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WIDER Working Paper 2019/91

Winning or buying hearts and minds?

Cash transfers and political attitudes in Pakistan

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November 2019

United Nations University World Institute for Development Economics Research
wider.unu.edu

Abstract: This paper studies how household-level receipts of cash transfers affect political attitudes in Pakistan. The paper exploits the locally exogenous eligibility cut-off of the flagship Benazir Income Support Programme to estimate causal effects. The main results show evidence of improved satisfaction with the government among beneficiaries of the programme. The paper discusses what potential mechanisms may explain this result and finds no evidence of changes in attitudes being associated with improvements in state capacity or better economic and security prospects. Instead, we find that the effect is present only when the programme has been in place in communities for over two years, which coincides with the switch to proxy-means test-based targeting from the earlier modality of nominations by parliamentarians. The main result is therefore driven by better connected and politically important communities that were favoured by incumbent parliamentarians for programme rollout before the introduction of objective targeting criteria.

Key words: Social protection, political attitudes, cash transfers, Pakistan

Figures and tables: at end of paper.

[edit: October 2020] This study is published within the UNU-WIDER project Inequality and governance in unstable democracies—the mediating role of trust, implemented by a consortium led by Institute of Development Studies (IDS). The support of the UK Economic and Social Research Council (ESRC) is gratefully acknowledged

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Information and requests: publications@wider.unu.edu

ISSN 1798-7237 ISBN 978-92-9256-727-9

https://doi.org/10.35188/UNU-WIDER/2019/727-9

Typescript prepared by Lesley Ellen.

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The Institute is funded through income from an endowment fund with additional contributions to its work programme from Finland, Sweden, and the United Kingdom as well as earmarked contributions for specific projects from a variety of donors.

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The views expressed in this paper are those of the author(s), and do not necessarily reflect the views of the Institute or the United Nations University, nor the programme/project donors.

1 Introduction

Perceptions about the legitimacy and efficiency of governments are largely conditional on their perceived ability to ensure the economic and security needs of citizens. This idea goes back several centuries to the formation of nation-states based on an exchange contract whereby subjects provided taxes and other resources in exchange for economic and physical security (Tilly 1992; Bates 2001). This simple exchange evolved in different ways in different parts of the world, including the expansion of the welfare state in Europe (Esping-Andersen 1990). As a result of this 'social contract', citizens make political decisions (about voting, supporting political parties, protesting, and so forth) based on their assessment of how well their governments fulfil their part of the contract (Lipset 1981; Pop-Eleches and Pop-Eleches 2012). One way in which governments attempt to gain support from citizens and/or strengthen the social contract is through welfare programmes (Boix 2003; Acemoglu and Robinson 2006).

Two longstanding sets of theories attempt to explain how welfare transfers may affect political attitudes. These theories are not mutually exclusive, and both capture important factors that may mediate the relationship between welfare gains and political attitudes in different contexts. The two sets of theories can be briefly illustrated by the emphasis each place on the role of cash transfers as a means of 'winning' hearts and minds, or 'buying' hearts and minds.

The first set of theories emphasizes the key role of the social contract by viewing income transfer programmes as a signal that the state is committed to improving the wellbeing of its citizens. Welfare spending may be associated with positive levels of support for the government when it signals the commitment of political elites to address the needs of ordinary citizens (Boix 2003; Acemoglu and Robinson 2006). Governments may therefore decide to implement welfare programmes because that earns them support and votes from citizens. Citizens in turn use their voting rights and voice to respond to government policy (Levitt and Snyder 1997), thereby creating mechanisms for political accountability. Government spending on welfare programmes is a particularly strong commitment signal because 'social security entitlement programmes appear to be more difficult to cut than other redistributive programs' (Acemoglu and Robinson 2000: 1194). This commitment signalling is in turn likely to improve beliefs about the capacity of state institutions (Pop-Eleches and Pop-Eleches 2012; Evans et al. 2019) and, therefore, improve citizens' perceptions and views about governments (North and Weingast 1989; Acemoglu and Robinson 2000, 2006; Besley and Persson 2009).

The second set of theories draws on a well-established body of research on 'clientelism' and the use of welfare spending (and other social and economic programmes) to buy votes and/or political support. There is no a priori reason to believe that welfare programmes will necessarily result in better living conditions. In fact, rises in inequalities and reductions in welfare may happen if the programmes are captured by elites to pursue their own interests and/or buy-out voters (Bénabou 2001; Piketty 1995, 1998) and welfare spending is captured as part of systems of clientelism and patronage (Centeno 2002; Diaz-Cayeros et al. 2012). Welfare programmes in these contexts can be used to buy out interest groups, rather than improving the social and economic conditions of programme beneficiaries (Acemoglu et al. 2004; Bueno de Mesquita et al. 2002, 2003; Piketty 1995). In settings where government programmes are part of widespread systems of patronage and clientelism, assessments about government performance may turn negative. The direction of the effect of welfare programmes on political attitudes is ambiguous under this second set of theories. Welfare transfers may lead to positive attitudes when citizens reward politicians for delivering on the electoral promises they voted for. However, although in the short term government expenditures may buy votes and support, ensuing forms of corruption and clientelism

may generate social tensions and discontent when welfare spending is not associated with political reform (World Bank 2017). Some studies have argued this to be the case in Syria and countries affected by the 'Arab Spring' events (Devarajan and Ianchovichina 2017) and by recent social upheaval in Latin America (Justino and Martorano 2019), where lack of political reform failed to address the expectations of ordinary citizens even as welfare spending increased.

Many governments make use of welfare programmes to 'win' and/or 'buy' hearts and minds. A large literature has analysed the effects of government welfare spending on political attitudes, preferences, and outcomes in the USA and Europe (for instance, Persson et al. 2000; Soss et al. 2007; Rothstein 2011; Chen 2013; Stokes et al. 2014). The recent expansion of cash transfer programmes across most developing countries has led to the emergence of new research in other parts of the world, particularly in Latin America where these programmes started. Studies in Latin America have found that cash transfer programmes have led to increases in political support for incumbent governments in Brazil (Zucco 2013), Colombia (Baez et al. 2012), Honduras (Linos 2013), Mexico (Diaz-Cayeros et al. 2012; De La O 2013), and Uruguay (Manacorda et al. 2011). Few studies find explicitly that this result is compatible with programmatic rather than clientelist politics (De La O 2013, Zucco 2013). There has, however, been limited research on the effects of these policy interventions on political outcomes in developing countries outside Latin America and on attitudes beyond voting outcomes. However, while cash transfer programmes were started in Latin America, they have expanded dramatically across Africa and some parts of Asia during the last decade (Fiszbein et al. 2009). The political effects of such expansion in cash transfers have remained under-researched. Cash transfer programmes have been also widely promoted in policy circles over the last few years as a way of strengthening the legitimacy of state institutions in conflict-affected and fragile countries, but this policy impetus has been supported by very limited research (UNHCR 2012; Ghorpade forthcoming).

This paper advances this literature by providing evidence on the effects of a large cash transfer programme implemented in Pakistan—the Benazir Income Support Programme (BISP)—on citizens' attitudes towards the government. Our main results show that cash transfers in Pakistan have been associated with positive views of the government among BISP beneficiaries when compared to non-beneficiaries. Our causal interpretation of this effect draws on the use of a discontinuity in programme eligibility based on an arbitrarily defined poverty score. This is an important contribution of the paper since causal inference at the household level is generally difficult to establish. De La O (2013) makes use of experimental data collected during the implementation of the *Progresa* programme in Mexico to explore the causal effects of the programme on voting outcomes but focusses on precinct- and village-level outcomes. Zucco (2013) discusses the effect of *Bolsa Familia* in Brazil on voting outcomes and turnout across municipalities and at the individual level using data matching methods rather than an exogenous source of policy change. Closer to our paper, Manacorda et al. (2011) use a research discontinuity design based on an exogenous eligibility score (similar to the poverty score used in the BISP in Pakistan) to assess the impact of the PANES programme in Uruguay on political attitudes at the household level. This paper focusses, however, on a short-lived programme (2005–07) designed to ameliorate the effects of the 2001–02 economic crisis. Our paper uses the change from politically nominated to proxy-means test (PMT) targeting methodology along with the exogenously determined eligibility PMT cut-off of the flagship BISP to determine causal effects of the cash transfer on political attitudes and likely mechanisms underlying such effects.

Distinguishing between the two sets of theories outlined above—cash transfers as a form of strengthening the social contract within programmatic politics or cash transfers as a form of vote buying—is not straightforward because both motivations entail some form of cash in exchange for political support (Stokes et al. 2014). In order to attempt to disentangle the two channels, we exploit a change in the targeting of the programme in 2011, when PMT-based targeting replaced

the earlier modality of nominations by parliamentarians. This switch in targeting modalities allows us to explore whether transfer-induced political attitudes may be due to changes in the perceived social contract between citizens and the government of Pakistan or reflect rewards towards clientelist politics. Although the data we use are not detailed enough, we find strong suggestive evidence that clientelism may explain this result: the analysis shows that the positive attitudes towards the government among BISP beneficiaries is present only when the programme has been in place in communities for over two years, which coincides with the switch to PMT-based targeting. The main result seems therefore to be driven by better connected and politically important communities that were favoured by incumbent parliamentarians (regardless of party affiliation) for programme rollout before the introduction of objective targeting criteria. In addition, we find no evidence that social contract mechanisms—in the form of better economic prospects or improvements in state capacity—drive the positive effect of cash transfers on government perceptions in Pakistan, except for a small effect on improved security perceptions. Robustness tests establish the exogeneity of the discontinuity in the eligibility criteria of the pre-determined poverty score and show similarity between the samples of households just below and above the poverty score. The main results also hold for the full sample.

Pakistan is an important case study to address this research question. Pakistan is at the centre of global security and development concerns, having grappled with several political conflicts with regional and global implications since its independence in 1947. These have ranged from the contestation of the Kashmir border with India to rises in sectarian violence in FATA (Federally Administered Tribal Areas), Balochistan, and Khyber Pakhtunkhwa since the involvement of Pakistan in the US-led War on Terror in 2001. Violence since 2010 has been particularly pronounced, in large part due to the confrontation between Islamist militant groups affiliated with the Tehrik-e-Taliban Pakistan (TTP, or Pakistani Taliban) and the state. Although the BISP was implemented to address chronic poverty in vulnerable areas, there was a strong expectation that the programme would also act to support state-building aims of the Government of Pakistan and reduce violent conflict. For instance, GoP (2010: 145) states:

> Conflict in NWFP [North-West Frontier Province, now Khyber Pakhtunkhwa], FATA and Balochistan has severely challenged the ability of the state as well as the legitimacy of the idea of a functioning state in Pakistan. Social protection must be part of the strategy to reclaim the space and legitimacy for the state in Pakistan, through protection to the basic entitlements of people in the conflict-affected areas […]. Expanded social protection programmes, particularly directed at the conflict-affected areas are essential to protect innocent victims of conflict, *and to regain legitimacy for the idea of a functioning state through creating, expanding and ensuring the delivery of citizenship-based entitlements* (our italics).

This reflects a growing perception in policy circles that cash transfers can support peace- and state-building objectives. However, the record of such policies has been mixed. A study in Afghanistan showed that income support programmes led to more favourable views about the governments in areas with initially high levels of violence (Beath et al. 2014), while Crost et al. (2016) found that cash transfers reduced insurgent violence in the Philippines. Two other studies noted that income support programmes can nudge civilians to share information with government forces, thus increasing the state's ability to defeat rebel groups in Iraq (Berman et al. 2011) and India (Khanna and Zimmermann 2014).[1] In contrast, other studies have shown a negative impact of economic

[1] Some studies have also linked the implementation and expansion of national welfare programmes to reductions in social and political conflict (Azam 2001; Justino 2015; Justino and Martorano 2018, 2019). Blattman et al. (2017) report

aid transfers on conflict and violence (Crost et al. 2014; Nunn and Qian 2014). Whether and how the BISP has strengthened the social contract between citizens and the government in Pakistan remains unknown—and is the focus of this paper.

The paper is organized as follows. Section 2 outlines briefly the political context in Pakistan, the process that led to the implementation of the BISP, and the data that will be used in the paper to analyse the questions above. Section 3 discusses the identification strategy and main results, while Section 4 focuses on the mechanisms. Section 5 shows the validity of the main results across several robustness tests. Section 6 concludes the paper.

2 Context and data

Pakistan's return of democratic rule in 2008 with the victory of the Pakistan People's Party (PPP) saw a focused effort by the PPP to leave their mark, most visibly by launching the BISP, named after its former leader Benazir Bhutto who had been assassinated just before the elections. The programme has three explicit aims: to eradicate extreme poverty, to empower women, and to achieve universal primary education (Ambler and de Brauw 2017). While state-building or conflict reduction were never stated as direct or explicit aims of the BISP, the programme was also expected to serve as a means of redressing alienation and the potential for future conflict—as outlined in official sources (GoP 2010).

The BISP is administered by the Government of Pakistan with technical and financial support from international donors, including USAID and the World Bank. The implementation of this large programme at a time of violent conflict across several parts of Pakistan provides a setting for studying how cash transfer programmes may affect attitudes of citizens towards the state. Payments amounting to PKR 1,000 (US$6.5) per month were made to women beneficiaries in eligible households at quarterly intervals. For the full sample, the BISP monthly transfer represented 5.9 per cent of total household expenditure; for recipient households it was 6.3 per cent.

The data we use is the Pakistan Rural Household Panel Survey (RHPS) collected by the International Food Policy Research Institute (IFPRI and IDS 2016).[2] This is a household survey dataset comprising two rounds, collected in 2012 and 2013, and covering 1,873 households across three of Pakistan's four main provinces. In each household, one male and one female respondent were interviewed. In addition to standard socio-economic modules on income, occupation, demographics, education, wealth, and expenditure, the second round of the survey contained data on the receipt of social protection and aid programmes (including on the BISP) and a detailed module on political attitudes. These data allow us to examine the effect of the receipt of cash transfers on citizen attitudes towards the state and its institutions, after suitably controlling for household and individual socio-economic characteristics.

We measure political attitudes using two questions in the survey. The first question asked each respondent about their overall level of satisfaction with the government. The second question asked about their level of satisfaction with the military. Respondents were asked to rate their

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a positive effect of cash payments on crime and violence among criminally engaged young men in Liberia. This literature is surveyed in Justino (2019) and Verwimp et al. (2018).

[2] This dataset has been used in several academic papers including Ambler and de Brauw (2017), Healy et al. (2017), and Kosec and Mo (2017).

satisfaction with the government and with the military on a seven-point scale, ranging from extremely dissatisfied (1) to extremely satisfied (7). These responses were converted into a dichotomized variable, where responses 'extremely dissatisfied', 'moderately dissatisfied', 'slightly dissatisfied', and 'neither satisfied nor dissatisfied' were coded as 0. Distinctly positive expressions of satisfaction (slightly/moderately/extremely satisfied) were coded as 1.[3] Table 1 lists the specific questions from the survey that form our main dependent variables, as well as sample means for each variable. The table shows that recipients of the programme are generally more satisfied with the government (36.3 per cent, versus 29.2 per cent among non-recipients). There are no substantial differences in attitudes between recipients and non-recipients with respect to the military. Similar results were observed when restricting the sample to the middle 50 per cent (which will be discussed later in the empirical analysis).

3 Identification strategy and main results

Establishing a causal relationship between cash transfers and political attitudes using household-level data is not a straightforward exercise. This is because a simple ordinary least squares (OLS) association between BISP receipts and household political attitudes may be biased, as cash transfers may selectively reach households or communities that are better disposed towards the government. Alternatively, BISP receipts can be selectively targeted to areas where support for government is known to be low. In order to address these endogeneity concerns, we exploit the eligibility criteria of the BISP based on a poverty scorecard—a PMT for household economic wellbeing. This poverty score is calculated by using the Poverty Census conducted in 2011 by the BISP, which was used to establish eligibly criteria and target relevant households. Households benefitted from the programme if they were below the strict cut-off of a score of 16.17.[4] Those above this cut-off threshold were not deemed eligible for the transfers. This sharp change in the likelihood of programme receipt at an arbitrary cut-off of 16.17 lends itself to a regression discontinuity design-based evaluation of the causal effects of the programme, since households that lay just above the locally random cut-off, and therefore were ineligible to receive BISP transfers, can serve as a good control group for households just below and which were eligible to receive the transfers.

We have been unable to directly observe the actual scores assigned to households through the poverty census conducted for the BISP in 2011. As a next best alternative, we reconstructed the poverty score based on survey data collected in 2013 (during the second round of the RHPS) using the same formula for the poverty score as was used in the 2011 Poverty Census of the BISP.[5] We then used the predicted eligibility status using the reconstructed score as an instrumental variable (IV) for actual programme receipts in the vicinity of the cut-off of the poverty score that determines eligibility. As Figure 1 shows, households below and above the cut-off score (16.17) have very similar distributions of monthly household consumption expenditure.

The indicators used in the construction of the poverty scorecard comprise correlates of chronic poverty such as land and asset ownership, the number of dependents in the household, education

[3] Robustness tests presented in the online appendix (Table A9) present estimates using the full (non-dichotomized) 7-point scale.

[4] BISP (n.d.).

[5] The formula used by BISP for generating the poverty score was provided to the authors by the World Bank. Poverty scores were replicated using the same formula (its constituent indicators and associated weights), applying it to household survey data collected in 2013.

levels of the head of the household, children's school attendance status, and room ratio and toilet access in the dwelling. These variables were chosen because targeting a long-term poverty alleviation programme using PMTs requires a clear identification of the chronic, rather than the transient poor. Therefore, these indicators are expected to continue to predict poverty status long after the period in which the poverty census survey was conducted. In other words, we assume that household poverty scores in 2011 are strongly and positively correlated with scores in 2013.

Conceptually, the predicted eligibility for the BISP based on the poverty score computed using 2013 data should satisfy the requirements of a strong IV for programme eligibility in 2011 under two conditions which we argue are reasonably likely. The first condition is that predicted eligibility based on scores calculated using the 2013 survey data is strongly (positively) correlated with actual eligibility determined by (the unobserved) poverty scores using the 2011 poverty census data. The second condition (the exclusion restriction) is that, in the vicinity of the cut-off score, predicted eligibility in 2013 affects political attitudes of households in 2013 only through the actual receipts of the BISP transfer—which is our causal channel of interest.

The first condition may not hold if poverty status between 2011–13 changes as a result of the BISP programme—such that households that were poor in the past are no longer so because of the programme. Prima facie, this may not seem an unreasonable outcome of an income support programme. However, as we argue above, the BISP scorecard focuses on identifying the chronic poor—whose long-term poverty status is not expected to change drastically in response to the income support received from BISP. This is reflected in the indicators used to construct the poverty scorecard (correlates of long-term economic wellbeing) and is further strengthened by the fact that between 2011–13 the programme did not undertake any recertification or revalidation of beneficiary status because of any expected upheavals in the composition of the chronic poor.[6]

Despite these reassurances, it is possible that three categories of households, which may have witnessed a change in the poverty score between 2011–13 as a result of the BISP programme, could potentially violate our identification assumptions. The first case is that of households that saw improvements in the poverty scores between 2011–13 and did not change their eligibility, i.e. the improvement did not change their poverty scores' position in relation to the cut-off score. This case has no serious implications for identification since scores in 2013 are strongly correlated with scores in 2011. The second is households below the cut-off in 2011 (and therefore eligible for BISP) that experienced an improvement in their economic conditions because of the positive effects of the programme, resulting in a new poverty score in 2013 that is higher than the cut-off. In this case, any effects we may find of the programme on political attitudes can be thought of as a lower bound to the true estimates. This is because such households will be deemed ineligible using 2013 scores whereas they in fact would have been eligible in 2011. This will lower the observed magnitude of the estimated treatment effect but will not change its direction. A third possible category would comprise households that were above the cut-off in 2011 (and therefore ineligible) and for whom the programme caused a worsening of their economic situation and a fall in the poverty score below the cut-off, making them potentially eligible in 2013. As we cannot identify any plausible mechanism through which the programme could cause such a worsening of poverty score among initially ineligible households, and in the absence of any evidence of any

[6] Donor notes confirm the lack of recertification between 2011 and 2014: www.iati.dfid.gov.uk/iati_documents/8492209.odt.

negative effects of the BISP programme on beneficiary and non-beneficiary households in general,[7] we rule this out as a possible threat to identification.

The salience of the exclusion restriction condition is relatively more straightforward. While household wealth or poverty can be directly correlated with political attitudes and support for government (Gelman et al. 2008; Bartels 2009), in our formulation poverty status (and implied programme eligibility) is established not by the direct extent of household poverty measured along a continuum, but by whether the household's poverty status falls above or below an arbitrarily set cut-off of the poverty score. The locally arbitrary nature of the eligibility threshold therefore satisfies the exclusion restriction requirement of the IV. In other words, *in the vicinity of the poverty score cut-off* used to determine eligibility, the predicted eligibility is treated as randomly assigned, allowing an estimation of the causal effects of BISP receipts on respondent attitudes. Since predicted eligibility varies from actual eligibility due to time lapse since the BISP poverty census, or due to random error in either the BISP poverty census, the RHPS survey, or both, we use predicted eligibility as an instrument for actual eligibility observed as the receipt of BISP transfers at the household level. To enforce the validity of the IV in the vicinity of the cut-off score, we restrict analysis to respondents whose household poverty score lay in the middle 50 per cent of the poverty score distribution, i.e. between the values 6 and 27, as shown in Figure 2.[8] Using an OLS regression of household monthly per capita expenditure on the poverty score, we find that the eligibility cut-off of the poverty score of 16.17 corresponds to a monthly per capita expenditure level of PKR 2,737 (US$27.93) in the sample.[9] The poverty score values of 6 and 27 correspond to household monthly per capita expenditures of PKR 2,147 (US$21.91) and PKR 3,365 (US$34.34), respectively.

The causal relationship of interest is expressed as:

$$Y_{ij} = \alpha + \beta_1 BISP_j + \varepsilon_{ij} \quad (1)$$

where Y is the dependent variable for individual *i* in household *j*. This is a dummy variable with value one for a favourable attitude towards the government in response to the two satisfaction questions outlined in Table 1. BISP is a dummy variable for household *j* that received the BISP transfer. Since the poverty score computed is a complex combination of several household and individual characteristics, we expressly exclude controls from the estimation of (1) to avoid multicollinearity. However, as a robustness test, we later show that the main results are robust to the inclusion of a set of controls. The simple probit estimates of the effect of BISP receipts on attitudes towards the government are presented in Table 2. In order to ensure comparability with results from the IV analysis, we restrict the analysis to the middle 50 per cent distribution of the BISP poverty score. The results show that BISP receipts are correlated with higher levels of satisfaction with the national government. There is no significant association between BISP receipts and attitudes towards the military.

We discussed how BISP receipts may be endogenous to political attitudes because transfers may be targeted to reward those with favourable, or win over those with unfavourable, attitudes towards the government. Therefore, we use the predicted enrolment in the BISP along the arbitrary eligibility threshold of 16.17 in the poverty score as an IV for actual programme receipt

[7] For instance, OPM (2014) provides a comprehensive review of programme impact and finds no evidence of any adverse effects.

[8] A limited sample size prevents us from choosing a narrower band.

[9] On 14 March 2013, the exchange rate was US$1 = PKR 97.98.

in the vicinity of the cut-off score estimates causal effects. The IV framework follows a standard two-stage procedure. The IV first-stage equation is as follows:

$$BISP_j \quad = \alpha \quad + \gamma_1 ELIG_{2013j} + u_{ij} \tag{2}$$

where $ELIG_{2013}$ is a dummy variable for a household deemed to be eligible to receive BISP based on the poverty score calculated using 2013 survey lying below the arbitrary cut-off of 16.17. Table 3 shows the first-stage IV estimates. These results indicate a positive and significant association between predicted programme receipts and actual programme receipts, as expected. The F-stat value of 16.58 also suggests the absence of weak instruments.

The IV second-stage equation below estimates $\beta_1{}'$, the instrumented (and therefore causal) effect of BISP receipt on individual attitudes:

$$Y_{ij} \quad = \alpha \quad + \beta_1{}' \widehat{BISP}_j + \varepsilon'_{ij} \tag{3}$$

Table 4 shows that the receipt of the BISP transfer results in a significantly higher likelihood of respondents expressing greater satisfaction with the government. No significant effects are observed on attitudes towards the military.

4 Mechanisms

What explains the results above? As discussed in the introduction, a large literature has argued that welfare programmes may be used by incumbent governments to strengthen the social contract with their citizens and ensure their support and approval. Cash transfers in Pakistan may have thus led to improvements in satisfaction with the government because they were perceived to proxy for improvements in the capacity and commitment of state institutions to ensuring better living conditions. In the case of countries affected by conflict and violence, such as Pakistan, this may include not only economic conditions but also better security. Alternatively, the effects we report above may reflect existing clientelist relations whereby citizens express satisfaction with politicians fulfilling their promises. We explore below these two alternative interpretations of our main results.

4.1 Social contract mechanisms: 'winning' hearts and minds

Table 5 reports the effect of the BISP on a range of economic indicators at the household level. The results show that, in the sample under consideration and using the same IV estimation of causal effects as above, the BISP had no effect on households' exposure to shocks, their resorting to harmful coping strategies in the 12 months preceding the survey, self-reported improvements in the households' financial condition over the past two years, or positive prospects for an improved economic situation in the near future. In fact, the latter two coefficients are negative. In Table 6, we report levels of satisfaction with the government among BISP recipients that experienced (or not) an economic shock, adopted (or not) harmful coping strategies, think that their economic situation is better (or worse) than two years ago, and expect (or not) their economic situation to improve. It appears that there is no causal effect of the BISP on political attitudes through pathways related to the improved financial conditions and expectations, or more effective social protection.

Despite the lack of results above, it is possible that the effects of the programme on overall assessments of the government are linked to changed attitudes towards underlying institutions that constitute the citizen–state relationship on a day-to-day basis rather than based on private

economic benefits. This analysis is shown in Table 7 and includes perceptions about security provision, police (one of the most visible arms of the state in Pakistan), law and order, influence of the government in community affairs, and helpfulness of the government and police in solving land disputes and addressing security concerns. Overall, we find no positive effects of BISP receipts on strengthened trust in the underlying state institutions. In fact, somewhat puzzlingly, BISP recipients are less likely to have faith in government institutions for solving land disputes and addressing security concerns in the community.

It is also possible that beneficiaries of the BISP may have a higher opinion about the government not because their own living conditions have improved but because the economic standards of their communities may have improved. The Planning Commission of Pakistan called upon the government to focus efforts to extend social protection programmes, including the BISP, in districts with poorer development indicators—deemed to render them vulnerable to radicalization (GoP 2010: 211). Districts in the Punjab and Sindh provinces with lagging development indicators were described as 'breeding grounds of alienation and conflict' and should therefore '… be designated as Nation-Building Regions of Pakistan, which must receive priority support in social protection programmes and policies' (GoP 2010: 145). Intensifying cash transfers in order to improve the capacity and reach of state institutions in lagging districts could lead to higher levels of political support for the government if it signals a positive effort on part of the latter. However, the prevailing conditions in such areas may make it harder to achieve such goals because of longstanding alienation and suspicion of the state. We examine which of these two possible factors may be at play by examining the differentiated impact of the BISP on attitudes towards the Pakistani government in the deemed 'nation-building' districts and other districts. Table 8 shows that the positive effects of BISP on attitudes towards the Pakistani government that were observed in Table 3 are driven not by the 'nation-building' districts, but by other districts—which are relatively more prosperous. In the lagging areas, the programme does not appear to have had any effect.

This result in not entirely surprising. While the government's assertions indicate a clear aim to link social protection with political aims, including quelling alienation and fostering support for itself, the implied direct link between lagging development and extremism is somewhat tenuous. Under-development has several implications for the successful implementation of cash transfer programmes and their ability to influence people's attitudes towards governments—beyond any possible links it may have with state-building objectives. A designated 'nation-building' district may in fact display several other characteristics due to which it may be harder for programmes in such areas to achieve changes in political attitudes among recipients—such as greater insularity and longstanding neglect making these areas less responsive (or slower to respond) to government outreach.

Even if the cash transfers do not reflect stronger state capacity, could they still indicate better physical security provision? We examine whether the effects observed in Table 3 are driven by respondents who expressed feeling unsafe at either a within-community or outside-community destination, or during social and religious events.[10] We find that the effects observed in Table 3 are stronger for respondents who report feeling unsafe at one or many locations within or outside their community (Table 9). This effect is only marginally significant (at the 10 per cent level) and could suggest that increased state presence may have improved security perceptions among those

[10] This includes homes, farms, and markets within the village and in neighbouring villages, wholesale markets, worksites, district/provincial hubs, and going out of the house to attend school, religious events, funeral, or other processions

that benefitted from the BISP. It may also be that the BISP generated reductions in violent conflict in the areas where it was implemented, which is an interesting question for future research.

4.2 Clientelist mechanisms: 'buying' hearts and minds

We can test for the clientelism hypothesis by taking advantage of a change in the targeting of the BISP that took place in 2011. The PMT-based targeting methodology was introduced only in 2011. Prior to that, between the years of its initiation in 2008 and the switch to PMT-based targeting starting in 2011, beneficiary selection for BISP was left entirely to the discretion of elected Members of National Assembly (MNA—lower house of parliament), who were assigned quotas to nominate 8,000 households from their parliamentary constituencies that they deemed poor/vulnerable, and therefore deserving of such assistance, regardless of their own political affiliation. No specific criteria for inclusion of beneficiaries were stipulated (OPM 2014; Gazdar 2011), and the exclusion criteria that were applied were hardly binding,[11] leaving parliamentarians free to exercise their own discretion (Haseeb and Vyborny 2016) and allowing them to potentially use programme benefits to extend patronage and reward selected, loyal constituents. For instance, while comparing beneficiary lists before and after the introduction of the PMT-based targeting methodology in Punjab (Pakistan's most populous province), Haseeb and Vyborny (2016) found that the switch to PMT-based targeting led to the disqualification of 75 per cent of BISP recipients who had been nominated by MNAs. They also found that before the PMT-based targeting was introduced, households in incumbent parliamentarians' home villages were 200–400 per cent more likely to receive BISP transfers than households in the home villages of the MNAs' nearest electoral rival in the 2008 National Assembly elections. This alludes to the strong presence of favouritism in the selection of BISP beneficiaries before the introduction of the PMT, and a considerable reduction thereafter.

Against this backdrop, the presence of BISP in communities in our sample prior to 2011 suggests that such communities may have been more important to parliamentarians from a political perspective, as households from such communities were chosen by them to receive benefits that need not have been guided by objective assessments of poverty and need. In other words, the presence or absence of BISP in communities prior to 2011 reflects a possible marker of heterogeneity, in terms of the political importance of communities, to either reward loyalist or win over opponent groups through the selective offering of BISP. Additionally, the presence of BISP before 2011 could also indicate greater social proximity between the community and the parliamentarian, or a greater ability of the community to pressure the representative for benefits. As we do not know since when a particular recipient household has been receiving BISP transfers (i.e. whether before or after the switch to PMT, or indeed whether current non-recipients were receiving BISP transfers before 2011), we cannot disaggregate effects at the household level to test whether households' own proximity to the MNA explains their favourable view of the government. We can, however, exploit the change in programme targeting in 2011 to investigate whether the effects of the cash transfer programme on attitudes towards the government may be affected only in communities that are higher priority for the incumbent parliamentary representative due to political reasons.[12] To that purpose, we disaggregated the effects in communities where BISP was present before 2011 (indicating greater proximity to the MNA) and

[11] Including the possession of machine-readable passports, foreign bank accounts, emigrant ID cards, and public sector employment.

[12] BISP was launched in 2008 after Pakistan's democratic transition. However, its implementation expanded gradually over the years resulting in considerable variation in exposure to the programme in communities in the sample. Further, the recertification exercise using the 2011 PMT census would have changed the profile of communities with(out) any BISP recipients.

where it was present only after. Table 10 shows that the effects observed in Table 3 are indeed driven by respondents in communities where the programme was present before 2011.

These results are further strengthened when we calculate how household satisfaction with the government differs across 'nation-building' and 'non-nation-building' districts before and after the switch to PMT in 2011. These results, shown in Table 11, indicate that satisfaction with the government among BISP beneficiaries is more prominent in the more prosperous (non-nation-building) districts before the introduction of the PMT-based eligibility in 2011.

While we cannot test whether the impact of BISP on attitudes towards the national (PPP-led) government is stronger among PPP voters (as the data does not ask about actual or intended voting behaviour), we are able to examine whether the impacts are stronger in PPP or in opposition-held National Assembly constituencies. This is particularly significant because in the BISP's initial rollout, MNAs directly elected to parliament were asked to nominate beneficiaries, before the PMT-based eligibility was introduced. If the effects of BISP on a favourable opinion of government is stronger in constituencies held by the PPP, especially in communities where the BISP was present prior to 2011 (suggesting the communities' higher political importance for the MNA), it could suggest that the BISP enhanced the people's support for government in areas where they were already pre-disposed to be close to the party leading the government. If, however, the effect is stronger in opposition party-held constituencies, it could suggest that the BISP enhanced the favourability of the government in areas where the PPP had not been elected to parliament.

As Table 12 shows, there appear to be no significant differences in effects between PPP- and opposition-held National Assembly constituencies at the aggregate level or when disaggregated by communities within constituencies where the BISP was present before or in/after 2011. However, within both PPP and opposition constituencies, effects appear stronger in communities where the BISP was present before 2011. While this result does not constitute conclusive evidence, it does suggest that the BISP increased satisfaction with the government in communities that were relatively more important to the elected representative (the MNA), regardless of their party affiliation. The higher importance of such communities for the MNA could be because of several factors, including shared ethnic or other identity, proximity to the MNA's native village, or higher voting or public engagement. We are not able to measure these factors distinctly to test their relative importance. However, as the BISP improves the perception of government across communities with stronger connection to the elected representative, even if they are from an opposition party, voters' general political engagement and ability to pressure any incumbent may be a stronger factor than the incumbent's affiliation with the party in power. The fact that the BISP is (and is viewed as) a federal programme, possibly connects BISP receipts with improved perceptions of the (then PPP-led) national government, more so in politically important and connected communities, and regardless of the party affiliation of the incumbent MNA. In other words, the BISP was able to generate goodwill for the government among politically engaged voter communities, in a manner that was not mediated by the political affiliation of the elected representative.

It is, of course, possible that the results above simply indicate that limited duration of exposure to a safety net programme that seeks to provide households with a modicum of assured income support to meet basic needs and smooth consumption would not allow beneficiaries to appreciate the full impact of such a programme, which could in turn make them supportive of the programme and the government. It is also possible that it will take time before beneficiaries fully attribute potential welfare benefits from the programme to the incumbent government (Zucco 2013). Therefore, only after sufficiently long exposure to the recurring cash transfer could it be expected to improve attitudes towards the government. The switch in satisfaction outcomes in 2011,

interpreted alongside the lack of results on the various social contract mechanisms, suggests that this may not be the case. In order to test this further, we repeat the analysis in Table 5 by splitting the economic effects pre- and post-2011. Table 13 shows no causal economic effect of the BISP pre- or post-2011 on households through pathways related to the improved financial conditions and expectations, or more effective social protection, suggesting that the clientelist interpretation may hold more convincingly than any interpretation related to improvements in the social contract.

5 Robustness analysis

The analysis conducted in the previous two sections shows that the BISP has led to positive attitudes towards the government among beneficiary households, likely as a reward to political actors in communities favoured by them before the programme switched to objective targeting methods. In this section, we test the validity of these results further by: (i) assessing whether the results may be due to an artefact of the choice of a specific cut-off of the reconstructed poverty score to determine eligibility; (ii) testing whether the results hold in narrower intervals of the sample close to the eligibility threshold; (iii) examining whether BISP receipts had any effect on attitudes towards political questions that we do not, prima facie, expect to change in response to BISP; and (iv) disaggregating the results by gender. We also run a series of alternative model specifications.

Alternative poverty score cut-offs. We examine how sensitive our results are to alternative eligibility-determining poverty score cut-offs in Table A1 in the online appendix. We find that the coefficient remains statistically significant as long as the threshold is between 15 and 20 (with the exception of the value of 19). The coefficient remains positive—even if not statistically significant—across alternative integer value cut-offs between 8 and 24. The loss of statistical significance may be partly due to a relatively more lopsided distribution of eligible households as we move away from the median poverty score of 16 in the 8–24 range,[13] and a reduced number of either eligible or ineligible households in the sample. This confirms that the significance of the results is robust to reasonable deviations of the eligibility threshold from the administratively set value of 16.17, within the poverty score range under consideration, while the direction of effects remains positive across wider thresholds.

Narrower sample intervals. The causal interpretation of our results depends on the exogenously determined cut-off value of the BISP poverty score to determine eligibility. In other words, it depends on whether, in the neighbourhood of the cut-off (i.e. around the value of 16.17), households are very similar to each other and that BISP receipts within that neighbourhood are close to random. We, therefore, confine our analysis to the middle 50 per cent of the sample around the cut-off score, corresponding to poverty scores between 6 and 27. While a narrower neighbourhood around the cut-off score would provide more precise estimates as the distinction between households becomes more random in smaller intervals, in a survey setting this comes at the cost of a sufficiently large sample size in the interval to test hypotheses. In Table A2 (in the online appendix) we estimate the IV regression coefficients in progressively narrower poverty score bands, starting from the 6–27 score interval that covers 54 per cent of sampled respondents, to the very narrow 16–17 score interval covering 3.5 per cent of the sample. We find that the coefficient on the dummy for overall satisfaction with government loses statistical significance in narrower bands as N decreases, but its sign and magnitude remain comparable in bands that

[13] Setting the eligibility threshold below 8 or above 24 within the 6–27 range provided too few observations for meaningful analysis.

include at least 30 per cent of the total sample respondents. Only when the band is as narrow as to contain only about 16 per cent of the sample (score 13–20) or smaller, does the coefficient change sign. Overall satisfaction with the military remains statistically non-significant in all poverty score bands.

Placebo test. Even though we are confident that our results indicate a causal effect of the BISP on political attitudes, it is still possible that the results may be driven by potential general equilibrium effects whereby other economic or political changes in Pakistan at the same time as the BISP was implemented may have led to positive attitudes towards the government. As a placebo test, we examine whether BISP receipts had any effect on perceptions about government performance and other political attitudes that are not expected to change in response to BISP. This analysis is shown in Table A3 (online appendix) and includes governments' support for independence in Kashmir (an ongoing issue in Kashmir that stirs strong opinions among the electorate), government's actions against extremist groups (which is also a strong determinant of how people assess government performance in Pakistan, particularly since 2001), and observance of purdah among women and honour killings, a gender-related issue that has generated much debate in Pakistan in recent years. While a cash transfer programme implemented by the national government can signal the responsiveness of the latter to the economic situation of the people, these transfers should not have an effect on wider political and social actions by the government. Indeed, we find that the BISP has no effect on any of these variables, giving us greater confidence in the validity of our main results.

Gender disaggregation. We also assessed whether our main results are affected by the gender of the respondent since each household had two respondents: one male and one female. Table A4 in the online appendix examines differences in political attitudes caused by BISP, by gender. Higher satisfaction with government causally associated with BISP appears to be driven by men more than women, although a reduced sample size possibly also reduces the precision of the coefficients. No statistically significant differences are observed between the effects on men's and women's attitudes with respect to the military.

Alternative model specifications. We also find that our main results are robust to several alternative model specifications including: (i) estimating IV effects for the full sample (Table A5 in online appendix); (ii) clustering standard errors at the village level (Table A6 in online appendix); (iii) using the recalculated (continuous) BISP poverty score (rather than the binary eligibility predicted by it) as the IV for BISP receipts (Table A7 in online appendix); (iv) assigning a higher weight to observations closer to the cut-off (Table A8 and Figure 1 in online appendix); and (v) replacing the dichotomized measure of satisfaction with government and military with the original 7-point Likert scale variable (Table A9).

6 Conclusion

Cash transfer programmes have been heralded as a means to win hearts and minds and strengthen the social contract between governments and citizens in countries affected by violent conflict. This paper attempts to isolate the causal effect of a cash transfer programme implemented in Pakistan—the Benazir Income Support Programme (BISP)—on individual political attitudes. The empirical identification leverages a discontinuity of the programme around a poverty threshold to examine differences in political attitudes among programme beneficiaries and a control group that did not benefit from the programme because its income was just below the poverty score used to target the cash transfers.

The analysis conducted in the paper shows that the programme led to improvements in attitudes towards the Government of Pakistan. When looking in more detail at heterogeneous effects of the programme, we find, however, that cash transfers improved attitudes towards the state but without any visible improvements in state capacity or the economic and security prospects of programme beneficiaries. We find that satisfaction with the government is conditional on the programme being in place for over two years, which coincides with the switch to PMT-based targeting from the earlier modality of nominations by parliamentarians. We interpret this result as indicating that government satisfaction among BISP beneficiaries is likely to be driven by better connected and politically important communities that were favoured by incumbent parliamentarians for programme rollout before the introduction of objective targeting criteria. This suggests that the more positive attitudes of programme beneficiaries (when compared to households just below the eligibility criteria) may reflect clientelist rewards towards politicians before the programme switched to objective eligibility criteria.

These findings challenge a longstanding view in policy circles that cash transfers may be used to improve relations between states and citizens in fragile states. In the case of Pakistan, this effect seems to be largely superficial and has no measurable impact on fundamental factors that shape the social contract between citizens and the state. This stands in contrast to other studies that have found positive effects of cash transfers on voting behaviour under programmatic politics (Manacorda et al. 2011) and on violence (Crost et al. 2016). These contrasting results suggest the need for more micro-level research on the relationship between income support programmes and political attitudes. Future research should, in general, pay attention to the social, political, and economic mechanisms that shape the relationship between income support and political attitudes, as well as the ways in which welfare programmes are delivered to different communities. The results in this paper suggest, in particular, that cash transfer programmes may need to be accompanied by changes in the underlying institutions of the state, an issue that needs urgent attention among scholars and policymakers.

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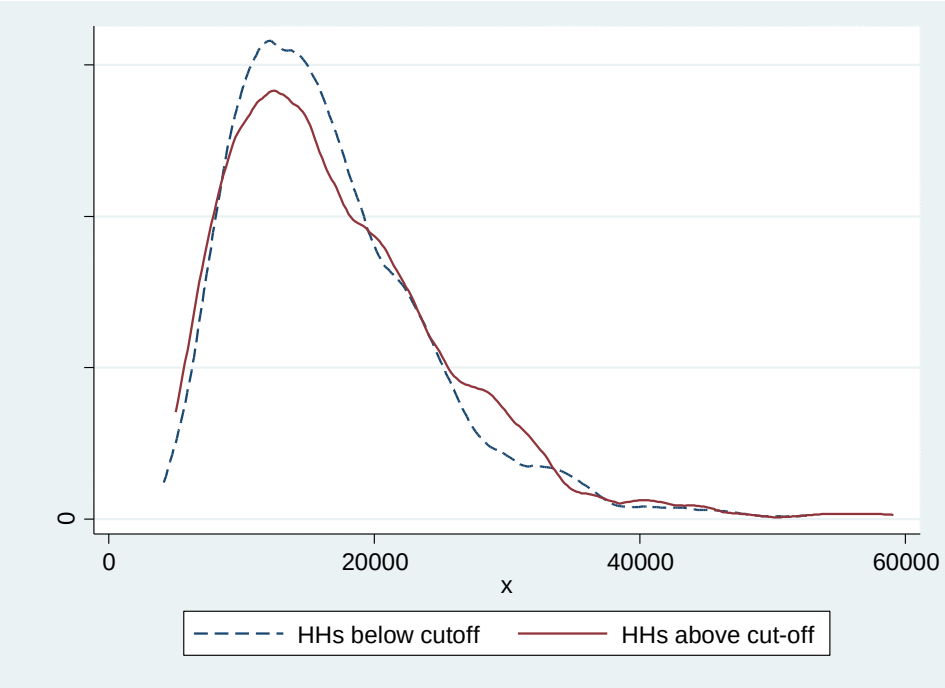
Figures and tables

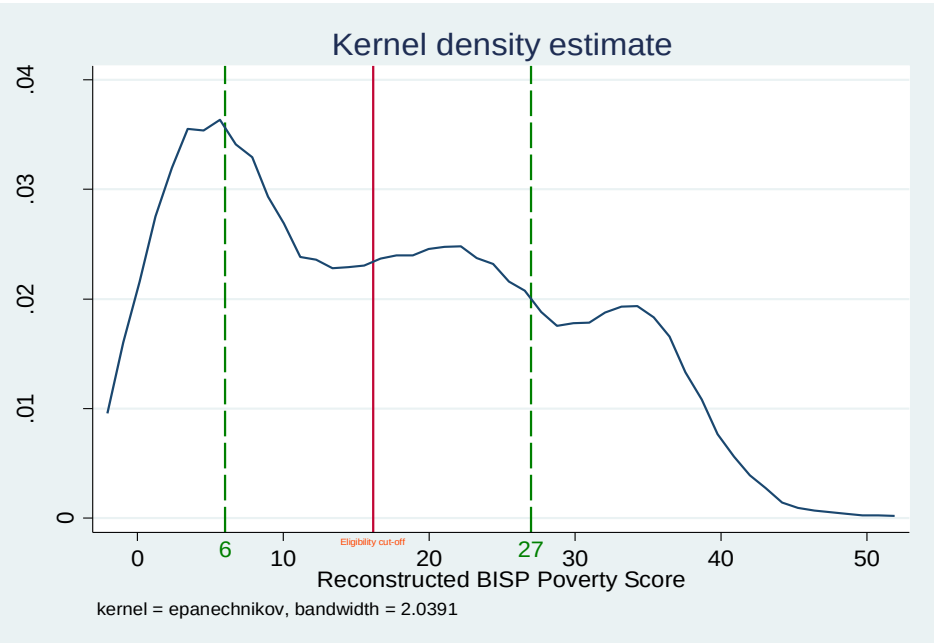
Figure 1: Kernel density of total monthly household expenditure (in PKR) for households in middle 50 per cent of the BISP score distribution (between values 6 and 27)



Note: Eligibility cut-off value is 16.17.

Source: Authors' calculations.

Figure 2: Kernel density of reconstructed BISP poverty score



Source: Authors' calculations.

Table 1: Main dependent variables

		Full sample			
Question		All	Non-recipient	Recipient	Diff
What is your overall level of satisfaction with the government?	1 = satisfied	30.31%	29.20%	36.33%	7.14%
What is your overall level of satisfaction with the military?	1 = satisfied	82.43%	82.56%	81.73%	-0.83%
N		3,907	3,335	572	
		Middle 50%			
Question		All	Non-recipient	Recipient	Diff
What is your overall level of satisfaction with the government?	1 = satisfied	29.82%	29.27%	33.39%	4.12%
What is your overall level of satisfaction with the military?	1 = satisfied	82.65%	83.04%	80.09%	-2.95%
N		2,126	1,850	276	

Source: Authors' calculations based on RHPS—Round 2 data.

Table 2: BISP receipt and attitudes towards the government: OLS estimates

	Overall satisfaction with government 1 = satisfied	Overall satisfaction with military 1 = satisfied
BISP recipient = 1	0.056*	-0.002
	(1.89)	(-0.08)
Mean (dep var)	0.307	0.838
N	2,125	2,125

Notes: *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table 3: IV first-stage estimation

	Coefficient
Predicted BISP receipt (1 = predicted eligible)	0.059***
Constant	0.099***
N	2,126
Adj. R-squared	0.0073
Cragg-Donald Wald F–stat	16.58

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table 4: IV estimates; second stage

	Overall satisfaction with government 1 = satisfied	Overall satisfaction with military 1 = satisfied
BISP recipient = 1	0.821**	-0.262
	(2.09)	(-0.93)
Mean (dep var)	0.307	0.838
N	2,125	2,125

Notes: *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table 5: Economic effects of BISP receipts

	HH faced any economic shock = 1	HH adopted a harmful coping strategy in response to shocks = 1	HH economic situation is better than two years ago = 1	HH expects economic situation to improve in the next 2 years = 1
BISP recipient = 1	-0.375	0.875	-0.460	-1.588***
	(-1.49)	(1.57)	(-1.31)	(-2.96)
Mean (dep var)	0.879	0.559	0.305	0.640
N	2,126	1,868	2,108	2,104

Source: Authors' calculations.

Table 6: Mechanisms—economic effects (dep var: satisfied with government = 1)

	HH experienced economic shock		HH adopted harmful coping strategy		HH economic situation is better than two years ago = 1		HH expects economic situation to improve in the next 2 years = 1	
	Yes	No	Yes	No	Yes	No	Yes	No
BISP recipient = 1	0.881*	0.702	0.465	1.923	1.921	0.666*	1.092	0.783*
	(1.72)	(1.26)	(1.04)	(0.98)	(1.05)	(1.73)	(1.43)	(1.71)
Mean (dep var)	0.306	0.318	0.318	0.291	0.304	0.308	0.314	0.292
N	1,867	258	1,045	822	642	1,465	1,346	757

Source: Authors' calculations.

Table 7: Mechanisms—perceptions about state institutions

	Satisfaction with govt to provide security for community	Satisfaction with police in community	Govt should make decisions on law and order	Govt should make decisions on community affairs	Govt will be helpful for solving land disputes	Govt will be helpful for addressing security concerns	Police will be helpful for solving land disputes	Police will be helpful for addressing security concerns
	1 = satisfied	1 = satisfied	1 = yes	1 = yes	1 = yes	1 = yes	1 = yes	1 = yes
BISP recipient = 1	-0.027	0.004	0.297	0.310	-0.773**	-0.683*	-0.047	-0.515
	(-0.07)	(0.01)	(1.02)	(0.90)	(-2.04)	(-1.80)	(-0.13)	(-1.47)
N	0.384	0.385	0.831	0.727	0.681	0.639	0.651	0.702
Mean (dep var)	2,125	2,125	2,125	2,125	2,126	2,126	2,126	2,126

Notes: *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table 8: Mechanisms—nation-building objectives

	Overall satisfaction with government 1 = satisfied	Overall satisfaction with military 1 = satisfied
Panel A: nation-building districts		
BISP recipient = 1	-0.000	-0.476
	(-0.00)	(-1.11)
Mean	0.282	0.866
N	714	714
Panel B: non-nation-building districts		
BISP recipient = 1	1.377**	-0.126
	(2.28)	(-0.33)
Mean	0.320	0.824
N	1,411	1,411

Notes: *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table 9: Mechanisms—perceptions of safety

	Overall satisfaction with government 1 = satisfied		Overall satisfaction with military 1 = satisfied	
	Respondent self-reported as unsafe	Respondent self-reported as safe	Respondent self-reported as unsafe	Respondent self-reported as safe
BISP recipient = 1	0.945*	0.743	-0.069	-0.431
	(1.72)	(1.34)	(-0.17)	(-1.05)
Mean	0.310	0.306	0.820	0.848
N	744	1,381	744	1,381

Notes: *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table 10: Mechanisms—duration of exposure to BISP

	Overall satisfaction with government	
	1 = satisfied	
	BISP present before 2011	BISP present since 2011
BISP recipient = 1	1.146*	0.369
	(1.84)	(0.81)
Mean	0.309	0.304
N	1,464	661

Notes: Marginal effects; *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table 11: Mechanisms—nation-building by year of BISP presence in community

	Non-nation-building districts		Nation-building districts	
BISP presence in community	Before 2011	2011 & after	Before 2011	2011 & after
BISP recipient = 1	1.436**	0.963	0.164	-0.043
	(1.96)	(1.05)	(0.17)	(-0.09)
Mean	0.309	0.369	0.309	0.259
N	1,140	271	324	390

Source: Authors' calculations.

Table 12: Mechanisms—effects of BISP on attitudes by party affiliation

	PPP^ MNA constituencies			Opposition MNA constituencies		
Presence of BISP in community	Combined	Pre-2011	2011 & after	Combined	Pre-2011	2011 & after
BISP recipient = 1	0.877	1.536	0.411	0.853	1.190	0.164
	(1.37)	(1.08)	(0.66)	(1.29)	(1.20)	(0.22)
Mean (dep var)	0.285	0.288	0.278	0.331	0.327	0.345
N	1,096	690	406	1,029	774	255

Notes: ^ includes MNA constituencies won by the Awami National Party (an ally of the PPP throughout its term in office, 2008–13). *t* statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table 13: Economic effects by presence of BISP in community before and after 2011

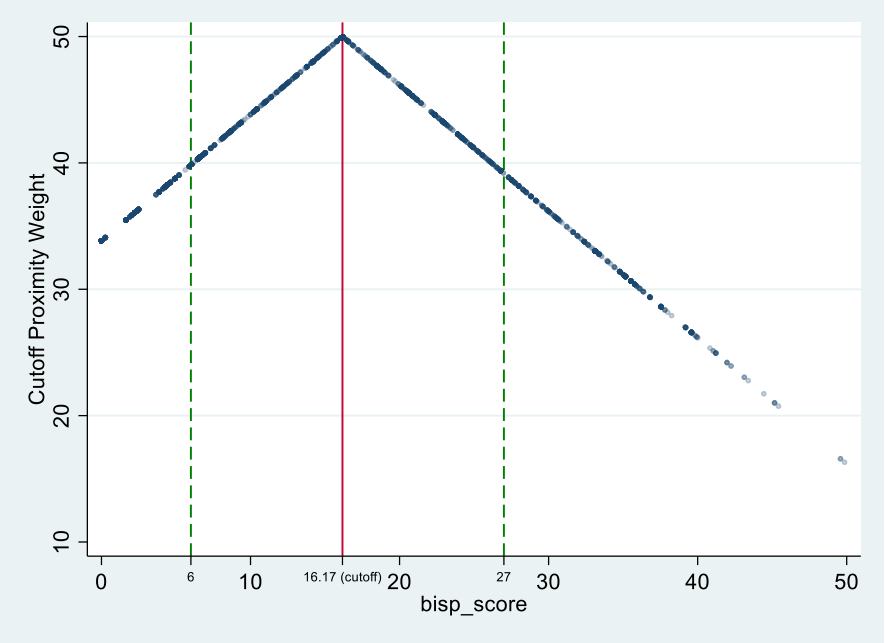
	HH experienced any economic shock in last year = 1		Conditional on experiencing shock, HH adopted harmful coping strategy = 1		HH economic situation is better than 2 years ago = 1		HH expects its economic situation to improve 2 years from now = 1	
Presence of BISP in community	Pre-2011	2011 & after	Pre-2011	2011 & after	Pre-2011	2011 & after	Pre-2011	2011 & after
BISP recipient = 1	-0.348	-0.160	1.465	-0.309	-0.412	-0.528	-2.992**	-0.054
	(-1.19)	(-0.40)	(1.43)	(-0.65)	(-0.83)	(-1.17)	(-2.41)	(-0.14)
Mean (dep var)	0.926	0.773	0.490	0.744	0.301	0.312	0.576	0.781
N	1,465	661	1,357	511	1,455	653	1,451	653

Notes: *t* statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Online Appendix

Figure A1: Cut-off proximity weight by BISP poverty score



Source: Authors' calculations.

Table A1: Alternative poverty score thresholds for BISP eligibility

Threshold for BISP score	IV regression coefficient (BISP received = 1) for overall satisfaction with the government	t - statistic	% eligible households (below threshold) in 6–27 range
8	0.557	0.58	0.151
9	0.488	1.38	0.224
10	0.563	1.48	0.267
11	0.402	1.25	0.313
12	0.422	1.35	0.343
13	0.432	1.56	0.385
14	0.395	1.43	0.416
15	0.493*	1.66	0.483
16	0.586*	1.71	0.506
16.17^	0.805**	2.09	0.520
17	1.068**	2.07	0.571
18	0.873**	2.06	0.588
19	0.799	1.6	0.659
20	0.787*	1.73	0.675
21	0.622	1.02	0.756
22	1.175	1.42	0.776
23	1.554	0.87	0.831
24	2.117	1.17	0.873

Note: ^16.17 is the actual cut-off of the poverty score used to determine eligibility.

Source: Authors' calculations.

Table A2: Estimates across sub-samples of households in alternative poverty score bands

Alternate poverty score bands	6–27	7–26	8–25	9–24	10–23	11–22	12–21	13–20	14–19	15–18	16–17
Overall Satisfaction with government^	0.821**	0.787	0.867*	0.867	0.899	1.419	2.393	-8.439	-10.00	-1.436	-23.75
Overall satisfaction with military^	-0.262	-0.282	-0.319	-0.540	-0.310	-0.380	0.114	-1.576	-0.833	-0.818	-12.00
N	2,125	1,746	1,648	1,379	1,200	985	879	616	518	223	138
N as % of total survey respondents (3,907)	54.4%	44.7%	42.2%	35.3%	30.7%	25.2%	22.5%	15.8%	13.3%	5.7%	3.5%
% respondents who receive BISP	13.00	12.70	13.00	11.60	11.30	11.20	11.00	10.70	9.80	9.00	7.20

Notes: ^ 1 = satisfied.

Source: Authors' calculations.

Table A3. Attitudes towards the government not related to BISP

	Pakistan government support for Kashmiri independence 1 = important	Military action against extremist groups improves Pakistani security 1 = agree	Government should force all women to observe purdah 1 = agree	Honour killings (*karo kari*) are justified 1 = yes
BISP recipient = 1	0.235	0.501	0.831	0.695
	(0.36)	(0.59)	(0.74)	(0.52)
N	2,125	2,100	2,124	2,123

Notes: Marginal effects; *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table A4: Results disaggregated by gender

	Overall satisfaction with government 1 = satisfied		Overall satisfaction with military 1 = satisfied	
	Men	Women	Men	Women
BISP recipient = 1	1.095*	0.577	-0.396	-0.126
	(1.75)	(1.15)	(-1.27)	(-0.29)
N	1,053	1,072	1,053	1,072

Notes: Marginal effects; *t* statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table A5: IV results with full sample

	Overall satisfaction with government 1 = satisfied	Overall satisfaction with military 1 = satisfied
BISP recipient = 1	0.421***	-0.135
	(3.40)	(-1.40)
N	3,906	3,906

Notes: Marginal effects; *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table A6: IV results with standard errors clustered at the village level

	Overall satisfaction with government 1 = satisfied		Overall satisfaction with military 1 = satisfied	
	SE clustered at Mauza (village) level	SE clustered at household level	SE clustered at Mauza (village/PSU) level	SE clustered at household level
BISP recipient = 1	0.821**	0.821*	-0.262	-0.262
	(2.09)	(1.87)	(-0.92)	(-0.90)
N	2,125	2,125	2,125	2,125

Notes: Marginal effects; *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table A7: Results using reconstructed poverty score as IV for BISP receipt

	Overall satisfaction with government 1 = satisfied	Overall satisfaction with military 1 = satisfied
BISP recipient = 1	0.688**	-0.269
	(2.04)	(-1.07)
N	2,125	2,125

Notes: Marginal effects; *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.

Table A8: Observations weighted by proximity to cut-off score (16.17)

	Overall satisfaction with government (1 = satisfied)	Overall satisfaction with military (1 = satisfied)
BISP recipient = 1	0.841**	-0.261
	(2.06)	(-0.90)
Mean	0.307	0.838
N	2,125	2,125

Notes: *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The weight assigned to observations for estimates in Table A9 is calculated as: Weight = $w = 50 - (|bisp_{_score} - 16.17|)$, where BISP score is the HH's BISP poverty score calculated by the authors using the 2013 survey data. 16.17 is the cut-off value for BISP eligibility. 50 is the maximum BISP score for households in the sample. Figure A1 in the Online Appendix shows the distribution of the weight calculated as Table A9, across the poverty score.

Source: Authors' calculations.

Table A9: IV estimates of BISP receipt on 7-point Likert scale (non-dichotomized)

	Satisfaction with government^	Satisfaction with military^
BISP recipient = 1	3.088*	-1.199
	(1.95)	(-1.05)
N	2,125	2,125

Notes: ^1= extremely dissatisfied, 7 = extremely satisfied. Marginal effects; *t* statistics in parentheses. (d) for discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Authors' calculations.