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

Katrina Kosec† Cecilia Hyunjung Mo‡

November 16, 2022

**Abstract**

Could perceived relative economic standing affect citizens' support for political leaders and institutions? We explore this question by examining Pakistan's national unconditional cash transfer program, the Benazir Income Support Program (BISP). 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 citizens do not feel relatively deprived, receiving cash transfers has little sustained effect on individuals' reported level of support for their political system and its leaders. However, when citizens feel relatively worse off, those receiving cash transfers become more politically satisfied, while those denied transfers become more politically disgruntled. Moreover, the magnitude of the reduction in political support among non-beneficiaries is larger than the magnitude of the increase in political support among beneficiaries. This has important implications for our understanding of the political ramifications of rising perceived inequality.

Word Count: 9,997

*We gratefully acknowledge insightful comments from Kate Ambler, Alan de Brauw, Jonathan Bendor, Matias Cattaneo, Yashodhan Ghorpade, Melissa Hidrobo, Rehan Jamil, Filip Jolevski, Seema Jayachandran, Saumitra Jha, Gabriel Lenz, Neil Malhotra, Brandon Marshall, Hope Michelson, Edward Miguel, Shalini Roy, Sudhir Singh, Rocio Titiunik, Kate Vyborny, and Romain Wacziarg. We also appreciate the helpful comments we received from participants of the Annual Meetings of APSA, EPSA, and MPSA; the Annual World Bank Conference on Development Economics; Korea University's Economics Speaker Series; the Stanford Political Economy Speaker Series; UC Berkeley's Center for Study of Law and Society Speaker Series; UC Berkeley's Institute of International Studies Speaker Series; and USC's Center for International Studies Working Paper Speaker Series. We also thank Lucia Carrillo, Hamza Haider, Brian Holtemeyer, Huma Khan, Bhumi Purohit, Jie Song, and Joseph Stigall for excellent research assistance. All remaining errors are our own. This research was part funded by the CGIAR Research Program on Policies, Institutions, and Markets led by IFPRI and the U.S. Agency for International Development (USAID).

†Katrina Kosec: Senior Research Fellow, International Food Policy Research Institute, Development Strategy and Governance Division, 1201 I Street, NW, Washington, D.C. 20005 (k.kosec@cgiar.org).

‡Cecilia Hyunjung Mo: Judith E. Gruber Associate Professor of Political Science and Public Policy, University of California, Berkeley, 210 Barrows Hall #740, Berkeley, CA 94720 (cecilia.h.mo@berkeley.edu).

Over the last three decades, income inequality *within* countries has risen (Ravallion 2014), and the global COVID-19 pandemic has likely created new inequalities and exacerbated preexisting income gaps within countries (Ferreira 2021). This economic trend stands to powerfully affect states given the key roles that inequality (e.g., Solt 2008) and perceived inequality (e.g., 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 redistributive 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 perceptions of inequality and where one fits in the income distribution moderates the relationship between social protection and political support.

Classic economic voting theory focuses on absolute rather than relative welfare, holding 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 in shaping political attitudes (Kahneman and Tversky 1979; Bendor 2016). 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 satisfaction and support is mixed. A number of studies demonstrate that receipt of targeted social protection programs increases support for policymakers delivering the program (e.g., Manacorda, Miguel, and Vigorito 2011; Chen 2013; De La O 2013). In contrast, Green (2006), Imai, King, and Velasco Rivera (2020), and Corrêa and Cheibub (2016) find that targeted government welfare programs do not always translate into political support. Ellis and Faricy (2011) show that U.S. public opinion is unresponsive to federal social welfare spending.

We consider how perceptions of relative deprivation moderate the effects of social protection on individuals' support for political leaders and state institutions 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 political leaders and state institutions. In 2010, the Pakistani government used a proxy means test to identify BISP beneficiaries; this generated a cutoff wealth score which we exploit. We then leverage an original survey priming experiment which subtly manipulated respondents' perceptions of their relative economic position, modeled on several recent studies (Fair et al. 2018; Healy, Kosec, and Mo 2017; Mo 2018; Kosec et al. 2021).

Individuals near the BISP cutoff who were not primed to feel relatively poor expressed similar levels of support for their political institutions and leaders regardless of BISP receipt status. However, when individuals near the cutoff were primed to feel relatively poor, BISP recipients displayed more positive political sentiment than do non-recipients. Effects are largely driven by those whose households did not get the BISP and were poverty-primed expressing significantly lowered support for their political system and leaders (as opposed to large increases in support among beneficiaries). These findings provide insights into how government provision of goods influences political satisfaction, and about the political ramifications of perceived relative economic standing. Moreover, the fact that we find impacts of even a subtle prime reflects the vulnerability of perceptions of one's relative economic position to external stimuli.

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 empirically support the link between inequality and public investment in goods that predominantly benefit the poor (Lupu and Pontusson 2011; Kosec 2014). Recent studies suggest that this focus on actual inequality is incomplete, however, highlighting that one's perception of their own position relative to others also alters citizen appetite for redistribution (Sands 2017; Condon and Wichowsky 2020). Relatedly, other studies have demonstrated that inequality and feelings of relative deprivation impact opposition to status quo political institutions and political violence (Tocqueville 1856; Gurr 1970; Healy, Kosec, and Mo 2017). We advance this literature by showing how is how peoples beliefs regarding their relative economic position in society can affect political responses to redistributive social protection programs. We also contribute to a rich scholarship explaining why citizens selectively reward their political leaders and systems for social protection that has focused on attribution challenges, biases, heuristics, and other psychological underpinnings; how social protection programs are branded and rolled out (see Appendix A.2 for more details).

The paper is organized as follows. We first 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 research exploring the effects of social protection programs on political satisfaction, considering how findings vary with the level of inequality of the study context. We then provide background on Pakistan and the BISP program. Next, we describe our dataset and empirical approach, followed by an explication of our results. We conclude by discussing their implications for future research and policy.

## Conceptual Framework

Social protection programs are branded and rolled out in a variety of ways. As such, it is useful to first provide scope conditions as we consider the role of perceived relative deprivation in people's assessment of such programs. First, we assume that information is not a binding constraint, and individuals are broadly aware of the existence of social protection programs. Second, we assume that the poor widely favor such programs, and do not perceive the program's implementation processes as corrupt; such perceptions may negatively affect judgements about a program (Lind and Tyler 1988). Finally, we assume aid is provided to eligible households soliciting it for as long as they are eligible (and not one-off in nature). We argue these assumptions are typically reasonable. For example, in Pakistan, according to the BISP database in 2013, 92.5 percent of households eligible for the BISP participated, indicating both broad awareness and a navigable process to access transfers. BISP provides quarterly transfers to households that remain eligible. Moreover, BISP is popular, and various political parties have taken steps to associate themselves with it.

We contend that citizens' beliefs regarding their relative economic position in society can affect political responses to social protection programs. States have limited resources, and as such, means-tested government social protection often generates two groups: (1) a beneficiary group that was targeted (potentially raising support for their political leaders and institutions) because they are poor and relatively deprived (an underlying condition that potentially lowers political satisfaction);

and (2) a non-beneficiary group that feels neglected by their political leaders and systems because public resources were insufficient to help them despite helping others (potentially lowering political satisfaction) which shares this underlying condition of feeling poor and relatively deprived.

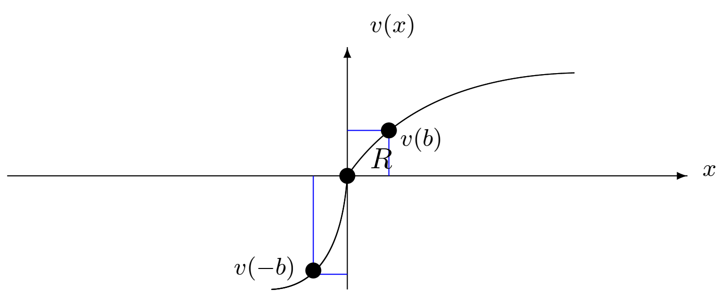
Neoclassical economics assumes that feeling relatively worse off is irrelevant for citizens attitudes. Utility depends on what one has, and if we considered individuals who were similarly poor prior to the program being rolled out, social protection beneficiaries will have a higher income than non-beneficiaries. As such, otherwise comparable beneficiaries will have a higher utility than their non-beneficiary counterparts. Given extant research establishing that political support is increasing in utility (Lewis-Beck and Nadeau 2011), if we follow traditional neoclassical models of economics, we would expect that beneficiaries will be more supportive of their political leaders and institutions than non-beneficiaries, and this relationship will not vary with the salience of relative deprivation.

Instead, if we assume reference points—a basis or standard for evaluation—factor into people's assessments (e.g., Festinger 1954; Adams 1965; Crosby 1976; Genicot and Ray 2020), our predictions change. According to prospect theory, reference points systematically affect people's judgements (Kahneman and Tversky 1979). The value function $v(x)$ of prospect theory integrates the following three principles: (1) a reference point partitions the space of outcomes into regions of gains versus losses; (2) there is loss aversion, where $v(x) < |v(-x)|$, $x > 0$; and (3) the function exhibits diminishing sensitivity, whereby $v"(x) < 0$, $x > 0$ and $v"(x) > 0$, $x < 0$. These three principles lead to an s-shaped value function that is asymmetrical; the value function is steeper for losses than gains (see Figure 1).

What occurs when people feel relatively worse off with regards to income? We posit that their relative economic need will be strongly felt, and they will be more likely to think about how they are or are not being helped by the state. Such a reaction is consistent with empirical evidence showing that exposure to economic disparity increases demand for redistribution among low-income individuals (Sands and de Kadt 2020). Assuming the aforementioned three scope conditions hold, the receipt or non-receipt of social protection assistance from the state will thus be more salient. Beneficiaries may then see that their present economic conditions are *better* than the economic conditions they would experience had they not received government assistance. The reference point ($R$ in Figure 1) for assessing their current utility and how the state affected that utility is their economic condition had they not received aid. Their utility is higher as a result of gaining assistance from government programs that increase their economic well-being ($v(b)$ in Figure 1). However, non-beneficiaries who are poor and feel relatively worse off compared to others may view themselves as having lost out with respect to government aid ($v(-b)$ in Figure 1), as their reference point is their economic condition had they received aid. Being insufficiently poor and ineligible for assistance in spite of their relative need, their present economic conditions are *worse* than the economic situation they would be in had they received government assistance.

In other words, when their relative deprivation is salient, beneficiaries may be more likely to think about the gain in utility stemming from aid as a result of state intervention. However, non-beneficiaries may be more likely to think about the loss in utility stemming from not having the aid that they think they should have, leading to different political attitudes between beneficiaries and non-beneficiaries. Moreover, given loss aversion ($v(b) < |v(-b)|$, $b > 0$), the difference in political satisfaction between beneficiaries and non-beneficiaries will largely be driven by non-beneficiaries becoming more politically disgruntled than beneficiaries becoming more politically satisfied.

Figure 1: Value Function According to Prospect Theory




When one does not feel relatively worse off, their relative need will *not* be salient and the need for redistribution will *not* be salient. Hence, they will be less likely to think about receipt or non-receipt of social protection. Consequently, beneficiaries' and non-beneficiaries' will not be considering different reference points, and their utility will be based upon their status quo condition as in the standard neoclassical economic model.

To better contextualize our theoretical contribution, we carried out a systematic review of empirical studies on the effects of cash transfers or social protection on support for diffuse or particularistic political support in high-impact journals in the last two decades, summarized in Appendix

Table A.1 and discussed in detail in Appendix A.1. We identified 26 studies using a defined set of search parameters, and find that they are concentrated in unequal countries (above median Gini). If actual inequality increases perceived inequality, our conceptual framework would suggest that we would *overestimate* the benefits of social protection on political support by consulting the studies in this review. Moreover, our conceptualization suggests that it would be problematic to interpret some of the observed positive effects noted in the literature review as evidence of social protection programs raising political satisfaction. If the estimated effect of the program on non-beneficiaries is not carefully considered and non-beneficiaries are simply treated as the "control" group, estimated treatment effects may be misinterpreted, as the positive difference between beneficiaries and non-beneficiaries may be driven by disgruntled non-beneficiaries.

## The Case of Pakistan

Pakistan has 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.

Like many developing countries, Pakistan relies on government social protection to reduce poverty and inequality. 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 recently-assassinated leader. In 2013, while the name of the program did not change, the BISP was re-branded to include a picture of the leader of the governing party in 2013, the Pakistan Muslim League-Nawaz (PML-N), and during the 2018 elections, Pakistan's three largest parties all pledged to enlarge the BISP. As a result, citizens do not associate the BISP with only the ruling political party. When 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. Today, the BISP is one of the largest unconditional cash transfer programs in the world.

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 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).[1] Within a year, the federal government began reforming the system to base beneficiary status on a family's wealth score, computed using a proxy means test (PMT) (Gazdar 2011). This revised, technocratic method of beneficiary selection was credited by third-party evaluators as being objective, and was used to distribute transfers starting in July 2011 (Cheema et al. 2014). 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, which are detailed in Appendix B. The average wealth score among respondents in our dataset is 22.8, and 35.4% of households are BISP recipients (see Table 1).[2] While there are several welfare programs in Pakistan besides the BISP that are targeted based on poverty, they were either rolled out after our study period or used a different eligibility cutoff point.[3]

The BISP delivers cash transfers to each ever-married woman in eligible families with a valid Computerized National Identity Card (CNIC)—a prerequisite for accessing public services (Gazdar 2011).[4] Throughout the period of our study, BISP beneficiaries received quarterly payments of PKR 3,000 (approximately $35.00 USD in early July 2011 (International Monetary Fund 2013))—equivalent to about 8.9% of average quarterly consumption expenditure per adult equivalent (Cheema et al. 2014).[5] Beneficiaries received payments either through the Pakistan Post or ATM cards (Ambler and De Brauw 2019).

---

[1] Details on BISP implementation can be found in Appendix B.

[2] The national average share is 23%, pointing to the deeper poverty of our rural sample.

[3] Appendix B discusses other welfare programs individually, explaining why they do not threaten our identification strategy.

[4] CNIC 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% of respondents reported no such barriers.

[5] BISP cash transfer amount is adjusted annually for inflation.

At the time of our survey, BISP beneficiaries had been receiving quarterly payments for between 1.75 and 4.5 years (depending on whether they were targeted in the first wave of the program starting in 2008, or not until 2011). As all of the BISP beneficiaries in our sample received the BISP for at least 1.75 years, we interpret our impacts as medium- to long-term ones. We lack data on when each household specifically began receiving transfers, and are thus unable to analyze impacts by the length of time one has received transfers. However, Evans, Holtemeyer, and Kosec (2019) show that the impacts of cash transfers on trust in government after 1.75 years are statistically indistinguishable from those after 2.75 years, suggesting that whether those in our sample received BISP for 1.75 years or for longer may not be a relevant moderator of the impacts of the BISP. Given governments' and international donors' frequent interest in institutionalizing social protection programs and retaining them for long periods of time, considering medium- to long-term impacts may be most useful from a policy perspective.

## 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 (described in detail in Appendix C). 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.[6] We combine this database with our survey, the Pakistan Rural Household Panel Survey (RHPS). Round 1 covered 2,090 households in 76 villages in Punjab, Sindh, and Khyber-Pakhtunkhwa provinces; 2,002 of these were re-interviewed in round 2. Households lacking a valid CNIC number in the BISP database were omitted from our analysis. Reassuringly, survey respondents that are missing a CNIC number or provided an invalid CNIC number are not systematically more disadvantaged

[6] Appendix C describes construction of this outcome in detail.

or advantaged than those who were in the BISP administrative database.[7]

The governance module was fielded 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).[8] Exact question wordings and response options are in Appendix D. They 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 offered five response options, re-coded to range from 0 (Not at all) to 1 (A great deal).[9] A potential concern is that our survey elicited social desirability bias, with respondents feeling compelled to respond favorably to government. If this were the case, responses would be substantially noisier and less reliable. Fortunately, this does not appear to be the case: a) For six of the seven questions, more citizens provide one of the lower two response options than one of the two highest response options; and b) For five of the seven questions, at least half of our respondents chose one of the bottom two response options (as opposed to the middle or the two highest).[10]

System support can be conceptualized as both support for a specific government regime, or as more diffuse attitudes towards the political system (Easton 1967; Lipset 1981). Following the Eastonian distinction, we construct a measure capturing the latter, as recipients of social protection programs often do not know what level of government deserves credit (e.g., León 2011; Gulzar and Pasquale 2017) and the BISP is not associated with a specific leader or party. In terms of the component measures of our index, five of the seven measures we employ are part of a cross-nationally validated system support index (Muller, Jukam, and Seligson 1982; Seligson 1983;

[7] The two groups are similar with respect to gender ($p = 0.893$), education level ($p = 0.788$), maternal education ($p = 0.274$), and being non-punjabi, non-sairaiki, and non-sindhi ($p = 0.238$). Nevertheless, there are a few differences. Those that matched with the BISP database perceive themselves as having lower social status than those who did not ($p < 0.01$) and report lower paternal education levels ($p = 0.023$). Those with matched or non-missing CNIC numbers are less likely to be punjabi ($p < 0.01$), and more likely to be sairaiki ($p < 0.01$) and sindhi ($p < 0.01$).

[8] Assignment of the experimental survey prime does not predict missingness of a valid CNIC number.

[9] We did not ask about specific political parties in our survey to avoid security risks for our enumerator team. Thus, we are unable to separate effects of BISP on attitudes toward government in general, as opposed to the ruling party. This is worth examining in future research.

[10] There is little evidence of social desirability bias among the four sub-groups: those who were poverty-primed, not poverty-primed, received the BISP, and did not receive the BISP. Statement (a) holds for all four groups, and statement (b) holds for all but BISP recipients, where at least 47% of respondents chose one of the bottom two options.

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 |
| 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 |
| Cash Loans Outstanding as Share of Yearly Expenditure | 2,639 | 0.114 | 0.402 | 0 | 9.032 |
| **Panel B: Attitudes Toward Government** | | | | | |
| Political 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 RHPS, Round 1 (2012) and Round 2 (2013), and BISP Database (2013).

Finkel, Muller, and Seligson 1989), which is predictive of important outcomes like political stability. According to Seligson and Carrion (2002), these five measures capture a single underlying attitude as confirmed by a factor analysis. In addition to these five measures, we asked about trust in the political system and views of whether political leaders in Pakistan are doing the best job possible for Pakistanis. These capture, respectively, individuals' "affective orientation toward the government" (Miller 1974, p.952) and specific perceptions of the individuals that work within political institutions. Our *political support index*, which is our principle outcome measure, averages all seven measures. Using an index avoids concerns with multiple hypothesis testing and nets out measurement error associated with any one of the index components (Ansolabehere, Rodden, and Snyder 2008). Our political support index and its seven component measures are summarized in Table 1, Panel B. It has a mean of 0.37, indicating average diffuse political support somewhere between "a little" and "somewhat," and its standard deviation is 0.19.

To assess whether the BISP had its intended welfare effects, we considered several outcomes (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 Appendix E for exact question wordings). Descriptive statistics of these measures appear in Table 1, Panel A. 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 the average individual's household is able to save the equivalent of 14.3% of their monthly expenditure. Additionally, 16% of respondents live in households that earn income from outside of the agricultural sector, and outstanding loans—if any—equal roughly 11.4% of annual expenditures.

### Regression Discontinuity Design

To estimate the causal effect of the BISP, we employ a quasi-experimental procedure exploiting the fact that the BISP relied on a wealth score threshold (PMT score of 16.17) with a few exceptions 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 with those that were marginally eligible, to evaluate the program's effects. See 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*, 2014b) and Calonico, Cattaneo, Farrell, and Titiunik (2017), we select an optimal bandwidth that minimizes the mean squared error (MSE).[11] 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. We also assess whether any of our results are sensitive to the presence or absence of clustering at the household level.[12]

This empirical strategy falls apart if the wealth score cutoff is not an appropriate instrument

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

[12] As shown in Table A.5, this choice makes no difference.

for receiving BISP aid. Reassuringly, this assumption is quite robust. We find that having a wealth score under 16.17 is an appropriate instrument for receiving BISP aid. We observe a 59 percentage point ($p < 0.001$) discrete jump in the BISP beneficiary rate at the cutoff, which we recode as 0 (see Figure A.1 in Appendix G, as well as Table 2, column (1). Further, to the left of the cutoff (i.e., among those eligible for the BISP), take-up was 96%. And to the right of the cutoff, take-up was only 5%. Reassuringly, all of those taking up the program with scores above the 16.17 cutoff further have a score strictly under 21.17, reflecting a rule that those with scores between 16.17 and 21.17 could still receive the BISP provided that their household met at least one of the following conditions: 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 (see Appendix B for more detail).[13]

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% significance level at the cutoff (see Figure A.3a (a) and Table A.2 in 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 if there 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. 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 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 ($p = 0.234$).

[13]Our data were collected after program roll-out, and the extent to which households met conditions (1)–(3) may have changed in the interim. However, it is encouraging that 63% of those we code as beneficiaries despite being to the right of the cutoff met at least one of criteria (1) – (3) at the time of our interview.

### 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 bracket choices such that half of respondents received answer choices for which they were likely to be in the bottom bracket and feel that they have less than what is typical and the other half received answer choices for which no particular response option was most likely.[14] 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:[15]

| 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. |

Figure 2 shows that, for those in the group we label as receiving the "relative poverty prime," 73% chose the first (lowest) income bracket response option (0–12,500 Rs. per month).[16] Hereafter, we refer to this group as poverty-primed. In contrast, among those in the group we label as receiving "no poverty prime" (hereafter, referred to as non-poverty-primed) there is a fairly uniform distribution of income bracket choices (i.e., at least 15%, and not more than 30%, choosing any given bracket). At the mean and the median, this group chose the middle income bracket.

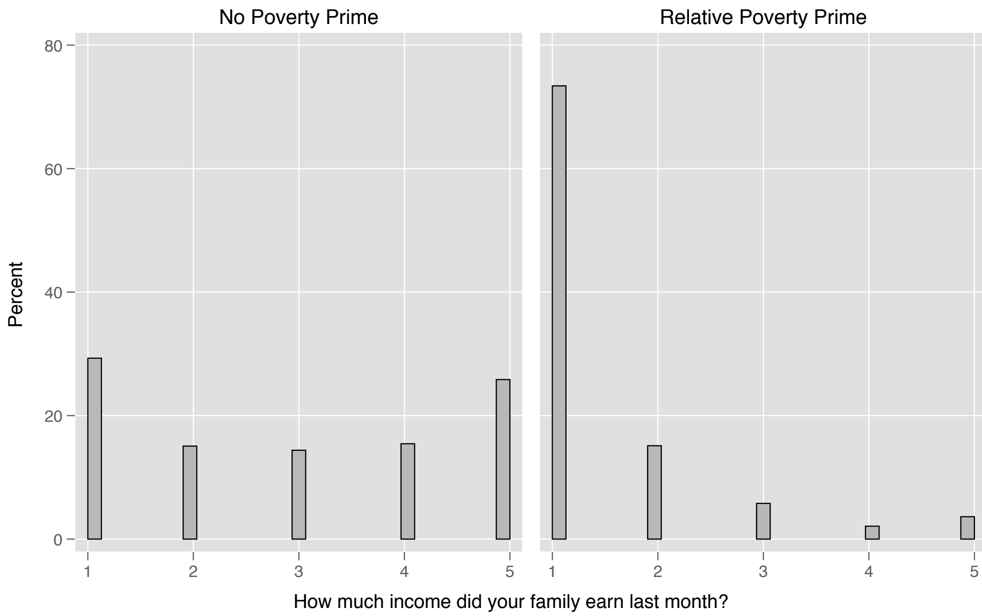
This experiment leverages the fact that individuals generally have uncertainty about the income distribution (Gimpelson and Treisman 2018), and as such, perceptions of inequality are malleable.

[14]To avoid spillovers, both respondents within a household received the same prime, and they were further interviewed separately and simultaneously (where possible).

[15]The 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).

[16]In 2013, the poverty line in Pakistan was 3,030 Rs. per adult equivalent per month; as the average household in our sample for analysis has 6.071 adult equivalents, this is a household monthly income of 18,395 Rs. (Ministry of Finance of Government of Pakistan 2013). Mean (median) monthly household income in our sample is 16,141 Rs. (14,755 Rs.); total monthly expenditures are below the poverty line for 62% of sample households.

Figure 2: Distribution of Income Bracket Choices by Relative Poverty Prime




The poverty prime is similar to one used by Haisley, Mostafa, and Loewenstein (2008) to study participation 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 et al. (2021) recently used it in Papua New Guinea. It is motivated by 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. Of note, our prime leverages a question that is commonplace in household surveys and censuses—a household income question that provides respondents with multiple income brackets from which to select. Thus, we do not anticipate impacting respondents any more than do other surveys asking questions about income.

Table A.3 in Appendix G shows that random assignment of this relative poverty prime was successful. Exactly 50% of the sample received the poverty prime, and there is balance on social status (i.e., where, on an 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.[17] Further, total household income is almost identical across the primed and non-primed groups ($p = 0.72$).[18]

## Results

Our results proceed in several parts. First, we assess how the BISP impacted citizens economically, to establish at least one basis for why the program might influence political attitudes. Next, we separately consider how the BISP influenced political attitudes among those who received our poverty prime and those who did not. Finally, we establish the robustness of our estimates.

### Effects of the BISP on economic and political outcomes

We find evidence of BISP increasing household expenditure and the propensity to engage in wealth-enhancing economic behaviors (see Table 2, Panel A). Receipt of the BISP increases total household expenditures ($p = 0.02$; $p_{robust} = 0.01$), in large part by increasing total food expenditures ($p < 0.01$; $p_{robust} < 0.01$).[19] 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 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 political satisfaction. We first consider our political support index. Figure A.4 (a) in Appendix G displays the average political support index value by wealth score for households near the cutoff. It depicts an increase in political support precisely after crossing the cutoff wealth score and thus

[17] Fuzzy RDD analyses for pre-treatment demographic characteristics and an indicator for perceiving barriers to acquiring a CNIC card show no differences at the cutoff for both the primed and non-primed groups that are significant at the 5% level (see Figure A.3a (b) and (c), respectively, in Appendix G).

[18] While our survey did not include a manipulation check question, when the same priming experiment was administered in Nepal (Boittin et al. 2020), the authors identified that it had its intended effects on perceptions of relative welfare.

[19] Throughout, we report conventional p-values based upon implementing a fuzzy RDD estimation strategy and robust p-values based upon the standard error adjustment recommended by Calonico, Cattaneo, and Titiunik (2014*b*).

Table 2: Effect of the 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 |
| 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 |
| Cash Loans Outstanding as Share of Yearly Expenditure | -0.619*** | -0.109*** | 0.001 | [-0.218, -0.059] | 2,639 | 351 | 429 |
| **Panel B: Attitudes Toward Government** | | | | | | | |
| Political 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 RHPS, Round 1 (2012) and Round 2 (2013), and BISP Database (2013)
*Notes:* In the first two columns, * p<0.10, **p<0.05, *** p<0.01. The estimate is the average treatment effect (ATE) at the cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik 2014*a*). 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. Panel A variables and Panel B variables were drawn from Round 1 and Round 2 of the RHPS, respectively.

becoming eligible for transfers. Indeed, when we estimate the effect of the BISP employing a fuzzy RDD estimation strategy, we find that household receipt of the BISP leads to an 8 percentage point increase in an individual's political support index value (see Table 2, Panel B, column (2), which is visually depicted in Figure A.5 in Appendix G); however, this effect misses the standard threshold of statistical significance ($p = 0.07$; $p_{robust} = 0.13$). When assessing each of the seven measures that comprise the index, we see that BISP receipt corresponds with a more positive appraisal of political leaders and institutions. However, only two (three) of the seven measures are statistically significant at the 10% level when employing bias-corrected (conventional) standard errors. Thus, we cannot confidently reject the null hypothesis of no overall effects of the BISP on political 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 political support index values near the wealth score cutoff for those primed versus not primed to feel relatively poor provides suggestive evidence that this is the case. Considering only the poverty-primed, Figure A.4(c) clearly shows a decline in political support as one crosses the wealth cutoff and their household becomes ineligible for the BISP. However, no differences are visually apparent among the non-poverty-primed group (see Figure A.4(b)).

In Table 3, we show our fuzzy RDD estimates of the effects of the BISP on two 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 group are depicted in Figure 3. Among the respondents randomly assigned to receive our relative poverty prime, the effect of their household receiving the BISP on their political support index value is nearly twice as large in magnitude than for the full sample (see Table 2), and this effect is statistically meaningful. We can further test whether the coefficient estimate on receipt of the BISP for the poverty-primed group is statistically significantly greater than the estimate for the non-poverty-primed group (a one-tailed test).[20] Considering our political support index, we can statistically reject the null hypothesis that the effect for the poverty-primed group is less than or equal to the effect for non-poverty-primed group ($p = 0.10$).

The BISP leads to a 16 percentage point increase in the political support index; this is equivalent to a 43% increase over the mean level of support ($p = 0.02$; $p_{robust} = 0.02$). 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 Table 3, column (2) (column (1)).[21] 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 (Ansolabehere, Rodden, and Snyder 2008). For the non-poverty-primed group, BISP recipients consistently have the same levels of political support as non-recipients; standard errors are substantially larger than they are for the poverty-

[20] As a simple interactive analysis is not feasible in an RDD setting, we use the formula $Z = |\tau_1 - \tau_2| / \sqrt{SE_1^2 + SE_2^2}$, where $\tau_1$ ($\tau_2$) is the bias-corrected local-polynomial RD estimate for the poverty-primed (non-poverty-primed) group and $SE_1$ ($SE_2$) is the robust standard error of the local-polynomial RD estimator for the poverty-primed (non-poverty-primed) group.

[21] Our findings hold when correcting for multiple hypothesis testing by controlling the false discovery rate (following either Benjamini and Hochberg (1995) or Benjamini, Krieger, and Yekutieli (2006)). All coefficients in Table 3 that were statistically significant at the 10% level or higher remain so.

primed group, and coefficients are always smaller—and in three cases negative.[22] Thus, higher political support due to BISP receipt is only apparent among those who are poverty-primed.

Table 3: Effect of the BISP on Political Support by Relative Poverty Prime (2SLS)

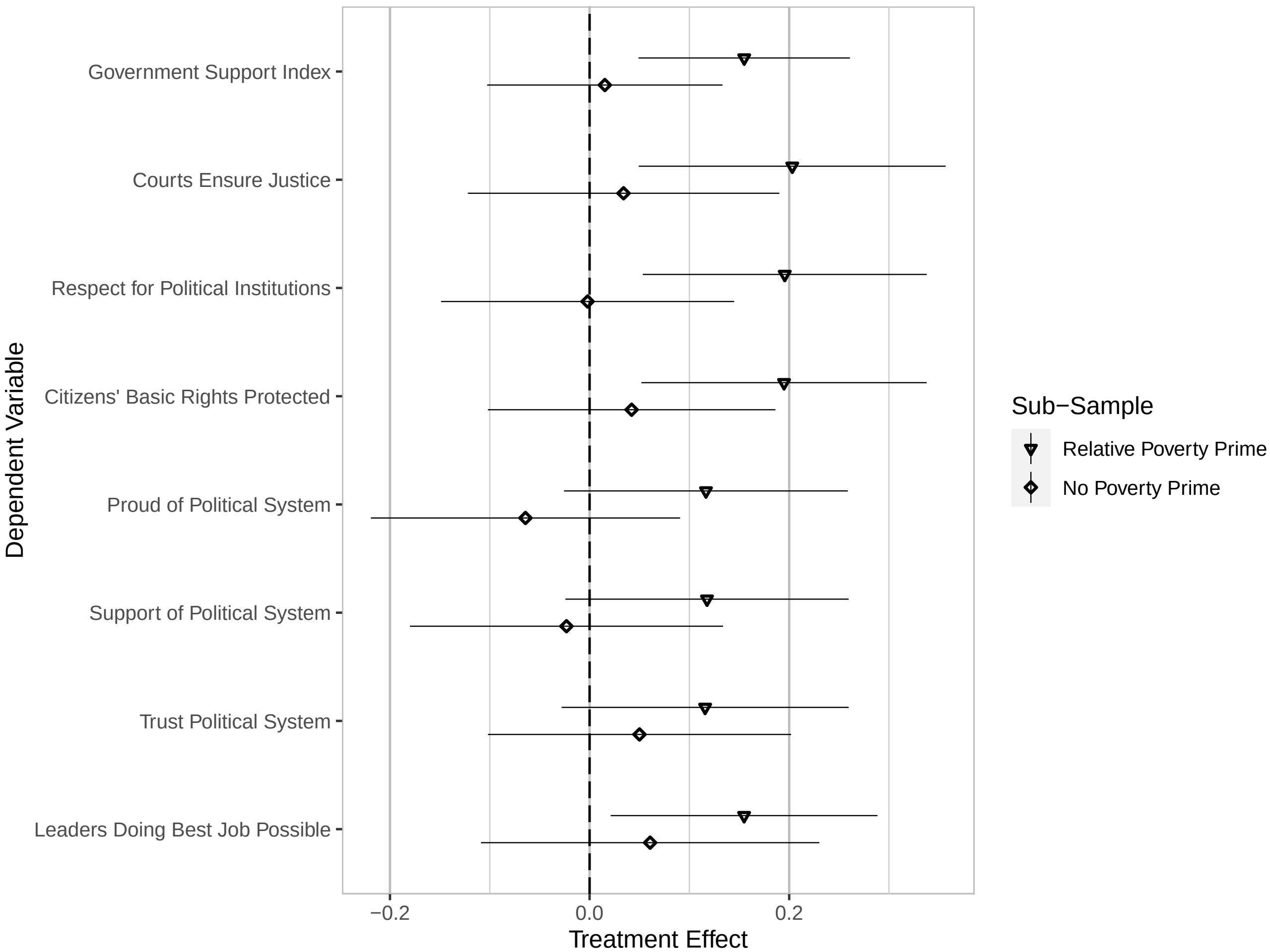
| Variable | (1) Estimate | (2) Robust P-Value | (3) Robust 95% CI | (4) N | (5) $N_{tr}$ | (6) $N_{co}$ |
|---|---|---|---|---|---|---|
| **Panel A: Effect of the BISP Among Those Who Received the Relative Poverty Prime** | | | | | | |
| Political Support Index | 0.155** | 0.021 | [0.026, 0.317] | 1,303 | 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 the BISP Among Those Who Did Not Receive the Relative Poverty Prime** | | | | | | |
| Political 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 RHPS, Round 2 (2013) and BISP Database (2013)
*Notes:* In the "Estimate" columns, * p<0.10, **p<0.05, *** p<0.01. The estimate is the ATE at the cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik 2014*a*). The robust p-value, 95% robust confidence intervals (CI), sample size ($N$), and the number of treated ($N_t r$) and control ($N_c$) observations within the optimal bandwidth are also reported. The running variable is the wealth score.

Is the positive effect of the BISP among the poverty-primed due to BISP recipients being relatively more appreciative of BISP assistance when they feel relatively worse off? Or is it an artifact of those from households that did not receive the BISP feeling relatively more disgruntled about the lack of government transfers when they feel relatively poor? When we examine the reduced form or "intent-to-treat" (ITT) local polynomial estimates to both sides of the cutoff for those who are and are not primed to feel relatively poor, we observe two separable shifts (see Table 4, columns (3) and (6)): (a) the relative poverty prime leads to a modest increase in political support among beneficiaries (for six of the eight measures, including the political support index);

[22]This pattern is not particularly sensitive to bandwidth selection (see Table A.4 in Appendix G), though power is reduced when we halve the optimal bandwidth.

Figure 3: Effect of the BISP on Political Support by Relative Poverty Prime (2SLS)




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

and (b) the relative poverty prime leads to a notable decrease in political support among those who did *not* receive cash transfers (for seven of the eight measures). Overall, for six of the eight measures, the observed decline (b) is of larger magnitude than the observed increase (a).

To consider magnitudes of our effects, we focus on the ITT estimates of the individual-level political support index at the cutoff (see Table 4, row (1), which is visualized in Figure A.4(b)-(c)). Among those just to the left of the cutoff, 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 political support due to being primed to feel relatively poor. This suggests that feeling relatively worse off is not unambiguously bad for engendering political support among social protection recipients. In contrast, among those just to

Table 4: Moderating Effect of Relative Poverty Prime

| | Estimates to Left of Cutoff (Eligible for BISP) | | | Estimates to Right of Cutoff (Not Eligible for BISP) | | |
|---|---|---|---|---|---|---|
| Variable | (1) $\mu_{primed}$ | (2) $\mu_{notprimed}$ | (3) Difference | (4) $\mu_{primed}$ | (5) $\mu_{notprimed}$ | (6) Difference |
| Political 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 RHPS, Round 2 (2013) and BISP Database (2013)
*Notes:* The difference ($\mu_{primed} - \mu_{notprimed}$) is computed by subtracting the local polynomial estimate for the subgroup that did *not* received the relative poverty prime ($\mu_{notprimed}$) from the estimate for the subgroup that received the relative poverty prime ($\mu_{primed}$).

the right of the cutoff, whose household barely missed receiving the BISP, we observe a sizable, 6 percentage point drop in political support due to receiving the relative poverty prime (0.3 vs. 0.36). Thus, feeling relatively poor, absent aid, can foment political dissatisfaction. Notably, the size of the drop in political support we observe among non-beneficiaries is 3.5 times larger than the magnitude of the increase in political support we see among beneficiaries. This difference in magnitudes is consistent with loss aversion, a key principle of prospect theory; the pain of not receiving social protection is psychologically more powerful than the pleasure of gaining social protection (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 politically disgruntled when they feel relatively poor rather than beneficiaries becoming more satisfied when feeling relatively poor. Absent perceptions of relative deprivation, receiving BISP aid has few effects on political 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.[23]

[23]While BISP aid is delivered to ever-married woman, female aid recipients are likely to share aid within the household. As such, there is no theoretical reason to believe that the effects are concentrated among women. Indeed, we see that there are no differences in results by gender (see Table A.6).

### Robustness

Our first robustness check examines whether the effects of the BISP among the poverty-primed are concentrated among those who did not feel relatively poor pre-treatment. Intuitively, if our relative poverty prime worked as intended, these individuals should be most susceptible to the relative poverty prime while those who *already* felt relatively poor should be (relatively) immune to its effects. We leverage a pre-experiment question about the individual's perceived economic standing relative to others: "[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. We thus divided our sample into two groups: those less than 5 (should be unaffected) and those at 5 or higher.[24]

As shown in Table 5 (and visualized in Figure A.6 in Appendix G), the observed effects among the poverty-primed are 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 political support index is larger in magnitude for this group; we estimate a 24 percentage point increase, which is equivalent to a 70% increase over the mean level of political support. We can further reject the null hypothesis that the effect for the poverty-primed who did *not* feel relatively poor pre-treatment is less than or equal to the effect for the poverty-primed who felt relatively poor pre-treatment ($p = 0.01$, one-tailed test).

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: whether violence to protect religious values is justified, whether military action against extremist groups is helpful, and whether Kashmiri independence is important. Indeed, neither the BISP nor the poverty prime affect any of these measures (see Figure A.7 in Appendix G).[25]

[24]This question was asked several modules before our survey experiment, making it unlikely that it affected our outcome measures.

[25]Exact question wordings are provided in the notes that accompany the figure.

Table 5: Effect of the BISP on Political Support by Perceived Relative Income Pre-Treatment Among Individuals Primed to Feel Relatively Poor (2SLS)

| Variable | (1) Estimate | (2) Robust P-Value | (3) Robust 95% CI | (4) N | (5) $N_{tr}$ | (6) $N_{co}$ |
|---|---|---|---|---|---|---|
| **Panel A: Effect of the BISP Among Those Who Did Not Feel Relatively Poor Pre-Treatment** | | | | | | |
| Political 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 the BISP Among Those Who Felt Relatively Poor Pre-Treatment** | | | | | | |
| Political 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 RHPS, Round 2 (2013) and BISP Database (2013)

*Notes:* In the "Estimate" columns, * p<0.10, **p<0.05, *** p<0.01. The estimate is the ATE at the cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik 2014*a*). 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.

# Conclusion

Our work seeks to advance a growing body of literature on the selective manner in which individuals reward government. Extant public administration research has shown that there is often a disconnect between government performance and public trust (e.g., Van Ryzin 2011). Previous literature has focused on habituation, attribution challenges, issues of political manipulation and capture, and recency bias to explain mixed effects. We posit and test the influence of another potential moderator: citizens' 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, considering the extent to which perceptions of one's relative economic standing affect the political consequences of the program.

We demonstrate that when feelings of relative poverty is not salient, cash transfers have little effect on citizens' political support one to four years after the household transfers were first initiated. But, when it is salient, beneficiaries have higher political support than do non-beneficiaries. Individuals' perceptions of their relative deprivation are thus an important moderator of program effects on political support. Feeling 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 appreciation for or the perceived value of the social protection. Moreover, we demonstrate that it is problematic to view non-recipients of aid as a "control" group against which to compare recipients when assessing program effects; even if programs are randomly assigned, non-recipients are affected by the *non-receipt* of aid, especially when they feel economically insecure. Overall, our research illustrates both the power of beliefs to change political perceptions, as well as the power and limitations of government to mold and shape those beliefs.

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 wealth distribution. Additional research should also examine how effects differ depending on whether the cash transfer program imposes conditions. Moreover, additional research is needed to explore the external validity of our findings. With that said, the theoretical underpinnings of the phenomena we observe is not unique to Pakistan, and we expect to observe similar effects elsewhere.

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, could limit its external validity. 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. 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.

## References


Adams, J. Stacey. 1965. "Inequity in Social Exchange." In *Advances in Experimental Social Psychology*, eds. L. Berkowitz. Vol. 2 New York, NY: Academic Press.

Ambler, Kate, and Alan De Brauw. 2019. The Impacts of Cash Transfers on Women's Empowerment: Learning from Pakistan's BISP Program. Technical report for the World Bank.

Ansolabehere, Stephen, Jonathan Rodden, and James M. Snyder. 2008. "The Strength of Issues: Using Multiple Measures to Gauge Preference Stability, Ideological Constraint, and Issue Voting." *American Political Science Review* 102 (2): 215–232.

Bendor, Jonathan. 2016. "Aspiration-Based Models of Politics." In *Political Psychology: New Explorations.* New York, NY: Taylor & Francis Group.

Benjamini, Yoav, Abba M. Krieger, and Daniel Yekutieli. 2006. "Adaptive Linear Step-Up Procedures that Control the False Discovery Rate." *Biometrika* 93 (3): 491–507.

Benjamini, Yoav, and Yosef Hochberg. 1995. "Controlling the False Discovery Rate: a Practical and Powerful Approach to Multiple Testing." *Journal of the Royal Statistical Society. Series B (Methodological)* 289–300.

Boittin, Margaret, Katrina Kosec, Cecilia Hyunjung Mo, and Soo Sun You. 2020. "The Unintended Consequences of Womens Empowerment Narratives: An Experimental Study of Gender Attitudes in Nepal." *EGAP Trial Registry* 20200604AC.

Booth, John A., and Mitchell A. Seligson. 2009. *The Legitimacy Puzzle in Latin America: Political Support and Democracy in Eight Nations.* New York, NY: Cambridge University Press.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014*a*. "Robust Data-Driven Inference in the Regression-Discontinuity Design." *Stata Journal* 14 (4): 909–946.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014*b*. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82 (6): 2295–2326.

Calonico, Sebastian, Matias D. Cattaneo, Max H. Farrell, and Rocio Titiunik. 2017. "rdrobust: Software for Regression Discontinuity Designs." *Stata Journal* 17 (2): 372–404.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma. 2020. "Simple Local Polynomial Density Estimators." *Journal of the American Statistical Association* 115 (531): 1449–1455.

Cheema, Iftikhar, Simon Hunt, Sarah Javeed, Tanya Lone, and Sean O'Leary. 2014. "Benazir Income Support Programme Final Impact Evaluation Report." Report prepared for the Government of Pakistan by Oxford Policy Management.

Chen, Jowei. 2013. "Voter Partisanship and the Effect of Distributive Spending on Political Participation." *American Journal of Political Science* 57 (1): 200–217.

Condon, Meghan, and Amber Wichowsky. 2020. "Inequality in the Social Mind: Social Comparison and Support for Redistribution." *The Journal of Politics* 82 (1): 000–000.

Corrêa, Diego Sanches, and José Antonio Cheibub. 2016. "The Anti-Incumbent Effects of Conditional Cash Transfer Programs." *Latin American Politics and Society* 58 (1): 49–71.

Crosby, Faye. 1976. "A Model of Egoistic Relative Deprivation." *Psychological Review* 83: 95–113.

De La O, Ana L. 2013. "Do Conditional Cash Transfers Affect Electoral Behavior? Evidence from a Randomized Experiment in Mexico." *American Journal of Political Science* 57 (1): 1–14.

Easton, David. 1967. *A Systems Analysis of Political Life.* New York, NY: Wiley.

Ellis, Christopher, and Christopher Faricy. 2011. "Social Policy and Public Opinion: How the Ideological Direction of Spending Influences Public Mood." *The Journal of Politics* 73 (4): 1095–1110.

Evans, David K., Brian Holtemeyer, and Katrina Kosec. 2019. "Cash Transfers Increase Trust in Local Government." *World Development* 114: 138–155.

Fair, Christine, Rebecca Littman, Neil Malhotra, and Jacob N. Shapiro. 2018. "Relative Poverty, Perceived Violence, and Support for Militant Politics: Evidence from Pakistan." *Political Science and Research Methods* 6 (1): 57–81.

Ferreira, Francisco, H.G. 2021. "Inequality in the Time of COVID-19." International Monetary Fund, Finance & Development, June.

Festinger, Leon. 1954. "A Theory of Social Comparison Processes." *Human Relations* 7: 117–140.

Finkel, Steven E., Edward N. Muller, and Mitchell A. Seligson. 1989. "Economic Crisis, Incumbent Performance and Regime Support: A Comparison of Longitudinal Data from West Germany and Costa Rica." *British Journal of Political Science* 19 (3): 329351.

Fiszbein, Ariel, Norbert Schady, Francisco H.G. Ferreira, Margaret Grosh, Niall Keleher, Pedro Olinto, and Emmanuel Skoufias. 2009. "Conditional Cash Transfers: Reducing Present and Future Poverty." A World Bank Policy Research Report 47603.

Gazdar, Haris. 2011. "Social Protection in Pakistan: In the Midst of a Paradigm Shift?" *Economic and Political Weekly* 59–66.

Genicot, Garance, and Debraj Ray. 2020. "Aspirations and Economic Behavior." *Annual Review of Economics* 12 (1): 715–746.

Gimpelson, Vladimir, and Daniel Treisman. 2018. "Misperceiving Inequality." *Economics & Politics* 30 (1): 27–54.

Gomez, Brad T., and J. Matthew Wilson. 2001. "Political Sophistication and Economic Voting in the American Electorate: A Theory of Heterogeneous Attribution." *American Journal of Political Science* 45 (4): 899–914.

Green, Tina Renee. 2006. *Essays on the Political Economy of Fiscal Policy in Developing Countries.* University of California, Berkeley.

Gulzar, Saad, and Benjamin J. Pasquale. 2017. "Politicians, Bureaucrats, and Development: Evidence from India." *American Political Science Review* 111 (1): 162–183.

Gurr, Ted Robert. 1970. *Why Men Rebel.* Princeton, NJ: Princeton University Press.

Haisley, Emily, Romel Mostafa, and George Loewenstein. 2008. "Subjective Relative Income and Lottery Ticket Purchases." *Journal of Behavioral Decision Making* 21: 283–295.

Haseeb, Muhammad, and Kate Vyborny. 2017. "Imposing Institutions: Evidence from Cash Transfer Reform in Pakistan." *CSAE Working Paper 2016-36* .

Healy, Andrew J., Katrina Kosec, and Cecilia Hyunjung Mo. 2017. "Economic Development, Mobility, and Political Discontent: An Experimental Test of Tocqueville's Thesis in Pakistan." *American Political Science Review* 11 (3): 605–621.

Imai, Kosuke, Gary King, and Carlos Velasco Rivera. 2020. "Do Nonpartisan Programmatic Policies have Partisan Electoral Effects? Evidence from Two Large Scale Randomized Experiments." *Journal of Politics* 82 (2): 714–730.

Imbens, Guido, and Karthik Kalyanaraman. 2011. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *The Review of Economic Studies* 79 (3): 933–959.

International Monetary Fund. 2013. "Representative Exchange Rates for Selected Currencies for June 2013." IMF.

Kahneman, Daniel, and Amos Tversky. 1979. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica* 47(March): 263–292.

Keefer, Philip. 2007. "Clientelism, Credibility, and the Policy Choices of Young Democracies." *American Journal of Political Science* 51 (4): 804–821.

Kosec, Katrina. 2014. "Relying on the Private Sector: The Income Distribution and Public Investments in the Poor." *Journal of Development Economics* 107: 320–342.

Kosec, Katrina, Cecilia Hyunjung Mo, Emily Schmidt, and Jie Song. 2021. "Perceptions of Relative Deprivation and Womens Empowerment." *World Development* 138: 105218.

León, Sandra. 2011. "Who is Responsible for What? Clarity of Responsibilities in Multilevel States: The Case of Spain." *European Journal of Political Research* 50 (1): 80–109.

Lewis-Beck, Michael Steven, and Richard Nadeau. 2011. "Economic Voting Theory: Testing New Dimensions." *Electoral Studies* 30 (2): 288–294.

Lind, E. Allen, and Tom R. Tyler. 1988. *The Social Psychology of Procedural Justice.* New York, NY: Plenum Press.

Lipset, Seymour Martin. 1981. *Political Man.* Baltimore: Johns Hopkins University Press.

Lupu, Noam, and Jonas Pontusson. 2011. "The Structure of Inequality and the Politics of Redistribution." *American Political Science Review* 105 (2): 316–336.

Manacorda, Marco, Edward Miguel, and Andrea Vigorito. 2011. "Government Transfers and Political Support." *American Economic Journal: Applied Economics* 3 (3): 1–28.

McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics* 142 (2): 698–714.

Meltzer, Allan H., and Scott F. Richard. 1981. "A Rational Theory of the Size of Government." *Journal of Political Economy* 89 (5): 914–927.

Miller, Arthur H. 1974. "Political Issues and Trust in Government: 1964–1970." *American Political Science Review* 68 (3): 951–972.

Ministry of Finance of Government of Pakistan. 2013. "Poverty Headcount Review for 2013–14 using New Methodology." IMF.

Mo, Cecilia Hyunjung. 2018. "The Effects of Perceived Relative Poverty on Risk: An Aspirations-Based Model of Trafficking Vulnerability." *Political Behavior,* 40 (1): 247–277.

Muller, Edward N., Thomas O. Jukam, and Mitchell A. Seligson. 1982. "Diffuse Political Support and Antisystem Political Behavior: A Comparative Analysis." *American Journal of Political Science* 26 (2): 240–264.

Ravallion, Martin. 2014. "Income Inequality in the Developing World." *Science* 344 (6186): 851–855.

Sands, Melissa L. 2017. "Exposure to Inequality Affects Support for Redistribution." *Proceedings of the National Academy of Sciences* 114 (4): 663–668.

Sands, Melissa L., and Daniel and de Kadt. 2020. "Local Exposure to Inequality Raises Support of People of Low Wealth for Taxing the Wealthy." *Nature* 586: 257–261.

Seligson, Mitchell A. 1983. "On the Measurement of Diffuse Support: Some Evidence from Mexico." *Social Indicators Research* 12 (1): 1–24.

Seligson, Mitchell A., and Julio F. Carrion. 2002. "Political Support, Political Skepticism, and Political Stability in New Democracies: An Empirical Examination of Mass Support for Coups d'Etat in Peru." *Comparative Political Studies* 35 (1): 58–82.

Shwarz, Norbert, Hans J. Hipper, Brigitte Deutsch, and Fritz Strack. 1985. “Response Scales: Effects of Category Range on Reported Behavior and Comparative Judgments.” *Public Opinion Quarterly* 49: 388–395.

Solt, Frederick. 2008. “Economic Inequality and Democratic Political Engagement.” *American Journal of Political Science* 52 (1): 48–60.

Suls, Jerry M., and Ladd Wheeler. 2000. *Handbook of Social Comparison: Theory and Research.* New York, NY: Kluwer Academic/Plenum Publishers.

Tocqueville, Alexis de. 1856. *The Old Regime and the Revolution.* trans. by John Bonner. New York, NY: Harper Press.

Van Ryzin, Gregg G. 2011. “Outcomes, Process, and Trust of Civil Servants.” *Journal of Public Administration Research and Theory* 21(02): 745–760.

Walker, Iain, and Heather J. Smith. 2001. *Relative Deprivation: Specification, Development and Integration.* Cambridge: Cambridge University Press.

World Bank. 2019. “World Development Indicators: GINI index (World Bank estimate).” World Bank.

# Appendix

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

## Table of Contents

# A Literature Review

## A.1 Studies on Social Protection and Political Attitudes

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 political 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 |
|---|---|---|---|---|---|---|
| Lyall, 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<br>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<br>2. Dummy for voting for the incumbent party<br>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 |
| Kweon (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<br>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<br>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 |
| Manacorda, 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, Emeriau, 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<br>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<br>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<br>2. Vote share for incumbent president | Positive |
| Cerda 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, Camacho, 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<br>2. Vote share of incumbent party candidate<br>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<br>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 closest year that pre-dates the first year of the data set, or the earliest year that the Gini index is available. For studies involving multiple countries, the Gini index is calculated by averaging the Gini indices of all countries.

## A.2 Scholarship on Why Citizens Selectively Reward Political Leaders

We contribute to a rich scholarship explaining why citizens selectively reward their political leaders and systems 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),[1] and may be overcome by provision of information, such as through regular community meetings (Evans, Holtemeyer, and Kosec 2019). Other literature focuses on biases, heuristics, and other psychological underpinnings. Voters may exhibit recency bias—or the tendency to credit government for recent provision of goods and services more than provision further in the past (Cole, Healy, and Werker 2012; Zucco 2013). Galiani et al. (2019) provide experimental evidence for peak-end heuristics in voter behavior, whereby citizens reward government for cash transfers based on the size of their most recent and largest transfers. Research on hedonic adaptation 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 re-occurring (as opposed to one-off) cash transfer programs are not 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). Another thread of research focuses on how social protection programs are branded and rolled out. Receipt may carry social stigma, complicating how recipients engage with the state (Mettler and Stonecash 2008; Soss 2000). Programs whose implementation is sometimes influenced by geography rather than rights or needs can also yield varied political responses (Michener 2018). And policymakers may use these programs for overtly political purposes, further complicating how they are perceived (e.g., Bruhn 1996; Dahlberg and Johansson 2002; Brollo and Nannicini 2012; Aytaç 2014). We contend that the perception that one is relatively deprived is an additional significant factor in citizens' selective rewarding of political leaders and institutions for social protection.[2]

[1] For example, the 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 helped register beneficiaries.

[2] This is consistent with the finding of Córdova and Layton (2016) that economic inequality can foment political distrust and dissatisfaction among low-income individuals, even when there is evidence of strong government performance.

# 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).[3] By including all opposition party politicians in the selection of beneficiaries and setting a quota for each politician, the government aimed to avoid favoritism, but they were only partly successful; for example, households from the origin villages of members of parliament were ultimately 200–400% 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

[3] 52% 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.[4] 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)[5] up to 4 years, 7 months (for those who received transfers since October 2008 and who we surveyed in May 2013).[6]

Because our RDD strategy relies on smooth changes in factors other than the BISP at the poverty cutoff of 16.17, it is important to establish that no other programs that operated concurrently share a similar cutoff. There are several programs in Pakistan besides the BISP unconditional cash transfer program we study that are targeted based on poverty, but they were generally either rolled out after our study period (e.g., the Prime Ministers National Health Programme provided healthcare services to poor families starting only in 2015) or used a different cutoff point for eligibility (e.g., a cutoff of 23 was employed by both the Pakistan Poverty Alleviation Fund, which has operated since 2000 providing healthcare financing, and the Rural Support Programmes Network, which has operated since 1992 and comprises 10 member programs that provide social guidance as well as technical and financial assistance to the rural poor). One exception is the BISP Waseela-e-Taleem, which was a conditional cash transfer program launched in 2012 employing the same cutoff, but it was only offered in one of our 19 sample districts (Jacobabad district in Sindh province), and only to the subset of BISP beneficiaries with 4-12 year old children who further accepted to join this add-on program. We take this as broad evidence that access to other social protection programs is not changing discontinuously at the cutoff we employ to examine the impacts of the BISP unconditional cash transfer program.

---

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

[5] In the administrative data given to us by the BISP Secretariat on the individuals we surveyed in our April–May 2013 household survey, over 95% received their PMT score in 2011 or earlier. The less than 5% that received a PMT score in 2012, and the less than 0.1% 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.

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

# C Background on Data Sources

## RHPS Dataset Sample

The RHPS[7] 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 Khyber-Pakhtunkhwa (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 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% (i.e., 2,639) were in the BISP administrative database.[8,9] Households lacking a CNIC number in the BISP database were omitted from our analysis.[10] 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,

---

[7]This study received IRB approval from the IFPRI Review Board (No. 00003487).

[8]CNIC 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).

[9]These valid and matched CNIC numbers came from 1,349 different households.

[10]Survey respondents that were missing a CNIC number or provided a CNIC number that did not match with the BISP database were not 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-sairaiki, 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 sairaiki ($p$ <0.01) and sindhi ($p < 0.01$).

our household-level beneficiary indicator is simply identical to this female's beneficiary indicator variable. In the administrative data, only 7.5% 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,[11] 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*.[12] 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% (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 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% 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; Oduro 2015), which may make individuals hesitant to admit that they receive social protection in a survey setting. Thinking that a "yes" answer would result in a set of follow-up questions, individuals may also wish to shorten the length of a survey by answering "no" upon being asked whether or not they received social protection programs—whether or not they do. Alternately, but equally problematic from a research standpoint, individuals may be eager to convey their need for additional welfare to enumerators—who they may suspect are providing information to government. This may manifest itself as under-reporting of what one currently receives—such as by saying one is not a BISP beneficiary when in fact they are. These potential sources of bias in self-reported information motivate our use of administrative data. It is nonetheless useful to consider the prevalence of conflicts between our administrative data source, which we use to code our beneficiary dummy, and responses to a question in each of rounds 1 and 2 of our household survey, which asked "Has [NAME] received any assistance in the last 12 months from the BISP?" Combining data from rounds 1 and 2 of our survey allow us to code a dummy variable for the household having received the BISP during *either* of the 12 month periods preceding these two survey rounds. Since individuals chosen by the PMT to be beneficiaries in 2011 almost universally remained beneficiaries (Haseeb and Vyborny 2017), coding a dummy in this way helps us minimize the likelihood of mis-coding a beneficiary during this two year period as a non-beneficiary purely due to, for example, a failure in one of the two years for the respondent to report receipt of BISP. Obviously, these two data sources are not fully comparable; while our survey tells us whether the individual claims to have received support at some point during a

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

[12] All sample households contained an adult female, so it is never the case that a household with a poverty score under 16.17 simply does not have adult female members that could register to receive BISP payments.

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% 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% 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% 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 political 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?
- Political 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 the BISP. This strategy helps attenuate selection bias concerns. To illustrate, consider the following empirical specification:

$$Y_i = \beta_0 + \tau B_i + \epsilon_i \tag{1}$$

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. $\epsilon_i$ is measurement error, and $\tau$ 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 $\tau$ 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:

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

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 Appendix B for greater details on program eligibility and take-up). We thus employ a fuzzy RDD, which does not require a doubling in the probability of receiving BISP transfers at the cutoff, and only requires the following to hold:

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

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 $\tau$ for outcome $Y_i$ as follows:

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

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 | 0.487 | [-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 | 463 | 538 |
| 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 |
| Sairaiki | -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 | 524 | 633 |
| 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 2014*a*). 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) $\mu_{Primed}$ | (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 |
| Sairaiki | 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 |
| *Proportion* | 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 ($\mu_{Primed}$-$\mu_{NotPrimed}$), which is computed by subtracting the mean value for the subgroup that did *not* received the relative poverty prime ($\mu_{notprimed}$) from the mean value for the subgroup that received the relative poverty prime ($\mu_{primed}$). 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: RDD Estimate by Bandwidth Selection Procedure

| Bandwidth Selection Procedure | (1)<br>Full Sample | (2)<br>No Poverty Prime | (3)<br>Relative Poverty Prime |
|---|---|---|---|
| (a) MSE-Optimal Bandwidth | 0.080* | 0.015 | 0.155** |
| (Calonico, Cattaneo, and Titiunik 2014*a*) | (0.044) | (0.072) | (0.065) |
| (b) MSE Minimizing Bandwidth | 0.077 | 0.007 | 0.152** |
| (Imbens and Kalyanaraman 2011) | (0.051) | (0.048) | (0.065) |
| (c) 1/2 the MSE Minimizing Bandwidth | 0.071 | -0.005 | 0.108 |
| (Imbens and Kalyanaraman 2011) | (0.067) | (0.068) | (0.086) |
| (d) 2X the MSE Minimizing Bandwidth | 0.064* | 0.015 | 0.093** |
| (Imbens and Kalyanaraman 2011) | (0.036) | (0.033) | (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 (2014*a*). Row (b) reports estimates when employing the optimal bandwith recommended in Imbens and Kalyanaraman (2011). Row (c) and (d) report estimates when the bandwidth selection procedure is half and double the optimal bandwith 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 relative poverty prime, and column (3) reports RDD estimates when analyzing the sample that was primed with the relative poverty prime.

Table A.5: RDD Estimate by Presence or Absence of Clustered Standard Errors

| | Full Sample | | No Poverty Prime | | Relative Poverty Prime | | Relative Poverty Prime (Felt Relatively Poor Pre-Treatment) | | Relative Poverty Prime (Did Not Feel Relatively Poor Pre-Treatment) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) Unclustered | (2) Clustered | (3) Unclustered | (4) Clustered | (5) Unclustered | (6) Clustered | (7) Unclustered | (8) Clustered | (9) Unclustered | (10) Clustered |
| Conventional | 0.080* | 0.083* | 0.015 | 0.012 | 0.155** | 0.153** | -0.047 | -0.047 | 0.239*** | 0.233*** |
| | (0.044) | (0.047) | (0.072) | (0.075) | (0.065) | (0.064) | (0.126) | (0.096) | (0.076) | (0.082) |
| Bias-corrected | 0.080* | 0.087* | 0.023 | 0.014 | 0.171*** | 0.174*** | -0.095 | -0.101 | 0.273*** | 0.271*** |
| | (0.044) | (0.047) | (0.072) | (0.075) | (0.065) | (0.064) | (0.126) | (0.096) | (0.076) | (0.082) |
| Robust | 0.080 | 0.087 | 0.023 | 0.014 | 0.171** | 0.174** | -0.095 | -0.101 | 0.273*** | 0.271*** |
| | (0.054) | (0.057) | (0.088) | (0.092) | (0.074) | (0.075) | (0.142) | (0.108) | (0.085) | (0.092) |
| N | 2636 | 2636 | 1333 | 1333 | 1303 | 1303 | 545 | 545 | 758 | 758 |

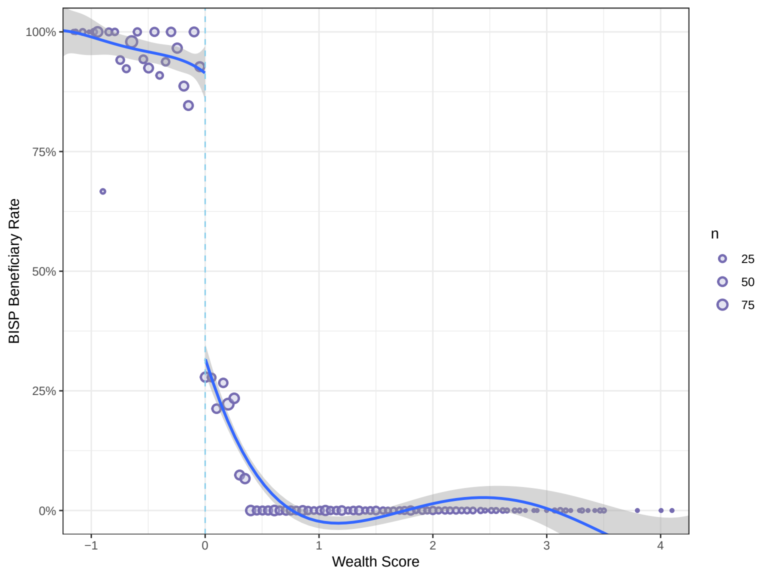
*Notes:* The outcome measure of interest is our diffuse political support index. Standard errors are in parentheses, and * $p<0.10$, ** $p<0.05$, *** $p<0.01$. We report coefficients with conventional standard errors, bias-corrected coefficients with conventional standard errors, and bias-corrected coefficients with robust standard errors. Columns (1)-(2) report results with the full sample; columns (3)-(4) report results among those who did not receive the poverty prime only; columns (5)-(6) report results among those who received the poverty prime only; columns (7)-(8) report results among those who received the poverty prime and felt relatively poor pre-treatment; and columns (9)-(10) report results among those who received the relative poverty prime and did not feel relatively poor pre-treatment.

Table A.6: RDD Estimate by Gender

| Subgroup | (1)<br>Full Sample | (2)<br>No Poverty Prime | (3)<br>Relative Poverty Prime |
|---|---|---|---|
| ***Female Respondents*** | | | |
| Coefficient | 0.065 | -0.004 | 0.106 |
| Conventional Standard Errors | (0.060) | (0.089) | (0.107) |
| Robust 95% CI | [-0.103, 0.190] | [-0.217, 0.205] | [-0.165, 0.334] |
| N | 1327 | 671 | 656 |
| ***Male Respondents*** | | | |
| Coefficient | 0.090 | 0.035 | 0.188* |
| Conventional Standard Errors | (0.072) | (0.103) | (0.106) |
| Robust 95% CI | [-0.072, 0.268] | [-0.197, 0.303] | [-0.042, 0.448] |
| N | 1309 | 662 | 647 |
| Test of Effect Heterogeneity by Gender | $Z = 0.48$ | $Z = 0.35$ | $Z = 0.66$ |

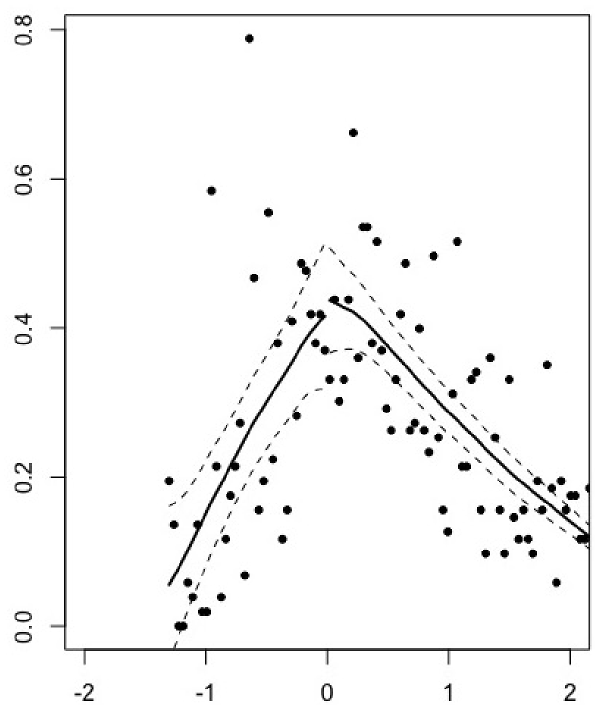
*Notes:* Conventional standard errors are in parentheses, and * p<0.10, **p<0.05, *** p<0.01. Row (a) reports estimates among female respondents. Row (b) reports estimates among male respondents. The last row displays the Z-statistic when estimating whether the RDD effects differ by gender.

Figure A.1: First Stage Results



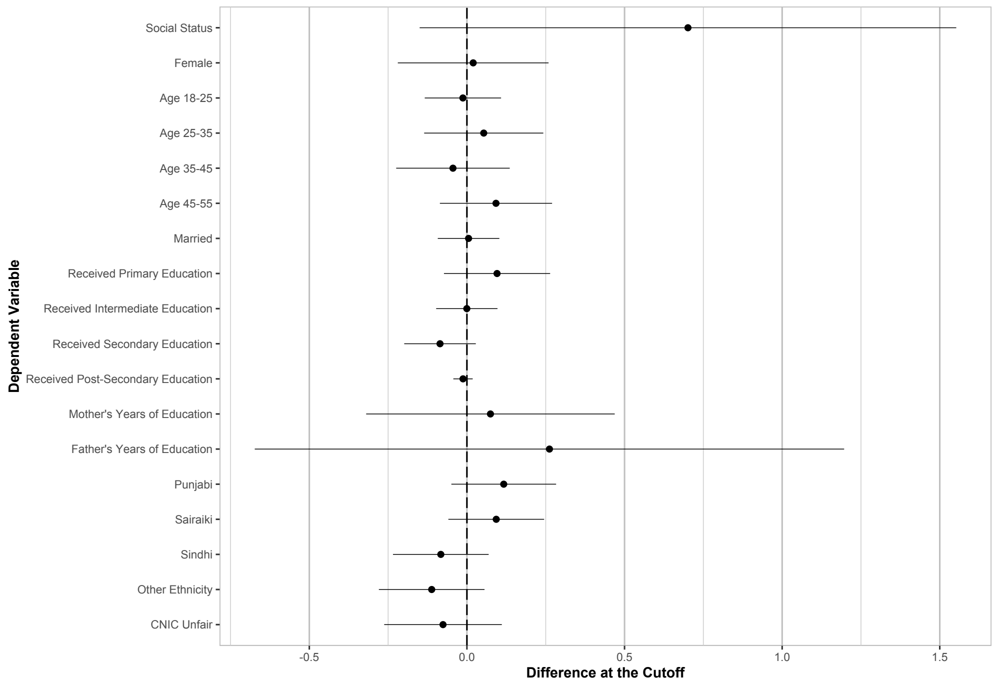

*Notes:* At the cutoff of 16.17, recoded as 0, the probability of being a BISP beneficiary increases by 59.4 percentage points ($p < 0.001$). The shaded area represents the 95% confidence interval of the fourth order polynomial fit line. The size of each bubble reflects the number of observations within each bin (bin size = 0.05).

Figure A.2: McCrary Density Plot

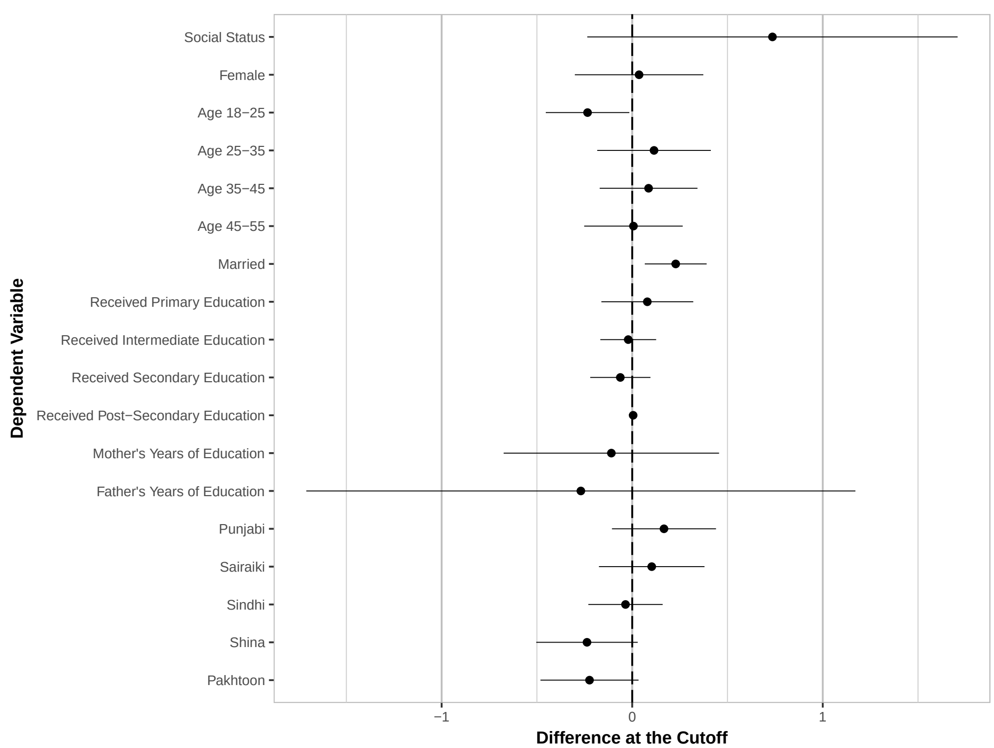



*Notes:* The figure is a density plot of the wealth score with 95% (two-tailed) confidence intervals, where the wealth score is recoded so that the cutoff is 0.

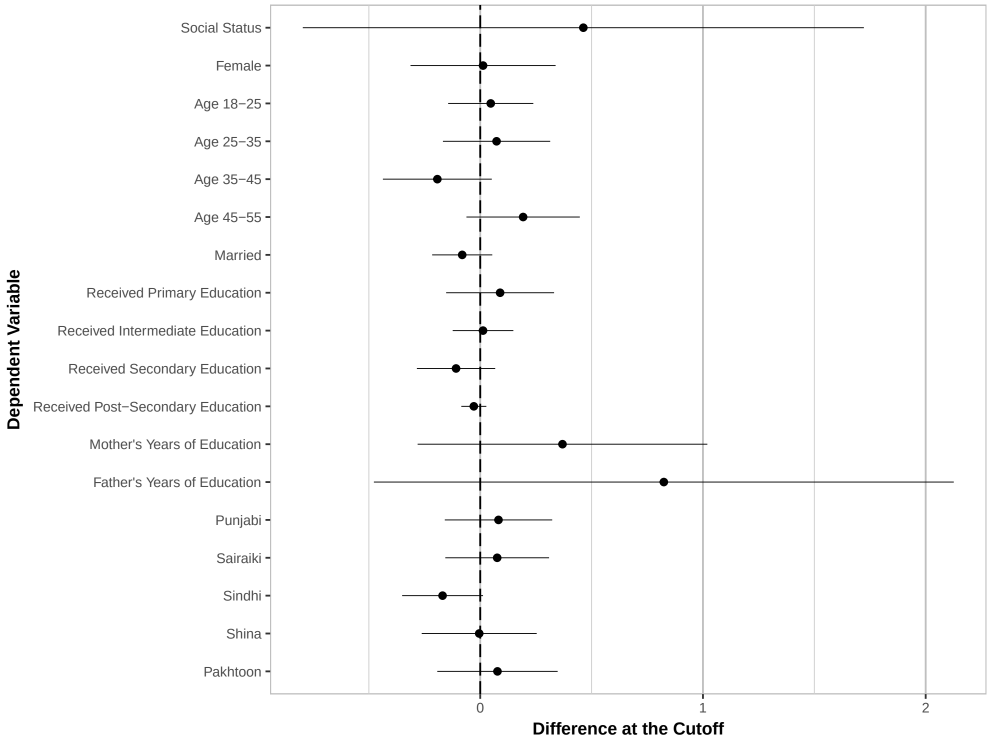
Figure A.3: Two-Stage Least Squares (2SLS) Estimates by Sample –
Baseline Pre-Treatment Characteristic




(a) *Full Sample*



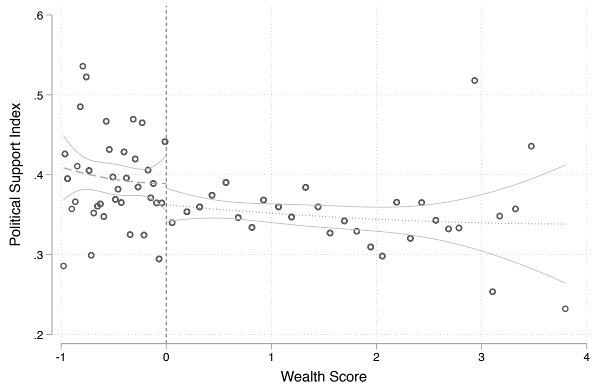

(b) *No Poverty Prime Sample*



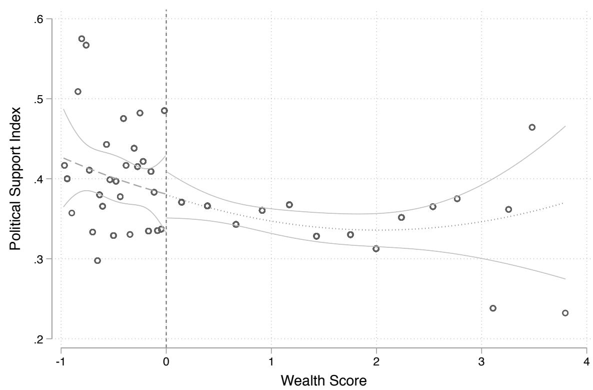

(c) *Relative Poverty Prime Sample*

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

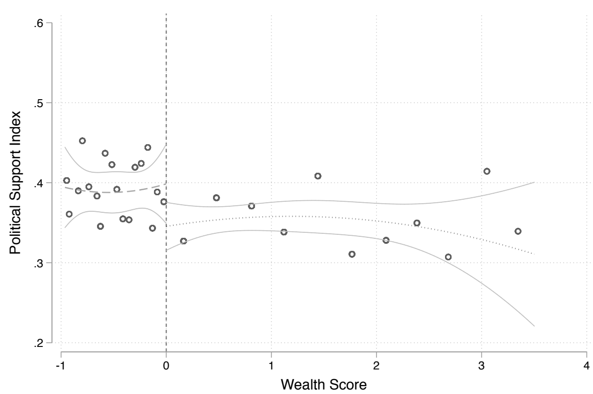
Figure A.4: Political Support Index by Wealth Score and Sample




(a) *Full Sample*



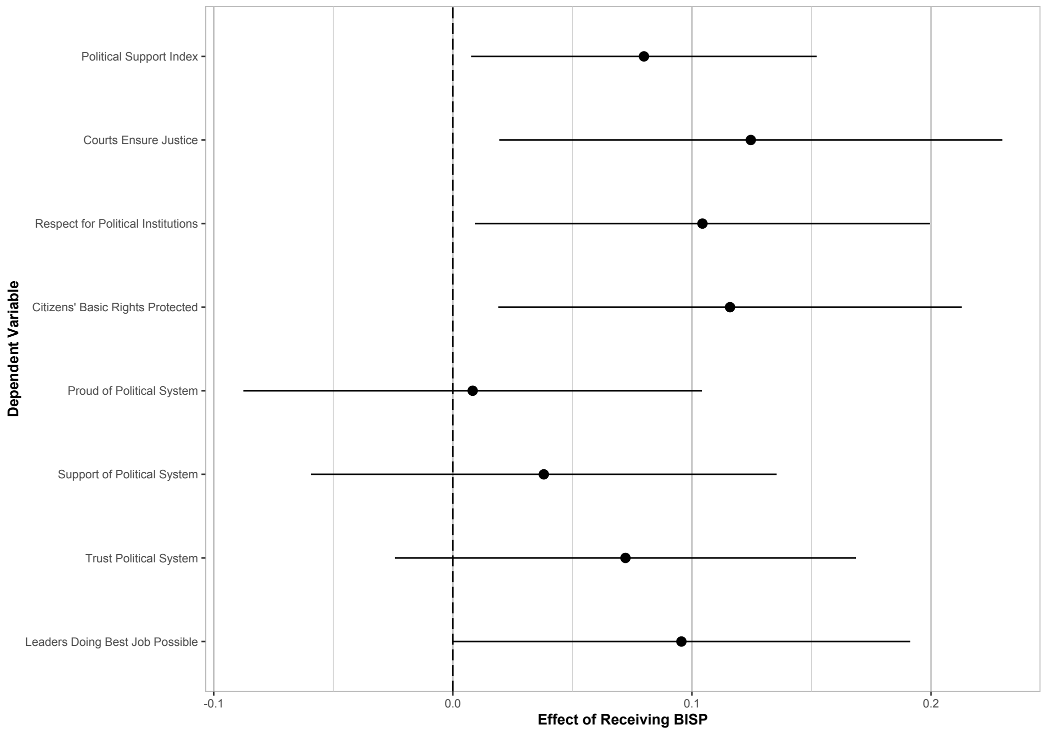

(b) *No Poverty Prime Sample*




(c) *Relative Poverty Prime Sample*

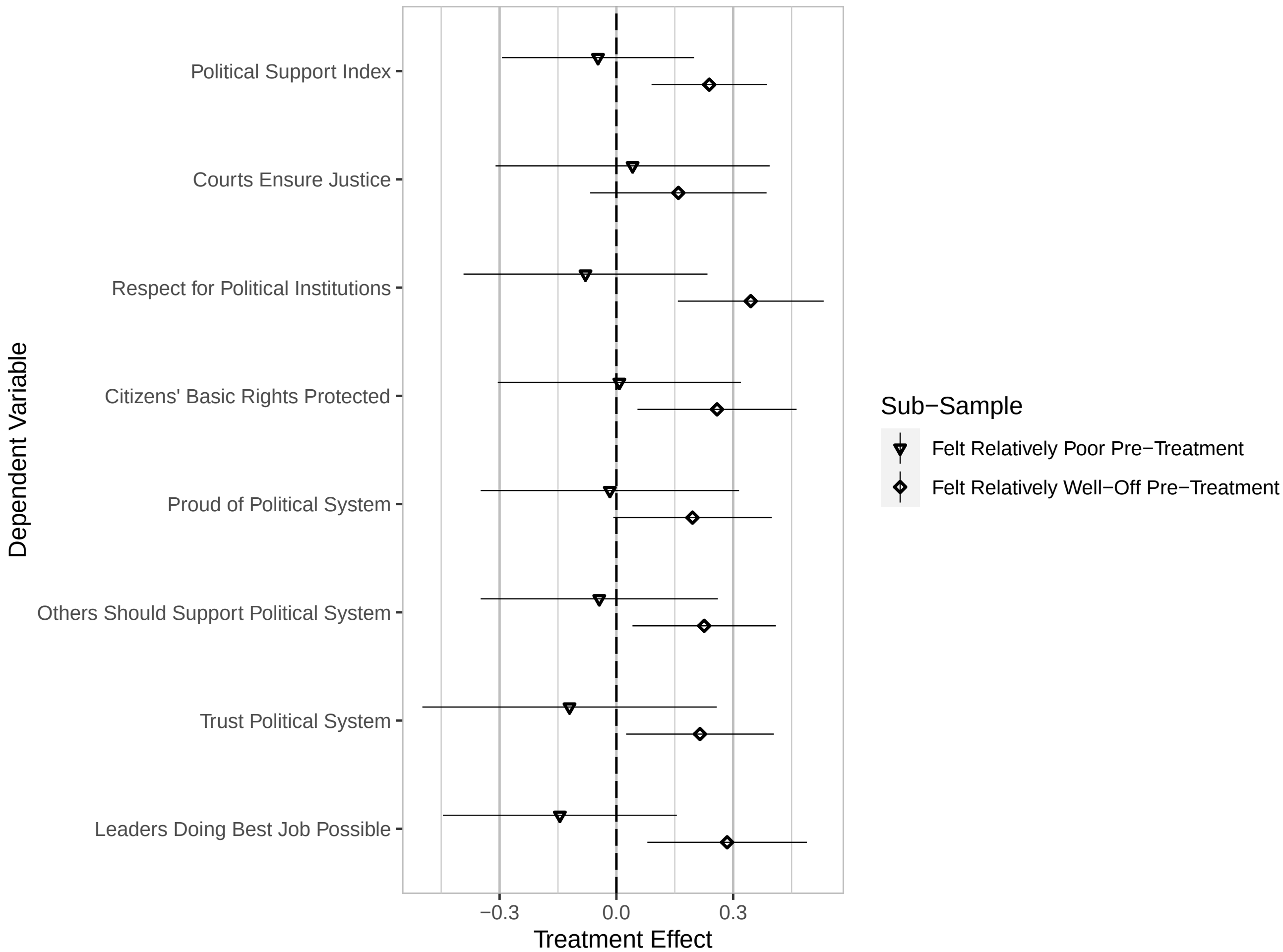
*Notes:* 95% confidence intervals surround the fitted line of the regression discontinuity plot.

Figure A.5: Two-Stage Least Squares (2SLS) Estimates – Attitudes Toward Government



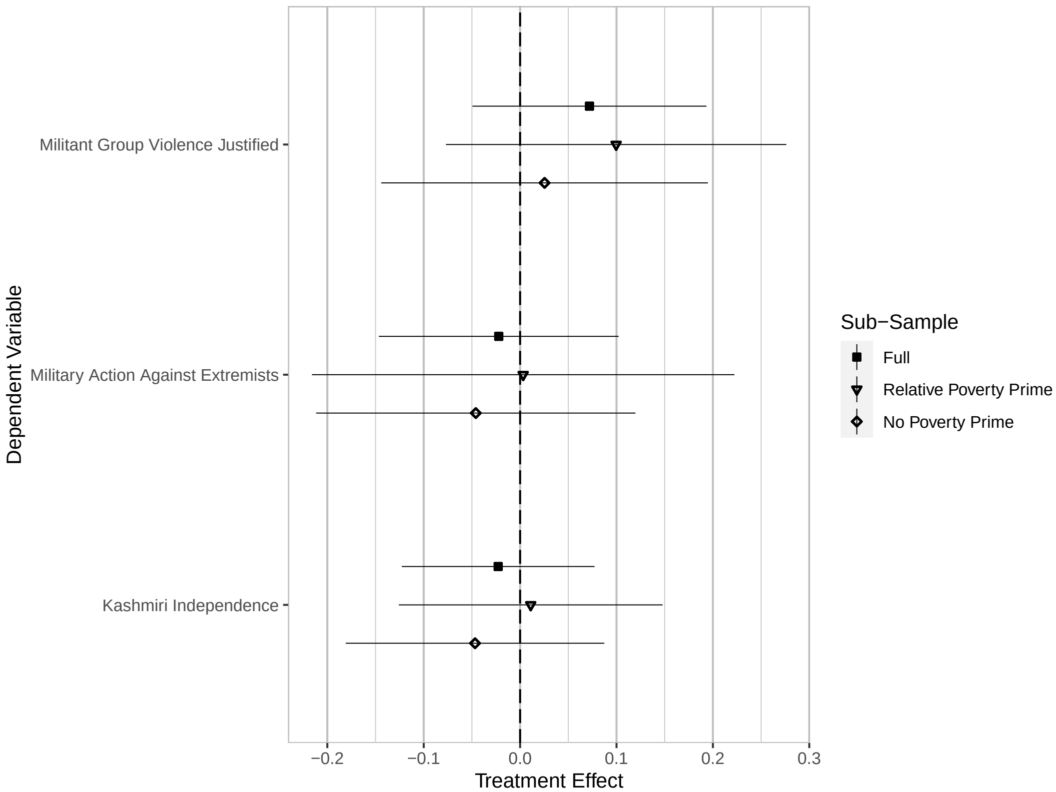

*Notes:* 95% confidence intervals surround local-polynomial RD treatment effect point estimates.

Figure A.6: Effect of the BISP on Political Support by Perceived Relative Income Pre-Treatment Among Individuals Primed to Feel Relatively Poor (2SLS)




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

Figure A.7: Placebo Test




*Notes:* 95% 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)).

## Appendix References


Ambler, Kate, and Alan De Brauw. 2019. The Impacts of Cash Transfers on Women's Empowerment: Learning from Pakistan's BISP Program. Technical report for the World Bank.

Aytaç, S Erdem. 2014. "Distributive Politics in a Multiparty System: The Conditional Cash Transfer Program in Turkey." *Comparative Political Studies* 47 (9): 1211–1237.

Bechtel, Michael M, and Jens Hainmueller. 2011. "How Lasting is Voter Gratitude? An Analysis of the Short-and Long-Term Electoral Returns to Beneficial Policy." *American Journal of Political Science* 55 (4): 852–868.

Blattman, Christopher, Mathilde Emeriau, and Nathan Fiala. 2018. "Do Anti-Poverty Programs Sway Voters? Experimental Evidence from Uganda." *Review of Economics and Statistics* 100 (5): 891–905.

Brazys, Samuel, Peter Heaney, and Patrick Paul Walsh. 2015. “Fertilizer and Votes: Does Strategic Economic Policy Explain the 2009 Malawi Election?” *Electoral Studies* 39: 39–55.

Brollo, Fernanda, and Tommaso Nannicini. 2012. “Tying Your Enemy’s Hands in Close Races: The Politics of Federal Transfers in Brazil.” *American Political Science Review* 106 (04): 742–761.

Bruhn, Kathleen. 1996. “Social Spending and Political Support: The “Lessons” of the National Solidarity Program in Mexico.” *Comparative Politics* 28 (2): 151–177.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014. “Robust Data-Driven Inference in the Regression-Discontinuity Design.” *Stata Journal* 14 (4): 909–946.

Cerda, Rodrigo, and Rodrigo Vergara. 2008. “Government Subsidies and Presidential Election Outcomes: Evidence for a Developing Country.” *World Development* 36 (11): 2470–2488.

Chen, Jowei. 2013. “Voter Partisanship and the Effect of Distributive Spending on Political Participation.” *American Journal of Political Science* 57 (1): 200–217.

Clinton, Joshua D, and Michael W Sances. 2018. “The Politics of Policy: The Initial Mass Political Effects of Medicaid Expansion in the States.” *American Political Science Review* 112 (1): 167–185.

Cole, Shawn, Andrew Healy, and Eric Werker. 2012. “Do Voters Demand Responsive Governments? Evidence from Indian Disaster Relief.” *Journal of Development Economics* 97 (2): 167–181.

Conover, Emily, Roman A Zarate, Adriana Camacho, and Javier Baez. 2018. “Cash and Ballots: Conditional Transfers, Political Participation and Voting Behavior.” *Economic Development and Cultural Change* .

Córdova, Abby, and Matthew L Layton. 2016. “When is Delivering the Goods Not Good Enough?” *World Politics* 68 (01): 74–110.

Dahlberg, Matz, and Eva Johansson. 2002. “On the Vote-Purchasing Behavior of Incumbent Governments.” *American Political Science Review* 96 (1): 27–40.

De La O, Ana L. 2013. “Do Conditional Cash Transfers Affect Electoral Behavior? Evidence from a Randomized Experiment in Mexico.” *American Journal of Political Science* 57 (1): 1–14.

Di Tella, Rafael, John Haisken-De New, and Robert MacCulloch. 2010. “Happiness Adaptation to Income and to Status in an Individual Panel.” *Journal of Economic Behavior  Organization* 76 (3): 834–852.

Dionne, Kim Yi, and Jeremy Horowitz. 2016. “The Political Effects of Agricultural Subsidies in Africa: Evidence from Malawi.” *World Development* 87: 215–226.

Evans, David K., Brian Holtemeyer, and Katrina Kosec. 2019. “Cash Transfers Increase Trust in Local Government.” *World Development* 114: 138–155.

Galiani, Sebastian, Nadya Hajj, Patrick J McEwan, Pablo Ibarrarán, and Nandita Krishnaswamy. 2019. “Voter Response to Peak and End Transfers: Evidence from a Conditional Cash Transfer Experiment.” *American Economic Journal: Economic Policy* 11 (3): 232–60.

Gazdar, Haris. 2011. “Social Protection in Pakistan: In the Midst of a Paradigm Shift?” *Economic and Political Weekly* 59–66.

Government of Pakistan. 2017. "National Socio Economic Registry Update: New Targeting Survey, Benazir Income Support Programme." Government of Pakistan.

Gulzar, Saad, and Benjamin J. Pasquale. 2017. "Politicians, Bureaucrats, and Development: Evidence from India." *American Political Science Review* 111 (1): 162–183.

Haseeb, Muhammad, and Kate Vyborny. 2017. "Imposing Institutions: Evidence from Cash Transfer Reform in Pakistan." *CSAE Working Paper 2016-36* .

Healy, Andrew, and Neil Malhotra. 2009. "Myopic Voters and Natural Disaster Policy." *American Political Science Review* 103 (3): 387–406.

Hobolt, Sara B, James Tilley, and Jill Wittrock. 2013. "Listening to the Government: How Information Shapes Responsibility Attributions." *Political Behavior* 35 (1): 153–174.

Imai, Kosuke, Gary King, and Carlos Velasco Rivera. 2020. "Do Nonpartisan Programmatic Policies have Partisan Electoral Effects? Evidence from Two Large Scale Randomized Experiments." *Journal of Politics* 82 (2): 714–730.

Imbens, Guido, and Karthik Kalyanaraman. 2011. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *The Review of Economic Studies* 79 (3): 933–959.

Kweon, Yesola. 2018. "Types of Labor Market Policy and the Electoral Behavior of Insecure Workers." *Electoral Studies* 55: 1–10.

Labonne, Julien. 2013. "The Local Electoral Impacts of Conditional Cash Transfers: Evidence from a Field Experiment." *Journal of Development Economics* 104: 73–88.

Layton, Matthew L, and Amy Erica Smith. 2015. "Incorporating Marginal Citizens and Voters: The Conditional Electoral Effects of Targeted Social Assistance in Latin America." *Comparative Political Studies* 48 (7): 854–881.

Lee, Seonghui, Carsten Jensen, Christoph Arndt, and Georg Wenzelburger. 2017. "Risky Business? Welfare State Reforms and Government Support in Britain and Denmark." *British Journal of Political Science* 1–20.

León, Sandra. 2011. "Who is Responsible for What? Clarity of Responsibilities in Multilevel States: The Case of Spain." *European Journal of Political Research* 50 (1): 80–109.

Linos, Elizabeth. 2013. "Do Conditional Cash Transfer Programs Shift Votes? Evidence From the Honduran PRAF." *Electoral Studies* 32 (4): 864–874.

Lü, Xiaobo. 2014. "Social Policy and Regime Legitimacy: The Effects of Education Reform in China." *American Political Science Review* 108 (2): 423–437.

Lyall, Jason, Yang-Yang Zhou, and Kosuke Imai. 2020. "Can Economic Assistance Shape Combatant Support in Wartime? Experimental Evidence from Afghanistan." *American Political Science Review* 114 (1): 126–143.

Malhotra, Neil, and Alexander G. Kuo. 2008. "Attributing Blame: The Public's Response to Hurricane Katrina." *The Journal of Politics* 70 (1): 120–135.

Manacorda, Marco, Edward Miguel, and Andrea Vigorito. 2011. "Government Transfers and Political Support." *American Economic Journal: Applied Economics* 3 (3): 1–28.

Margalit, Yotam. 2011. "Costly Jobs: Trade-Related Layoffs, Government Compensation, and Voting in US Elections." *American Political Science Review* 105 (1): 166–188.

Marschall, Melissa, Abdullah Aydogan, and Alper Bulut. 2016. "Does Housing Create Votes? Explaining the Electoral Success of the AKP in Turkey." *Electoral Studies* 42: 201–212.

Mettler, Suzanne, and Jeffrey M Stonecash. 2008. "Government Program Usage and Political Voice." *Social Science Quarterly* 89 (2): 273–293.

Michener, Jamila. 2018. *Fragmented Democracy: Medicaid, Federalism, and Unequal Politics.* Cambridge University Press.

Oduro, Razak. 2015. "Beyond Poverty Reduction: Conditional Cash Transfers and Citizenship in Ghana." *International Journal of Social Welfare* 24 (1): 27–36.

Pop-Eleches, Cristian, and Grigore Pop-Eleches. 2012. "Targeted Government Spending and Political Preferences." *Quarterly Journal of Political Science* 7 (3): 285–320.

Soss, Joea. 2000. *Unwanted Claims: The Politics of Participation in the U.S. Welfare System.* University of Michigan Press.

World Bank. 2019. "World Development Indicators: GINI index (World Bank estimate)." World Bank.

Zucco, Cesar Jr. 2013. "When Payouts Pay Off: Conditional Cash Transfers and Voting Behavior in Brazil 2002–10." *American Journal of Political Science* 57 (4): 810–822.