

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

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Abstract

Could perceived economic standing relative to others affect citizens' support for and confidence in government leaders and institutions? We explore this question by examining Pakistan's national unconditional cash transfer program, the Benazir Income Support Program (BISP). The BISP is one of the largest unconditional cash transfer programs in the world, and employs a selection model that allows for causal inference. Leveraging a regression discontinuity approach using BISP's administrative data and an original survey experiment, we find that perceptions of relative deprivation color citizen reactions to social protection. When relative deprivation is not salient, receiving cash transfers has little effect on individuals' reported level of support for government and its leaders. 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 perceived inequality.

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While income inequality *across* countries has declined worldwide over the last three decades, there has been a simultaneous rise in income inequality *within* countries (Ravallion 2014). This economic trend stands to powerfully affect states given the key roles that inequality (Solt 2008; Newman, Johnston, and Lown 2015) and perceived inequality (Healy, Kosec, and Mo 2017; Gimpelson and Treisman 2018) play in determining citizens’ support for and confidence in government. At the same time, a political trend has emerged: governments are increasingly addressing poverty and inequality through social protection programs—including cash transfer programs (Fiszbein et al. 2009). By reallocating wealth, these programs, too, may affect citizen attitudes towards government (Evans, Holtemeyer, and Kosec 2019). Yet little is known about how perceived income inequality moderates the relationship between social protection and political support.

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 (Gomez and Wilson 2001; Lewis-Beck and Nadeau 2011). Conversely, literature from behavioral economics, sociology, and psychology suggests that relative welfare considerations are important, highlighting the role of reference points (Kahneman and Tversky 1979; Bendor 2016)—a basis or standard for evaluation—in shaping political attitudes (Healy, Kosec, and Mo 2017). Several theories have focused on how people’s evaluations of their well-being are significantly affected by comparisons with others (Mo 2018; Condon and Wichowsky 2020): Equity theory (Adams 1965), relative deprivation theory (Crosby 1976; Walker and Smith 2001), and social comparison theory (Festinger 1954; Suls and Wheeler 2000). Do such comparisons impact the relationship between social protection and political attitudes?

The empirical literature on how social protection influences political support and trust in government is mixed. A number of studies demonstrate that receipt of targeted social protection programs increases voter turnout and support for policymakers delivering the program (e.g., Manacorda, Miguel, and Vigorito 2011; Chen 2013; De La O 2013; Layton and Smith 2015). Indeed, even campaign promises to provide such programs can elicit voter support (Elinder, Jordahl, and Poutvaara 2008). In contrast, Green (2006) and Imai, King, and Velasco Rivera (2020) challenge the conclusion that targeted government welfare programs always translate into political support. Ellis and Faricy (2011) find that U.S. public opinion is unresponsive to federal social welfare spending. Lyall, Zhou, and Imai (2020) demonstrate that cash transfers can push recipients to support anti-

government groups (e.g., the Taliban) in the long-term. And Corrêa and Cheibub (2016) find net *anti*-incumbent effects, driven by increases in support for the opposition among non-beneficiaries. Further, even if social protection raises satisfaction with government, Ghorpade and Justino (2019) argue it may not visibly impact trust in state institutions.

We consider how perceptions of relative deprivation moderate the effects of social protection on individuals’ support for government using a quasi-experimental regression discontinuity design (RDD) overlaid with a survey experiment in Pakistan. Specifically, we evaluate the effects of Pakistan’s national unconditional cash transfer program, the Benazir Income Support Program (BISP), on citizen support for and confidence in government leaders and institutions. In 2010, the Pakistani government used a proxy means test to identify BISP beneficiaries; this generated a wealth score used as a cutoff which our empirical strategy exploits. We then leverage an original survey priming experiment which subtly manipulated respondents’ perceptions of relative deprivation.¹ We draw from a growing number of studies suggesting that subtle primes can influence individuals’ perceptions of their relative economic position (Mo 2018; Fair et al. 2018; Healy, Kosec, and Mo 2017; Kosec et al. Forthcoming).

Individuals near the BISP cutoff who were assigned to a condition in which they would feel neutrally about their income (hereafter referred to as the “non-primed group”) express similar levels of support for government regardless of BISP receipt status. However, when individuals near the cutoff are primed to feel relatively poor compared to the average citizen, we find that BISP recipients have more pro-government sentiment than do non-recipients. The effects are largely driven by those whose households did not get the BISP and were primed to feel relatively poor expressing significantly lowered support for government. These findings provide insights into how government provision of goods influences attitudes toward government, and about the political ramifications of perceived inequality. Absent perceptions of relative deprivation, cash transfers do little to alter support for government. However, in times of salient relative deprivation, cash transfers engender greater differences in support for government among recipients relative to non-recipients, in large part due to disgruntled non-recipients. That we find impacts of even a subtle prime reflects the vulnerability of perceptions of one’s relative economic position to external stimuli

¹While all respondents participated in the survey experiment, our central RDD analysis of BISP effects and how perceptions of relative inequality moderate observed effects focuses on data from those near the cutoff.

and further indicates the power of these changed perceptions to affect government satisfaction. The prime’s subtlety potentially signals an even larger impact from more sizeable or durable changes in such perceptions.

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 from rich to poor under majority-rule voting. A number of studies have empirically supported the theory that inequality and public investment in goods that predominantly benefit the poor are inextricably linked (Lupu and Pontusson 2011; Kosec 2014; Newman 2014). Recent studies suggest that this focus on actual inequality is incomplete, highlighting that one’s perception of their own position relative to others alters citizen appetite for redistribution (Sands 2017; Condon and Wichowsky 2020). Other studies have demonstrated that inequality and feelings of relative deprivation directly impact opposition to status quo political institutions and political violence (Tocqueville 1856; Gurr 1970; Healy, Kosec, and Mo 2017). This literature speaks to the endogeneity challenges that accompany studies of the political consequences of inequality, as inequality is rarely exogenous to political attitudes, underscoring the valued added by our experimental approach.

We also contribute to scholarship explaining why citizens selectively reward government for social protection. Some of this literature has focused on attribution challenges, which are often heightened by an intertwined distribution of powers, financing, and responsibilities (León 2011; Hobolt, Tilley, and Wittrock 2013; Gulzar and Pasquale 2017),² and may be overcome by provision of information, such as through regular community meetings (Evans, Holtemeyer, and Kosec 2019). Such meeting may be especially useful precisely because government officials typically have comparatively few interactions with poor households compared to those who are better off (Li-aqat, Cheema, and Mohmand 2020). Voters may also exhibit recency bias—or the tendency to credit government for recent provision of goods and services more than provisions further in the past (Cole, Healy, and Werker 2012). Building on this work, Galiani, Hajj, McEwan, Ibararán, and Krishnaswamy (2019) provide experimental evidence for peak-end heuristics in voter behavior,

²For example, the provision of BISP has involved multiple political parties, as BISP has continued despite changes in government; international organizations and foreign governments have helped fund BISP; and multiple levels of government have been involved in registering beneficiaries.

whereby citizens reward government for a cash transfer program based on the size of their most recent transfers as well as their largest transfer. When considering re-occurring cash transfer programs, as opposed to one-off transfers, research on hedonic adaption and income habituation – the tendency to return to a relatively stable level of happiness after positive or negative events, suggests that any observed effects of social protection programs will not be durable (Di Tella, Haisken-De New, and MacCulloch 2010). Citizens also have biased assessments of government performance based upon whether the party in power is the one with which they identify (Malhotra and Kuo 2008). Participation in social protection programs may also carry social stigma and thus decrease civic engagement (Mettler and Stonecash 2008). Finally, policymakers may also use these programs for overtly political purposes, complicating how they are perceived (e.g., Bruhn 1996; Dahlberg and Johansson 2002; Costa 2011; Brollo and Nannicini 2012; Aytac 2014).

We contend that citizens’ perceptions of their relative economic position—and, in particular, the perception that one is relatively deprived—are an additional significant factor in their selective rewarding of government for provision of social protection. When a citizen does not feel relatively deprived, receipt of goods has minimal sustained effect on attitudes toward government. However, when it is made salient, we demonstrate that social protection causes beneficiaries to have greater support for government than non-beneficiaries for two potential reasons. First, when people feel relatively poor, beneficiaries of social protection may feel especially appreciative of this aid. Second, non-beneficiaries experience greater disdain for government, feeling that their government is ignoring their economic well-being when they feel relatively deprived.

The paper is organized as follows. In the next section, 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 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 of the study context. We then provide background on our study context of Pakistan and the BISP program. Next, we describe our dataset and empirical approach. Our empirical results follow. We conclude by discussing their implications for future research and policy.

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 BISP beneficiaries. However, due to habituation (i.e., becoming accustomed to the benefits, making the program lose salience as a source of funds), attribution challenges (i.e., individuals do not know who funds the program),³ or concerns with respect to political manipulation or capture individuals may fail to credit the standing government or political system.

When inequality is salient, the relationship between government programs and citizen attitudes are further complicated. On the one hand, feelings of relative deprivation may activate anxiety, prompting citizens to blame government for their current status. As citizens make social comparisons to assess their well-being (Festinger 1954; Adams 1965; Crosby 1976; Suls and Wheeler 2000), citizens should sanction government for making them feel inferior. On the other hand, the value of redistribution programs rises when individuals feel relatively deprived (de Kadt and Sands 2019).

States have limited resources, and as such, targeted government social protection often generates two groups: (1) a beneficiary group that was targeted (raising support for government) because they are poor and deprived (an underlying condition that lowers support for government); and (2) a non-beneficiary group that is poor in an absolute and/or relative sense (underlying conditions that may lower support for government) but feels neglected by government because public resources were insufficient to help them despite helping others in their networks (lowering support for government). When relative need is not salient, we contend that non-beneficiaries should not feel as neglected, and the beneficiary group should not feel as appreciative. Feelings of deprivation should (1) decrease habituation effects; and (2) create a beneficiary group that feels supported and a non-beneficiary group that feels neglected, assuming that scarce resources do not allow all citizens desiring aid to receive it, which is typically the case. As such, evidence of social protection engendering durable pro-government sentiment among beneficiaries relative to similar non-beneficiaries will be more likely to be observed when perceptions of one's relative deprivation are salient. The extent to which the net effects of social protection are due to reduced utility among the non-beneficiaries, as

³For example, Zucco (2013) notes how citizens may reward those who initially introduced a social protection program or those currently dispensing it.

opposed to the increased utility among beneficiaries, is an empirical question. Our conceptualization is consistent with the finding in Latin America that the poor evaluate local government on the basis of government performance *and* the observation of economic disparities in one’s neighborhood (Córdova and Layton 2016).

To better contextualize our contribution, we carried out a systematic review of empirical studies on the effects of cash transfers or social protection on support for government in high-impact journals in the last two decades, summarized in Online Appendix Table A.1 and discussed in detail in Online Appendix A. We identified 26 studies using a detailed set of search parameters, and find that they are concentrated in unequal countries (above median Gini). This could be due to greater data availability in high-inequality contexts, more use of social protection programs there, or publication biases (e.g., if null findings that were not published were concentrated in low-inequality contexts). If actual inequality increases perceived inequality, our conceptual framework would predict that we would *overestimate* the benefits of social protection for government support by consulting the studies in this review.

Background

While the implications of our study apply more broadly, Pakistan is an interesting study context 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—like many developing countries—relies on government social protection to reduce poverty and inequality. Finally, eligibility for Pakistan’s national social protection program is a discontinuous function of a family’s poverty score, allowing us to leverage a quasi-experiment to identify its causal impacts.

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), a center-left, socialist-progressive, and social democratic political party in Pakistan. The government subsequently experienced a peaceful democratic transition shortly after our April–May 2013 survey.

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 leader who had just been assassinated.⁴ At the time the BISP was launched, Pakistan was in the midst of a food, fuel, and financial crisis (Cheema et al. 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 (Ambler and De Brauw 2019). Donors providing support included the UK’s Department for International Development and the World Bank.

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 distribute 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, the federal government reformed the system to base beneficiary status on a family’s wealth score, computed using a proxy means test (PMT) (Gazdar 2011). This revised, technocratic method of beneficiary selection was credited by third-party evaluators as being objective (Cheema et al. 2014). This was key as perceptions of an unfair process undermine political trust (Van Ryzin 2011). The PMT used data on 23 variables to compute a family wealth score ranging from 0 to 100 (Ambler and De Brauw 2019). Eligible families were those with scores below 16.17—with some exceptions, detailed in Online Appendix B. The average wealth score among respondents in our dataset is 22.8, and 35.4 percent of households are BISP recipients (Table 1).⁶

The BISP aims to deliver cash transfers to each ever-married woman in eligible families who possess a valid Computerized National Identity Card (CNIC), which is a prerequisite for accessing governmental services (Gazdar 2011).⁷ Throughout the period of our study, BISP beneficiaries

⁴The program continued to bear her name and thus to be associated with the PPP despite political party transitions in 2013—a feature of the program which had made “its acceptability to the rest of the political parties...questionable,” as Marvi Memon (2016) wrote while serving as Chairperson of the BISP. She notes that a custom of respecting the deceased had slowed proposals of a change despite potential for other parties to benefit politically from the program (Memon 2016). By 2019, initiatives to change its name materialized.

⁵Details on BISP implementation can be found in Online Appendix B.

⁶The national average share is 23 percent, pointing to the deeper poverty of our rural sample.

⁷CNIC numbers, among other things, are necessary to register to vote and obtain a passport. To measure any barriers to receiving a CNIC card, we asked: “To get a CNIC card, do members of your community have to pay any extra fees, bribes, or facilitation payments?” 77 percent of respondents reported no such barriers.

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).⁸ Beneficiaries received payments either through the Pakistan Post or ATM cards (Ambler and De Brauw 2019). At the time of our survey, beneficiaries had received payments anywhere from 1.75 to 4.5 years.

Empirical Strategy

Data and Measurement

We employ two data sources: (1) individual-level administrative data on the poverty score of one’s family and one’s individual participation in the BISP; and (2) original survey data we collected in rural Pakistan during March–April 2012 (round 1) and April–May 2013 (round 2) from a male and a female respondent in each household. The Secretary of the BISP maintains an individual-level administrative database of all CNIC numbers in Pakistan and two key pieces of information for each: their family’s wealth score and BISP beneficiary status. We code a household-level indicator for having at least one member that receives the BISP.⁹ We combine this database with a survey, which we refer to as the Pakistan Rural Household Panel Survey (RHPS). Round 1 covered 2,090 households in 76 villages in Punjab, Sindh, and Khyber-Pakhtunkhwa (KPK) provinces; 2,002 of these were re-interviewed in round 2. The RHPS is described in detail in Online Appendix C.

The governance module was in round 2, and began with a priming experiment described below before asking respondents seven questions about their level of support for government, which comprise the “system support” battery in Booth and Seligson (2009). Exact question wordings and response options are enumerated in Online Appendix D. These questions probe the extent to which individuals feel that the courts guarantee a fair trial, respect political institutions, feel basic rights are protected, feel proud of the political system, feel that others should support the political system, trust the political system, and feel that leaders are doing the best job possible. Each question had five response options, 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).

⁸BISP cash transfer amount is adjusted annually for inflation.

⁹Online Appendix C describes construction of this outcome in detail.

Table 1: Summary Statistics

Variable	(1) Obs	(2) Mean	(3) Std. Dev.	(4) Min	(5) Max
BISP Household	2,639	0.354	0.478	0.0	1
Wealth Score	2,639	22.816	12.560	0.0	80.660
Poverty Prime	2,637	0.494	0.500	0.0	1
Panel A: Economic Well-Being					
Total Food Expenditures per Month (Rupees)	2,610	9,902.174	5,808.545	1,955.357	113,153.6
Total Expenditures per Month (Rupees)	2,610	16,140.83	7,758.07	3,316.28	120,302.2
Cash Loans Outstanding as Share of Yearly Expenditure	2,639	0.114	0.402	0	9.032
Total Savings as a Share of Monthly Expenditure	2,610	0.143	1.275	0.0	21.40
Household Earns Income from Outside Agriculture	2,608	0.160	0.366	0.0	1
Panel B: Attitudes Toward Government					
Government Support Index	2,636	0.367	0.193	0.0	1
Courts Guarantee Fair Trial	2,637	0.412	0.290	0.0	1
Respect for Political Institutions	2,637	0.499	0.272	0.0	1
Citizens' Basic Rights Protected	2,636	0.356	0.259	0.0	1
Proud of Political System	2,636	0.354	0.268	0.0	1
Others Should Support Political System	2,637	0.369	0.267	0.0	1
Trust Leaders	2,637	0.321	0.262	0.0	1
Leaders Doing Best Job Possible	2,637	0.255	0.258	0.0	1

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

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 Lægveid 2005). The seven items have a high Cronbach's alpha score of 0.88, indicating high levels of internal consistency. Given this high reliability coefficient, as well as concerns with multiple hypothesis testing, we construct a government support index which is the average of all seven measures, and use this as our principal outcome measure. Our government support index and each of its seven component measures are summarized in Panel B of 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.

To assess whether the BISP had the intended welfare effects that could ultimately explain effects on citizen attitudes towards political leaders and the government, we considered several measures of economic well-being and productivity (see Panel A in Table 1): 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 E for exact question wordings). Descriptive statistics of these measures are provided in Table 1. In our sample, 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 11.4 percent of annual expenditures.

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 (PMT score of 16.17) to determine program eligibility: a fuzzy regression discontinuity design (RDD). Receipt of BISP aid is a discontinuous function of a household’s wealth score, enabling us to compare the outcomes of households that were marginally ineligible to be beneficiaries with those that were marginally eligible, to evaluate the program’s effects. See Online Appendix F for additional details on our empirical strategy.

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 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 (2014*a*, 2014*b*) and Calonico, Cattaneo, Farrell, and Titiunik (2017), we select an optimal bandwidth that minimizes the mean squared error (MSE).¹⁰ We then employ the robust confidence intervals developed by Calonico, Cattaneo, and Titiunik (2014*b*), which estimate the asymptotic bias ignored by conventional inference and correct the standard errors appropriately to produce valid inferences.

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

¹⁰We also consider the optimal bandwidth recommended by Imbens and Kalyanaraman (2011) and consider results when halving and doubling bandwidth size (see Table A.4 in Online Appendix G).

to be at 0, and at this cutoff, there is a 59 percentage point ($p < 0.001$) discrete jump in the BISP beneficiary rate (see Figure A.1 in Online Appendix G, as well as 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, the take-up was only 5 percent.

Additionally, if there is a discontinuous difference in respondent characteristics around the score threshold, we worry that the exclusion restriction is violated, and our instrument is correlated with our outcome. Reassuring, we find that 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. 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 shows that no measure is significantly different at a 5 percent significance level at the cutoff (see Figure A.3a (a) and Table A.2 in Online Appendix G). Thus, the assumption that there are no meaningful differences in pre-treatment measures at the cutoff appears to hold.

Another threat to causal interpretation is household and/or government manipulation of wealth scores near the cutoff. This would imply that households differ in discrete ways precisely at the cutoff; for example, those just below the cutoff might be significantly more politically-connected. We can empirically test for such manipulation. First, we test the null hypothesis of continuity of the density of the forcing variable—the wealth score—at the cutoff (McCrary 2008). As shown in Figure A.2 in Online Appendix G, we find no evidence of discontinuity at the cutoff ($p = 0.604$). Results of a test described by Cattaneo, Jansson, and Ma (2020) similarly leads us to reject the null of density discontinuity ($p = 0.234$).

Survey Experiment

To study the moderating effects of perceived relative deprivation, we carried out a priming experiment with all households. We asked respondents which of five income brackets describes their household’s income, and manipulated the range of the bracket choices such that half of respondents were likely to be in the bottom bracket and feel that they have less than what is typical (i.e., poverty primed) and the other half were likely to be in the middle or a higher bracket and perceive their income to be average or higher. This experiment leverages the fact that individuals generally

have uncertainty about the income distribution (Gimpelson and Treisman 2018), and as such, perceptions of inequality are malleable and core to the study of the political effects of inequality.

To avoid spillovers, both respondents within a household received the same prime assignment, and they were further interviewed separately and simultaneously by dual-gender enumerator teams. 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.¹¹

No Poverty Prime	Relative Poverty Prime
0-2,000 Rs.	0-12,500 Rs.
2,001-4,000 Rs.	12,501-25,000 Rs.
4,001-6,000 Rs.	25,001-45,000 Rs.
6,001-10,000 Rs.	45,001-60,000 Rs.
More than 10,000 Rs.	More than 60,000 Rs.

Our relative poverty prime is a variation on one used by Haisley, Mostafa, and Loewenstein (2008) to study the decision to participate in lotteries. Mo (2018) first employed this design to study the effects of relative deprivation on political behavior in Nepal, Healy, Kosec, and Mo (2017) and Fair et al. (2018) replicated that design in Pakistan, and Kosec, Mo, Schmidt, and Song (2021) recently used it in Papua New Guinea. The logic of this prime derives from previous research showing that response options to ordinal or interval questions can send (often unintended) cues to respondents about what are normal responses (e.g., Shwarz, Hipper, Deutsch, and Strack 1985). Respondents frequently assume that the middle response is the modal response, and it thus becomes the reference point by which they assess their own economic well-being.

The middle income bracket in the non-primed group is 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 non-primed 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 non-primed group ($p < 0.001$).¹² In the non-primed

¹¹The dollar–Rs. exchange rate at the time of our survey was 98.5 Rs. per 1 USD. This is from May 2, 2013 (International Monetary Fund 2013).

¹²In 2013, the poverty line in Pakistan was 3,030 Rs. per adult equivalent per month; as the average household

group, the average and median respondent placed themselves in the middle income bracket. Actual total household income is almost identical across the group primed to feel relatively poor and the non-primed group ($p = 0.72$). Table A.3 in Online Appendix G shows that random assignment of this relative poverty prime worked as intended. There is balance with respect to social status (i.e., where, on a integer scale from 1 to 10 describing one’s social standing and influence in the community, an individual locates herself), gender, age, marital status, education level, parents’ education, and ethnicity. Moreover, exactly 50 percent of the sample received the poverty prime.¹³

Results

To assess the extent to which perceived relative poverty conditions the effects of BISP on governmental support, we estimate equation (4) 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.

Effects of the BISP on economic and political outcomes

It is first helpful to examine whether we can observe any economic benefits from BISP transfers. If transfers do not affect household welfare, it is difficult to theorize that they affect individuals’ political attitudes. Accordingly, we examined whether or not the BISP increased household expenditure and the propensity to engage in wealth-enhancing economic behaviors. 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 ($p < 0.01$; $p_{robust} < 0.01$).¹⁴ It also increases total household savings, normalized as a share of expenditures ($p = 0.03$; $p_{robust} = 0.02$), 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

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.

¹³Fuzzy RDD analyses for each pre-treatment demographic characteristic and an indicator for the respondent perceiving barriers to acquiring a CNIC card for both the primed group and non-primed group show that no measure is significantly different at a 5 percent significance level at the cutoff (see Figure A.3a (b) and (c), respectively, in Online Appendix G). Thus, the assumption that there are no meaningful differences in pre-treatment measures at the cutoff by prime condition holds.

¹⁴Throughout, 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).

Table 2: Effect of BISP (2SLS)

Variable	(1) First-Stage	(2) Estimate	(3) Robust P-Value	(4) Robust 95% CI	(5) N	(6) N_{tr}	(7) N_{co}
Panel A: Economic Well-Being							
Total Food Expenditures per Month (Rupees)	-0.595***	2,596***	0.003	[1,078.56, 5,206.14]	2,610	481	569
Total Expenditures per Month (Rupees)	-0.594***	3,107.5**	0.014	[815.647, 7,088.83]	2,610	487	586
Cash Loans Outstanding as Share of Yearly Expenditure	-0.619***	-0.109***	0.001	[-0.218, -0.059]	2,639	351	429
Total Savings as a Share of Monthly Expenditure	-0.600***	0.403**	0.016	[0.087, 0.859]	2,610	418	524
Household Earns Income from Outside Agriculture	-0.628***	0.382***	0.000	[0.259, 0.556]	2,608	331	357
Panel B: Attitudes Toward Government							
Government Support Index	-0.595***	0.080*	0.134	[-0.025, 0.185]	2,636	494	590
Courts Guarantee Fair Trial	-0.595***	0.125*	0.088	[-0.019, 0.283]	2,637	490	590
Respect for Political Institutions	-0.595***	0.104*	0.151	[-0.037, 0.241]	2,637	494	590
Citizens' Basic Rights Protected	-0.596***	0.116**	0.058	[-0.004, 0.267]	2,636	486	573
Proud of Political System	-0.594***	0.008	0.932	[-0.148, 0.135]	2,636	500	590
Others Should Support Political System	-0.595***	0.038	0.501	[-0.091, 0.186]	2,637	492	590
Trust Leaders	-0.594***	0.072	0.303	[-0.065, 0.210]	2,637	500	598
Leaders Doing the Best Job Possible	-0.595***	0.096	0.224	[-0.053, 0.227]	2,637	500	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 (Calonico, Cattaneo, and Titiunik 2014a). The robust p-value, 95% robust confidence intervals, sample size (N), as well as the number of treated (N_{tr}) and control (N_c) observations that lie within the optimal bandwidth in our regression discontinuity estimates are also reported. The running variable is the wealth score. Note that Panel A variables were drawn from Round 1 and Panel B variables were drawn from Round 2.

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 ($p < 0.01$; $p_{robust} < 0.01$). BISP transfer payments might substitute alternate sources of financing investments—most of them likely requiring payment of interest. The BISP thus led to observable improvements to the economic well-being of vulnerable households.

Next, we examine how one's household receiving the BISP influences individuals' downstream support for government. We first consider as outcomes both our government support index as well as the seven variables used to create it. These estimates are reported in Table 2, Panel B, column (2) (and depicted in Figure A.4 in Online Appendix G). Household receipt of the BISP leads to an 8 percentage point increase in an individual's government support index value; however, this effect misses the standard threshold of statistical significance ($p = 0.07$; $p_{robust} = 0.13$). Figure A.5 (a) in Online Appendix G displays the average government support index value by wealth score for households near the cutoff. It depicts a drop in government support precisely after crossing the

cutoff wealth score and thus becoming ineligible for 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.4), we see that BISP receipt corresponds with a more positive appraisal of political leaders and institutions. Two (three) of the seven measures are statistically significant at the 10 percent level when employing bias-corrected (conventional) standard errors. However, none (only one) are statistically significant at the 5 percent level 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.

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

Might perceived relative poverty affect the relationship between household BISP receipt and an individual’s political attitudes? Examining the individual government support index value averages near the wealth score cutoff for those primed versus not primed to feel relatively poor provides suggestive evidence that this is the case (see Figures A.5(b)-(c) in Online Appendix G). Considering only those individuals primed to feel relatively poor, Figure A.5(c) shows a decline in government support as one crosses the wealth cutoff and their household becomes ineligible for the BISP. However, no differences are visually apparent among those not primed (see Figure A.5(b)).

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 (Panel A) and not primed (Panel B) to feel relatively poor. The coefficients and confidence intervals based upon conventional standard errors for each of these two groups are depicted in Figure 1. Among the respondents randomly chosen to receive our relative poverty prime, our estimates of the effects of their household receiving the BISP on their government support index value are now nearly twice as large in magnitude than for the full estimation sample in Table 2, and this effect is statistically meaningful. Household receipt of the BISP leads to a 16 percentage point increase in the government support index; this is equivalent to a 43 percent increase over the mean level of support for government ($p = 0.02$; $p_{robust} = 0.02$). For those who received the relative poverty prime, household receipt of the BISP predicts increases in all seven individual measures of support that make up the index, with the effects of three (four) measures being statistically meaningful when using robust (conventional) standard errors, as shown in column (2) (column (1)) of Table 3.

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 component measure (Ansolabehere, Rodden, and Snyder 2008).¹⁵

Table 3: Effect of BISP on Attitudes Toward Government by Poverty Prime (2SLS)

Variable	(1) Estimate	(2) Robust P-Value	(3) Robust 95% CI	(4) N	(5) N_{tr}	(6) N_{co}
Panel A: Effect of BISP Among Those Who Received the Poverty Prime						
Government Support Index	0.155**	0.021	[0.026, 0.317]	1,303	252	310
Courts Guarantee Fair Trial	0.203**	0.045	[0.005, 0.436]	1,303	250	308
Respect for Political Institutions	0.196**	0.035	[0.015, 0.420]	1,303	246	298
Citizens’ Basic Rights Protected	0.195**	0.036	[0.014, 0.418]	1,303	246	298
Proud of Political System	0.117	0.270	[-0.089, 0.320]	1,303	259	318
Others Should Support Political System	0.118	0.177	[-0.062, 0.337]	1,303	248	306
Trust Political System	0.116	0.283	[-0.095, 0.325]	1,303	278	326
Leaders Doing the Best Job Possible	0.155*	0.121	[-0.040, 0.346]	1,303	309	349
Panel B: Effect of BISP Among Those Who Did Not Receive the Poverty Prime						
Government Support Index	0.015	0.791	[-0.149, 0.195]	1,333	185	174
Courts Guarantee Fair Trial	0.034	0.819	[-0.197, 0.249]	1,334	181	160
Respect for Political Institutions	-0.002	0.911	[-0.223, 0.199]	1,334	185	196
Citizens’ Basic Rights Protected	0.042	0.715	[-0.169, 0.246]	1,333	185	190
Proud of Political System	-0.064	0.679	[-0.272, 0.177]	1,333	185	188
Others Should Support Political System	-0.023	0.904	[-0.242, 0.214]	1,334	185	196
Trust Political System	0.050	0.507	[-0.148, 0.299]	1,334	185	174
Leaders Doing the Best Job Possible	0.061	0.630	[-0.170, 0.331]	1,334	185	174

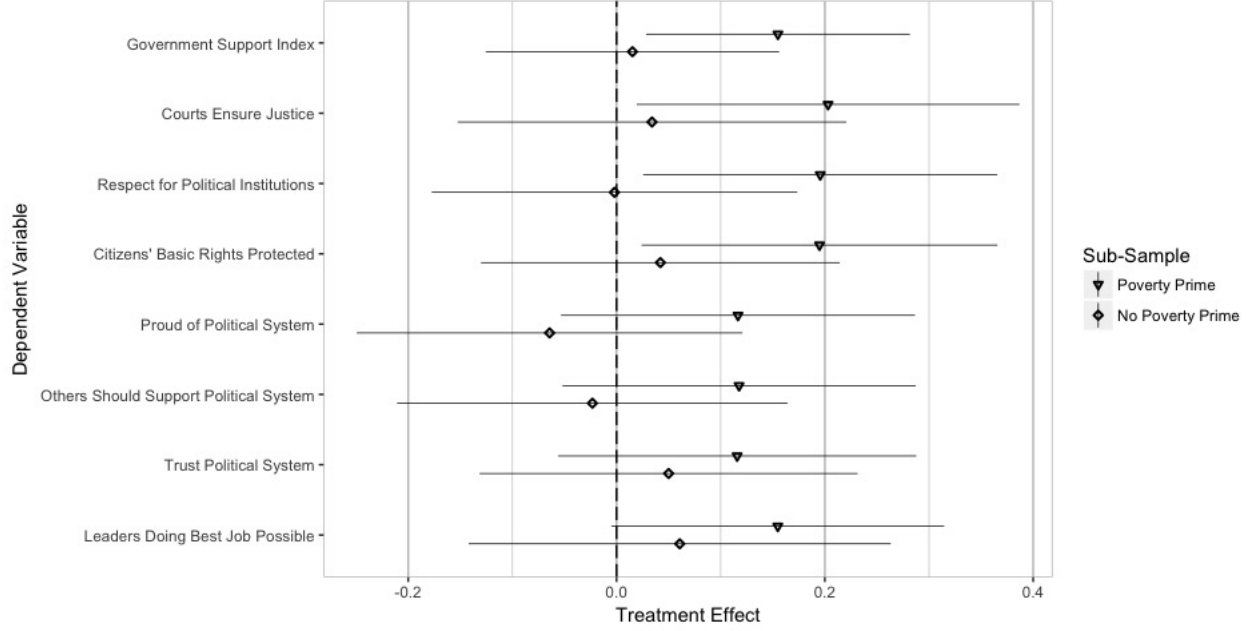
Source: Pakistan Rural Household Panel Survey (RHPS), Round 2 (2013) and Benazir Income Support Program Database (2013)

Notes: In the “Estimate” columns, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The estimate is the average treatment effect at the cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik 2014a). The robust p-value, 95% robust confidence intervals (CI), sample size (N), and the number of treated (N_{tr}) and control (N_c) observations within the optimal bandwidth are also reported. The running variable is the wealth score.

For those not primed to feel relatively poor, BISP recipients have the same levels of government confidence as non-recipients for all measures; standard errors are substantially larger than they are for the poverty primed group, and coefficients are always smaller—and in three cases negative. This is consistent with our conjecture that social protection would not have a strong effect on political

¹⁵Our findings hold up to correction for multiple hypothesis testing. We employ two 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. Regardless of method, all coefficients on receipt of the BISP that were statistically significant at the 10 percent level or higher remain so.

Figure 1: 2SLS Estimates – Attitudes Toward Government by Poverty Prime



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

support when relative deprivation is not salient.¹⁶ Thus, higher support for government due to receipt of the BISP is only apparent when one's relative deprivation is made salient.¹⁷

Is the positive effect of BISP among the poverty primed due to BISP recipients being relatively more appreciative of BISP assistance when they see themselves as relatively poor? Or is it an artifact of those from households that did not receive the BISP feeling relatively more disgruntled with government for the lack of transfers when they feel greater perceived relative poverty? When we examine the local polynomial estimates to the right and left of the cutoff for those who are and are not primed to feel relatively poor, we observe two separable shifts (see Table 4): (a) the relative 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 notable 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

¹⁶This pattern is not sensitive to bandwidth selection (see Table A.4 in Online Appendix G), though there are problems with statistical power when we halve the optimal bandwidth.

¹⁷That there are no impacts of the BISP on the non-primed group provides reassurance that the effects of the BISP we identify above are not simply due to demand effects stemming from the prime explicitly noting BISP as a potential source of household income. Rather, individuals whose household receives the BISP *selectively* report higher confidence in the political system than non-recipients only when primed to feel relatively poor.

Table 4: Moderating Effect of Relative Poverty Prime

Variable	Estimates to Left of Cutoff (Eligible for BISP)			Estimates to Right of Cutoff (Not Eligible for BISP)		
	(1) μ_{primed}	(2) $\mu_{notprimed}$	(3) Difference	(4) μ_{primed}	(5) $\mu_{notprimed}$	(6) Difference
Government Support Index	0.388	0.371	0.017	0.3	0.36	-0.06
Courts Guarantee Fair Trial	0.428	0.439	-0.01	0.314	0.415	-0.101
Respect for Political Institutions	0.507	0.501	0.006	0.397	0.502	-0.105
Citizens' Basic Rights Protected	0.401	0.414	-0.013	0.292	0.386	-0.094
Proud of Political System	0.368	0.334	0.034	0.302	0.378	-0.075
Others Should Support Political System	0.383	0.355	0.028	0.317	0.371	-0.054
Trust Leaders	0.338	0.313	0.026	0.273	0.279	-0.006
Leaders Doing the Best Job Possible	0.295	0.24	0.055	0.207	0.198	0.009

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

Notes: The difference ($\mu_{primed} - \mu_{notprimed}$) is computed by subtracting the local polynomial estimate for the subgroup that did *not* the prime ($\mu_{notprimed}$) from the estimate for the subgroup that received the prime (μ_{primed}).

effect that is of larger magnitude than the effect of (a)—seen by comparing columns (3) and (6).¹⁸

To consider magnitudes of our effects, we examine the individual-level government support index (Table 4, row (1)). Among those close to the cutoff but just to its left, whose households accordingly received the BISP, there was a negligible difference between those who were and were not primed to feel relatively poor: 0.371 versus 0.388, indicating a 1.7 percentage point increase in government satisfaction due to being poverty primed. This suggests that feeling relatively poor is not unambiguously bad for engendering government support among social protection recipients. It is consistent with the poverty prime instilling gratitude in beneficiaries, though more research is needed to uncover the exact mechanism. In contrast, among those close to the cutoff but just to its right, whose household thus barely missed receiving the BISP, we observe a sizable, 6 percentage point drop in government support due to receiving the relative poverty prime (0.3 vs. 0.36). Thus, feeling relatively poor, absent aid, can foment dissatisfaction with government. Notably, the size of the drop in government support we see among non-beneficiaries is larger than the increase in government support we see among beneficiaries elicited by feelings of relative economic insecurity. This difference in magnitudes is consistent with loss aversion, a key principal of prospect theory;

¹⁸We conduct this exercise to assess whether the statistically significant effects we observe among those who are primed and do not observe among those who are not primed are driven by those who did not receive BISP becoming more disgruntled when relative deprivation is salient or those who did receive BISP becoming more satisfied when relative deprivation is salient. We do not conduct it to test whether the average levels of political support among the primed and non-primed groups are statistically meaningful on the same side of the cutoff.

the pain of not receiving social protection aid is psychologically more powerful than the pleasure of gaining social protection aid (Kahneman and Tversky 1979).

Overall, we take our results as evidence that perceptions of relative poverty moderate the effects of receiving cash transfers on support for government, and further, most of the difference is driven by non-beneficiary households becoming disgruntled with government when they are primed to feel relatively poor rather than beneficiaries becoming more satisfied when primed to feel relatively poor. Absent perceptions of deprivation, receiving BISP aid has few effects on government support 1.75 to 4.5 years later. Our results contribute a scope condition for when one is more likely to detect a positive effect of a social protection program, and highlight that positive effects of a social protection program should not be viewed as unambiguously positive.

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. Those who *already* felt relatively poor before receipt of the poverty prime should be (relatively) immune to its effects.

To test this, 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 was 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.¹⁹ As

¹⁹This pre-treatment question was asked several modules before our key survey experiment, making it unlikely that this pre-treatment question had an effect on our outcome measures. However, if there were an effect, it would bias our results downward among those who were primed, as this relative economic standing question would presumably make the effect of the relative poverty prime less potent.

Table 5: Effect of BISP on Attitudes Toward Government by Perceived Income Standing Pre-Treatment Among Primed Individuals (2SLS)

Variable	(1) Estimate	(2) Robust P-Value	(3) Robust 95% CI	(4) N	(5) N_{tr}	(6) N_{co}
Panel A: Effect of BISP Among Those Who Did Not Feel Relatively Poor Pre-Treatment						
Government Support Index	0.239***	0.001	[0.107, 0.439]	758	178	195
Courts Guarantee Fair Trial	0.159	0.151	[-0.070, 0.456]	758	190	203
Respect for Political Institutions	0.345***	0.000	[0.190, 0.699]	758	145	174
Citizens' Basic Rights Protected	0.258**	0.012	[0.064, 0.525]	758	178	199
Proud of Political System	0.196*	0.061	[-0.010, 0.449]	758	149	180
Others Should Support Political System	0.225**	0.016	[0.048, 0.466]	758	178	199
Trust Political System	0.215**	0.025	[0.031, 0.460]	758	178	199
Leaders Doing the Best Job Possible	0.284***	0.009	[0.079, 0.555]	758	190	203
Effect of BISP Among Those Who Felt Relatively Poor Pre-Treatment						
Government Support Index	-0.047	0.505	[-0.373, 0.184]	545	61	92
Courts Guarantee Fair Trial	0.042	0.879	[-0.415, 0.356]	545	49	81
Respect for Political Institutions	-0.079	0.467	[-0.483, 0.222]	545	61	95
Citizens' Basic Rights Protected	0.008	0.803	[-0.403, 0.312]	545	61	92
Proud of Political System	-0.017	0.767	[-0.448, 0.330]	545	61	92
Others Should Support Political System	-0.044	0.754	[-0.420, 0.304]	545	71	109
Trust Political System	-0.120	0.447	[-0.607, 0.268]	545	61	92
Leaders Doing the Best Job Possible	-0.145	0.312	[-0.527, 0.168]	545	61	95

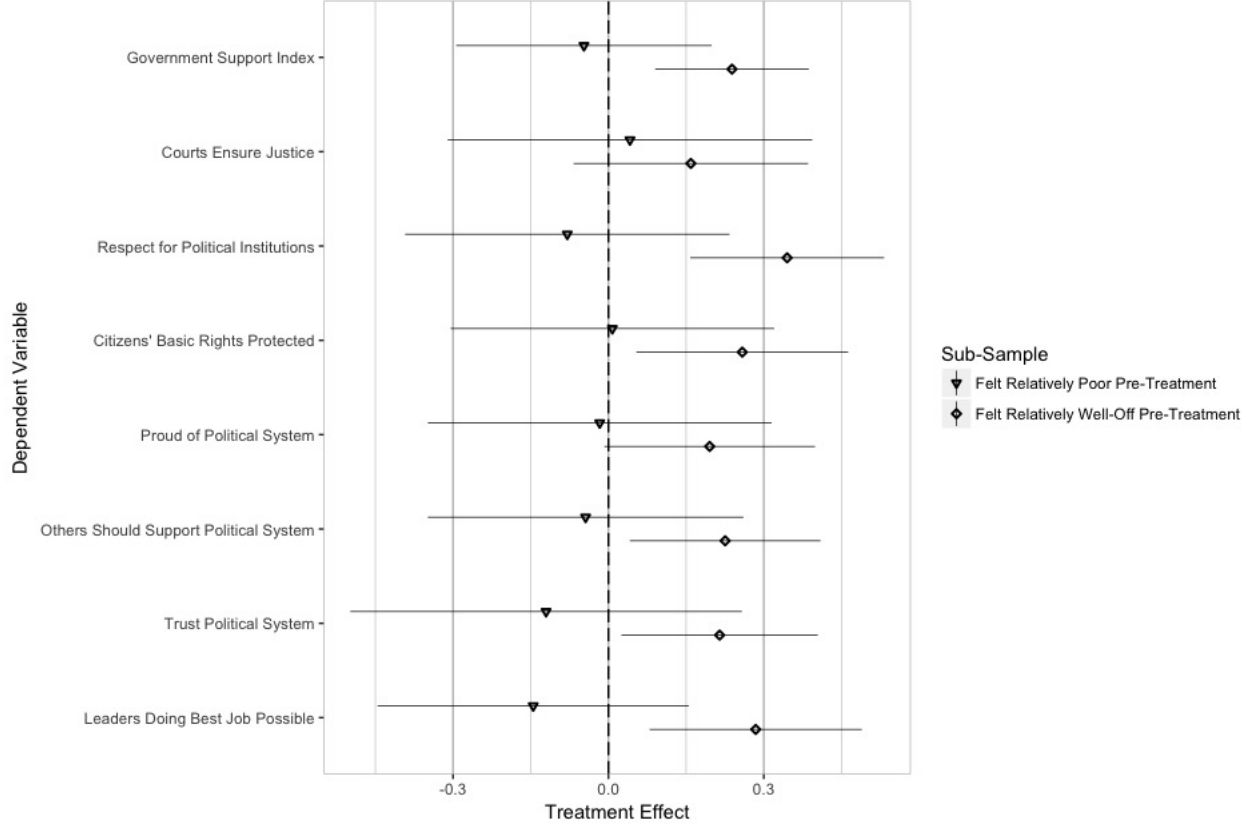
Source: Pakistan Rural Household Panel Survey (RHPS), Round 2 (2013) and Benazir Income Support Program Database (2013)

Notes: In the "Estimate" columns, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The estimate is the average treatment effect at the cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik 2014a). The robust p-value, 95% robust confidence intervals, sample size (N), and the number of treated (N_{tr}) and control (N_c) observations within the optimal bandwidth are also reported. The running variable is the wealth score.

shown in Table 5 and Figure 2, the effects we observe among those primed to feel relatively poor are predominantly driven by those who did not feel relatively poor pre-treatment. For this group, the effects of BISP receipt on the index, as well as on six of the seven individual components, are statistically significant. Moreover, the effect on the government support index is larger in magnitude for this group; we estimate a 24 percentage point increase, which is equivalent to a 70 percent increase over the mean level of government support. This pattern is consistent with the argument that voters' perceptions of their relative well-being condition their reactions to receipt of social protection.

As an additional robustness check, we conducted a set of placebo tests. We considered measures of political attitudes unlikely to be affected by social protection and feelings of relative deprivation

Figure 2: 2SLS Estimates – Attitudes Toward Government by Perceived Relative Income Pre-Treatment Among Primed Individuals



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

including whether violence to protect religious values is justified, whether military action against extremist groups is helpful, and whether Kashmiri independence is important. Figure A.6 in Online Appendix G confirms that neither the BISP nor the poverty prime affect these measures.²⁰

Conclusion

Our work seeks to advance a growing body of literature on the selective manner in which individuals reward government for public investments. A rich literature in public administration has shown that there is often a disconnect between government performance and public trust (e.g., Van Ryzin 2011; Yang and Holzer 2006). Previous literature has focused on habituation, attribution challenges, issues of political manipulations and capture, and recency bias to explain mixed effects of social protection. We posit and test the influence of another potential moderator: citizens'

²⁰Exact question wordings are provided in the notes that accompany the figure.

perceptions of their relative economic standing. We evaluate the effects of Pakistan’s nation-wide, unconditional cash transfer program, the BISP, on support for government leaders and institutions when we do and do not experimentally make perceptions of relative deprivation salient. We exploit the fact that BISP receipt is a discontinuous function of an individual’s wealth score to use an RDD approach to assess its causal impacts. Beyond assessing the average treatment effects of the program, our combination of an experimental and a quasi-experimental research design allows us to consider the extent to which perceptions of one’s relative economic standing 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 one to four years later. But, when it is salient, beneficiaries have higher support for government than do non-beneficiaries. Individuals’ perceptions of their relative deprivation 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 the shift is largest for non-beneficiaries, for whom receiving no aid in the face of feeling relatively deprived meaningfully 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 gratitude for or 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. Moreover, we demonstrate that it is problematic to view non-recipients of social protection as a “control” group to compare recipients of social protection against to assess the effect of social protection programs, even if the program is randomly assigned as non-recipients are affected by the non-receipt of aid, especially when non-recipients feel economically insecure. This observation suggests that the positive effects of social protection program on political attitudes noted in previous studies may be overestimated; the positive effects may reflect non-recipients becoming more politically disillusioned as opposed to recipients becoming more satisfied with their political leaders and institutions.

One drawback of our study is the fact that we estimate a local average treatment effect that may be sensitive to the cutoff chosen. That is, we have learned about individuals close to the poverty score determining eligibility, but not the broader set of poor individuals in Pakistan. Future

work could examine settings with multiple cutoffs to understand how impacts of social protection on political attitudes differ at different points in the income or wealth distribution. Additional research should also examine how effects differ depending on whether the cash transfer program is unconditional (like BISP) or imposes conditions. Moreover, additional research is needed to explore the external validity of our findings beyond Pakistan, where relationships with government may be influenced by a history of military rule. With that said, the theoretical underpinnings of the phenomena we observe is not unique to Pakistan, and expect to observe similar effects in other countries.

Priming individuals to feel relatively poor via a survey experiment has the advantage that we isolate one thing—immediate perceptions of one’s relative position—from other factors or responses that may come with actual shifts in inequality. This is helpful to overcome endogeneity challenges and to isolate causal pathways. At the same time, it is important to acknowledge that experimental effects are bound to differ from those observed in everyday life, in large part because of the many endogenous shifts likely to accompany shifts in inequality, such as explicit actions by the power class to relax the lower class’s conflict emotions and thus reduce perceived inequality. Conclusions from our experiment, by virtue of it being free from any concomitant responses to inequality by others (e.g., citizens, government, or NGOs), could limit its external validity in settings in which such endogenous changes do occur. Further research is needed on how actual shifts in inequality affect perceived inequality and how this then mediates the impacts of social protection.

As building faith in government is often a key objective of social protection programs, our research has important policy implications. When perceived relative deprivation is quite low, social protection is unlikely to have sustained effects on political attitudes in the medium- or long-terms. Of course, building support for government is only one of several goals of social protection. In contrast, when perceived relative deprivation 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 neglected while others are getting the program). As such, positive effects of social protection programs detected from observing those who received aid expressing more pro-government sentiment than who did not receive aid may not be unambiguously good news, as the positive effect is, in part, driven by the decline in support for government stemming from non-beneficiaries.

Might there be ways to distribute benefits in such a way that resentment among non-beneficiaries is mitigated? This remains an open policy question. 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. Regardless, our study adds to the growing body of scholarship noting that there are large political ramifications of increased perceived relative deprivation (e.g., Zmerli and Castillo 2015), and helps clarify why social protection programs do not consistently produce political wins for the government providing such protections.

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Online Appendix

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

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A Literature Review

To generate a comprehensive list of studies on the effect of cash transfer and other social protection programs on political attitudes, in May 2019, we had a research assistant conduct a systematic review of articles written in high-impact journals in the last two decades. Search terms included a combination of terms related to our independent variable of interest (“cash transfer programs” and “social protection programs”) with terms related to our outcome variable of interest (“political attitudes” and “elections”). Targeted economics journals included *The American Economic Review*, *Journal of Political Economy*, *Quarterly Journal of Economics*, *Review of Economic Studies*, *Review of Economics and Statistics*, *American Economic Journal (AEJ): Applied*, *AEJ: Economic Policy*, *Journal of Development Economics*, *Journal of Public Economics*, *The Economic Journal*, and *Economic Development and Cultural Change*. Targeted political science and interdisciplinary journals included *American Journal of Political Science*, *American Political Science Review*, *Journal of Politics*, *British Journal of Political Science*, *Comparative Political Studies*, *Electoral Studies*, *Political Behavior*, *World Politics*, *Quarterly Journal of Political Science*, *Science*, *Proceedings of the National Academy of Sciences*, *World Development*, and *Social Science Quarterly*.

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 26 studies. Of these, 19 (i.e. almost 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.

If actual inequality has a positive (as opposed to negative) average impact on increases perceived inequality, our conceptual framework would predict that we would *overestimate* the benefits of social protection for government support by consulting the studies in this review. In keeping with this prediction, among studies we reviewed finding a positive impact of social protection, the Gini index is on average 3.49 points (0.40 standard deviations) higher than it is for studies identifying null and/or some negative impacts. Null and mixed studies come predominately from low-inequality contexts.

Table A.1: Evidence Review of the Effect of Cash Transfer Programs on Political Attitudes

Author (Year)	Country Context	Dataset	Population	Gini Index	Outcome(s)	Effect
Lyal, Zhou, and Imai (2020)	Afghanistan	Pre-baseline enrollment form (2015), baseline survey (2015), and two endline surveys (2016)	Individuals who were young, underemployed, displaced, and shared Pashtun ethnicity with the Taliban	27.8 (2013)	1. Relative support for the Taliban versus the Afghan government 2. Binary and frequency variables of violence	Mixed
Pop-Eleches and Pop-Eleches (2012)	Romania	Gallup public opinion survey of program participants (2007), Basic information about program participants (2005)	Comparable eligible and ineligible applicants	28.2 (1994)	1. Dummy for voting 2. Dummy for voting for the incumbent party 3. Trust in government	Positive
Bechtel and Hainmueller (2011)	Germany	Flood report (2002), State election and constituency data (1994, 1998, 2002, 2005, 2009)	All voters	29.2 (1994)	Vote share for the incumbent party	Positive
Lee, Jensen, Arndt, and Wenzelburger (2017)	United Kingdom and Denmark	Polling data for government support in United Kingdom (1946-2014) and Denmark (1957-2014)	British and Danish poll respondents	30.5	Mean percentage of support for governing parties	Positive
Kwon (2018)	18 European countries	European Social Survey (1999-2015)	Representative sample of Europeans	30.7	Dummy for having voted for leftist parties	Mixed
Cole, Healy, and Werker (2012)	India	Election and rainfall data (1977-1999)	Representative sample of Indian voters	32.1 (1983)	Vote share for the ruling coalition	Positive
Healy and Malhotra (2009)	United States	Presidential election results, natural disaster, and government spending data (1984-2004)	All voters	34.6 (1979)	Presidential vote share for the incumbent party	Positive
Brazys, Heaney, and Walsh (2015)	Malawi	Malawian Welfare Monitoring Survey (2008), Election data (2004, 2009)	All voters	39.9 (2004)	Vote share for the incumbent party	Positive
Dionne and Horowitz (2016)	Malawi	Panel survey of rural Malawians (2008, 2010)	Representative sample of rural Malawians	39.9 (2004)	Dummy for supporting the incumbent party	Positive
Margalit (2011)	United States	Data of applications for compensation for trade-related job loss (1996-2004)	Representative sample of workers hurt by trade	40.2 (1994)	Change in Republican presidential vote share	Positive
Evans, Holtemeyer, and Kosec (2019)	Tanzania	Survey on beneficiaries and would-be beneficiaries (2009, 2011, 2012)	Representative sample of rural households with vulnerable children and elderly individuals	40.3 (2007)	Dummy for trusting political leaders	Positive
Chen (2013)	United States	Data on hurricane disaster aid awards (2004), Election data (2002, 2004)	Applicants to hurricane disaster aid	40.4 (2000)	Voter turnout for the incumbent party	Positive
Clinton and Sances (2018)	United States	Dave Leips Atlas of US Presidential Elections (2010, 2012, 2014, 2016)	Representative sample of residents in Medicare expansion and non-expansion states	40.4 (2010)	1. Voter registration 2. Voter turnout	Positive
Mettler and Stonecash (2008)	United States	Maxwell Poll (2005)	Poll respondents	40.5 (2004)	Voter turnout	Mixed
Labonne (2013)	Philippines	Precinct-level electoral data (2007, 2010), Poverty statistics (2003, 2007)	All voters	41.5 (2003)	Vote share for incumbent	Positive
Lü (2014)	China	Chinese Attitudes toward Inequality and Distributive Injustice (2004, 2009)	Representative sample of rural and urban Chinese	42.1 (2002)	1. Trust in central government 2. Trust in local government	Positive
Marschall, Aydogan, and Bulut (2016)	Turkey	Mayoral election data (2004, 2009, 2014), Data of housing projects (2003-2014)	All voters	42.2 (2003)	# of times ruling party won mayoral election over last three elections	Positive
Mancoorda, Miguel, and Vigorito (2011)	Uruguay	Baseline and 2 follow-up survey amongst applicants for the cash transfer program (2005-2008)	Applicants to the cash transfer program	42.4 (1989)	Support for the current government	Positive
Blattman, Eneria, and Fiala (2018)	Uganda	Baseline and two follow-up survey (2008, 2010, 2012)	Representative sample of applicants to the program	45.2 (2008)	Index of presidential support	Mixed
Layton and Smith (2015)	24 countries in Latin America and Caribbean	AmericasBarometer survey (2012)	Representative sample of Latin Americans	47.3	Dummies of voting for the incumbent in a hypothetical voting	Positive
Imai, King, and Velasco Rivera (2020)	Mexico	Presidential election data (2000, 2006), Baseline and follow-up survey (2005, 2006) of the SPS program, Poverty data of Progresa (1990, 1995)	Representative sample of poor Mexicans	48.9 (1984)	1. Voter turnout in presidential election 2. Vote share of incumbent party	Null
De La O (2013)	Mexico	Precinct level election data (2000), Poverty data (1990, 1995)	Representative sample of poor Mexicans	48.9 (1984)	1. Voter turnout 2. Vote share of the incumbent party	Positive
Linos (2013)	Honduras	Presidential and mayoral election data (1993-2005), Survey of targeted municipalities (2000, 2002)	All voters	51.8 (1992)	1. Vote share for incumbent mayor 2. Vote share for incumbent president	Positive
Cerdas and Vergara (2008)	Chile	Election data (1989, 1993, and 1999), Survey of Socioeconomic Characteristics of the Chilean Population (1990, 1992, 1998)	All voters	56.2 (1987)	Vote share for the incumbent	Positive
Conover, Zarate, Canacho, and Baez (2018)	Colombia	Electoral census and booth-level electoral results (2010), the CCT's management information system of beneficiaries (2001-2010)	All voters	57.2 (2001)	1. Turnout 2. Vote share of incumbent party candidate 3. Margin of victory	Positive
Zucco (2013)	Brazil	National household survey on program enrollment (2000), Municipal election data (2002, 2006, 2010)	All voters	59 (1999)	1. Vote share for incumbent 2. Vote share for incumbent party candidate	Positive

Notes: Papers are ordered according to the value of the Gini index for the study context (lowest to higher). The Gini index is taken from the first year in the data set used, 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).¹ 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. Unsurprisingly, opponents of the PPP party objected that the program was politicized (Haseeb and Vyborny 2017).

The federal government ended the system of members of parliament selecting beneficiaries in April 2009 and 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. 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). The PMT used data on 23 variables to compute a family wealth score ranging from 0 to 100 (Ambler and De Brauw 2019). 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 2019).

Eligible families were those with scores below 16.17 or with scores between 16.17 and 21.17 who met at least one of the following three criteria: 1) at least one disabled member; 2) at least one senior citizen (65 years of age or older) and less than three total family members; or 3) four or more children under age 12 (Ambler and De Brauw 2019). 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 2019). The federal government began using these data to distribute transfers in July 2011, and the number of beneficiary families rapidly expanded from 1.8 million in 2009 to 5.3 million in 2011 (Government of Pakistan 2017). The switch to use of a PMT naturally ended BISP access for some while simultaneously extending it to previous non-beneficiaries. Recipients selected under the old system (i.e., by members of parliament) who did not qualify under the new criteria had their payments stopped. From 2011 up to our household survey (April–May 2013), citizen removal from the program has been almost non-existent (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

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

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.² 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)³ up to 4 years, 7 months (for those who received transfers since October 2008 and who we surveyed in May 2013).⁴

C Background on Data Sources

RHPS Dataset Sample

The RHPS provides village-, household-, and individual-level data on a range of economic, political, and social topics. It includes a common set of topics across rounds, plus select topics in certain rounds—including a governance module asked only in round 2. Two respondents per household—the head and their spouse—completed surveys. When the head or spouse was not available, a second visit was made to the household. If the individual was still not available, another knowledgeable member of the same gender was selected. Interviews with the male respondent and female respondent were carried out simultaneously by dual-gender enumerator teams, reducing biases that can be generated from overhearing responses.

The 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.

Both rounds collected Computerized National Identity Card (CNIC) numbers of respondents—though in round 2, we only gathered CNIC data if they were not received in round 1. Most of our

²A 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.

³In 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.

⁴We 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.

analysis makes use of round 2 data, with two exceptions: we use CNIC data from both rounds; and we use round 1 data on indicators of economic well-being to ascertain whether or not the BISP impacted economic livelihood shortly after its implementation.

Construction of BISP Beneficiary Variable from Administrative Data

3,914 individuals from 2,002 different households were provided with the governance module during round 2 of our survey. Of those, 3,292 individuals from 1,921 households reported a “plausible” (i.e., correct number of digits) CNIC number. Of the 3,292, 80 percent (i.e., 2,639) were in the BISP administrative database.^{5,6} Households lacking a CNIC number in the BISP database were omitted from our analysis.⁷ We then coded, for households in the administrative database, a household-level BISP beneficiary variable.

The Office of the Secretary of the BISP provided us with individual-level administrative data which we used to construct a household-level BISP beneficiary variable as follows. For each individual in the 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 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,⁸ given that his beneficiary status was always 0, we had to make use of his poverty score—plus demographic data from our households survey—to code a household-level BISP beneficiary indicator. This is non-ideal since the poverty score and household demographics only tell us his family’s *eligibility* to receive the BISP—not whether in fact a family member actually *registered*.⁹ 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, only 6.7 percent (i.e., 176 out of 2,639) 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

⁵CNIC numbers missing from the database may have been missing for several reasons: misreporting, data entry problems, or the individual could have only recently received a CNIC number (and not yet provided to the BISP).

⁶These valid and matched CNIC numbers came from 1,349 different households.

⁷Survey respondents that were missing a CNIC number or provided a CNIC number that did not match with the BISP database were not necessarily more disadvantaged or advantaged than those who were in the BISP administrative database. They were similar with respect to gender ($p = 0.893$), education level ($p = 0.788$), maternal education ($p = 0.274$), and being non-punjabi, non-sairai, and non-sindhi ($p = 0.238$). Nevertheless, there were some differences. Those that did not match with the BISP database saw themselves as having higher social status than those who did ($p < 0.01$) and reported higher paternal education levels ($p = 0.023$). Those with un-matched or missing CNIC numbers were more likely to be punjabi ($p < 0.01$), and less likely to be sairai ($p < 0.01$) and sindhi ($p < 0.01$).

⁸In all such households, there was an adult female, but we simply did not have administrative data on her.

⁹All 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.

BISP, as a small set of individuals who we count as BISP recipients actually received no aid due to their failure to register. On average, 10 percent of households with a BISP beneficiary had more than one recipient (Cheema et al. 2014).

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

D Question Wording: Political Attitude Outcome Measures

Exact question wording for political attitude outcome measures from Round 2 of our survey 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.

E Question Wording: Economic Outcome Measures

The exact questions used to construct our economic outcome measures from Round 1 of our survey 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

F Regression Discontinuity Design Details

We pursue a regression discontinuity design (RDD) to estimate the causal effect of BISP. This strategy helps attenuate selection bias concerns. To illustrate, consider the following empirical specification:

$$Y_i = \beta_0 + \tau B_i + \epsilon_i \quad (1)$$

where i indexes households. Y_i denotes our outcome of interest—support for government—and B_i is an indicator representing receipt of BISP cash transfers. ϵ_i is measurement error, and τ is our parameter of interest—the relationship between BISP receipt and our outcome of interest. If individuals receive aid because of unobservable characteristics like political connectedness, which are correlated with political support, direct estimation of τ via 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 , the PMT, 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:

$$\lim_{\Delta \downarrow 0} E[\epsilon_i | X_i = c + \Delta] = \lim_{\Delta \uparrow 0} E[\epsilon_i | X_i = c + \Delta]. \quad (2)$$

If equation (2) holds, an indicator variable for having a score below the cutoff c , D_i , can serve as an instrumental variable for receipt of the BISP. In our case, the threshold is not a sharp cutoff given that a few exceptions were made if households had greater need (e.g., having a large number of children or disabled persons) and a few families with wealth scores below the cutoff had not yet received BISP transfers at the time of our survey, as detailed in our background section (see Online Appendix B for greater details on program eligibility and take-up). We thus employ a fuzzy RDD,

which does not require a 100 percent jump in the probability of receiving BISP transfers at the cutoff, and only requires the following to hold:

$$\lim_{\Delta \downarrow 0} Pr[D_i = 1|X_i = c + \Delta] \neq \lim_{\Delta \uparrow 0} Pr[D_i = 1|X_i = c + \Delta]. \quad (3)$$

As the probability of BISP receipt, or our “treatment,” jumps by less than one at the threshold, the jump in the relationship between outcome Y_i and wealth score X_i can not 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 τ for outcome Y_i as follows:

$$\tau_F = \frac{\lim_{\Delta \downarrow 0} E[Y_i|X_i = c + \Delta] - \lim_{\Delta \uparrow 0} E[Y_i|X_i = c + \Delta]}{\lim_{\Delta \downarrow 0} E[D_i|X_i = c + \Delta] - \lim_{\Delta \uparrow 0} E[D_i|X_i = c + \Delta]} \quad (4)$$

where the F subscript refers to the fuzzy RDD.

G Supplementary Figures and Tables

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

Demographic Characteristics	(1) First-Stage	(2) Estimate	(3) Robust P-Value	(4) Robust 95% CI	(5) N	(6) N_{tr}	(7) N_{co}
Social Status	-0.595***	0.701	0.084	[-0.119, 1.895]	2,637	490	590
Female	-0.593***	0.020	0.914	[-0.275, 0.307]	2,637	510	618
Age 18-25	-0.588***	-0.013	0.656	[-0.180, 0.113]	2,600	521	629
Age 25-35	-0.592***	0.053	0.695	[-0.144, 0.301]	2,600	475	555
Age 35-45	-0.589***	-0.044	0.512	[-0.293, 0.146]	2,600	508	617
Age 45-55	-0.590***	0.092	0.277	[-0.095, 0.332]	2,600	489	583
Married	-0.593***	0.005	0.777	[-0.099, 0.133]	2,608	501	607
Received Primary Education	-0.625***	0.096	0.223	[-0.073, 0.314]	2,464	401	503
Received Intermediate Education	-0.612***	-0.0003	0.993	[-0.116, 0.117]	2,464	494	584
Received Secondary Education	-0.615***	-0.085	0.203	[-0.232, 0.049]	2,464	472	552
Received Post-Secondary Education	-0.617***	-0.012	0.406	[-0.054, 0.022]	2,464	472	552
Mother’s Years of Education	-0.606***	0.075	0.864	[-0.442, 0.527]	2,455	593	651
Father’s Years of Education	-0.611***	0.262	0.487	[-0.706, 1.482]	2,456	460	543
Punjabi	-0.592***	0.117	0.332	[-0.103, 0.305]	2,603	524	633
Sairai	-0.592***	0.093	0.385	[-0.106, 0.274]	2,603	563	666
Sindhi	-0.606***	-0.083	0.528	[-0.222, 0.114]	2,603	404	503
Other Ethnicity	-0.592***	-0.112	0.165	[-0.350, 0.060]	2,603	404	503
CNIC Unfair	-0.597***	-0.076	0.114	[-0.306, 0.141]	2,337	369	476

Notes: In columns (1) and (2), * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The estimate is the average treatment effect at the wealth score cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik 2014a). 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.

Table A.3: Balance Test: Relative Poverty Prime Assignment

Demographic Characteristic	(1) $\mu_{NotPrimed}$	(2) μ_{Primed}	(3) Difference in Means	(4) Test of Balance (P-Value)
Social Status	4.084	4.012	-0.072	0.238
Female	0.505	0.504	-0.001	0.924
Age 18-25	0.105	0.107	0.002	0.869
Age 25-35	0.254	0.260	0.006	0.663
Age 35-45	0.252	0.243	-0.009	0.520
Age 45-55	0.208	0.218	0.01	0.447
Married	0.897	0.899	0.002	0.874
Received Primary Education	0.159	0.151	-0.008	0.472
Received Intermediate Education	0.077	0.067	-0.01	0.238
Received Secondary Education	0.104	0.108	0.004	0.698
Received Post-Secondary Education	0.018	0.015	-0.003	0.462
Mother's Years of Education	0.171	0.174	0.003	0.919
Father's Years of Education	1.009	1.034	0.025	0.780
Punjabi	0.359	0.364	0.005	0.738
Sairaiiki	0.209	0.219	0.01	0.448
Sindhi	0.131	0.113	-0.018	0.098
Other Ethnicity	0.301	0.304	0.003	0.880
CNIC Unfair	0.225	0.233	0.008	0.612
<i>Proportion</i>	0.501	0.499	.	.

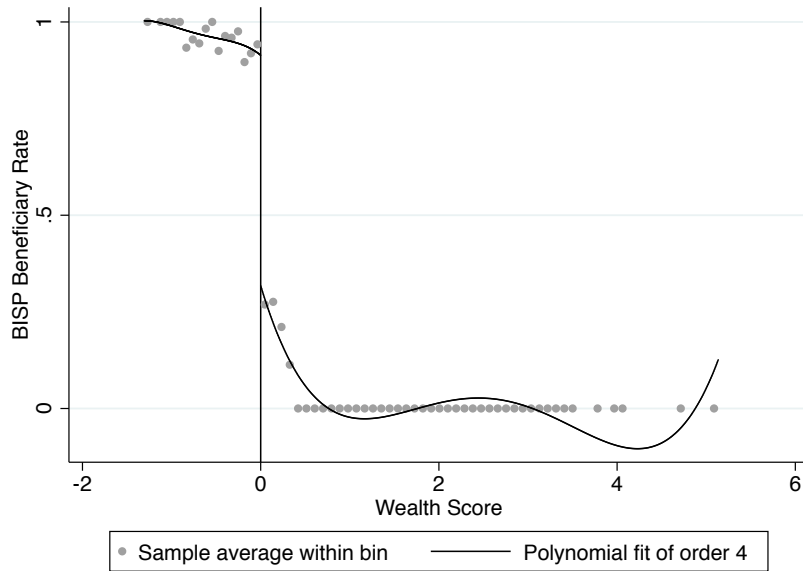
Notes: For each of the observable demographic characteristics, Columns (1) and (2) report means by the experimental condition. Column (3) reports the difference in means ($\mu_{Primed} - \mu_{NotPrimed}$), and Column (4) reports the p-value when conducting a difference in means test by experimental condition. The *proportion* row indicates what share of the total sample was assigned to each of the two conditions.

Table A.4: RD Estimate by Bandwidth Selection Procedure

Bandwidth Selection Procedure	(1) Full Sample	(2) No Poverty Prime	(3) Poverty Prime
(a) MSE-Optimal Bandwidth (Calonico, Cattaneo, and Titiunik 2014a)	0.080* (0.044)	0.015 (0.072)	0.155** (0.065)
(b) MSE Minimizing Bandwidth (Imbens and Kalyanaraman 2011)	0.077 (0.051)	0.007 (0.048)	0.152** (0.065)
(c) 1/2 the MSE Minimizing Bandwidth (Imbens and Kalyanaraman 2011)	0.071 (0.067)	-0.005 (0.068)	0.108 (0.086)
(d) 2X the MSE Minimizing Bandwidth (Imbens and Kalyanaraman 2011)	0.064* (0.036)	0.015 (0.033)	0.093** (0.043)
N	2636	1333	1303

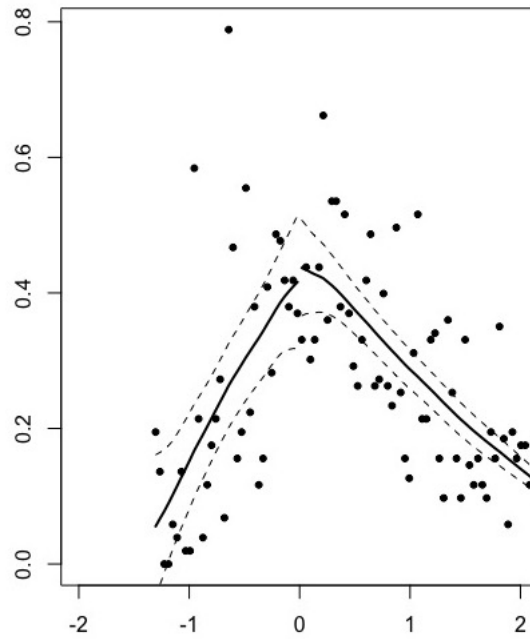
Notes: Standard errors are in parentheses, and * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each row presents the regression discontinuity (RDD) estimate when employing different bandwidth strategies. Row (a) reports estimates when employing the MSE-optimal bandwidth procedure recommended in Calonico, Cattaneo, and Titiunik (2014a). Row (b) reports estimates when employing the optimal bandwidth recommended in Imbens and Kalyanaraman (2011). Row (c) and (d) report estimates when the bandwidth selection procedure is half and double the optimal bandwidth recommended in Imbens and Kalyanaraman (2011), respectively. Column (1) reports RDD estimates when analyzing the full sample, column (2) reports RDD estimates when analyzing the sample that was not primed with the poverty prime, and column (3) reports RD estimates when analyzing the sample that was primed with the poverty prime.

Figure A.1: First Stage Results



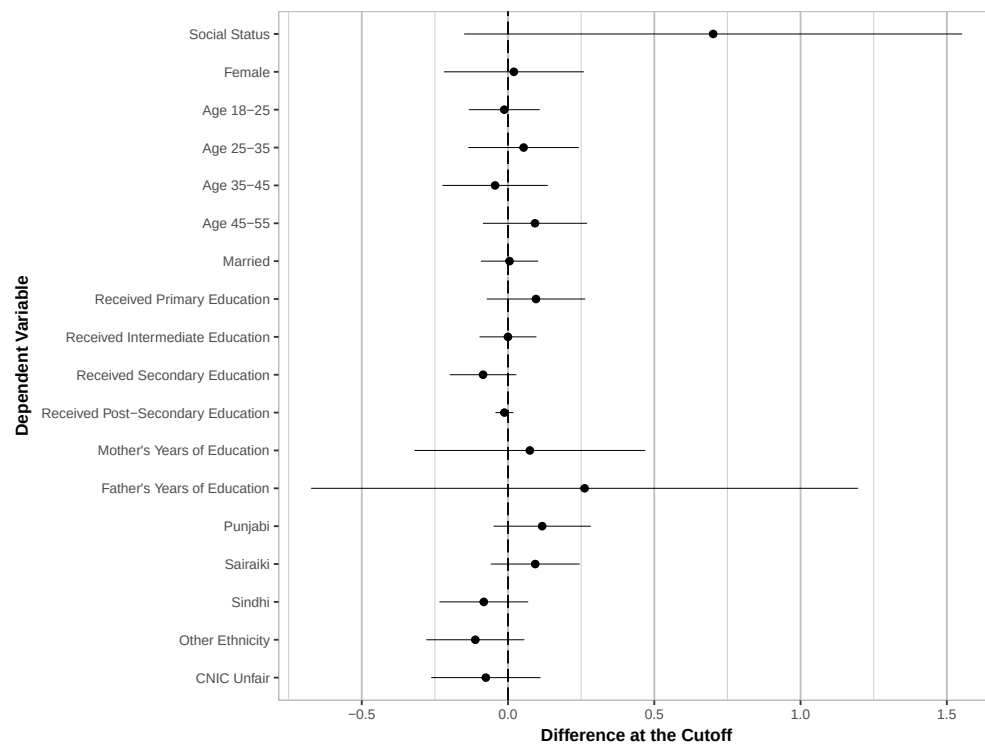
Notes: At the cutoff (recoded as 0), the probability of being a BISP beneficiary increases by 59.4 percentage points ($p < 0.001$).

Figure A.2: McCrary Density Plot

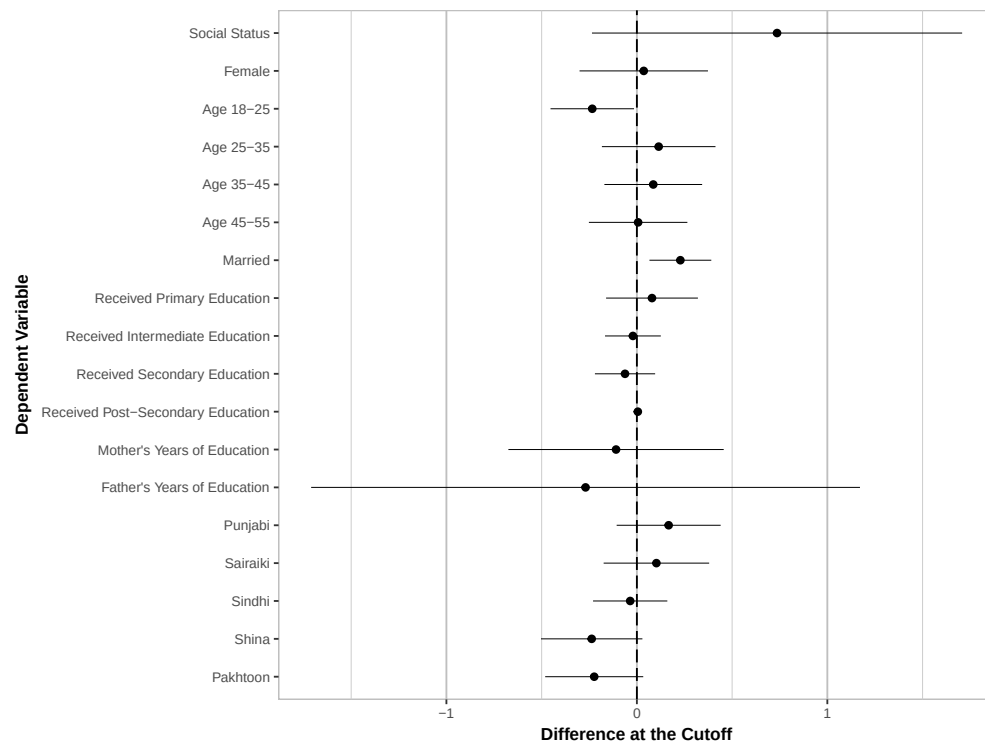


Notes: The figure is a density plot of the wealth score with 95 percent (two-tailed) confidence intervals.

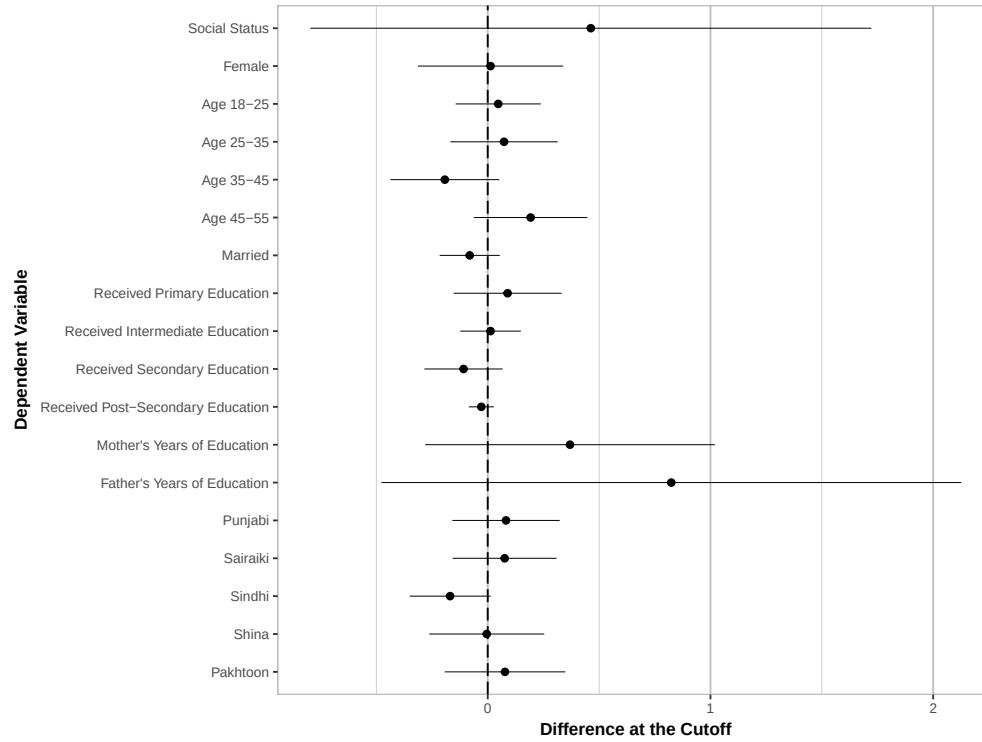
Figure A.3: Two-Stage Least Squares (2SLS) Estimates – Baseline Pre-Treatment Characteristic



(a) *Full Sample*



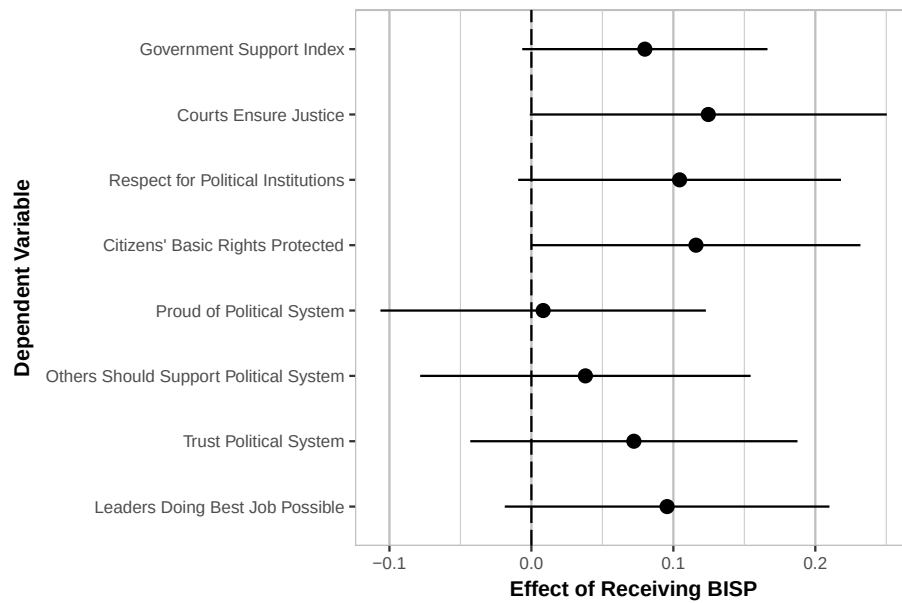
(b) *Sample that Received No Prime*



(c) Sample that Received Relative Deprivation Prime

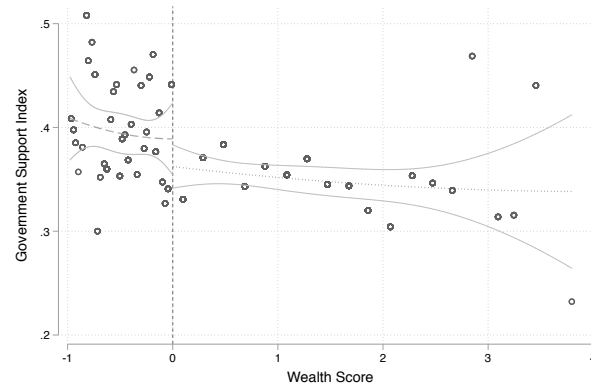
Notes: 95 percent confidence intervals surround local-polynomial RD treatment effect point estimates.

Figure A.4: Two-Stage Least Squares (2SLS) Estimates – Attitudes Toward Government

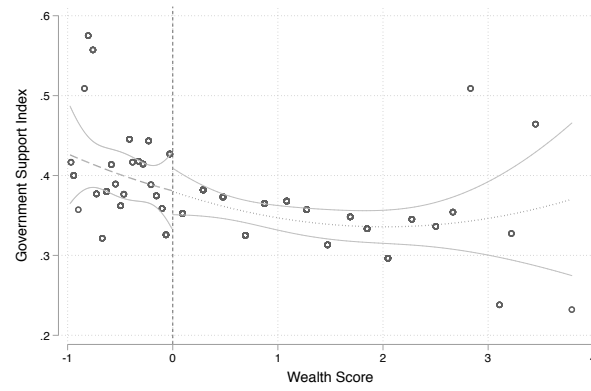


Notes: 95 percent confidence intervals surround local-polynomial RD treatment effect point estimates.

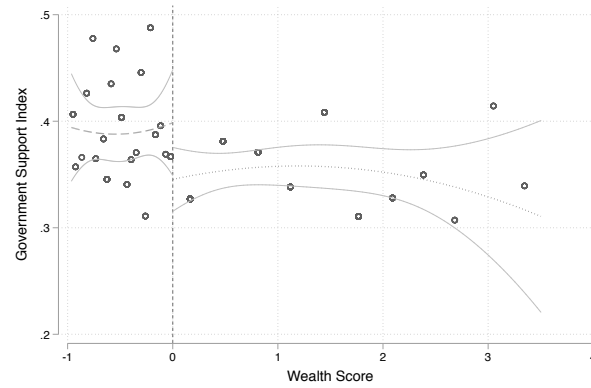
Figure A.5: Government Support Index by Wealth Score



(a) *Full Sample*



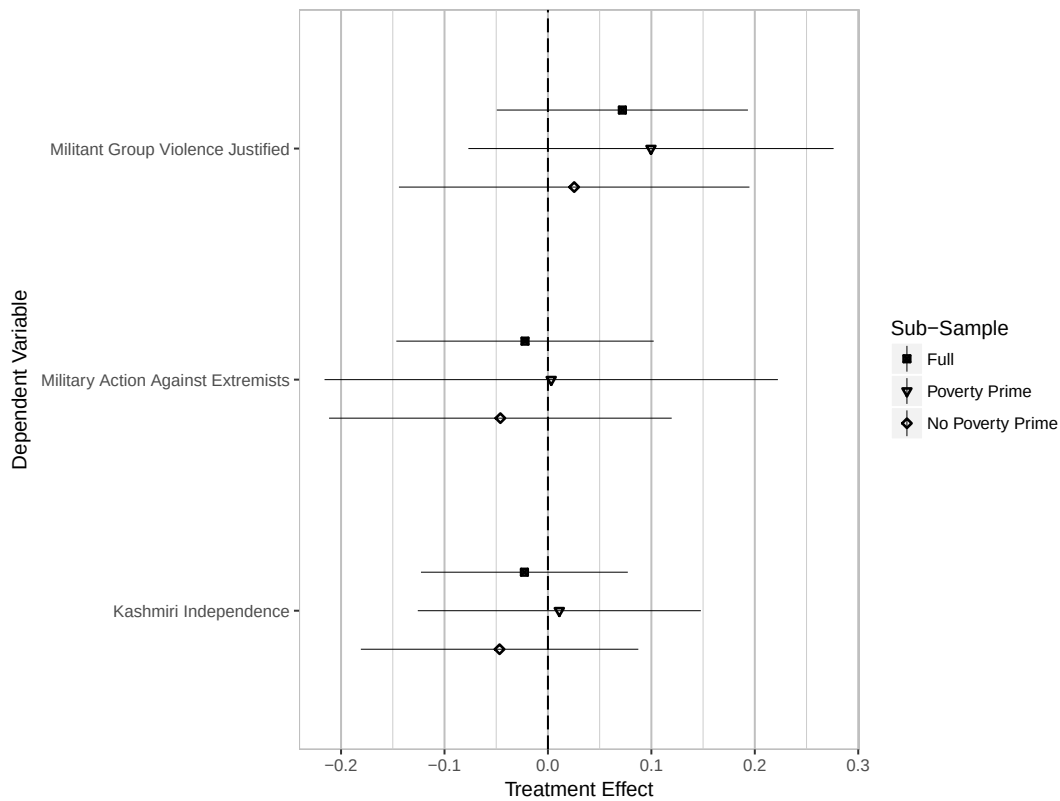
(b) *Sample that Received No Prime*



(c) *Sample that Received Relative Deprivation Prime*

Notes: 95 percent confidence intervals surround the fitted line of the regression discontinuity plot.

Figure A.6: Placebo Test



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)).

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