enact smaller budgets than they otherwise would. However, even if voters know policies’ costs and benefits, Cantril and Free (1967) highlight how individuals may irrationally say that they want more spending on social programs while simultaneously demanding smaller government—a point echoed in later literature (e.g., Ladd et al. (1979); Sears and Citrin (1982); Bennett and Bennett (1990)). Civic education may help. While Kramon (2011) finds that less educated voters in Kenya are more likely to prefer vote-buying candidates, Gottlieb (2016) shows that citizens in Mali are more likely to hold leaders accountable to a higher standard if they are educated about local government capacity and responsibility, as well as how local politicians perform relative to others. Relatedly, Fujiwara and Wantchekon (2013) show that town hall meetings held in Benin based on programmatic, non-clientelist platforms reduce the prevalence of clientelism and lower the vote shares of candidates with a political stronghold.
in the village. Evans, Holtemeyer, and Kosec (2019) make a related point in the context of a cash transfer program in Tanzania, showing that effective communication about the program with citizens through frequent village meetings can maximize its beneficial impacts on trust in leaders.

Some scholars have also hypothesized that citizens may fail to reward government for the provision of social protection because of the very nature of these programs. While they deliver monetary and other benefits to individuals and their households, many impose conditions which have been criticized for robbing beneficiaries of a sense of agency and partnership with the state (Freeland 2007). Further, participation in such programs may carry a social stigma which foments feelings of social isolation and powerlessness, and decreases civic engagement (Mettler and Stonecash 2008; Oduro 2015). Policymakers may also use social protection programs for overtly political purposes (Bruhn 1996; Dahlberg and Johansson 2002; Guo 2009; Costa 2011; Brollo and Nannicini 2012; Aytaç 2014), which has implications for how they are perceived.1

We posit that the literature on the provision of social protection programs is mixed, in part, because perceived inequality affects the relationship between receipt (or non-receipt) of a program and political attitudes. Perceived inequality has been shown to have a direct impact on political attitudes (Tocqueville 1856; Healy, Kosec, and Mo 2017). Moreover, citizens make social comparisons (Festinger 1954; Adams 1965; Crosby 1976; Suls and Wheeler 2000), and as such, when feelings of relative deprivation are acute, targeted government programs can generate (1) a non-beneficiary group that feels relatively poor and left behind by their government; and (2) a beneficiary group that feels relatively poor and heard by their government. When people feel left economically behind by their state, recipients of government social protection may feel more grateful for this support; however, non-recipients may feel greater frustration towards their government, as their economic needs are being neglected while others are being helped. The creation of these two groups would correspond with a positive difference in the political attitudes of beneficiaries relative to non-beneficiaries.

While inequality level is an imperfect measure of perceived inequality, as a suggestive exercise, we consider the inequality levels—measured by the country’s Gini coefficient—in each of the study contexts that comprise our systematic literature review of targeted government social protection

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1In contrast, Fried (2012) and Sugiyama and Hunter (2013) find little evidence that political criteria influenced the distribution of transfers through the Bolsa Família program in Brazil.
programs and political attitudes. We find a positive correlation between inequality levels and research findings—consistent with our hypothesis (see a summary of research findings, country context(s), inequality level of the country context(s) of recent studies on citizen reactions to social protection programs in Table A.3 in Online Appendix D). Among studies finding a positive impact of social protection on attitudes toward government, the Gini coefficient is on average 0.092 standard deviations higher than it is in studies which identify null and/or some negative impacts.\(^2\)

**Background on the Benazir Income Support Program (BISP)**

Rural Pakistan provides a useful context in which to study how social protection affects attitudes toward government, and whether or not perceived inequality moderates this relationship. The country is governed under a parliamentary system where the president is head of state and a popularly-elected prime minister leads the government. Since independence in 1947, the country has frequently switched between democratically elected civilian governments and military-led governments. The latest transition to civilian rule occurred following elections in February 2008 that brought to power a coalition led by the Pakistan People’s Party (PPP). The government subsequently experienced a peaceful democratic transition in 2013—immediately following our April–May 2013 survey. While inequality in Pakistan is relatively low (its Gini coefficient is the world’s 130th largest—about equal to that of the European Union), poverty, particularly in rural areas, is exceptionally high (its GDP per capita is the world’s 171st highest) (Central Intelligence Agency 2017). In such an environment, citizens are unlikely to know actual levels of inequality (Gimpelson and Treisman 2018), which makes it a context that is ripe for a study like ours that aims to subtly manipulate perceptions of inequality.

Pakistan is also an excellent setting for studying the impacts of social protection policies. As the world’s sixth largest country, with over 207 million people, Pakistan is home to a sizable share of the world’s poor. The Pakistani federal government launched its first ever nation-wide social protection program, the Benazir Income Support Program (BISP), in July 2008. The PPP named the program after Benazir Bhutto—their late leader who had been assassinated just before the

\(^2\)Possibly owing to publication bias, of 25 total studies, only four identify null and/or some negative impacts of social protection. As such, this difference in means (the mean Gini coefficient in studies identifying positive impacts is 42.109, compared to a mean Gini of 41.325 in the four other studies), which amounts to 0.092 standard deviations of the Gini coefficient, is not statistically significant at conventional levels. This motivates more analysis of the role of inequality in moderating the impacts of social protection on political attitudes.
2008 elections. Pakistan was in the midst of a food, fuel, and financial crisis (Cheema, Hunt, Javeed, Lone, and O’Leary 2014) and GDP per capita had declined since 2007 (World Bank 2017). Donors providing support included the UK’s Department for International Development and the World Bank. The BISP’s stated goals were to eradicate extreme poverty, empower women, and achieve universal primary education by providing cash transfers to poor women, and disbursements commenced in October 2008 (Gazdar 2011; Ambler and De Brauw 2017; Haseeb and Vyborny 2017).

Social protection programs may be vulnerable to capture or clientelism (Keefer 2007). In the case of the BISP, senior PPP party leaders agreed to use an objective, donor-designed system to select beneficiaries, but were eager to start distributing funds before such a system could be feasibly developed (Haseeb and Vyborny 2017). As a result, members of parliament were asked to identify a set number of beneficiaries (8,000 each for members of the Senate and the National Assembly and 1,000 each for members of the Provincial Assembly) (Government of Pakistan 2017). They were provided with minimal criteria, all of which were readily verifiable using the existing national ID database: beneficiaries should not have a machine readable passport, an ID card for emigrants, an account with a foreign-owned bank, or a household member who is a public servant. 4.2 million targeted individuals subsequently filled out application forms, and, following a screening process carried out by Pakistan’s National Database and Registration Authority—an independent and autonomous agency under the Ministry of the Interior—1.8 million beneficiaries had been selected by 2009 (Gazdar 2011; Government of Pakistan 2017).³ By including all opposition party politicians in the selection of beneficiaries and setting a quota for each politician, the government aimed to avoid favoritism, but they were only partly successful; for example, households from the origin villages of members of parliament were ultimately 200–400 percent more likely to receive BISP transfers than were those in rival politicians’ villages (Haseeb and Vyborny 2017). Unsurprisingly, opponents of the PPP party objected that the program was politicized (Haseeb and Vyborny 2017).

To make targeting more transparent and fair, in 2009, the federal government began reforming the system to base beneficiary status on a family’s wealth score, computed using a proxy means test (PMT) (Gazdar 2011). The federal government carried out a BISP Poverty Census to collect data

³52 percent of nominations were disqualified for not meeting these four basic criteria (Haseeb and Vyborny 2017).
for the PMT during October 2010 – December 2011, covering 155 million people from 27 million families.\textsuperscript{4} They began using these data to distribute transfers in July 2011 (Haseeb and Vyborny 2017), and the number of beneficiary families rapidly expanded to 5.3 million (Government of Pakistan 2017).\textsuperscript{5} This Poverty Census gathered data on 23 variables, which were used to compute a family wealth score ranging from 0 to 100 (Ambler and De Brauw 2017).\textsuperscript{6} Eligible families were those with scores below 16.17, or with scores between 16.17 and 21.17 who met at least one of the following three criteria: 1) at least one disabled member; 2) at least one senior citizen (65 years of age or older) and less than three total family members; or 3) four or more children under age 12 (Ambler and De Brauw 2017). Recipients selected under the old system (i.e. by members of parliament) who did not qualify under the new criteria were removed from the list, notified by a letter, and their payments stopped. Since this reform and up to the date of our household survey (April–May 2013), citizen removal from the program (e.g., due to an increase in wealth) has been almost non-existent (Haseeb and Vyborny 2017). The average wealth score among respondents in our dataset is 22.8, a score indicating ineligibility for the BISP. As shown in Table 1, 35 percent of the households in our sample are BISP recipients.\textsuperscript{7}

The BISP aims to deliver cash transfers to each ever-married female in eligible families, as long as they possess a valid Computerized National Identity Card (CNIC) (Gazdar 2011). While individuals did not need to apply for the BISP, upon receipt of a qualifying wealth score, beneficiaries had to register at their local BISP office to receive transfers (Ambler and De Brauw 2017). Throughout the period of our study, BISP beneficiaries received quarterly payments of PKR 3,000 (approximately $39.00 USD in early October 2008)—an amount equivalent to about 8.9 percent of average quarterly consumption expenditure per adult equivalent (Cheema et al. 2014). Beneficiaries received payments either through the Pakistan Post (mainly in rural areas) or ATM cards that can be used at ATMs or Point-of-Sale machines (Ambler and De Brauw 2017). Our data analyze

\textsuperscript{4}While the main wave of data collection began in October 2010, in June 2010, the government collected data in 15 pilot districts (out of 106 total districts in the country at the time) (Haseeb and Vyborny 2017).

\textsuperscript{5}This switch naturally ended BISP access for some while simultaneously extending it to previous non-beneficiaries.

\textsuperscript{6}All individuals in a family have the same wealth score. Weights placed on each of the 23 variables were developed using the 2007/8 Pakistan Living Standards Measurement Survey, but have not been publicly released (Ambler and De Brauw 2017).

\textsuperscript{7}The national average share of households that are BISP recipients is 23 percent, reflecting the deeper poverty of our entirely rural sample.
the BISP after implementation of PMTs to target beneficiaries; we check using both a McCrary (2008) test and a Cattaneo, Jansson, and Ma (2017) test, and find no evidence in our data of elite manipulation of access to the BISP.

Our regression discontinuity design (RDD) identification strategy importantly circumvents any identification concerns stemming from endogenous spatial variation in the timing of families’ access to the BISP. Spatial variation may occur either due to members of parliament initially (in 2008) targeting some mauzas (i.e. villages) but not others, or due to uneven migration to the PMT system of targeting.\(^8\) Regardless, it is important to note that at the time of our April – May 2013 household survey which we use in all analysis, beneficiaries of the BISP may have had it for anywhere from 1 year, 9 months (for those never targeted by members of parliament, who received PMT scores in June 2011 and their first transfers in July 2011, and who we surveyed in April 2013)\(^9\) up to 4 years, 7 months (for those who received transfers since October 2008 and who we surveyed in May 2013).\(^{10}\)

**Data and Measurement**

Our results come from marrying two data sources: (1) administrative data on eligibility for and participation in the BISP provided to us by the Office of the Secretary of the BISP; and (2) an original household panel survey data we collected in rural Pakistan during March–April 2012 (round

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\(^8\)A household survey we collected during April–May 2013, across 76 rural mauzas, revealed that 15 percent of respondents lived in a mauza where a community focus group claimed that the BISP arrived in their mauza starting in 2008, while 26 percent lived in mauzas where it started in 2009, 37 percent in 2010, 18 percent in 2011, and 2 percent in 2012. Two percent lived in mauzas where the program supposedly arrived in 2007, which is not possible.

\(^9\)In the administrative data given to us by the BISP Secretariat on the individuals we surveyed in our April–May 2013 household survey, over 95 percent received their PMT score in 2011 or earlier. The less than 5 percent that received a PMT score in 2012, and the less than 0.1 percent that received it in 2013, appear to be cases of individuals who could not be reached during the 2010–11 BISP Poverty Census (e.g., due to being temporarily away from their mauza at the time)—meaning a small share of our sample may have received the BISP for less than 1 year, 9 months. One other possible reason for a beneficiary receiving the BISP for less than 1 year, 9 months would be if they delayed, after receiving their poverty score, going to the BISP office to register to receive transfers. While this is unlikely given the ease and financial incentives of registering, it does not invalidate our RDD identification strategy.

\(^{10}\)We lack data on whether or not an individual received BISP transfers prior to the development and use of the PMT methodology. This means that some share of individuals we identify as non-beneficiaries when analyzing our April–May 2013 survey data may have in fact been beneficiaries in 2010 or earlier, even though they have not been beneficiaries during the last 1 year and 9 months. This would be the case only where members of parliament targeted an individual in 2008 who in 2011 received a wealth score above the BISP cutoff (making them ineligible for transfers). After nearly two and a half years of receiving no BISP transfers, the effects of the BISP on political attitudes should largely have worn off—especially for relatively less-poor households not qualifying for the BISP in 2011, for whom BISP transfers should accordingly be a relatively small share of their income. Regardless, receipt of BISP in 2008 by non-beneficiaries in 2011 could **downward-bias** our estimates of any beneficial effects of the BISP on government support, as some of our non-recipients received this aid in earlier years.
1) and April–May 2013 (round 2). Hereafter, we refer to this surveying effort as the International Food Policy Research Institute’s (IFPRI’s) Pakistan Rural Household Panel Survey (RHPS).

The RHPS included a common set of topics across rounds, plus select topics in certain rounds—including a governance module asked only in round 2. In all rounds, the survey gathered information on individual demographics, household income generation, credit and savings, and access to social protection programs. The RHPS covered 2,090 households in 76 villages in Punjab, Sindh, and Khyber-Pakhtunkhwa (KPK) provinces.\footnote{The RHPS provides village-, household-, and individual-level data on a range of economic, political, and social topics. The RHPS sample was selected using a multi-stage, stratified sampling technique. 19 districts were selected: 12 from Punjab, five from Sindh, and two from KPK. The sampling frame excluded Balochistan, the Federally Administered Tribal Areas, and 13 of KPK’s 24 districts due to safety concerns. Districts in each province were selected using a probability proportionate to size approach. In each district, four mauzas (villages) were randomly selected, and then 28 households were randomly chosen from each village. Urban villages and those with populations greater than 25,000 were excluded from the sampling frame.} Two respondents per household–the head of each household and his/her spouse–completed household surveys.\footnote{In cases where the head or spouse was not available, a second visit was made to the household. If the individual was still not available, another knowledgeable household member of the same gender was selected instead.}

The RHPS further collected the Computerized National Identity Card (CNIC) numbers of household heads and their spouses in both rounds.\footnote{If either was unavailable, another knowledgeable person of the same gender was interviewed.} The Secretary of the BISP maintains an administrative database of all CNIC numbers in Pakistan and two key pieces of information which we requested for each CNIC number we collected: their family’s wealth score and whether they are a registered BISP beneficiary. As household income is typically shared across members, we used the administrative data to code a household-level indicator for having at least one member that was a recipient of the BISP.\footnote{It is possible and not uncommon for multiple families to live in one household in Pakistan, meaning there may be multiple distinct wealth scores in a household (e.g., imagine one family owns land but another does not) (Cheema et al. 2014). Also, even in households with a single family, multiple women might be eligible to be BISP beneficiaries. On average, 10 percent of households with a BISP beneficiary had more than one (Cheema et al. 2014).} Ultimately, we were able to collect data on our governance outcomes for 3,908 individuals from 2,002 different households. Of those individuals, 3,300 individuals from 1,921 households reported to us a “plausible” CNIC number.\footnote{349 individuals reported a single-digit CNIC number, which is not valid and likely reflects enumerator or data entry error, and 259 either did not have a CNIC number or declined to provide it.} Of the 3,300 plausible CNIC numbers, 80...
percent (2,639) were in the BISP administrative database.\textsuperscript{16,17} Households lacking a CNIC number in the BISP database had to be omitted from our analysis.

We constructed our household-level BISP beneficiary variable as follows. For each individual in the BISP administrative database, we had a wealth score and a beneficiary indicator variable. A wealth score is a family-level variable, while the beneficiary indicator is an individual-level variable. The beneficiary indicator was uniformly 0 for all males, in keeping with the program’s targeting of women. In households for which we had a female respondent in the BISP administrative database, our household-level beneficiary indicator is simply identical to this female’s beneficiary indicator variable. In the administrative data, only 7.5 percent of the time (in 34 out of 452 cases) did a woman with a wealth score under 16.17 have a beneficiary indicator variable equal to 0, suggesting a high rate of registering to receive BISP transfers among the eligible. In households for which we had a male but not a female respondent in the administrative database,\textsuperscript{18} given that his beneficiary status was always 0, we had to make use of his poverty score—plus demographic data from our households survey—to code a household-level BISP beneficiary indicator. This is non-ideal since the poverty score and household demographics only tell us his family’s eligibility to receive the BISP—not whether in fact a family member actually registered.\textsuperscript{19} Fortunately, there were few households with administrative data for only a male household member (and not a female) and for which the household’s poverty score is under 21.17. Specifically, in our sample of 2,639 individuals (male and female), only 176 came from households with only a male in the BISP database and a family poverty score under 16.17. Among these 176 individuals, only 38 came from households with only a male in the BISP database, with a family poverty score between 16.17 and 21.17, and with household demographics indicating that the household was eligible despite having a poverty score above 16.17. While a small share of these 214 individuals we code as beneficiaries may have

\textsuperscript{16}CNIC numbers missing from the database may have been missing for several reasons: individuals may have knowingly or accidentally reported false numbers, enumerators may have incorrectly recorded numbers (we used a pen and paper survey), data entry may have introduced errors, the individual could have only recently received a CNIC number (and it was not yet provided to the BISP), or the database from the Secretary of the BISP may have been incomplete for reasons unclear to us.

\textsuperscript{17}These valid and matched CNIC numbers came from 1,349 different households (i.e. 67 percent of the original 2,002 households).

\textsuperscript{18}In all such households, there was an adult female, but we simply did not have administrative data on her.

\textsuperscript{19}All sample households contained an adult female, so it is never the case that a household with a poverty score under 16.17 simply does not have adult female members that could register to receive BISP payments.
failed to register, this would if anything generate a downward bias in any estimates of the benefits of the BISP, as a small set of individuals who we count as BISP recipients actually received no aid due to their failure to register and take-up the program.

While we collected data during both rounds 1 and 2 of our survey on self-reported receipt of the BISP, research shows that participation in social protection programs often carries a social stigma (Mettler and Stonecash 2008; Oduro 2015), which may make individuals hesitant to admit that they receive social protection in a survey setting. Thinking that a “yes” answer would result in a set of follow-up questions, individuals may also wish to shorten the length of a survey by answering “no” upon being asked whether or not they received social protection programs—whether or not they do. Alternately, but equally problematic from a research standpoint, individuals may be eager to convey their need for additional welfare to enumerators—who they may suspect are providing information to government. This may manifest itself as under-reporting of what one currently receives—such as by saying one is not a BISP beneficiary when in fact they are. These potential sources of bias in self-reported information motivate our use of administrative data. It is nonetheless useful to consider the prevalence of conflicts between our administrative data source, which we use to code our beneficiary dummy, and responses to a question in each of rounds 1 and 2 of our household survey, which asked “Has [NAME] received any assistance in the last 12 months from the BISP?” Combining data from rounds 1 and 2 of our survey allow us to code a dummy variable for the household having received the BISP during *either* of the 12 month periods preceding these two survey rounds. Since individuals chosen by the PMT to be beneficiaries in 2011 almost universally remained beneficiaries (Haseeb and Vyborny 2017), coding a dummy in this way helps us minimize the likelihood of mis-coding a beneficiary during this two year period as a non-beneficiary purely due to, for example, a failure in one of the two years for the respondent to report receipt of BISP. Obviously, these two data sources are not fully comparable; while our survey tells us whether the individual claims to have received support at some point during a two year period, the administrative database tells us who were beneficiaries of the BISP when we inquired—i.e. in the database as it stood in March 2013. However, we would expect these numbers to be largely similar—which is precisely what we find. Among the 2,639 individuals in the BISP database, 84 percent of the time, their beneficiary status in our administrative database matched what was constructed using data from our two household survey rounds. Among the 1,705 individuals who our administrative data lead us to
code as non-beneficiaries, only 9 percent claimed (during either round 1 or 2) on our survey to be beneficiaries. However, among the 934 individuals who our administrative data lead us to code as beneficiaries, a larger, 28 percent claimed on our survey to be non-beneficiaries. It is hard to assess whether these disagreements are due to inaccurate respondent reports (e.g., imagine a respondent who wants to shorten a lengthy survey by responding “No” to a filter question asking if they have received any support from government programs) or due to legitimate reasons (e.g., imagine the beneficiary and her husband moved out of her in-laws’ household in 2012 to form their own household; our constructed dummy would code the original household as a beneficiary while the BISP administrative database would not). Nevertheless, this discrepancy with the 934 individuals, which includes non-beneficiaries in the group of beneficiaries, would lead to a downward bias in our findings if BISP receipt does lead to positive effects on government support.

The round 2 governance module began with an experiment, described in the next section, before asking respondents seven questions related to their level of support for government, drawn from the “system support” battery of the AmericasBarometer survey of Latin American countries (Booth and Seligson 2009). Exact question wordings and response options are enumerated in Online Appendix A. These questions probe the extent to which individuals feel that the courts guarantee a fair trial, they respect political institutions, they feel basic rights are protected, they feel proud of the political system, they feel that others should support the political system, they trust the political system, and they feel that leaders are doing the best job possible. Each of these questions had five response options that were coded to range from 0 (Not at all) to 1 (A great deal).

System support can be conceptualized as both support for a specific government regime, or as more diffuse attitudes towards democracy (Easton 1967; Lipset 1981; Booth and Seligson 2009). Previous research has shown that trust in government is of a general character, whereby a high level of trust in one institution usually extends to trust in other institutions (Christensen and Lægreid 2005). The seven items have a high Cronbach’s alpha score of 0.88, indicating high levels of internal consistency. Given this high reliability coefficient, as well as our concerns with multiple hypothesis testing—whereby, upon testing a sufficiently large number of null hypothesis, at least one is likely to be significantly affected by a key explanatory variable merely by chance—we construct a government support index which is the average of all seven measures, and use this as our principal measure of government support. Additionally, an advantage of an averaged measure is that it nets
out measurement error associated with any one of the index components (Ansolabehere, Rodden, and Snyder 2008). Our government support index and each of the seven individual measures comprising it are summarized in Table 1. Among those with wealth scores, the index has a mean of 0.37, indicating average support for government somewhere between ‘a little’ and ‘somewhat’; and its standard deviation is 0.19.

If receipt of the BISP impacts citizens attitudes, we would expect it to change at least some outcomes related to citizen welfare. To ensure that the BISP has these hypothesized effects, we included several economic measures in our survey that may be associated with greater economic well-being and productivity (see Panel A in Table 2): total expenditure on food per month, total expenditure per month, total cash loans outstanding as a share of yearly total expenditure, total savings as a share of monthly expenditure, and a dummy for the household operating a non-agricultural enterprise (see Online Appendix B for exact question wordings). Descriptive statistics of these measures are provided in Table 1. In our sample for analysis, the average household food expenditure per month is 9,902.17 Pakistani Rupees (Rs.), the average household expenditure per month is Rs. 16,140.83, and 16 percent of respondents live in households that earn income from outside of the agricultural sector. Additionally, the average individual’s household is able to save the equivalent of 14.3 percent of their monthly expenditure, and outstanding loans—if any—equal roughly 32.3 percent of their annual expenditures.

[Table 1 about here]

Research Design

Our study combines quasi-experimental and experimental methods. Specifically, we leverage both a regression discontinuity design (RDD) and a priming survey experiment. In this section, we discuss each of these in turn, explaining how they allow us to estimate the causal effects of being a beneficiary of an unconditional cash transfer program, as well as how these effects vary according to an individual’s perception of how poor their household is relative to others.

Regression Discontinuity Design

To estimate the causal effect of being a beneficiary of an unconditional cash transfer, we employ a quasi-experimental procedure that exploits the fact that BISP relied on a wealth score threshold
(16.17) to determine program eligibility. Receipt of BISP aid is a discontinuous function of a household’s wealth score. Essentially, this regression discontinuity design allows for an identification strategy that compares the outcomes of households that were marginally ineligible to be beneficiaries with those that were marginally eligible to evaluate the program’s effects. This is significant because of selection bias concerns. Consider the following empirical specification:

\[ Y_i = \beta_0 + \tau B_i + \epsilon_i \]  

(1)

where \( i \) indexes households. We denote by \( Y_i \) our outcome of interest—support for government—and by \( B_i \) receipt of cash transfers through the BISP. \( \epsilon_i \) is measurement error, and \( \tau \) is our parameter of interest—the relationship between receipt of BISP aid and our outcome of interest. If individuals receive aid because of political connectedness or other unobservable characteristics of individuals, which would correlate with the political attitudes we are interested in, direct estimation of \( \tau \) by estimating equation (1) would be biased.

To overcome this aforementioned selection bias, our empirical analysis leverages the fact that receipt of cash transfers from the BISP is based on how a household’s wealth score \( X_i \) resulting from a proxy means test compares with a cutoff score \( c \). In other words, \( X_i \) is our forcing variable; households for which \( X_i \leq c \) receive the BISP while most of those for which \( X_i > c \) do not. We can estimate the causal effect of the BISP if the distributions of unobserved characteristics of individuals just above the cutoff score and just below are essentially drawn from the same population. Formally, this condition requires:

\[ \lim_{\Delta \downarrow 0} E[\epsilon_i | X_i = c + \Delta] = \lim_{\Delta \uparrow 0} E[\epsilon_i | X_i = c + \Delta] \]  

(2)

where \( \epsilon_i \) is the unobserved determinant of future outcomes.

If equation (2) holds, the following indicator variable for having a score below the cutoff, \( D_i \), can serve as an instrumental variable for receipt of the BISP:

\[ D_i = \begin{cases} 
1, & \text{if } X_i \leq c \\
0, & \text{if } X_i > c. 
\end{cases} \]  

(3)

In our case, the threshold is not a sharp cutoff given that a few exceptions were made, such as allowing households above 16.17 and below 21.17 with many young children, at least one senior citizen, and/or at least one disabled member to receive the program despite having a wealth score
above the cutoff. Also, a few families with wealth scores below the cutoff had not yet received BISP transfers at the time of our survey (see the section above about the BISP for greater details on program eligibility and take-up). We thus employ a fuzzy RDD, which does not require a 100 percent jump in the probability of receiving the treatment at the cutoff, and only requires the following to hold:

\[
\lim_{\Delta \downarrow 0} Pr[D_i = 1 | X_i = c + \Delta] \neq \lim_{\Delta \uparrow 0} Pr[D_i = 1 | X_i = c + \Delta].
\] (4)

As the probability of treatment jumps by less than one at the threshold, the jump in the relationship between outcome \(Y_i\) and wealth score \(X_i\) can no longer be interpreted as an average treatment effect. As in an instrumental variable setting, however, the treatment effect can be estimated by dividing the jump in the relationship between \(Y_i\) and \(X_i\) at \(c\) (the reduced form estimate) by the fraction induced to take up the treatment at the threshold (the first stage estimate). Thus, we can estimate our parameter of interest \(\tau\) for outcome \(Y_i\) as follows:

\[
\tau_F = \frac{\lim_{\Delta \downarrow 0} E[Y_i | X_i = c + \Delta] - \lim_{\Delta \uparrow 0} E[Y_i | X_i = c + \Delta]}{\lim_{\Delta \downarrow 0} E[D_i | X_i = c + \Delta] - \lim_{\Delta \uparrow 0} E[D_i | X_i = c + \Delta]}
\] (5)

where we assume equations (2) and (4) hold, and the \(F\) subscript refers to the fuzzy RDD.

In operationalizing a fuzzy RDD design, we use local polynomial methods to fit two separate regression functions below and above the cutoff. We weight observations by applying a kernel function to the distance between each observation’s wealth score \(X_i\) and the cutoff. These kernel-based estimators require selection of a bandwidth, whereby observations outside the bandwidth receive zero weight. Following Calonico, Cattaneo, and Titunik (2014a, 2014b) and Calonico, Cattaneo, Farrell, and Titunik (2017), we select an optimal bandwidth that minimizes the mean squared error (MSE). We then employ the robust confidence intervals developed by Calonico, Cattaneo, and Titunik (2014b), which estimate the asymptotic bias ignored by conventional inference and correct the standard errors appropriately to produce valid inferences.\(^{20}\)

This empirical strategy falls apart if the wealth score cutoff is not an appropriate instrument for receiving BISP aid. Reassuringly, this assumption is quite robust. We recode our 16.17 cutoff to be at 0, and at this cutoff, there is a 59 percentage point (\(p < 0.001\)) bump in the BISP transfers.
beneficiary rate (see Figure 1 and column (1) of Table 2). Moreover, if there is a discontinuous difference in respondent characteristics around the score threshold, Equation (2) will not hold, thereby compromising our empirical design. We test for this by assessing whether observable pre-treatment measures of the study participants trend smoothly at the cutoff. We consider a number of pre-treatment demographic characteristics that were collected in our survey: gender, age, marital status, education, parental education, and ethnicity. When we conduct a fuzzy RDD analysis for each of the 17 pre-treatment demographic characteristics, there is not one measure that is significantly different at a 5 percent significance level at the cutoff (see Figure A.2 and Table A.1 in the Online Appendix). Thus, the assumption that there are no meaningful differences in pre-treatment measures at the cutoff appears to hold.

Another threat to causal interpretation would result if households and/or the government actors generating wealth scores could manipulate scores near the cutoff. Since receipt of cash transfers from the BISP is based on these scores, such manipulation would imply that households on either side of the cutoff differ in discrete ways precisely at the cutoff; for example, those with wealth scores just below the cutoff might be substantially more politically-connected. Fortunately, we can empirically test for manipulation at the cutoff. Specifically, we test the null hypothesis of continuity of the density of the forcing variable—the wealth score, \( X_i \)—at the cutoff (McCrary 2008). Reassuringly, as shown in Figure A.1 in the Online Appendix, we find that there is no discontinuity at the cutoff in the density function of the wealth score \( p = 0.604 \). We also conduct a manipulation test based on density discontinuity (Cattaneo, Jansson, and Ma 2017), and find no evidence of a discontinuity \( p = 0.234 \).

Survey Experiment

Study participants were either induced to feel relatively poor, which we refer to as receiving a poverty prime, or were assigned to a control condition designed to frame their economic position more neutrally. Both respondents within a household received the same treatment, reducing the potential for spillovers. The half which received the poverty prime, which we refer to as the “treatment” group, were primed to feel as if their income was in the bottom part of the income

\[ ^{21} \text{This test is conducted using the authors’ rddensity software.} \]
distribution. The half assigned to the “control” condition were made to feel that their income was more typical (roughly at the median of the distribution). Specifically, we asked respondents the following question: “Income is the amount of cash income you earn from all agricultural and non-agricultural activities, and money from the Benazir Income Support Programme (BISP) or other programs. How much income did your family earn last month?” We then randomly assigned them to one of the following two sets of response options:

<table>
<thead>
<tr>
<th>Control (No Poverty Prime)</th>
<th>Treatment (Relatively Poor Group) (Poverty Prime)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0-2,000 Rs.</td>
<td>0-12,500 Rs.</td>
</tr>
<tr>
<td>2,001-4,000 Rs.</td>
<td>12,501-25,000 Rs.</td>
</tr>
<tr>
<td>4,001-6,000 Rs.</td>
<td>25,001-45,000 Rs.</td>
</tr>
<tr>
<td>6,001-10,000 Rs.</td>
<td>45,001-60,000 Rs.</td>
</tr>
<tr>
<td>More than 10,000 Rs.</td>
<td>More than 60,000 Rs.</td>
</tr>
</tbody>
</table>

Our poverty prime is a variation of one used by Haisley, Mostafa, and Loewenstein (2008) to study the decision to participate in lotteries. Mo (2012; 2018) first employed this design to study the effects of relative poverty on political behavior in the context of Nepal, and both Healy, Kosec, and Mo (2017) and Fair et al. (2018) replicated that design in Pakistan. The logic of this prime derives from previous research showing that response options to ordinal or interval questions can send cues to respondents—often unintended ones—about what are normal responses (e.g. Courneya, Jones, Rhodes, and Blanchard 2003; Menon, Raghubir, and Schwarz 1997; Rockwood, Sangster, and Dillman 1997; Shwarz, Hipper, Deutsch, and Strack 1985). Specifically, respondents frequently assume that the ranges present in a question were purposely selected so that the middle response is the modal or most typical response. As such, the middle response changes the respondent’s reference point, and they then assess their level of economic well-being in relation to that point. Research in decision-making, economics, and psychology has repeatedly found that people do not simply evaluate outcomes like poverty in absolute terms, but rather in comparison to others (Crosby 1976; Festinger 1954; Suls and Wheeler 2000; Walker and Smith 2001). Reference points can therefore significantly impact how people feel and what decisions they make (Heath, Larrick, and Wu 1999; Kahneman and Tversky 1979).

The different income brackets used in our treatment (primed to feel relatively poor) versus
control groups are meant to offer respondents different perceptions around their economic position. Those in the treatment group should feel relatively deprived and like their economic status is quite low, while those in the control group should feel that their economic status is typical, as they answer subsequent questions. The middle income bracket in the control group is only 4,001–6,000 Rs., whereas the middle income bracket in the treatment group is much higher, at 25,001–45,000 Rs. In other words, respondents in the treatment group are more likely, compared to the control group, to place themselves in lower income brackets. This is indeed what we see; 73 percent of study participants primed to feel poor assigned themselves to the bottom income bracket, compared to only 29 percent of participants in the control group ($p < 0.001$). Actual total household income is almost identical across the group treated to feel relatively poor and the control group ($p = 0.72$). Table A.2 in the Online Appendix shows that random assignment of this relative poverty prime worked as intended. There is balance with respect to social status, gender, age, marital status, education level, mother’s education, father’s education, and ethnicity; there is no statistically meaningful difference in observable demographic characteristics gathered before treatment assignment. Moreover, exactly 50 percent of the sample received no relative poverty prime and 50 percent of the sample received the relative poverty prime.

To assess the extent to which perceived relative poverty conditions the effects of BISP on governmental support, we estimate equation (5) separately for two sub-groups: those who received the poverty prime and those who did not. Comparing the intercepts with the vertical cutoff line of each of these regression functions allows us to properly interpret any positive impacts of the BISP on support for government. In other words, we can observe both the effects of the poverty prime on those who did versus those who did not receive the BISP, as well as the effects of the BISP on those who did versus those who did not receive the poverty prime.

**Results**

Before examining how receipt of cash transfers through the BISP affects support for government, it is helpful to examine whether we can observe any economic benefits from the cash transfers. If BISP transfers do not affect individuals welfare, it is difficult to theorize that they affect political attitudes. Accordingly, we examined whether or not the BISP increased household expenditure and the propensity to engage in wealth-enhancing economic behaviors, as governments often hope
social protection expenditures will do. We indeed find evidence that this is the case (see Panel A of Table 2). Receipt of the BISP increases total household expenditures \((p = 0.02; \text{robust } p < 0.01)\), in large part due to increasing total food expenditures \((p < 0.01; \text{robust } p = 0.01)\).\(^{22}\) It also increases total household savings, normalized as a share of expenditures \((p = 0.03; \text{robust } p = 0.016)\), and income diversification \((p < 0.01; \text{robust } p = 0.01)\), as measured by whether or not one’s household earns income outside of the agricultural sector—the dominant economic sector in rural Pakistan.

When we consider the impacts of BISP receipt on accessing credit, as evidenced by total cash loans outstanding as a share of yearly expenditure, we see a reduction in debt \((p = 0.08; \text{robust } p < 0.01)\). BISP transfer payments might substitute alternate sources of financing investments—most of them likely requiring the payment of interest. While on the one hand low access to credit markets by the poor has been associated with poverty traps (Azariadis and Stachurski 2005), on the other hand the poor are often susceptible to usurious interest rates, and these themselves can lead to a poverty trap (Piketty 1997). These results suggest that the poor are able to make more lower-interest non-predatory investments in the future as a result of the receipt of the BISP. Observable improvements to the economic well-being of vulnerable households can be attributable to BISP transfers.

Next, we examine how receipt of the BISP influences downstream support for government nearly two to five years later. We consider as outcomes both our government support index, as well as the seven constituent variables we used to create it. These estimates are reported in Table 2, Panel B, column (2) (and visualized in Figure A.3 in the Online Appendix). We find that receipt of the BISP leads to an 8 percentage point increase in our government support index several years after first receiving BISP transfers, which is a 22 percent increase relative to the mean level of support for government \((p = 0.070; \text{robust } p = 0.134)\). Figure A.4 (a) in Online Appendix C displays the average government support index value \((y\)-axis) by wealth score \((x\)-axis) for households near the cutoff. The figure depicts a drop in government support precisely after crossing the cutoff wealth score and thus becoming ineligible for BISP transfers. When assessing each of the seven measures that make up the index (also reported in Table 2, Panel B, column (2) and visualized

\(^{22}\)Throughout, we report conventional p-values based upon implementing a fuzzy RDD estimation strategy and robust p-values based upon the standard error adjustment recommended by Calonico, Cattaneo, and Titiunik (2014b). See the research design section above for more details.
in Figure A.3), we see that BISP receipt corresponds with a more positive appraisal of political leaders and institutions. Two (three) of the seven measures are statistically significant at the 10 percent level when employing bias-corrected (conventional) standard errors. However, none (only one) are statistically significant at the 5 percent level or higher when employing bias-corrected (conventional) standard errors. Thus, we cannot confidently reject the null hypothesis of no overall effects of the BISP on political attitudes.

Might perceived inequality affect the relationship between BISP receipt and political attitudes? We gain insight into this question by examining government support index value averages near the wealth score cutoff for those who are primed versus those who are not primed to feel relatively poor in Figures A.4 (b)-(c). Considering only those who were primed, Figure A.4 (c) clearly shows a decline in government support as one crosses the wealth cutoff and becomes ineligible for this BISP; the polynomial on the left reflects higher government support than does the polynomial on the right. However, no differences are visually apparent among those who were not primed to feel relatively deprived; Figure A.4 (b) shows polynomials that nearly intersect at the cutoff.

Regression analyses allow us to more precisely quantify these magnitudes. In Table 3, we examine the effects of the BISP on two separate sub-groups: those primed to feel poor (columns (1)–(6)) and those not primed (columns (7)–(2)). The coefficients and confidence intervals based upon conventional standard errors for each of these two groups are visualized in Figure 2. We find that for the half of respondents randomly chosen to receive our poverty prime, our estimates of the effects of the BISP on their government support index value are now nearly twice as large in magnitude and more statistically significant than they were for the full estimation sample (see Table 2). The greater statistical significance is especially noteworthy given that we now have significantly less power to detect significant effects because we are halving our sample size when we consider primed individuals and unprimed individuals separately. Receipt of the BISP leads to a 16 percentage point increase in support, which is equivalent to a 43 percent increase over the mean level of support ($p = 0.016; \hat{p}_{\text{robust}} = 0.021$). For those who received the poverty prime, receipt of the BISP has a positive effect on all seven individual measures of support that make up the index, with the effects of three (four) measures being statistically meaningful when using robust (conventional) standard errors, as shown in column (3) (column (2)) of Table 3. For the half of the sample not primed to feel poor, the BISP increases none of the seven individual measures of confidence in
government, nor the government support index; standard errors are substantially larger than they are for the poverty prime group, and coefficients are smaller (and at times even negative) than they are for the poverty prime group for every measure. Thus, support for government due to receipt of the BISP is more pronounced when one’s relative poverty is made salient.

To further assess whether it is indeed feelings of relative deprivation that explain poverty-primed beneficiaries’ increased support for government relative to non-beneficiaries following receipt of the BISP, we test a key implication of such an explanation. Specifically, the effects of the BISP among those primed to feel poor should be concentrated among those who did not feel relatively poor pre-treatment. If the prime effectively causes individuals to feel relatively poor, than those who felt that they were typical or well-off at the outset and receive the relative deprivation prime should suddenly feel different about their relatively standing. In contrast, those who already felt relatively poor before receipt of the deprivation prime should be immune to its effects.

To test this implication of our theory, we leverage a relative standing question collected prior to the experiment: “[Showing the picture of a ladder] Please look at this ladder, which has 10 steps. Suppose we say that the top of this ladder represents the best possible life for you and the bottom step represents the worst possible life for you. Where on the ladder do you feel you personally stand at present?” The median and mean response is 5, the mid-point of the scale. We thus divided our sample into two groups: (1) those who chose a number of less than 5, and as such, should not feel the effects of the prime; and (2) those who chose 5 or higher, representing those who feel fairly typical or well-off pre-treatment, and as such, have room for their subjective assessments of economic well-being to fall due to the prime.23 As summarized in Table 4 and shown in Figure 3, the effects we observe among those primed to feel relatively poor only arise among those who had the potential to feel the effect of the prime: those who did not feel relatively poor pre-treatment. The effect we observed among those who received the relative deprivation prime is only observed

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23This pre-treatment question of interest was asked several modules before participating in our key survey experiment. A series of health questions were asked in between the pre-treatment relative standing question and our survey experiment, making it unlikely that this pre-treatment question had an effect on our outcome measures of interest. However, if there were an effect, it would bias our results against finding major differences in the effect of BISP receipt on political attitudes among those who were primed, as this relative standing question would presumably make the effect of the relative poverty prime less potent. Recall that our effects are driven by those who did not feel relatively poor pre-treatment.
among those who felt relatively typical or well-off pre-treatment. The effect of BISP receipt on the
index, as well as five of the seven measures that comprise the index, are statistically significant at
the 5 percent level or better (whether we use robust or conventional standard errors), and six of
the seven are statistically significant at the 10 percent level or better. Moreover, the effect size of
the government support index is larger in magnitude; we estimate a 24 percentage point increase,
which is equivalent to a striking 70 percent increase over the mean level of support for government.
This provides us with reassurance that voters’ perceptions of their relative well-being condition
their reactions to receipt of social protection.

[Table 4 and Figure 3 about here]

What is driving this observed difference in the effect by perceptions of one’s relative deprivation?
Is the positive BISP effect due to BISP recipients becoming more satisfied with their political
leaders and system? Or is the positive BISP effect an artifact of non-BISP recipients becoming
more disgruntled due to the feeling that they are being economically left behind? When we examine
the local polynomial estimates (values of our outcome variables) to the right and left of the cutoff for
those not primed to feel poor and for those primed to feel poor, we indeed see that overall effects are
being driven by two separable shifts (see Table 5): (a) the prime leads to an increase in government
support among those who received the cash transfer (for 6 out of the 8 outcome measures, including
the government support index); and (b) the prime leads to a decrease in government support among
those who did not receive cash transfers (for 7 out of the 8 outcome measures). However, for 6
out of the 8 outcome measures, (b) has a moderating effect that is of larger magnitude than the
effect of (a)—seen by comparing columns (3) and (6). In other words, the observed difference
we see between BISP and non-BISP recipients at the cutoff when we manipulation perceptions of
relative poverty is driven by two distinct and divergent effects: first, receipt of the poverty prime
among BISP recipients increased government support; second, receipt of the poverty prime among
non-BISP recipients decreased government support. For the majority of the outcome measures, the
second effect is larger.

To see this, consider the government support index (Table 5, row (1)). Among those close to
the cutoff but just to its left, who accordingly received the BISP, there was a negligible difference
between those who were not and were primed to feel poor (0.371 versus 0.388). Namely, among
those who just met the threshold for receiving the cash transfer, receiving the relative poverty
prime resulted in a 1.7 percentage point increase in government satisfaction. This is an interesting result in itself as it suggests that being made to feel poor is not unambiguously bad for government support among recipients of targeted governmental welfare assistance. Perhaps beneficiaries who feel relatively poor feel more gratitude for the support they receive than those who do not. With that said, this is a conjecture at best, and further exploration is needed to test this hypothesis. Moreover, this effect is admittedly very small.

In contrast, among those close to the cutoff but just to its right, who thus barely missed receiving the BISP, we observe a sizable 6 percentage point drop in government support due to having received the relative poverty prime. Let us consider one of the 7 individual outcomes that make up the index: respect for political institutions. This is the individual outcome with the smallest robust p-value among those primed to feel poor. Among those who barely met the cutoff to be cash transfer recipients, the relative poverty prime led to a 0.6 percentage point increase in government satisfaction. Among those who barely missed the cutoff, and received no cash transfer assistance, we observe a 10.5 percentage point drop in government confidence stemming from the relative poverty prime. Once again, we find that being primed to feel poor leading to a decrease in government support among those who did not receive cash transfers is overwhelmingly the dominant explanation of our finding that primed BISP recipients have higher support for government than primed non-BISP recipients. In contrast, for two individual outcomes related to political leadership (trust in leaders and a perception that leaders are doing the best job possible) rather than political institutions, the moderating effect is largely driven by an increase in satisfaction among BISP recipients stemming from the prime.

Overall, we take these results as evidence that receipt of BISP transfers largely neutralizes the negative effect of feeling relatively poor on satisfaction with government institutions and, to a smaller degree, fosters positive affect toward political leaders. With that said, beneficiaries and non-beneficiaries may feel differently towards the state when feelings of relative deprivation are salient because non-beneficiaries feel resentment that they do not have access to economic assistance from the state that others have access to, despite their economic need. Future research is necessary to disentangle the extent to which the observed differences between beneficiaries and non-beneficiaries that feel relatively poor are driven by resentment among non-beneficiaries stemming from their economic anxiety as opposed to the awareness that others are receiving aid that they would like,
but are not eligible to receive. More inquiry is also necessary to assess whether the mechanism underlying our findings operates differently when we consider perceptions of political leaders (who are in power for a particular period of time) versus perceptions of the political system (i.e., enduring formal and informal institutions).

[Table 5 about here]

Conclusion

Our work leverages an original survey experiment in Pakistan and exploits the fact that the provision of a targeted cash transfer program is a discontinuous function of an individual’s wealth score to contribute to a growing literature on the effects of social protection programs on citizens’ political attitudes. Beyond assessing the average treatment effects of the program—intended to alleviate poverty—we importantly consider the extent to which economic conditions can affect the political consequences of a cash transfer program. We demonstrate that individuals’ perceptions of the degree of inequality present in society and their relative poverty importantly moderate the effects of the program on attitudes toward government for both non-beneficiaries and beneficiaries. We circumvent the endogeneity of economic conditions to political attitudes using an original survey experiment in which we primed half of respondents to feel that their income is relatively low while half were primed to feel neutrally about their income. We can further make causal statements about the cash transfer program’s impacts by using a regression discontinuity design. By combining both an experimental and quasi-experimental research design, we are able provide valuable empirical insights about whether and when government provisions of goods can translate into greater support for and confidence in government leaders and institutions, and help clarify why the body of evidence regarding the political consequences of social protection programs is mixed.

Extant literature has largely focused on attribution challenges and low levels of civic education and engagement as reasons that citizens fail to reward policymakers for providing public and private goods. We point to the importance of an important behavioral mechanism. Specifically, we find that when relative deprivation is not salient, receipt of goods has little durable effect on attitudes toward government. But, when it is salient, beneficiaries have higher support for government than do non-beneficiaries. Moreover, we find that the positive difference in support for government when relative deprivation is apparent is largely driven by the social protection program attenuating a
drop in government support that feelings of relative deprivation can trigger. Feelings of deprivation can spark negative political attitudes and resentment towards government (Healy, Kosec, and Mo 2017); however, we find evidence that social protection can aid in neutralizing the disgruntled affect towards political institutions and leaders that relative deprivation engenders. The extent to which perceived inequality conditions the ability to detect an effect of social protection and political attitudes has been largely ignored by existing literature.

Overall, our study reveals that reactions to government provision of public and private goods is sensitive to individual beliefs about their own relative poverty level. Inequality translates to greater anxiety and dissatisfaction with the political system and political leaders among citizens. We see that targeted social protection programs can be an effective intervention to attenuate this drop in incumbent and institutional support. Nevertheless, when perceptions of relative need are high, non-recipients of government social protection may feel disgruntled as they feel left behind while other citizens are receiving social protection. That is, if perceived inequality is sufficiently higher, targeted government provision of social protection may actually reduce support for government among some citizens. With that said, if investments in these programs are expansive enough, it can be politically advantageous in the face of inequality, as they can assuage political anger among recipients that may simmer and explode without it. While non-beneficiaries may feel some resentment, feelings of economic anxiety that are not tended to would foment discontent among a larger base. We illustrate both the power of beliefs—to drastically change perceptions of government—as well as the power and limitations of government to mold and shape those beliefs.
References


Table 1: Summary Statistics

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<tr>
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<th>(3)</th>
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<th>(5)</th>
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<td>Mean</td>
<td>Std. Dev.</td>
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<td>Max</td>
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**Panel A: Economic Well-Being**

<table>
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<tbody>
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<td>Total Food Expenditures per Month (Rupees)</td>
<td>2,610</td>
<td>9,902.174</td>
<td>5,808.545</td>
<td>1,955.357</td>
<td>113,153.6</td>
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<tr>
<td>Total Expenditures per Month (Rupees)</td>
<td>2,610</td>
<td>16,140.83</td>
<td>7,758.07</td>
<td>3,316.28</td>
<td>120,302.2</td>
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<td>Cash Loans Outstanding as Share of Yearly Expenditure</td>
<td>930</td>
<td>0.323</td>
<td>0.626</td>
<td>0</td>
<td>9.032</td>
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<td>Total Savings as a Share of Monthly Expenditure</td>
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<td>0.143</td>
<td>1.275</td>
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<td>0.160</td>
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**Panel B: Attitudes Toward Government**

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<td>Government Support Index</td>
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<td>0.193</td>
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<td>Courts Guarantee Fair Trial</td>
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<td>0.412</td>
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<td>Respect for Political Institutions</td>
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<td>0.499</td>
<td>0.272</td>
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<td>Citizens’ Basic Rights Protected</td>
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<td>Others Should Support Political System</td>
<td>2,637</td>
<td>0.369</td>
<td>0.267</td>
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</tr>
<tr>
<td>Trust Leaders</td>
<td>2,637</td>
<td>0.321</td>
<td>0.262</td>
<td>0.0</td>
<td>1</td>
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<td>Leaders Doing Best Job Possible</td>
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<td>0.255</td>
<td>0.258</td>
<td>0.0</td>
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Table 2: Effect of BISP (2SLS)

<table>
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<tr>
<th>Variable</th>
<th>(1) First-Stage</th>
<th>(2) Estimate</th>
<th>(3) Robust P-Value</th>
<th>(4) Robust 95% CI</th>
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<th>(6) Ntr</th>
<th>(7) Nco</th>
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<tr>
<td>Panel A: Economic Well-Being</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total Food Expenditures per Month (Rupees)</td>
<td>-0.595***</td>
<td>2.596***</td>
<td>0.003</td>
<td>[1.078.56, 5.206.14]</td>
<td>2,610</td>
<td>481</td>
<td>569</td>
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<tr>
<td>Total Expenditures per Month (Rupees)</td>
<td>-0.594***</td>
<td>3.107.5**</td>
<td>0.014</td>
<td>[815.647. 7,088.83]</td>
<td>2,610</td>
<td>487</td>
<td>586</td>
</tr>
<tr>
<td>Cash Loans Outstanding as Share of Yearly Expenditure</td>
<td>-0.283**</td>
<td>-0.379*</td>
<td>0.004</td>
<td>[-1.083. -0.172]</td>
<td>930</td>
<td>125</td>
<td>183</td>
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<tr>
<td>Total Savings as a Share of Monthly Expenditure</td>
<td>-0.600***</td>
<td>0.403**</td>
<td>0.016</td>
<td>[0.087, 0.859]</td>
<td>2,610</td>
<td>418</td>
<td>524</td>
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<tr>
<td>Household Earns Income from Outside Agriculture</td>
<td>-0.628***</td>
<td>0.382***</td>
<td>0.000</td>
<td>[0.259, 0.556]</td>
<td>2,608</td>
<td>331</td>
<td>357</td>
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<tr>
<td>Panel B: Attitudes Toward Government</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Government Support Index</td>
<td>-0.595***</td>
<td>0.080*</td>
<td>0.134</td>
<td>[-0.025, 0.185]</td>
<td>2,636</td>
<td>494</td>
<td>590</td>
</tr>
<tr>
<td>Courts Guarantee Fair Trial</td>
<td>-0.595***</td>
<td>0.125*</td>
<td>0.088</td>
<td>[-0.019, 0.283]</td>
<td>2,637</td>
<td>490</td>
<td>590</td>
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<tr>
<td>Respect for Political Institutions</td>
<td>-0.595***</td>
<td>0.104*</td>
<td>0.151</td>
<td>[-0.037, 0.241]</td>
<td>2,637</td>
<td>494</td>
<td>590</td>
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<td>Citizens’ Basic Rights Protected</td>
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<td>0.116**</td>
<td>0.058</td>
<td>[-0.004, 0.267]</td>
<td>2,636</td>
<td>486</td>
<td>573</td>
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<td>Proud of Political System</td>
<td>-0.594***</td>
<td>0.008</td>
<td>0.932</td>
<td>[-0.148, 0.135]</td>
<td>2,636</td>
<td>500</td>
<td>590</td>
</tr>
<tr>
<td>Others Should Support Political System</td>
<td>-0.594***</td>
<td>0.038</td>
<td>0.501</td>
<td>[-0.091, 0.186]</td>
<td>2,637</td>
<td>492</td>
<td>590</td>
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<tr>
<td>Trust Leaders</td>
<td>-0.594***</td>
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<td>[-0.065, 0.210]</td>
<td>2,637</td>
<td>500</td>
<td>598</td>
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<td>Leaders Doing the Best Job Possible</td>
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<td>0.096</td>
<td>0.224</td>
<td>[-0.053, 0.227]</td>
<td>2,637</td>
<td>500</td>
<td>590</td>
</tr>
</tbody>
</table>


Notes: In the first two columns, * p<0.10, **p<0.05, *** p<0.01. The estimate is the average treatment effect at the cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth. The robust p-value, 95% robust confidence intervals, sample size, and the number of treated and control observations within the optimal bandwidth are also reported. The running variable is the wealth score.
### Table 3: Effect of BISP on Attitudes Toward Government by Poverty Prime (2SLS)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Effect of BISP Among Those Who Received the Poverty Prime</th>
<th>Effect of BISP Among Those Who Did Not Receive the Poverty Prime</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimate (1)</td>
<td>P-Value (2)</td>
</tr>
<tr>
<td>Government Support Index</td>
<td>0.155**</td>
<td>0.021</td>
</tr>
<tr>
<td>Courts Guarantee Fair Trial</td>
<td>0.203**</td>
<td>0.045</td>
</tr>
<tr>
<td>Respect for Political Institutions</td>
<td>0.196**</td>
<td>0.035</td>
</tr>
<tr>
<td>Citizens' Basic Rights Protected</td>
<td>0.195**</td>
<td>0.036</td>
</tr>
<tr>
<td>Proud of Political System</td>
<td>0.117</td>
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<td>Others Should Support Political System</td>
<td>0.118</td>
<td>0.177</td>
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<tr>
<td>Trust Political System</td>
<td>0.116</td>
<td>0.283</td>
</tr>
<tr>
<td>Leaders Doing the Best Job Possible</td>
<td>0.155*</td>
<td>0.121</td>
</tr>
</tbody>
</table>

*Source: Pakistan Rural Household Panel Survey (RHPS), Round 2 (2013) and Benazir Income Support Program Database (2013)*

*Notes: In the “Estimate” columns, * p<0.10, ** p<0.05, *** p<0.01. The estimate is the average treatment effect at the cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth. The robust p-value, 95% robust confidence intervals (CI), sample size, and the number of treated and control observations within the optimal bandwidth are also reported. The running variable is the wealth score.*
Table 4: Effect of BISP on Attitudes Toward Government by Perceived Income Standing Pre-Treatment Among Primed Individuals (2SLS)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Effect of BISP Among Those Who Did Not Feel Relatively Poor Pre-Treatment</th>
<th>Effect of BISP Among Those Who Felt Relatively Poor Pre-Treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
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<tr>
<td></td>
<td>Estimate</td>
<td>P-Value</td>
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<td>Government Support Index</td>
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<tr>
<td>Courts Guarantee Fair Trial</td>
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<td>0.151</td>
</tr>
<tr>
<td>Respect for Political Institutions</td>
<td>0.345***</td>
<td>0.000</td>
</tr>
<tr>
<td>Citizens’ Basic Rights Protected</td>
<td>0.258**</td>
<td>0.012</td>
</tr>
<tr>
<td>Proud of Political System</td>
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<tr>
<td>Others Should Support Political System</td>
<td>0.225**</td>
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<td>Trust Political System</td>
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<td>Leaders Doing the Best Job Possible</td>
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</table>


Notes: In the “Estimate” columns, * p<0.10, **p<0.05, *** p<0.01. The estimate is the average treatment effect at the cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth. The robust p-value, 95% robust confidence intervals, sample size, and the number of treated and control observations within the optimal bandwidth are also reported. The running variable is the wealth score.
Table 5: Moderating Effect of Relative Poverty Prime

<table>
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<tr>
<th>Variable</th>
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<th>Estimates to Right of Cutoff</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>(1) µprimed</td>
<td>(2) µnotprimed</td>
<td>(3) Difference</td>
<td>(4) µprimed</td>
<td>(5) µnotprimed</td>
<td>(6) Difference</td>
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<td>0.273</td>
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<td>0.24</td>
<td>0.055</td>
<td>0.207</td>
<td>0.198</td>
<td>0.009</td>
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</table>


Notes: The difference ($\mu_{primed} - \mu_{notprimed}$) is computed by subtracting the local polynomial estimate for the subgroup that did not the prime ($\mu_{notprimed}$) from the estimate for the subgroup that received the prime ($\mu_{primed}$).
Notes: At the cutoff (recoded as 0), the probability of being a BISP beneficiary increases by 59.4 percentage points ($p < 0.001$).
Figure 2: 2SLS Estimates – Attitudes Toward Government by Poverty Prime

Notes: 95 percent confidence intervals surround local-polynomial RD treatment effect point estimates.
Figure 3: 2SLS Estimates – Attitudes Toward Government by Perceived Income Standing Pre-Treatment Among Primed Individuals

Notes: 95 percent confidence intervals surround local-polynomial RD treatment effect point estimates.
Online Appendix

Does Relative Deprivation Condition the Effects of Social Protection Programs on Political Attitudes? Experimental Evidence from Pakistan
A Question Wording: Political Attitude Outcome Measures

Exact question wording for political attitude outcome measures are as follows, where response options were 0 (Not at all), 1 (A little), 2 (Somewhat), 3 (A lot), and 4 (A great deal), recoded as 0, 0.25, 0.5, 0.75, and 1:

- Courts Guarantee Fair Trial: To what extent do you think the courts in Pakistan guarantee a fair trial?
- Respect for Political Institutions: To what extent do you respect the political institutions of Pakistan?
- Citizens’ Basic Rights Protected: To what extent do you think citizens’ basic rights are protected by the political system of Pakistan?
- Proud of Political System: To what extent do you feel proud of living under the political system of Pakistan?
- Others Should Support Political System: To what extent do you think that one should support the political system of Pakistan?
- Trust Political System: To what extent do you trust the political system of Pakistan?
- Leaders Doing Best Job Possible: To what extent do you feel your leaders are doing the best job possible for Pakistanis?
- Government Support Index: Average of the aforementioned seven measures.
B  Question Wording: Economic Outcome Measures

The questions used to construct our economic outcome measures are the following:

- **Total Food Expenditures Per Month (Rupees)**
  - For each food item (of 67 listed food items), how much of [ITEM] did your household consume that was paid for during the last two weeks? (List number of units and unit code–i.e. kilograms, grams, liters, number, value, or other)
  - For each food item (of 67 listed food items), what was the price per [UNIT FROM PREVIOUS QUESTION]

  Calculations made: All values are measured in Pakistani Rupees. We summed up the total food expenditures across all 67 items to obtain a total amount of expenditure per two weeks. We then multiplied by 2.167 to convert to monthly expenditures rather than bi-weekly expenditures.

- **Total Expenditures Per Month (Rupees)**
  - Total food expenditures per month were obtained as described above; below we list the questions related to total non-food expenditures.
  - What is your total expenditure on seed in the last 12 months
  - What is your total expenditure on pesticide and weedicide in the last 12 months
  - What is your total expenditure on fertilizer in the last 12 months
  - What is your total expenditure on irrigation in the last 12 months
  - What is your total expenditure on hired labor for land preparation in the last 12 months
  - What is your total expenditure on hired labor for sowing in the last 12 months
  - What is your total expenditure on hired labor for irrigation in the last 12 months
  - What is your total expenditure on hired labor for fertilizer application in the last 12 months
  - What is your total expenditure on hired labor for pesticide application in the last 12 months
  - What is your total expenditure on hired labor for weeding activity in the last 12 months
  - What is your total expenditure on hired labor for harvesting/ picking in the last 12 months
  - What is your total expenditure on hired labor for thrashing in the last 12 months
  - What is your total expenditure on hired labor for transportation and storage in the last 12 months
  - What is your total expenditure on livestock feed in the last 12 months
  - What is your total expenditure on building rental in the last 12 months
  - What is your total expenditure on electricity/ gas in the last 12 months
  - What is your total expenditure on tools and machinery in the last 12 months
  - What is your total expenditure on veterinary services/medicines in the last 12 months
- How much monthly rent do you pay for this dwelling?
- For each non-durable good (of 10 listed items, including items such as firewood, coal, furnace oil, and tobacco), what number of units of [ITEM] did your household consume that was paid for during the last month? (List number of units and unit code—i.e. kilograms, liters, maunds, or other)
- For each item (of 10 listed items), what was the reported value per [UNIT FROM PREVIOUS QUESTION]
- For each household good or service (of 23 listed items, including items such as clothing, medicines, housing improvements, and ceremonies), what was the reported value paid for and consumed during the last year?
- How much did your household spend on meals outside home during last week?

Calculations made: All values are measured in Pakistani Rupees. For expenditures measured per year, we divided the amount by 12. For expenditures measured per week, we multiplied the total by 4.333. For expenditures measured per month, we kept the amounts as-is. We then summed up all non-food expenditures per month, and added this to total food expenditures per month to obtain total expenditures per month.

- Cash Loans Outstanding as Share of Yearly Expenditure
  - For each loan outstanding, list the total amount that still needs to be repaid, including all interest and fees. (Rs.)
  - Consumption and expenditure module of women’s questionnaire (available online at https://dataverse.harvard.edu/dataverse.xhtml?alias=IFPRI)

Calculations made: We summed up the total amount that still needs to be repaid across all loans and divided this by 12 times the sum of all monthly food and non-food expenditure items.

- Total Savings as a Share of Monthly Expenditure
  - For each saver, and for each “account,” or location of savings (possible locations include home, NGO, bank, shop, post office/ government institution, employer’s provident fund, insurance company, relative/ friend/ neighbor, committee/ bisi, prize bond/ saving certificate, and other), list the total amount that is currently saved in this account (Rs.)
  - Consumption and expenditure module of women’s questionnaire (available online at https://dataverse.harvard.edu/dataverse.xhtml?alias=IFPRI)

Calculations made: We summed up the total amount of savings across all accounts of all individuals and divided this by the sum of all monthly food and non-food expenditure items.

- Household Earns Income from Outside Agriculture
  - Total earnings from a primary non-farm job during the last 12 months
  - Total earnings from a secondary non-farm job during the last 12 months

Calculations made: If at least one household member earned at least some income from a primary or secondary non-farm job in the last 12 months, we counted them as earning income from outside agriculture
## Table A.1: Differences in Baseline Pre-Treatment Characteristics

<table>
<thead>
<tr>
<th>Demographic Characteristics</th>
<th>(1) First-Stage Estimate</th>
<th>(2)</th>
<th>(3) Robust P-Value</th>
<th>(4) Robust 95% CI</th>
<th>(5) N</th>
<th>(6) N_co</th>
<th>(7) N_{tr}</th>
</tr>
</thead>
<tbody>
<tr>
<td>Social Status</td>
<td>-0.595***</td>
<td>0.701</td>
<td>0.084 [ -0.119, 1.895]</td>
<td>2,637</td>
<td>490</td>
<td>590</td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>-0.593***</td>
<td>0.020</td>
<td>0.914 [-0.275, 0.307]</td>
<td>2,637</td>
<td>510</td>
<td>618</td>
<td></td>
</tr>
<tr>
<td>Age 18-25</td>
<td>-0.588***</td>
<td>-0.013</td>
<td>0.656 [-0.180, 0.113]</td>
<td>2,600</td>
<td>521</td>
<td>629</td>
<td></td>
</tr>
<tr>
<td>Age 25-35</td>
<td>-0.592***</td>
<td>0.053</td>
<td>0.695 [-0.144, 0.301]</td>
<td>2,600</td>
<td>475</td>
<td>555</td>
<td></td>
</tr>
<tr>
<td>Age 35-45</td>
<td>-0.589***</td>
<td>-0.044</td>
<td>0.512 [-0.293, 0.146]</td>
<td>2,600</td>
<td>508</td>
<td>617</td>
<td></td>
</tr>
<tr>
<td>Age 45-55</td>
<td>-0.590***</td>
<td>0.092</td>
<td>0.277 [-0.095, 0.332]</td>
<td>2,600</td>
<td>489</td>
<td>583</td>
<td></td>
</tr>
<tr>
<td>Married</td>
<td>-0.593***</td>
<td>0.005</td>
<td>0.777 [-0.099, 0.133]</td>
<td>2,608</td>
<td>501</td>
<td>607</td>
<td></td>
</tr>
<tr>
<td>Received Primary Education</td>
<td>-0.625***</td>
<td>0.096</td>
<td>0.223 [-0.073, 0.314]</td>
<td>2,464</td>
<td>401</td>
<td>503</td>
<td></td>
</tr>
<tr>
<td>Received Intermediate Education</td>
<td>-0.612***</td>
<td>-0.0003</td>
<td>0.993 [-0.116, 0.117]</td>
<td>2,464</td>
<td>494</td>
<td>584</td>
<td></td>
</tr>
<tr>
<td>Received Secondary Education</td>
<td>-0.615***</td>
<td>-0.085</td>
<td>0.203 [-0.232, 0.049]</td>
<td>2,464</td>
<td>472</td>
<td>552</td>
<td></td>
</tr>
<tr>
<td>Received Post-Secondary Education</td>
<td>-0.617***</td>
<td>-0.012</td>
<td>0.406 [-0.054, 0.022]</td>
<td>2,464</td>
<td>472</td>
<td>552</td>
<td></td>
</tr>
<tr>
<td>Mother's Years of Education</td>
<td>-0.606***</td>
<td>0.075</td>
<td>0.864 [-0.442, 0.527]</td>
<td>2,455</td>
<td>593</td>
<td>651</td>
<td></td>
</tr>
<tr>
<td>Father's Years of Education</td>
<td>-0.611***</td>
<td>0.262</td>
<td>0.487 [-0.706, 1.482]</td>
<td>2,456</td>
<td>460</td>
<td>543</td>
<td></td>
</tr>
<tr>
<td>Punjabi</td>
<td>-0.592***</td>
<td>0.117</td>
<td>0.332 [-0.103, 0.305]</td>
<td>2,603</td>
<td>524</td>
<td>633</td>
<td></td>
</tr>
<tr>
<td>Sairaiki</td>
<td>-0.592***</td>
<td>0.093</td>
<td>0.385 [-0.106, 0.274]</td>
<td>2,603</td>
<td>563</td>
<td>666</td>
<td></td>
</tr>
<tr>
<td>Sindhi</td>
<td>-0.606***</td>
<td>-0.083</td>
<td>0.528 [-0.222, 0.114]</td>
<td>2,603</td>
<td>404</td>
<td>503</td>
<td></td>
</tr>
<tr>
<td>Other Ethnicity</td>
<td>-0.592***</td>
<td>-0.112</td>
<td>0.165 [-0.350, 0.060]</td>
<td>2,603</td>
<td>404</td>
<td>503</td>
<td></td>
</tr>
</tbody>
</table>

*Notes: In columns (1) and (2), * p<0.10, **p<0.05, *** p<0.01. The estimate is the average treatment effect at the cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth. The robust p-value, 95% robust confidence intervals, sample size, and the number of treated and control observations within the optimal bandwidth are also reported. The running variable is the wealth score.*
Table A.2: Balance Test: Relative Poverty Prime Assignment

<table>
<thead>
<tr>
<th>Demographic Characteristic</th>
<th>(1) $\mu_{\text{NotPrimed}}$</th>
<th>(2) $\mu_{\text{Primed}}$</th>
<th>(3) Difference in Means</th>
<th>(4) Test of Balance (P-Value)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Social Status</td>
<td>4.084</td>
<td>4.012</td>
<td>-0.072</td>
<td>0.238</td>
</tr>
<tr>
<td>Female</td>
<td>0.505</td>
<td>0.504</td>
<td>-0.001</td>
<td>0.924</td>
</tr>
<tr>
<td>Age 18-25</td>
<td>0.105</td>
<td>0.107</td>
<td>0.002</td>
<td>0.869</td>
</tr>
<tr>
<td>Age 25-35</td>
<td>0.254</td>
<td>0.260</td>
<td>0.006</td>
<td>0.663</td>
</tr>
<tr>
<td>Age 35-45</td>
<td>0.252</td>
<td>0.243</td>
<td>-0.009</td>
<td>0.520</td>
</tr>
<tr>
<td>Age 45-55</td>
<td>0.208</td>
<td>0.218</td>
<td>0.01</td>
<td>0.447</td>
</tr>
<tr>
<td>Married</td>
<td>0.897</td>
<td>0.899</td>
<td>0.002</td>
<td>0.874</td>
</tr>
<tr>
<td>Received Primary Education</td>
<td>0.159</td>
<td>0.151</td>
<td>-0.008</td>
<td>0.472</td>
</tr>
<tr>
<td>Received Intermediate Education</td>
<td>0.077</td>
<td>0.067</td>
<td>-0.01</td>
<td>0.238</td>
</tr>
<tr>
<td>Received Secondary Education</td>
<td>0.104</td>
<td>0.108</td>
<td>0.004</td>
<td>0.698</td>
</tr>
<tr>
<td>Received Post-Secondary Education</td>
<td>0.018</td>
<td>0.015</td>
<td>-0.003</td>
<td>0.462</td>
</tr>
<tr>
<td>Mother’s Years of Education</td>
<td>1.009</td>
<td>1.034</td>
<td>0.025</td>
<td>0.780</td>
</tr>
<tr>
<td>Father’s Years of Education</td>
<td>0.171</td>
<td>0.174</td>
<td>0.003</td>
<td>0.919</td>
</tr>
<tr>
<td>Punjabi</td>
<td>0.359</td>
<td>0.364</td>
<td>0.005</td>
<td>0.738</td>
</tr>
<tr>
<td>Sairaiki</td>
<td>0.209</td>
<td>0.219</td>
<td>0.01</td>
<td>0.448</td>
</tr>
<tr>
<td>Sindhi</td>
<td>0.131</td>
<td>0.113</td>
<td>-0.018</td>
<td>0.098</td>
</tr>
<tr>
<td>Other Ethnicity</td>
<td>0.301</td>
<td>0.304</td>
<td>0.003</td>
<td>0.880</td>
</tr>
<tr>
<td>Proportion</td>
<td>0.501</td>
<td>0.499</td>
<td>.</td>
<td>.</td>
</tr>
</tbody>
</table>

Notes: For each of the observable demographic characteristics, Columns (1) and (2) report means by the experimental condition. Column (3) reports the difference in means, and Column (4) reports the p-value when conducting a difference in means test by experimental condition. The proportion row at the bottom of the table indicates what share of the total sample was assigned to each of the two conditions.
Figure A.1: McCrary Density Plot

Notes: The figure is a density plot of the wealth score with 95 percent (two-tailed) confidence intervals.
Notes: 95 percent confidence intervals surround local-polynomial RD treatment effect point estimates.
Figure A.3: Two-Stage Least Squares (2SLS) Estimates – Attitudes Toward Government

Notes: 95 percent confidence intervals surround local-polynomial RD treatment effect point estimates.
Figure A.4: Government Support Index by Wealth Score

(a) Full Sample

(b) No Prime

(c) Relative Deprivation Prime
D Literature Review

To generate a comprehensive list of studies that examine the effect of cash transfer programs on political attitudes, we had a research assistant conduct a literature search in top political science and economic journal websites for articles written in the last 20 years. Search terms included a combination of terms related to our independent variable of interest (“cash transfer programs” and “social protection programs”) with terms related to our outcome variable of interest (“political attitudes” and “elections”). Targeted political science journals included American Journal of Political Science, American Political Science Review, British Journal of Political Science, Comparative Political Studies, Electoral Studies, Political Behavior, Quarterly Journal of Political Science, Journal of Politics, and World Politics. Targeted economics and interdisciplinary journals included American Economic Journal (AEJ): Applied, AEJ: Economic Policy, American Economic Review, Economic Development and Cultural Change, Journal of Development Economics, Journal of Political Economy, Journal of Public Economics, Quarterly Journal of Economics, Proceedings of the National Academy of Sciences, Review of Economics and Statistics, Review of Economic Studies, Science, Social Science Quarterly, The Economic Journal, and World Development. For each study, we documented the context, major data sets used, population studied, Gini index, outcome measures, and sign of the effect. The Gini index is taken from the closest year that pre-dates the first year of the major data set, and if unavailable, the earliest year available that post-dates the first year (World Bank 2019). For studies involving multiple countries, we took the average Gini index of all countries (unweighted by population). The sign of effect is recorded “positive” if the effect is positive for at least one outcome and never negative for any outcome, “null” if the results are null for all outcomes, and “mixed” if there are mixed findings of positive and negative results. We reviewed a total of 25 studies.
<table>
<thead>
<tr>
<th>Author (Year)</th>
<th>Country</th>
<th>Dataset</th>
<th>Population</th>
<th>Gini Index</th>
<th>Outcome Measures</th>
<th>Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lee, Jensen, Arndt, and Wenningamberger (2017)</td>
<td>United Kingdom and Denmark</td>
<td>Polling data for government support in United Kingdom (1946-2014) and Denmark (1957-2014)</td>
<td>British and Danish poll respondents</td>
<td>30.5</td>
<td>Mean percentage of support for governing parties</td>
<td>Positive</td>
</tr>
</tbody>
</table>

Notes: Gini index is taken from the first year in the data set used, a closest year that pre-dates the first year of the data set, or the earliest year that Gini index is available. For studies involving multiple countries, Gini index is calculated by averaging the Gini indices of all countries.
Appendix References


ALL IFPRI DISCUSSION PAPERS

All discussion papers are available here.

They can be downloaded free of charge.