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

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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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November 15, 2019

Abstract

Why do citizens often fail to reward policymakers for providing targeted social protection? While existing literature has focused on attribution challenges and information asymmetries to explain mixed effects on support for government, we propose an additional explanation related to an individual’s perceived economic standing relative to others. To test this explanation, we evaluate the effects of Pakistan’s national unconditional cash transfer program, the Benazir Income Support Program (BISP), on support for and confidence in government leaders and institutions. In 2010, the government used a proxy means test to identify BISP beneficiaries; as receipt of BISP transfers is a discontinuous function of an individual’s wealth score, we can use a regression discontinuity design to assess the program’s causal impacts. We then leverage an original survey experiment we carried out in 2013 which subtly manipulated individuals’ perceptions of their relative poverty in order to assess how these perceptions moderate the effects of social protection. When relative deprivation is not salient, we find that receiving cash transfers has little effect on individuals’ reported level of support for government. However, when relative deprivation is salient, those receiving cash transfers experience increases in support for government, while those denied transfers simultaneously become more politically disgruntled. This has important implications for our understanding of the political ramifications of rising inequality, as well as how the positive effects of social protection programs should be interpreted.

*We gratefully acknowledge insightful comments from Kate Ambler, Matias Cattaneo, Yashodhan Ghorpade, Melissa Hidrobo, Rehan Jamil, Seema Jayachandran, Saumitra Jha, Gabriel Lenz, Brandon Marshall, Hope Michel-son, Shalini Roy, Rocio Titiunik, and Romain Wacziarg. We thank Matthew Brundage, Hamza Haider, Brian Holtemeyer, Huma Khan, Bhumi Purohit, Jie Song, and Joseph Stigall for excellent research assistance. All remaining errors are our own. This research was part funded by the CGIAR Research Program on Policies, Institutions, and Markets led by IFPRI and the U.S. Agency for International Development (USAID).

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1 Introduction

While income inequality across 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 economic trend stands to powerfully affect developing countries given the influence that inequality (Solt 2008; 2010) and perceived inequality (Gimpelson and Treisman 2018) have over public opinion and political engagement. At the same time, a political trend has emerged: governments are increasingly addressing poverty and inequality through social protection programs—including cash transfer programs that carefully target the poor using proxy means tests (Fiszbein et al. 2009; Garcia and Moore 2012). By reallocating wealth within a society, social protection programs, too, have vast potential to change political attitudes. However, little is known about how perceived income inequality moderates the relationship between social protection and political attitudes.

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 (e.g., Lewis-Beck and Nadeau 2011; Nadeau, Bélanger, and Didier 2013; Lewis-Beck, Nadeau, and Foucault 2013). Conversely, literature from behavioral economics, sociology, and political psychology suggests that reference points, (Kahneman and Tversky 1979; Levy 2003; Bendor 2010) which are affected by perceived relative welfare, may influence attitudes toward both the state (Healy, Kosec, and Mo 2017) and non-state actors (Fair et al. 2018). Several strands of social science research have focused on the concept of reference points being affected by social comparisons (Mo 2018): 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 income or achievements are significantly affected by comparisons with others. If perceptions of relative economic standing impact the relationship between social protection and political attitudes, then relative poverty needs to be incorporated into models of how social protection impacts support for government.

The empirical literature on how social protection programs influence political attitudes and trust in government is mixed. A number of studies demonstrate that receipt of targeted social protection programs translates into increased voter turnout and increased support for the policymakers
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; Blattman, Emeriau, and Fiala 2018; Marschall, Aydogan, and Bulut 2016; Conover, Zarate, Camacho, and Baez 2018). Indeed, even campaign promises to provide such programs have been found to elicit voter support (Elinder, Jordahl, and Poutvaara 2008). In contrast, work by Green (2006) and recent work by Imai, King, and Velasco Rivera (2019) in Mexico challenges the conclusion that targeted government welfare programs translate into political support. 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. Further, even if social protection leads to improvements in satisfaction with government, Ghorpade and Justino (2019) find that cash transfer programs have few visible impacts on trust in state institutions. Could inequality and perceptions of relative economic standing partially explain why we see mixed empirical findings?

We consider how perceptions of relative economic standing moderate the effects of social protection on support for government using a quasi-experimental regression discontinuity design (RDD) overlaid with a survey experiment we carried out in Pakistan. Specifically, we evaluate the effects of Pakistan’s national unconditional cash transfer program, the Benazir Income Support Program (BISP), on support for and confidence in government leaders and institutions. While it is challenging to study the effects of social protection programs since targeting may be politically motivated, in 2010, the Pakistani government used a proxy means test to identify BISP beneficiaries. If an individual’s family had a wealth score below a given cutoff, they received transfers. We find no evidence of manipulation of scores at the cutoff. We then leverage an original survey experiment we carried out in 2013 which subtly manipulated individuals’ perceptions of their relative poverty in order to assess how these perceptions moderate the effects of social protection. We asked respondents which of five income brackets describes their income, and then manipulated the range of the bracket choices such that half of respondents were likely to be in the bottom bracket and feel that the income distribution is wider than it actually is (poverty primed group) and the other half were likely to be in the middle or a higher bracket and perceive the income distribution to be narrower.
Gimpelson and Treisman (2018) provide motivation for a focus on perceptions of inequality 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 neutrally about their income (the control group), we find that receiving cash transfers has little effect on individuals’ reported level of support for government. However, when individuals are primed to feel poor and thus relative deprivation is made salient, we find that the BISP significantly increases support for government. The effects are largely driven by those who did not get the BISP and yet were primed to feel poor experiencing severely lowered support for government. These findings provide rich insights into how government provision of public and private goods influences attitudes toward government, and about the political ramifications of inequality—or perceived inequality. Absent perceptions of relative deprivation, cash transfers do very little in fostering long-term support for government. Moreover, in times of salient relative deprivation, *ceteris paribus*, cash transfers raise support for government. However, this positive effect is largely driven by non-recipients being disgruntled more so than recipients experiencing increased support.

Our study relates to a burgeoning 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 (Tocqueville 1856; Alesina and Perotti 1994; Cramer 2005). This literature points to endogeneity challenges when studying how inequality affects the relationship between redistributive social welfare policies and 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 regarding whether perceived inequality affects the relationship between
targeted government programs and political attitudes.

We also contribute to literature aiming to explaining why citizens only selectively reward government for social protection. Some of this literature has focused on attribution challenges. If citizens do not know who deserves credit for public investments, 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, Nielsen, and Pickering 2018). Attributing blame and credit to government has also 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 (Brender 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 broader role of low levels of civic education and 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 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. Kramon (2011) finds that less educated voters in Kenya are more likely to prefer vote-buying candidates, and 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 informative 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 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.\footnote{In contrast, Fried (2012) and Sugiyama and Hunter (2013) find little evidence that political criteria influenced the distribution of transfers through the Bolsa Familia program in Brazil.}

We contend that citizens’ perceptions of their relative economic position—and, in particular, the perception that one is relatively poor in the context of a relatively wide income distribution—are an additional significant factor in their selective rewarding of government for the provision of social protection. When a citizen does not feel relatively deprived, receipt of goods has minimal long-term effects on attitudes toward government. However, when it is made salient, social protection causes beneficiaries to support for government more than non-beneficiaries for two reasons. First, when people feel relatively poor, beneficiaries of social protection feel especially appreciative of this aid. Second, non-beneficiaries experience a relatively higher level of concern with government appearing to ignore their economic well-being when they feel relatively deprived.

The paper is organized as follows. In Section 2, we outline our conceptual framework, linking perceived inequality with how citizens reward government for public investments. We bolster our conceptual framework through a systematic review of the literature exploring the effects of cash transfers and other social protection programs on support for government, and then consider how study findings vary with the level of inequality (measured by the Gini index) of the study context. Section 3 provides background on our study context of Pakistan and the BISP, Pakistan’s national...
unconditional cash transfer program whose effects we evaluate. Section 4 describes our dataset and empirical approach—which marries a regression discontinuity design and a survey experiment to generate two sources 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. Section 5 presents our empirical results, and Section 6 concludes by discussing their implications for future research and policy.

2 Conceptual Framework

We posit that individuals are broadly aware of the existence of social protection programs. In our dataset from rural Pakistan, for example, over 35 percent of individuals are direct beneficiaries of the BISP cash transfer program. However, due to habituation (i.e., individuals become accustomed to the benefits, making the program lose salience as a source of the funds), attribution challenges (i.e., individuals do not know who is funding the program and thus deserves credit for its existence), or a lack of civic education such that individuals have incorrect beliefs about the program costs, how the benefits are targeted, or some other factor, individuals may fail to credit the standing government or political system for the program’s existence.

When inequality is salient, however, and a subset of citizens feel that they are relatively poor and deprived, these processes are disrupted. When individuals are economically anxious, a rich set of studies have found that individuals are more information-seeking and interested in political learning, reducing reliance on habitual cues for voting (Marcus and Mackuen 1993). The effect of anxiety on information-processing and learning may reduce attribution errors and the lack of civic education. Moreover, the value of redistribution programs may become more salient to a population when feeling relatively deprived, reducing the likelihood of habituation effects.

Additionally, as citizens make social comparisons to assess their own well-being (Festinger 1954; Adams 1965; Crosby 1976; Suls and Wheeler 2000), feeling relatively deprived should reduce support for government. This is consistent with models of economic voting—which posit that citizens reward government for good economic outcomes and sanction them for bad ones (Lewis-Beck and Nadeau 2011; Nadeau, Bélanger, and Didier 2013; Lewis-Beck, Nadeau, and Foucault 2013). However, theories that espouse that people’s sense of well-being are affected by comparisons with others suggest that citizens reward government for conditions that make them feel they are economically
superior or equal to individuals with whom they typically compare themselves (e.g., neighbors and other community members), and sanction government for making them feel economically inferior. Moreover, these conditions of feeling relatively deprived often lead to elevated demands from those on the lower end of the income distribution for government efforts to reduce inequality—such as through social protection programs.²

The question of how social protection will affect attitudes toward state actors that provide this protection in this context of inequality and salient feelings of relative deprivation among some is further complicated by the fact that states have limited resources, and targeted government social protection programs often generate two groups: (1) a beneficiary group that feels relatively poor and deprived (resulting in declining support for government) but appreciative of the protection they receive from the government (leading to increased support for government); and (2) a non-beneficiary group that feels relatively poor and deprived (resulting in declining support for government) and neglected by their government (because they have observed a social protection program that benefits others and not themselves). When relative need is not salient, the non-beneficiaries should not feel as neglected, and the beneficiary group should not feel as appreciative. Both groups are affected by the creation of a social protection program. As such, the effect of the program itself needs to assess the change in attitudes among both the beneficiary group and non-beneficiary group. As feelings of deprivation should decrease habituation effects, compel individuals to overcome attribution challenges and seek civic information, and create a beneficiary group that feels supported by government in addressing their deprivation and a non-beneficiary group that feels neglected (through failing to address their deprivation), we argue that evidence of social protection causing increased support for government among beneficiaries, relative to similar non-beneficiaries, will be stronger during times in which inequality and one’s relative poverty is more salient. The extent to which the net effects of social protection are due to reduced utility among the non-beneficiaries due to not receiving assistance, as opposed to the increased utility among beneficiaries due to receiving the social protection is an empirical question.

A systematic review of empirical studies on the effects of cash transfers or social protection on support for government indeed suggests the importance of considering feelings of relative depri-

²Though rising inequality does not always correspond with increased demands for inequality, as is the case in the United States (Ashok, Kuziemko, and Washington 2015).
vation. In doing this exercise, we first established a set of detailed search parameters, aimed at capturing empirical studies from the last two decades appearing in top journals; these are described in Online Appendix A. While by no means comprehensive, this provides useful evidence from some of the more prominent outlets. We next documented key features of each study: country context, dataset, population considered, Gini index (a proxy for when relative deprivation would be more salient among the mass population), outcome(s) considered, and overall conclusions about the direction of the effect of social protection (positive, mixed, negative, or null). The studies, and these details, are recorded in Table A.1.

Notably, extant studies evaluating social protection programs are concentrated in unequal study contexts; while the median Gini index across countries was around 36.7 in 2013 (World Bank 2019), 19 of 25 studies we identified had an above-median Gini index. Pakistan, with a Gini of 30.7 in our study year of 2013, is in contrast a relatively low-inequality country (World Bank 2019). Studies published in prominent outlets have thus focused predominantly on unequal contexts. This could be due to greater data availability in high-inequality contexts, more use of social protection programs in high-inequality contexts, or publication biases (e.g., if null effects were concentrated in low-inequality contexts); we cannot say definitively which of these is the case. However, it is interesting to consider how the direction of the effect of social protection varies with the Gini index of the study context among these 25 studies. We find a positive correlation between inequality levels and finding positive overall effects of social protection—consistent with the predictions of our conceptual framework. Among studies finding a positive impact of social protection on attitudes toward government, the Gini index is on average 0.092 standard deviations higher than it is for studies which identify null and/or some negative impacts. Acknowledging inequality is an imperfect proxy for perceived inequality, this suggests that previous literature is predominantly focused on contexts in which perceived inequality would be high—and thus, for the reasons outlined above, that social protection would be more likely to have a positive net effect on support for government.

While Gini data are not available for all years for all countries, this is computed by taking the latest available data for each country as of 2015; when we do this, the median year is 2013, and the median Gini index is 36.7.

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 index 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 index, 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.
3 Background

3.1 Study context of Pakistan

While the implications of our study apply more broadly, Pakistan is an interesting context in which to conduct an empirical test of our hypothesis for several reasons. First, as the world’s sixth largest country, Pakistan is home to a sizable share of the world’s poor. Second, Pakistan has a fragile security situation, and lessons from this setting may thus be useful for understanding how to foment political stability in similar contexts. For example, a number of militant organizations operate in Pakistan, threatening both Pakistan’s own growth prospects and international stability (Lamb 2008; Ghani and Lockhart 2009; Blair, Neumann, and Olson 2014). Third, Pakistan—like much of the developing world—relies on government social protection as a means of reducing poverty and inequality. Finally, eligibility for Pakistan’s national social protection program is a discontinuous function of a person’s poverty score, allowing us to leverage a quasi-experimental design to causally identify the effects of the program. In the next sections, we describe in detail the roll-out of Pakistan’s Benazir Income Support Program (BISP), its flagship national social protection program, and our identification strategy.

Pakistan 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 the April–May 2013 survey we use in our analysis. While inequality in Pakistan is relatively low (its Gini index 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.
3.2 The Benazir Income Support Program (BISP)

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 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 2019). The BISP’s stated goals were to eradicate extreme poverty, empower women, and achieve universal primary education by providing cash transfers to poor women (Gazdar 2011; Ambler and De Brauw 2017; Haseeb and Vyborny 2017). Donors providing support included the UK’s Department for International Development and the World Bank. Disbursements commenced in October 2008.

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 developed. Initial targeting was thus carried out by each member of parliament identifying a set number of beneficiaries (Haseeb and Vyborny 2017).\(^5\) Within a year, however, the federal government reformed the system to make targeting more transparent and fair by basing 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 for the PMT during October 2010 – December 2011, covering 155 million people from 27 million families.\(^6\) They began using these data to distribute transfers in July 2011 (Haseeb and Vyborny 2017), and the number of beneficiary families rapidly expanded from 1.8 million in 2009 to 5.3 million (Government of Pakistan 2017).\(^7\) 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).\(^8\) 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

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\(^5\)Details on how the BISP was initially implemented can be found in Online Appendix B.

\(^6\)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).

\(^7\)This switch naturally ended BISP access for some while simultaneously extending it to previous non-beneficiaries.

\(^8\)All individuals in a family have the same wealth score. Weights placed on each of the 23 variables were developed using the 2007–08 Pakistan Living Standards Measurement Survey, but have not been publicly released (Ambler and De Brauw 2017).
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 had payments stopped. Since this reform in 2011 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, 35.4 percent of the households in our sample are BISP recipients.9

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).10 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 $35.00 USD in early July 2011 (International Monetary Fund 2013))—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 (Ambler and De Brauw 2017). 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) up to 4 years, 7 months (for those who received transfers since October 2008 and who we surveyed in May 2013).11

9The national average share of households that are BISP recipients is 23 percent, reflecting the deeper poverty of our entirely rural sample, described in more detail below.

10To assess whether there were any barriers to receiving a CNIC card, we asked the following question: “To get a CNIC card, do members of your community have to pay any extra fees, bribes, or facilitation payments?” The vast majority of our study participants (77 percent) reported there being no such barriers.

11Details on how our RDD identification strategy circumvents any potential identification concerns stemming from endogenous spatial variation in the timing of families’ access to the BISP can be found in Online Appendix B.
4 Empirical Strategy

4.1 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) original household panel survey data we collected in rural Pakistan during March–April 2012 (round 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. Round 1 of the RHPS covered 2,090 households in 76 villages in Punjab, Sindh, and Khyber-Pakhtunkhwa (KPK) provinces; 2,002 of these were also interviewed in round 2. Two respondents per household—the head of each household and their spouse—completed household surveys.

Both round 1 and round 2 of the RHPS collected the Computerized National Identity Card (CNIC) numbers of both respondents—though in round 2, we only gathered CNIC data if they were not received in round 1. Most of our analysis makes use of round 2 data, though we rely on CNIC data collected from round 1. The Secretary of the BISP maintains an administrative database of all CNIC numbers in Pakistan and two key pieces of information which we requested that they provide to us 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

\[12\] 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.

\[13\] 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.
a recipient of the BISP. 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. Of the 3,300 plausible CNIC numbers, 80 percent (2,639) were in the BISP administrative database. Households lacking a CNIC number in the BISP database had to be omitted from our analysis. We were then able to code, for households in the administrative database, a household-level BISP beneficiary variable. Further details on the construction of this beneficiary variable, and efforts to validate it using our own survey data, can be found in Online Appendix B.

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 C. 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 we re-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

14It 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).

15349 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.

16CNIC 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.

17These valid and matched CNIC numbers came from 1,349 different households.
consistency. Given this high reliability coefficient, as well as our concerns with multiple hypothesis testing—whereby, upon testing a sufficiently large number of null hypotheses, 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 measures comprising it are summarized in Table 1. 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 the economic effects that would have the potential to lead to effects on citizen attitudes, 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 D 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.

4.2 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.
4.2.1 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 RDD 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 helpful because of selection bias concerns generally characterizing access to social protection programs. Consider the following empirical specification:

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

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 they possess which are correlated with the political outcomes we study, direct estimation of \( \tau \) by estimating equation (1) would be biased.

Our RDD identification strategy 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 requires:

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

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:

\[
D_i = \begin{cases} 
1, & \text{if } X_i \leq c \\
0, & \text{if } X_i > c.
\end{cases}
\]
In our case, the threshold is not a sharp cutoff given that a few exceptions were made and also given that a few families with wealth scores below the cutoff had not yet received BISP transfers at the time of our survey—all detailed in sub-section 3.2. 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:

\[
\lim_{\Delta \downarrow 0} P[D_i = 1 | X_i = c + \Delta] \neq \lim_{\Delta \uparrow 0} P[D_i = 1 | X_i = c + \Delta].
\] (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 Titiunik (2014a, 2014b) and Calonico, Cattaneo, Farrell, and Titiunik (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 Titiunik (2014b), which estimate the asymptotic bias ignored by conventional inference and correct the standard errors appropriately to produce valid inferences.\(^\text{18}\)

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 (\(p < 0.001\)) discrete jump in the BISP aid.

\(^{18}\)These estimations are implemented using their rdrobust software. The software is available at https://sites.google.com/site/rdpackages/rdrobust.
beneficiary rate (see Figure 1, as well as the first-stage results reported for each outcome measure of interest in column (1) of Table 2). Further, to the left of the cutoff (i.e., among those eligible for the BISP), take-up is 96 percent. And to the right of the cutoff, where an exception was made for those with a cutoff score between 16.17 and 21.17 with higher family needs (see discussion above), the take-up was only 5 percent.

If there is a discontinuous difference in respondent characteristics around the score threshold, equation (2) will not hold, 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 and an indicator for the respondent perceiving barriers to acquiring a CNIC card, there is not one measure that is significantly different at a 5 percent significance level at the cutoff (see Figure A.1 and Table A.2 in Online Appendix E). 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 government could manipulate wealth scores near the cutoff. 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 such manipulation. 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). As shown in Figure A.2 in Online Appendix E, we find 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 ($p = 0.234$).\footnote{This test is conducted using the authors’ rddensity software.}

### 4.2.2 Survey Experiment

To study the moderating effects of perceived relative poverty, we carried out a survey experiment called a priming experiment. In this, we asked respondents which of five income brackets describes
their income, and then manipulated the range of the bracket choices such that half of respondents were likely to be in the bottom bracket and feel that the income distribution is wider than it actually is (poverty primed, or treatment group) and the other half were likely to be in the middle or a higher bracket and perceive the income distribution to be narrower (control group). Both respondents within a household received the same treatment, reducing the potential for spillovers. Specifically, the income question was: “Income is the amount of cash income you earn from all agricultural and non-agricultural activities, and money from the 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 (the prevailing dollar–Rs. exchange rate at the time of our survey was 98.5 Rs. per 1 USD): ²⁰

<table>
<thead>
<tr>
<th>Control (No Poverty Prime)</th>
<th>Treatment (Relatively Poor Group)</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>

²⁰This is from May 2, 2013 (International Monetary Fund 2013).

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). 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 behavioral 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).21

The middle income bracket in the control group is only 4,001–6,000 Rs., whereas for the treatment group, it is 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 those primed to feel poor assigned themselves to the bottom income bracket, compared to only 29 percent 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.3 in Online Appendix E 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 the 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 impacts of the BISP on support for government. 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.

5 Results

5.1 Effects of the BISP on economic and political outcomes

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 21In 2013, the poverty line in Pakistan was 3,030 Rs. per adult equivalent per month; as the average household in our sample for analysis has 6.071 adult equivalents, this is a household monthly income of 18,395 Rs. (Ministry of Finance of Government of Pakistan 2013). Mean (median) monthly household income in our sample is 16,141 Rs. (14,755 Rs.); total monthly expenditures are below the poverty line for 62 percent of sample households.
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 ($p = 0.02; p_{robust} < 0.01$), in large part due to increasing total food expenditures ($p < 0.01; p_{robust} = 0.01$).\footnote{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.} It also increases total household savings, normalized as a share of expenditures ($p = 0.03; p_{robust} = 0.016$), and income diversification ($p < 0.01; p_{robust} = 0.01$), 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 ($p = 0.08; p_{robust} < 0.01$). BISP transfer payments might substitute alternate sources of financing investments—most of them likely requiring the payment of interest.\footnote{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). While some interpretations of the Quran (and in particular the word “riba”) say interest rates should be zero, others say that the Quran simply bans usurious interest rates (Noorzoy 1982). Accordingly, we find ample evidence of non-zero interest rates in our study context.} Observable improvements to the economic well-being of vulnerable households can thus be attributed to BISP transfers.

Next, we examine how receipt of the BISP influences downstream support for government. We first consider as outcomes both our government support index as well as the seven variables we used to create it. These estimates are reported in Table 2, Panel B, column (2) (and depicted in Figure A.3 in Online Appendix E). 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 ($p = 0.070; p_{robust} = 0.134$). Figure A.4 (a) in Online Appendix E 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 depicted 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 or higher 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 government support.

5.2 Perceptions of poverty as a moderator of the effects of the BISP

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 the 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 poor; 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 to feel relatively poor (columns (7)–(2)). The coefficients and confidence intervals based upon conventional standard errors for each of these two groups are depicted 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 in 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 the government support index that combines all seven of our individuals measures; this is equivalent to a 43 percent increase over the mean level of support for government \( p = 0.016; \ p_{\text{robust}} = 0.021 \). 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) of these seven measures being statistically meaningful when using robust (conventional) standard errors, as shown in column (3) (column (2)) of Table 3. The outcomes for which receipt of the BISP has the largest and most statistically significant effects relate to the belief that courts guarantee a fair trial, to citizens’ respect for political institutions, and to the belief that citizens’ basic rights are protected. Existing research shows that beliefs about various domains of the political system are highly inter-linked, with perceptions of unfairness in one domain permeating others—possibly explaining consistent findings across arguably diverse measures of support for government (Lind and Tyler 1988). That said, the measure we should most focus on is the index, as the component measures are highly correlated (Cronbach’s alpha score of 0.88) and the index reduces measurement error associated with any one of the components (Ansolabehere, Rodden, and Snyder 2008).  

[Table 3 and Figure 2 about here]

For those not primed to feel poor (the control group), 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 primed group, and coefficients are always smaller—and in three cases negative. Thus, support for government due to receipt of the BISP is only apparent when one’s relative poverty is made salient.

Is the positive effect of BISP among those primed to feel relatively poor (but not among those in the control group) due to BISP recipients being relatively more appreciative of BISP assistance due to their greater perceived poverty (stemming from the prime)? Or is the positive effect of BISP among those primed to feel poor an artifact of non-BISP recipients feeling relatively more disgrun-

---

24If one is not convinced that the index is the best measure to assess, given the mixed significance of our results for those primed to feel poor across the seven measures that make up our government support index, we tested whether the findings hold up to correction for multiple hypothesis testing. We employ two popular methods of controlling the false discovery rate: those of Benjamini and Hochberg (1995) (BH) and Benjamini, Krieger, and Yekutieli (2006) (BKY). We compute the q-values (i.e., p-values corrected for multiple testing) for each method. We use our larger (and thus more conservative) robust p-values as inputs into each correction method. For both methods, all four of the coefficients on receipt of the BISP that were statistically significant at the 10 percent level or higher remain so—including the coefficient on BISP when our outcome is our overall government support index. We conclude that our findings are robust and not simply due to multiple testing.

25That there are no impacts of the BISP on the non-primed (control) group further provides reassurance that the effects of the BISP we identify above are not simply due to demand effects or social desirability bias stemming from the prime explicitly noting BISP as a potential source of household income. Rather, BISP recipients selectively increase their reported confidence in the political system: only when primed to feel relatively poor.
tled with government for the lack of transfers when they experience greater perceived poverty? 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 observe two separable shifts (see Table 4): (a) the poverty prime leads to a small increase in government support among beneficiaries (for 6 out of the 8 outcome measures, including the government support index); and (b) the prime leads to a generally larger decrease in government support among those who did not receive cash transfers (for 7 out of the 8 outcome measures). Overall, 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).

[Table 4 about here]

To consider magnitudes of our effects, we examine the government support index (Table 4, row (1)), which is our preferred outcome as it captures information from across the host of different government and institution support questions and is less prone to measurement error. 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 transfers, receiving the relative poverty prime resulted in a 1.7 percentage point increase in government satisfaction. This suggests that feeling relatively 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 does the control group. 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 (0.3 vs. 0.36). This suggests that feeling relatively poor absent aid engenders dissatisfaction with government.

Overall, we take the aforementioned results as evidence that perceptions of relative poverty strongly moderate the effects of receiving cash transfers on support for government, and further, most of the effects come from those above the cutoff (i.e. non-beneficiaries) becoming disgruntled with government when they are primed to feel poor. The poverty prime has modest moderating effects among BISP beneficiaries. Absent perceptions of deprivation, receiving BISP aid has few
effects, as can be seen by the lack of a BISP effect when relative poverty was not primed. Our results (1) contribute a scope condition for when one is more likely to detect a positive effect of a social protection program; and (2) highlight that positive effects of a social protection program should not be viewed as an unambiguously positive finding, as part of the positive effect stems from non-recipients being disgruntled about not receiving assistance. 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 of non-beneficiaries that others are receiving aid that they would like, but are not eligible to receive.

5.3 Robustness

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 (relatively) immune to its effects.

To test this implication of our theory, we leverage a question about the individual’s perceived economic standing relative to others, 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 less than 5, and should thus not be affected by the prime; and (2) those who chose 5 or higher, representing those who feel fairly typical or well-off pre-treatment, and as such, would more probably have their subjective assessment of their economic well-being fall due to receiving the poverty prime. As summarized in Table 5 and shown in Figure 3, the effects

26This pre-treatment question of interest was asked several modules before participating in our key survey experi-
we observe among those primed to feel relatively poor are predominately due to 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 5 and Figure 3 about here]

As an additional robustness check, we conducted a set of placebo tests. We considered a number of measures in a survey module pertaining to conflict that are orthogonal to social protection and feelings of relative deprivation. Namely, we considered a set of questions that pertain to whether violence to protect religious values is justified, whether military action against extremist groups is helpful, and whether Kashmiri independence is important. As shown in Figure A.5 in Online Appendix E, as we would expect, neither the BISP nor the poverty prime affect these measures.27

6 Conclusion

Our work seeks to advance a growing body of literature on the selective manner in which individuals reward government for public investments. We posit and test the influence of a novel moderator of the effects of a social protection program on support for government: citizens’ perceptions of their own relative economic standing. Previous literature has predominately focused on habituation, attribution challenges, and other information asymmetries to explain mixed effects of social protection. We evaluate the effects of Pakistan’s nation-wide, unconditional cash transfer program, the BISP, on support for and confidence in government leaders and institutions. Our work

A series of health questions were asked in between the pre-treatment relative economic 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 economic 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.

27Exact question wordings are provided in the notes that accompany the figure.
leverages an original survey experiment and exploits the fact that receipt of the BISP is a discontinuous function of an individual’s wealth score—permitting use of a regression discontinuity design to assess its causal impacts. Our survey incorporated a priming experiment in which we asked respondents which of five income brackets describes their income, and then manipulated the range of the bracket choices such that half of respondents were likely to be in the bottom bracket and feel that the income distribution is wider than it actually is (poverty primed group) and the other half were likely to be in the middle or a higher bracket and perceive the income distribution to be narrower (control group). In this way we exogenously vary perceptions of relative poverty. Beyond assessing the average treatment effects of the program, our combination of a quasi-experimental and experimental research design importantly allows us to consider the extent to which perceptions of one’s relative economic standing can affect the political consequences of the program.

We demonstrate that when relative deprivation is not salient, receipt of cash transfers has little effect on support for government. But, when it is salient, beneficiaries have higher support for government than do non-beneficiaries. Individuals’ perceptions of their relative poverty are thus an important moderator of the effects of the program on government support. Being primed to feel relatively poor influences both beneficiaries’ and non-beneficiaries’ level of support for government, but these effects are largest for non-beneficiaries, for whom receiving no aid in the face of feeling relatively deprived substantially lowers support for government. Feeling relatively deprived as they receive governmental aid modestly raises support for government among beneficiaries—for whom their vulnerability may increase the perceived value of the social protection. Overall, our research illustrates 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.

The political attitude shifts that we study have behavioral implications. When we examine the effect of BISP on a respondent’s self-reported intention to participate in the 2013 presidential elections, we see that BISP receipt increases intentions to vote among women ($p = 0.082; p_{\text{robust}} = 0.134$), and this effect is stronger when relative deprivation is salient ($p = 0.068; p_{\text{robust}} = 0.071$; see Figure A.6 in Online Appendix E). However, there are null effects on men’s voting intentions. The gendered nature of this effect may be due to Pakistan ranking at the bottom of the global distribution in terms of the political participation gender gap Solijonov (2016); nearly all of our male survey participants (94 percent) indicated an intention to vote, and as such, there is very
little room for improvement in voter turnout. Conversely, only 74 percent of women reported an intention to vote. Future research should further explore the effects of feelings of relative poverty and social protection programs on political engagement.

A number of factors could contribute to perceptions that one is at the lower end of the income distribution and that the distribution is furthermore highly unequal. One source may be real increases in inequality in a society—which is on the rise in developing countries according to recent research (Ravallion 2014). Another is the proliferation of social media and information technology, which permit quicker comparisons not only with others in one’s community, but also with more distant individuals including those in richer countries. Finally, Tiebout sorting that leads to segregation on socio-economic status may lead to perceptions that the distribution of income is flatter than it actually is. To the extent that these factors fuel or reduce perceived inequalities, they have implications for how public investments affect attitudes toward government. Government itself may also be able to move some levers that influence citizens’ perceptions of their relative economic standing, such as through communication efforts or other programs to increase citizens’ sense of solidarity with fellow citizens. It could also well include other forms of public investments, such as in broad-based public goods that predominately benefit the poor in many contexts—like public education and healthcare (Kosec 2014).

To the extent that building trust in government is a goal of social protection programs, our research has important policy implications. In a setting in which perceived relative poverty is quite low, social protection is unlikely to affect political attitudes. (Of course, building support for government is likely to be only one of several goals of a cash transfer program.) In contrast, when perceived relative poverty is high, support for government is significantly lower among non-beneficiaries of a social protection program (either because of their relative poverty and/or because they feel forgotten while others are getting the program—we cannot mediate between these two drivers). As such, positive effects of social protection programs detected from rigorous experimental and quasi-experimental studies where there is a treated group that received aid and a similar untreated group that did not receive aid, may not be unambiguously good news, as the positive effect may be driven by the decline in support for government stemming from non-beneficiaries. This raises the important need for a social protection program to have sufficient funding to ensure that aid is provided to as many citizens who feel relatively deprived as possible, and any potential
discord sowed among non-beneficiaries is offset by support garnered from beneficiaries. However, our results also indicate that simply relieving feelings of relative deprivation in non-beneficiaries—possibly through means other than social protection—can raise support for government. Coupling targeted social protection, such as cash transfers, with other investments that reduce citizens’ sense of relative deprivation may well yield great dividends from the standpoint of citizen support for government.
References


Table 1: Summary Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Obs</td>
<td>Mean</td>
<td>Std. Dev.</td>
<td>Min</td>
<td>Max</td>
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<tr>
<td>BISP Household</td>
<td>2,639</td>
<td>0.354</td>
<td>0.478</td>
<td>0.0</td>
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<tr>
<td>Wealth Score</td>
<td>2,639</td>
<td>22.816</td>
<td>12.560</td>
<td>0.0</td>
<td>80.660</td>
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<td>Poverty Prime</td>
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<td>0.494</td>
<td>0.500</td>
<td>0.0</td>
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</tr>
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</table>

Panel A: Economic Well-Being

<table>
<thead>
<tr>
<th>Variable</th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<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>
</tr>
<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>
</tr>
<tr>
<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>
</tr>
<tr>
<td>Total Savings as a Share of Monthly Expenditure</td>
<td>2,610</td>
<td>0.143</td>
<td>1.275</td>
<td>0.0</td>
<td>21.40</td>
</tr>
<tr>
<td>Household Earns Income from Outside Agriculture</td>
<td>2,608</td>
<td>0.160</td>
<td>0.366</td>
<td>0.0</td>
<td>1</td>
</tr>
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</table>

Panel B: Attitudes Toward Government

<table>
<thead>
<tr>
<th>Variable</th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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</thead>
<tbody>
<tr>
<td>Government Support Index</td>
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<td>0.367</td>
<td>0.193</td>
<td>0.0</td>
<td>1</td>
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<tr>
<td>Courts Guarantee Fair Trial</td>
<td>2,637</td>
<td>0.412</td>
<td>0.290</td>
<td>0.0</td>
<td>1</td>
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<tr>
<td>Respect for Political Institutions</td>
<td>2,637</td>
<td>0.499</td>
<td>0.272</td>
<td>0.0</td>
<td>1</td>
</tr>
<tr>
<td>Citizens’ Basic Rights Protected</td>
<td>2,636</td>
<td>0.356</td>
<td>0.259</td>
<td>0.0</td>
<td>1</td>
</tr>
<tr>
<td>Proud of Political System</td>
<td>2,636</td>
<td>0.354</td>
<td>0.268</td>
<td>0.0</td>
<td>1</td>
</tr>
<tr>
<td>Others Should Support Political System</td>
<td>2,637</td>
<td>0.369</td>
<td>0.267</td>
<td>0.0</td>
<td>1</td>
</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>
</tr>
<tr>
<td>Leaders Doing Best Job Possible</td>
<td>2,637</td>
<td>0.255</td>
<td>0.258</td>
<td>0.0</td>
<td>1</td>
</tr>
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</table>

Table 2: Effect of BISP (2SLS)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Panel A: Economic Well-Being</th>
<th>Panel B: Attitudes Toward Government</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) First-Stage</td>
<td>(2) Estimate</td>
</tr>
<tr>
<td></td>
<td>(3) Robust</td>
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<td>(4) Robust</td>
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</tr>
<tr>
<td></td>
<td>(5) N</td>
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</tr>
<tr>
<td></td>
<td>(6) N_t</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(7) N_c</td>
<td></td>
</tr>
</tbody>
</table>

**Panel A: Economic Well-Being**

- **Total Food Expenditures per Month (Rupees)**: 
  - Estimate: -0.595***
  - First-Stage: 2,596***
  - Robust P-Value: 0.003
  - Robust 95% CI: [1,078.56, 5,206.14]
  - N: 2,610
  - N_t: 481
  - N_c: 569

- **Total Expenditures per Month (Rupees)**: 
  - Estimate: -0.594***
  - First-Stage: 3,107.5**
  - Robust P-Value: 0.014
  - Robust 95% CI: [815.647, 7,088.83]
  - N: 2,610
  - N_t: 487
  - N_c: 586

- **Cash Loans Outstanding as Share of Yearly Expenditure**: 
  - Estimate: -0.283**
  - First-Stage: -0.379*
  - Robust P-Value: 0.004
  - Robust 95% CI: [-1.083, -0.172]
  - N: 2,610
  - N_t: 125
  - N_c: 183

- **Total Savings as a Share of Monthly Expenditure**: 
  - Estimate: -0.600***
  - First-Stage: 0.403**
  - Robust P-Value: 0.016
  - Robust 95% CI: [0.087, 0.859]
  - N: 2,610
  - N_t: 418
  - N_c: 524

- **Household Earns Income from Outside Agriculture**: 
  - Estimate: -0.628***
  - First-Stage: 0.382***
  - Robust P-Value: 0.000
  - Robust 95% CI: [0.259, 0.556]
  - N: 2,610
  - N_t: 331
  - N_c: 357

**Panel B: Attitudes Toward Government**

- **Government Support Index**: 
  - Estimate: -0.595***
  - First-Stage: 0.080*
  - Robust P-Value: 0.134
  - Robust 95% CI: [-0.025, 0.185]
  - N: 2,636
  - N_t: 494
  - N_c: 590

- **Courts Guarantee Fair Trial**: 
  - Estimate: -0.595***
  - First-Stage: 0.125*
  - Robust P-Value: 0.088
  - Robust 95% CI: [-0.019, 0.283]
  - N: 2,637
  - N_t: 490
  - N_c: 590

- **Respect for Political Institutions**: 
  - Estimate: -0.595***
  - First-Stage: 0.104*
  - Robust P-Value: 0.151
  - Robust 95% CI: [-0.037, 0.241]
  - N: 2,637
  - N_t: 494
  - N_c: 590

- **Citizens’ Basic Rights Protected**: 
  - Estimate: -0.596***
  - First-Stage: 0.116*
  - Robust P-Value: 0.058
  - Robust 95% CI: [-0.004, 0.267]
  - N: 2,636
  - N_t: 486
  - N_c: 573

- **Proud of Political System**: 
  - Estimate: -0.594***
  - First-Stage: 0.008
  - Robust P-Value: 0.932
  - Robust 95% CI: [-0.148, 0.135]
  - N: 2,636
  - N_t: 500
  - N_c: 590

- **Others Should Support Political System**: 
  - Estimate: -0.595***
  - First-Stage: 0.038
  - Robust P-Value: 0.501
  - Robust 95% CI: [-0.091, 0.186]
  - N: 2,637
  - N_t: 492
  - N_c: 590

- **Trust Leaders**: 
  - Estimate: -0.594***
  - First-Stage: 0.072
  - Robust P-Value: 0.303
  - Robust 95% CI: [-0.065, 0.210]
  - N: 2,637
  - N_t: 500
  - N_c: 598

- **Leaders Doing the Best Job Possible**: 
  - Estimate: -0.595***
  - First-Stage: 0.096
  - Robust P-Value: 0.224
  - Robust 95% CI: [-0.053, 0.227]
  - N: 2,637
  - N_t: 500
  - N_c: 590

**Source:** Pakistan Rural Household Panel Survey (RHPS), Round 1 (2012) and Round 2 (2013), and Benazir Income Support Program Database (2013)

**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)

| Variable                              | Effect of BISP Among Those | Effect of BISP Among Those |
|                                      | Who Received the Poverty Prime | Who Did Not Receive the Poverty Prime |
|                                      | Estimate | P-Value | 95% CI | N | N_tr | N_co | Estimate | P-Value | 95% CI | N | N_tr | N_co |
| Government Support Index              | 0.155**  | 0.021   | [0.026, 0.317] | 1,303 | 252  | 310  | 0.015    | 0.791   | [-0.149, 0.195] | 1,333 | 185  | 174  |
| Courts Guarantee Fair Trial           | 0.203**  | 0.045   | [0.005, 0.436] | 1,303 | 250  | 308  | 0.034    | 0.819   | [-0.197, 0.249] | 1,334 | 181  | 160  |
| Respect for Political Institutions    | 0.196**  | 0.035   | [0.015, 0.420] | 1,303 | 246  | 298  | -0.002   | 0.911   | [-0.223, 0.199] | 1,334 | 185  | 196  |
| Citizens’ Basic Rights Protected      | 0.195**  | 0.036   | [0.014, 0.418] | 1,303 | 246  | 298  | 0.042    | 0.715   | [-0.169, 0.246] | 1,333 | 185  | 190  |
| Proud of Political System             | 0.117    | 0.270   | [-0.089, 0.320] | 1,303 | 259  | 318  | -0.064   | 0.679   | [-0.272, 0.177] | 1,333 | 185  | 188  |
| Others Should Support Political System| 0.118    | 0.177   | [-0.062, 0.337] | 1,303 | 248  | 306  | -0.023   | 0.904   | [-0.242, 0.214] | 1,334 | 185  | 196  |
| Trust Political System                | 0.116    | 0.283   | [-0.095, 0.325] | 1,303 | 278  | 326  | 0.050    | 0.507   | [-0.148, 0.299] | 1,334 | 185  | 174  |
| Leaders Doing the Best Job Possible   | 0.155*   | 0.121   | [-0.040, 0.346] | 1,303 | 309  | 349  | 0.061    | 0.630   | [-0.170, 0.331] | 1,334 | 185  | 174  |


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: Moderating Effect of Relative Poverty Prime

<table>
<thead>
<tr>
<th>Variable</th>
<th>Estimates to Left of Cutoff</th>
<th></th>
<th>Estimates to Right of Cutoff</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) µ\text{primed}</td>
<td>(2) µ\text{notprimed}</td>
<td>(3) Difference</td>
<td>(4) µ\text{primed}</td>
</tr>
<tr>
<td>Government Support Index</td>
<td>0.388</td>
<td>0.371</td>
<td>0.017</td>
<td>0.3</td>
</tr>
<tr>
<td>Courts Guarantee Fair Trial</td>
<td>0.428</td>
<td>0.439</td>
<td>-0.01</td>
<td>0.314</td>
</tr>
<tr>
<td>Respect for Political Institutions</td>
<td>0.507</td>
<td>0.501</td>
<td>0.006</td>
<td>0.397</td>
</tr>
<tr>
<td>Citizens’ Basic Rights Protected</td>
<td>0.401</td>
<td>0.414</td>
<td>-0.013</td>
<td>0.292</td>
</tr>
<tr>
<td>Proud of Political System</td>
<td>0.368</td>
<td>0.334</td>
<td>0.034</td>
<td>0.302</td>
</tr>
<tr>
<td>Others Should Support Political System</td>
<td>0.383</td>
<td>0.355</td>
<td>0.028</td>
<td>0.317</td>
</tr>
<tr>
<td>Trust Leaders</td>
<td>0.338</td>
<td>0.313</td>
<td>0.026</td>
<td>0.273</td>
</tr>
<tr>
<td>Leaders Doing the Best Job Possible</td>
<td>0.295</td>
<td>0.24</td>
<td>0.055</td>
<td>0.207</td>
</tr>
</tbody>
</table>


Notes: The difference (µ\text{primed} − µ\text{notprimed}) is computed by subtracting the local polynomial estimate for the subgroup that did not the prime (µ\text{notprimed}) from the estimate for the subgroup that received the prime (µ\text{primed}).
Table 5: 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) Estimate</td>
<td>P-Value</td>
</tr>
<tr>
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<td>0.001</td>
</tr>
<tr>
<td>Courts Guarantee Fair Trial</td>
<td>0.159</td>
<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>
<td>0.106*</td>
<td>0.061</td>
</tr>
<tr>
<td>Others Should Support Political System</td>
<td>0.225**</td>
<td>0.016</td>
</tr>
<tr>
<td>Trust Political System</td>
<td>0.215**</td>
<td>0.025</td>
</tr>
<tr>
<td>Leaders Doing the Best Job Possible</td>
<td>0.284***</td>
<td>0.009</td>
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</tbody>
</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.
Figure 1: First Stage Results

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 Literature Review


For each study, we documented the country context, major datasets used, population studied, Gini index of the country context and year of study, outcome measure(s), and the study’s broad finding on the overall effects of social protection (positive, mixed, negative, or null). The Gini index (World Bank 2019) is taken from the closest year that pre-dates the first year of the major dataset, and if unavailable, the earliest year available that post-dates the first year. For studies involving multiple countries, we took the average Gini index of all countries (unweighted by population). The overall effect is recorded as “positive” if the effect is positive for at least one outcome and never negative for any outcome, “negative” if the effect is negative for at least one outcome and never positive 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 identified a total of 25 studies. Of these, 19 (i.e. more than three-quarters) have a Gini index above the 2013 global median (specifically—while Gini data are not available for all years for all countries—if we take the latest available data for each country as of 2015, the median year is 2013, and the median Gini index is 36.7). Thus, studies are mostly from high-inequality contexts.
<table>
<thead>
<tr>
<th>Author (Year)</th>
<th>Country Context</th>
<th>Dataset</th>
<th>Population</th>
<th>Gini Index</th>
<th>Outcome(s)</th>
<th>Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lee, Jensen, Aastveit, and Wenneberger (2017)</td>
<td>United Kingdom and Denmark</td>
<td>Polling data for government support in United Kingdom (1946-2014) and Denmark (1937-2014)</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: Papers are ordered according to the value of the Gini index for the study context (lowest to highest). The Gini index is taken from the first year in the data set used, the closest year that pre-dates the first year of the data set, or the earliest year that the Gini index is available. For studies involving multiple countries, the Gini index is calculated by averaging the Gini indices of all countries.
B Background on the Benazir Income Support Program (BISP)

When the BISP was first rolled out in 2008, while party leaders agreed to the construction of an objective system to select beneficiaries, they were eager to start distributing funds before such a system could be 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).

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).

We employ a regression discontinuity design (RDD) identification strategy to study the effects of BISP, which 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. 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

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28 52 percent of nominations were disqualified for not meeting these four basic criteria (Haseeb and Vyborny 2017).

29 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.
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)\textsuperscript{30} up to 4 years, 7 months (for those who received transfers since October 2008 and who we surveyed in May 2013).\textsuperscript{31}

### Construction of BISP Beneficiary Variable from Administrative Data

We constructed our household-level BISP beneficiary variable from the administrative data we received from the BISP Secretariat 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{32} 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

\textsuperscript{30}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.

\textsuperscript{31}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.

\textsuperscript{32}In all such households, there was an adult female, but we simply did not have administrative data on her.
member actually registered. 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 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.

Validation of the BISP Administrative Data

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

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33 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.
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.
C 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.
D  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
### E  Additional Figures and Tables

#### Table A.2: Differences in Baseline Pre-Treatment Characteristics

<table>
<thead>
<tr>
<th>Demographic Characteristics</th>
<th>(1) First-Stage Estimate</th>
<th>(2) Estimate</th>
<th>(3) P-Value</th>
<th>(4) 95% CI</th>
<th>N</th>
<th>N&lt;sub&gt;tr&lt;/sub&gt;</th>
<th>N&lt;sub&gt;co&lt;/sub&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Social Status</td>
<td>-0.595***</td>
<td>0.701</td>
<td>0.084</td>
<td>[-0.119, 1.895]</td>
<td>2,637</td>
<td>490</td>
<td>590</td>
</tr>
<tr>
<td>Female</td>
<td>-0.593***</td>
<td>0.020</td>
<td>0.914</td>
<td>[-0.275, 0.307]</td>
<td>2,637</td>
<td>510</td>
<td>618</td>
</tr>
<tr>
<td>Age 18-25</td>
<td>-0.588***</td>
<td>-0.013</td>
<td>0.656</td>
<td>[-0.180, 0.113]</td>
<td>2,600</td>
<td>521</td>
<td>629</td>
</tr>
<tr>
<td>Age 25-35</td>
<td>-0.592***</td>
<td>0.053</td>
<td>0.695</td>
<td>[-0.144, 0.301]</td>
<td>2,600</td>
<td>475</td>
<td>555</td>
</tr>
<tr>
<td>Age 35-45</td>
<td>-0.589***</td>
<td>-0.044</td>
<td>0.512</td>
<td>[-0.293, 0.146]</td>
<td>2,600</td>
<td>508</td>
<td>617</td>
</tr>
<tr>
<td>Age 45-55</td>
<td>-0.590***</td>
<td>0.092</td>
<td>0.277</td>
<td>[-0.095, 0.332]</td>
<td>2,600</td>
<td>489</td>
<td>583</td>
</tr>
<tr>
<td>Married</td>
<td>-0.593***</td>
<td>0.005</td>
<td>0.777</td>
<td>[-0.099, 0.133]</td>
<td>2,608</td>
<td>501</td>
<td>607</td>
</tr>
<tr>
<td>Received Primary Education</td>
<td>-0.625***</td>
<td>0.096</td>
<td>0.223</td>
<td>[-0.073, 0.314]</td>
<td>2,464</td>
<td>401</td>
<td>503</td>
</tr>
<tr>
<td>Received Intermediate Education</td>
<td>-0.612***</td>
<td>-0.0003</td>
<td>0.993</td>
<td>[-0.116, 0.117]</td>
<td>2,464</td>
<td>494</td>
<td>584</td>
</tr>
<tr>
<td>Received Secondary Education</td>
<td>-0.615***</td>
<td>-0.085</td>
<td>0.203</td>
<td>[-0.232, 0.049]</td>
<td>2,464</td>
<td>472</td>
<td>552</td>
</tr>
<tr>
<td>Received Post-Secondary Education</td>
<td>-0.617***</td>
<td>-0.012</td>
<td>0.406</td>
<td>[-0.054, 0.022]</td>
<td>2,464</td>
<td>472</td>
<td>552</td>
</tr>
<tr>
<td>Mother's Years of Education</td>
<td>-0.606***</td>
<td>0.075</td>
<td>0.864</td>
<td>[-0.442, 0.527]</td>
<td>2,455</td>
<td>593</td>
<td>651</td>
</tr>
<tr>
<td>Father's Years of Education</td>
<td>-0.611***</td>
<td>0.262</td>
<td>0.487</td>
<td>[-0.706, 1.482]</td>
<td>2,456</td>
<td>460</td>
<td>543</td>
</tr>
<tr>
<td>Punjabi</td>
<td>-0.592***</td>
<td>0.117</td>
<td>0.332</td>
<td>[-0.103, 0.305]</td>
<td>2,603</td>
<td>524</td>
<td>633</td>
</tr>
<tr>
<td>Sairaki</td>
<td>-0.592***</td>
<td>0.093</td>
<td>0.385</td>
<td>[-0.106, 0.274]</td>
<td>2,603</td>
<td>563</td>
<td>666</td>
</tr>
<tr>
<td>Sindhi</td>
<td>-0.606***</td>
<td>-0.083</td>
<td>0.528</td>
<td>[-0.222, 0.114]</td>
<td>2,603</td>
<td>404</td>
<td>503</td>
</tr>
<tr>
<td>Other Ethnicity</td>
<td>-0.592***</td>
<td>-0.112</td>
<td>0.165</td>
<td>[-0.350, 0.060]</td>
<td>2,603</td>
<td>404</td>
<td>503</td>
</tr>
<tr>
<td>CNIC Unfair</td>
<td>-0.597***</td>
<td>-0.076</td>
<td>0.114</td>
<td>[-0.306, 0.141]</td>
<td>2,337</td>
<td>369</td>
<td>476</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.3: 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>1.099</td>
<td>1.034</td>
<td>0.025</td>
<td>0.780</td>
</tr>
<tr>
<td>Father’s Years of Education</td>
<td>1.019</td>
<td>1.034</td>
<td>0.016</td>
<td>0.738</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>Sairaki</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>CNIC Unfair</td>
<td>0.225</td>
<td>0.233</td>
<td>0.008</td>
<td>0.612</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 ($\mu_{\text{Primed}} - \mu_{\text{NotPrimed}}$), 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.
Notes: 95 percent confidence intervals surround local-polynomial RD treatment effect point estimates.
Figure A.2: McCrary Density Plot

Notes: The figure is a density plot of the wealth score with 95 percent (two-tailed) confidence intervals.
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
Notes: 95 percent confidence intervals surround local-polynomial RD treatment effect point estimates. The three measures are: (1) To what extent do you agree with the statement: “violence by militant groups is justified if it is in defense of religious values?” (Response Options: 0 (Disagree strongly), 0.25 (Disagree), 0.5 (Neither Agree nor Disagree), 0.75 (Agree), 1 (Agree strongly)); (2) To what extent do you agree with the statement: ”Military action against extremist groups like that taken in Bajaur, improves Pakistani security”? (Response Options: 0 (Disagree strongly), 0.25 (Disagree), 0.5 (Neither Agree nor Disagree), 0.75 (Agree), 1 (Agree strongly)); (3) “How important is Pakistani government support for Kashmiri independence?” (Response Options: 0 (Not important at all), 0.25 (Somewhat important), 0.5 (Neither important nor unimportant), 0.75 (Somewhat important), 1 (Extremely important)).
Figure A.6: Intention to Vote in 2013

Notes: 95 percent confidence intervals surround local-polynomial RD treatment effect point estimates. The question wording for the outcome measure is: “Do you plan to vote in the upcoming (2013) general elections?” (Response Options: 0 (No), 1 (Yes)).
Appendix References


