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EXPERIMENTAL EVIDENCE ON ANTI-AMERICANISM IN PAKISTAN

Leonardo Bursztyn
Michael Callen
Bruno Ferman
Ali Hasanain
Noam Yuchtman

Working Paper 20153
<http://www.nber.org/papers/w20153>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2014

We would like to thank Chris Blattman, Sylvain Chassang, Ernesto Dal Bò, Stefano DellaVigna, Lena Edlund, Ray Fisman, Paola Giuliano, Macartan Humphreys, Matthew Kahn, Dean Karlan, Asim Khwaja, David Levine, Aprajit Mahajan, Edward Miguel, Petra Persson, Daniel Posner, Jacob Shapiro, Jesse Shapiro, Nico Voigtlaender, Romain Wacziarg, Reed Walker, and seminar participants at CEGA (Berkeley), CGU, CU Denver, NYU, Princeton, RAND, Sao Paulo School of Economics, and USP for helpful comments and suggestions. Sarah Brierley and Saad Gulzar provided excellent research assistance. We also thank the Center for Global Management at UCLA Anderson for financial support. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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A Revealed Preference Approach to the Elicitation of Political Attitudes: Experimental Evidence on Anti-Americanism in Pakistan

Leonardo Bursztyn, Michael Callen, Bruno Ferman, Ali Hasanain, and Noam Yuchtman

NBER Working Paper No. 20153

May 2014

JEL No. C90,P16

ABSTRACT

We develop an indirect, revealed preference method of eliciting attitudes and apply it in an experiment in Pakistan designed to understand the expression of anti-American views. Following the completion of a personality survey, we offer subjects a bonus payment for completing the survey. We find that around one-quarter of subjects forgo a 100 Rupee payment (roughly one-fifth of a day's wage) to avoid anonymously checking a box indicating gratitude toward the United States government for providing funds. We experimentally vary the identity of the funder, the payment size, and subjects' expectations of privacy, and find that rejection of the payment is responsive to all of these treatments. Rejection of the U.S. government bonus payment is an indirect measure of anti-American attitudes. This approach mitigates concerns with experimenter demand, social desirability, and other biases, which can distort reported attitudes. We discuss and present suggestive evidence of the advantages of our methodology.

Leonardo Bursztyn
Anderson School of Management
University of California, Los Angeles
110 Westwood Plaza, C 513
Los Angeles, CA 90095
and NBER
bursztyn@ucla.edu

Michael Callen
Department of Political Science
University of California, Los Angeles
4289 Bunche Hall
Los Angeles, CA, 90095
mcallen719@gmail.com.

Bruno Ferman
Sao Paulo School of Economics, FGV
Rua Itapeva n 474
Sao Paulo, Brazil, 01332-000
bruno.ferman@fgv.br

Ali Hasanain
Lahore University of Management Sciences
Street 29 Lahore 54792, Pakistan
hasanain@lums.edu.pk

Noam Yuchtman
Haas School of Business
University of California, Berkeley
Berkeley, CA 94720
and NBER
yuchtman@haas.berkeley.edu

1 Introduction

Economists have devoted increased attention in recent years to investigating individuals' beliefs and attitudes, from preferences for redistribution, to trust, moral values, religious beliefs, and political ideology.¹ Often, empirical studies of individuals' attitudes are forced to rely on subjective, stated preference measures in response to direct questions.² Such measures have well-known problems, however: especially when questions are sensitive, or responses are stigmatized, respondents may answer in ways that are socially acceptable, rather than answer truthfully.³ Even when questions are not sensitive, responses may be affected by the fact that respondents know that their attitudes are being studied; they may also tend to answer in a way that seems to be desired by the experimenter.⁴ The latter is of particular concern in the monitoring and evaluation of interventions *designed* to shape attitudes.⁵ Finally, one may be concerned that responses to direct survey questions do not reveal a preference, because there is no incentive to respond in the way that one actually feels.

In this paper, we develop an *indirect* method for eliciting political attitudes, based on *revealed* preference. Not only are subjects in our study faced with a financial cost to express their political attitudes, but they also are unaware of the elicitation when they act to reveal their preferences. We

¹Theoretical work on beliefs, attitudes, and identity includes Piketty (1995), Akerlof and Kranton (2000), Alesina and Angeletos (2005), and Bénabou and Tirole (2006); empirical work includes Luttmer (2001), Alesina and Fuchs-Schündeln (2007), and Giuliano and Spilimbergo (Forthcoming) on preferences for redistribution, Guiso et al. (2009) and Nunn and Wantchekon (2011) on trust, and Alesina and Ferrara (2002) and Di Tella et al. (2007) on beliefs related to the workings of capitalist society.

²Recent work by Augenblick et al. (2012) is an exception: the authors elicit individuals' religious beliefs regarding an apocalyptic prophecy by presenting them with real-stakes choices.

³Recent work eliciting potentially stigmatized attitudes includes Bullock et al. (2011a), Diaz-Cayeros et al. (2011), Fair et al. (2012), Coffman et al. (2013), Blair et al. (2013a), and Blair et al. (2013b). Chassang and Padró-i-Miquel (2014) model the circumstances under which an agent who has paid a bribe with truthfully report. In their model, if the threat of retribution for whistleblowing is real, then potential informants will only report malfeasance when afforded a sufficient degree of plausible deniability. We compare our method of preference elicitation to other methods, including methods used to reduce concerns about revealing stigmatized attitudes, in Section 5.

⁴Zizzo (2010) reviews the problem of experimenter demand effects as they relate to economic experiments. Crowne and Marlowe (1964) discuss the implications for survey research and Mayo (1933) provides the classical example of subjects changing their behavior merely because they are being observed (Hawthorne effects) based on experiments in the Western Electric factory at Hawthorne, Illinois. A recent reevaluation of the Hawthorne study is presented in Levitt and List (2011).

⁵Direct survey questions are used to evaluate a broad range of large scale interventions. For example, the U.S. military's progress in Afghanistan—an enterprise on the order of hundreds of billions of dollars—is often evaluated via surveys of Afghan citizens, which suffer from the concerns outlined above (see United States Department of Defense, 2014). While use of randomized control trials is spreading among development and aid agencies, the use of methods to address concerns with survey data is much more limited. We reviewed the current official evaluation policies of six major international bilateral donor agencies: the United States Agency for International Development (USAID); the Department for International Development (DFID); the Japan International Cooperation Agency (JICA); the Canadian International Development Agency (CIDA); the Australian Agency for International Development (AusAID), and the Swedish International Development Agency (SIDA). Five of these six policies recommend surveys as a best practice for collecting outcome data (and random assignment to overcome selection bias). One—the USAID policy—mentions a form of survey bias that could be driven by experimenter demand: recall bias. None of the policies mention the possible *interaction* between experimental treatment and survey bias. Some recent work has used behavioral measures to assess the impact of interventions aimed at changing attitudes and norms; see, for example, Fearon et al. (2009), Casey et al. (2012), and Beath et al. (forthcoming).

call this method an *offer experiment*. In addition to developing a novel elicitation methodology, we use the method to study anti-American attitudes in Pakistan, focusing on the responsiveness of political expression to price and to social visibility.

To implement our methodology, we conducted an experiment in Pakistan in July, 2013, with 1,152 participants.⁶ During each experimental session, groups of Pakistani men, aged between 18 and 35, were brought into a room where they were asked to complete a standard “Big Five” personality survey. The intervention of interest occurred *after* subjects had completed the survey, though subjects were unaware of this fact.⁷ In return for completing the survey, study participants were offered a “bonus” payment (above a show-up fee they had received upon arrival). Receiving the bonus payment required checking a box in a form that indicated (from the subject’s perspective): “I gratefully thank the [funding agency] for its generosity and I accept the bonus payment offer.” Rejecting the payment required checking a box in the same form that indicated (again, from the subject’s perspective): “I choose not to accept the bonus payment offer.” The experiment randomly varied three separate components of the form, at the individual level, in a $2 \times 2 \times 2$ design:

The identity of the funding agency: The funding agency was either the U.S. government or the Lahore University of Management Sciences (LUMS), a leading Pakistani university.⁸

The amount of money offered: Subjects were either offered a bonus payment of 100 Pakistani Rupees (Rs.) or of 500 Rs.; both payments represented a sizable fraction of a day’s wage.⁹

The expectation of privacy: In the “public” condition, we emphasized that subjects would submit their experimental materials in front of the other subjects in the room, while in the “private” condition, we emphasized the privacy of their responses. In the public condition, subjects were told: “If you choose to accept the bonus payment, in order to receive this additional payment, you will be asked to turn the letter in to the survey coordinator in the front of the room, so other participants will see you turn in the letter.” In the private condition, subjects were told, “If you choose to accept the bonus payment, in order to receive this additional payment, **your decision will be completely private**; you will simply replace the letter in envelope 2 and submit it with your other survey materials at the end of the study, so no other participants will know your choice.”¹⁰

⁶All survey personnel in the field were Pakistani, and no mention was made of the involvement of American faculty in designing and analyzing survey responses.

⁷Social psychologists have long been aware of problems created by experimenter demand effects (Rosenthal, 1963, 1966). Reflecting this, many studies in social psychology make use of indirect elicitations in part to avoid experimenter demand effects (e.g., Cohen et al., 1996).

⁸Funds for bonus payments in fact came from the (public, so government-funded) University of California or from LUMS.

⁹Irfan (2008) finds that in 2003–2004, the average monthly wage among Pakistani men was 4,278 Rs., or around 200 Rs. per day. Conversations with local research assistants indicate that the daily wage for a manual worker today is roughly 400–500 Rs.

¹⁰In fact, all subjects would turn in their materials in exactly the same way: all subjects turned in their envelopes in the front of the room, and were seen doing so by other participants (as emphasized in the public condition). But, no subject’s decision regarding the bonus payment was ever observed by any other participant, because all survey materials were submitted inside subjects’ envelopes. The goal of the intervention was to manipulate subjects’ expectations, without telling them anything that was factually incorrect in order to minimize the use of deception in

Subjects’ willingness to reject payment in order to avoid expressing gratitude to the U.S. government is our measure of expressions of anti-American attitudes. To place this decision within a simple economic framework, we consider political expression to be a function of three different components. First, political expression can be explicitly aimed at changing the world—individuals derive utility from changing (expected) outcomes for themselves, for their children, or for others whom they care about.¹¹ Second, because many political acts occur publicly, social influence may shape individuals’ choices of political expression—this may be due to a desire to conform (Bernheim, 1994), to a desire to send a signal to a particular group, or because of the utility derived from social activity.¹² Third, individuals may engage in costly political actions for reasons of *ideology*—they would express their views, even at some cost, even in the absence of social influence, and even in the absence of any potential impact on the world around them.¹³

In our experiment, the first, “consequential,” determinant of political expression is shut down, since accepting or rejecting the money offer is not likely to have any real-world impact. We use experimental variation in an individual’s private financial cost associated with rejecting the U.S. government money offer, and the social visibility of that rejection decision, to estimate the importance of the second and the third determinants of political expression: social influence and an individual’s (anti-American) ideology. Of course, subjects may wish to reject payment for reasons other than anti-Americanism, for example, because they do not want to feel indebted to another party. We thus compare subjects’ rates of rejecting money from the U.S. government to rates of rejecting money from LUMS in order to “difference out” a propensity to reject bonus payments from a relatively neutral funder.¹⁴

It is important to highlight several virtues of our method of eliciting subjects’ ideology. First, we elicit individuals’ ideological views in a setting in which subjects are *unaware* of the elicitation. Not only was no subject aware of the purpose of the study, but also, the action through which individuals’ preferences were revealed appeared, from the subjects’ perspective, simply to be part of the process of receiving payment for completing the survey. Because the choice of whether to accept the bonus payment does not appear to be of scientific interest to the researcher, we are able to observe subjects’ (relatively) natural behavior, reducing concerns about experimenter demand effects or Hawthorne effects (though these concerns are not completely eliminated, as subjects’ choices are still “ideological” and are still made in an artificial setting). Second, we

the study.

¹¹This is the case in rational voting models (Downs, 1957, Palfrey and Rosenthal, 1983, Ledyard, 1984, and Palfrey and Rosenthal, 1985), and would encompass both pure altruism (Nagel, 1970.), where the agent cares only about the utility of others, and warm glow (Andreoni, 1989, 1990, 1993), where the agent gets utility from the act of giving.

¹²This is true even of the (often) private act of voting (Gerber et al., 2008, DellaVigna et al., 2013, and Gerber et al., 2013).

¹³See, for instance, the literature on expressive voting (Riker and Ordeshook, 1968, Tullock, 1971, Brennan and Buchanan, 1984, Brennan and Lomasky, 1993, Scheussler, 2000, Feddersen and Sandroni, 2006, Feddersen et al., 2009, and Kamenica and Brad, 2014).

¹⁴We discuss other possible confounding factors leading to the rejection of the bonus payment in Section 4.2, below.

create a setting in which a meaningful financial cost, the magnitude of which is experimentally varied across individuals, is imposed on an individual’s *private* political expression. This provides a revealed preference measure of subjects’ ideology, and allows us to *price* individuals’ willingness to express anti-American views. Third, by experimentally varying expectations of anonymity, we can manipulate the anticipated social costs of expressing one’s ideology, allowing us to study how social context affects political expression.

We find that when individuals express their ideology *privately*, a significant minority—around one quarter of subjects—are willing to forgo 100 Rs. to avoid taking an action that would undermine their ideology: checking a box and thus thanking the U.S. government for its generosity.¹⁵ We also find that public expression significantly differs from private expression. When subjects believe that their decision to accept the payment will be observed by the other study participants, significantly *fewer* individuals reject the bonus payment—the rejection rate falls by nearly 10 percentage points.¹⁶ Next, we find that individuals’ willingness to check the box thanking the U.S. government is responsive to the size of the payment. While 25% of subjects are willing to forgo a 100 Rs. payment rather than check the box indicating gratitude toward the U.S., only around 10% of subjects are willing to forgo a 500 Rs. payment (this difference is highly statistically significant). Exploiting the experimental variation in prices, we are able to estimate that the cost of publicly rejecting payment is equivalent to around 200 Rs.

Responses to direct survey questions, administered following our main intervention, support our interpretation of rejection of payment from the U.S. government as an expression of anti-American ideology. We find, first, that individuals who rejected the U.S. bonus payment report significantly more negative views of the U.S. government, and of aid provided by the U.S. government.¹⁷ Importantly, individuals who rejected the U.S. payment offer are *no more likely* to report negative views of Japan’s government or of aid from Japan, a relatively neutral foreign country. Survey evidence, too, is consistent with our findings of “moderating” effects of public expression: individuals who reject the bonus payment from the U.S. government not only report being anti-American, but they also view the other experimental subjects—correctly—as less “extremist” than themselves.¹⁸ Furthermore, the moderating effects of public expression do not appear to result from our construction

¹⁵In a sense, individuals’ expression of their ideology functions much like an element of their identity (Akerlof and Kranton, 2000); individuals are willing to pay a private financial cost rather than undertaking action that undermines their ideology (identity).

¹⁶In our analysis below, we present results comparing rejection rates for the U.S. government vs. LUMS as the funding agency. The results are very similar to the raw rejection rates presented here. By differencing out LUMS rejection rates across conditions we account for rejection for reasons other than anti-Americanism and for other sources of private/public differences in rejection rates. In fact, LUMS rejection rates are slightly (insignificantly) *higher* in the public condition than in the private condition.

¹⁷We also find that rejection of the U.S. government offer is correlated with subjects’ visible religiosity.

¹⁸While this moderating effect of the majority may be specific to our study, the finding that private ideological preferences and public expression can differ has implications for understanding ideology, and its expression, in different social contexts. Our findings also suggest that even subjects with relatively “extreme” views may moderate the expression of their attitudes due to social pressure.

of artificial social contexts: moderation effects are strongest among subjects who report knowing most of the other participants in their session, suggesting that the effects of public expression arise from *naturally occurring* social ties.

These findings contribute to a growing body of empirical evidence on, and economic analysis of, social and political outcomes in South Asia, an area of geopolitical importance.¹⁹ Our results indicate that there is a significant minority of literate young men who are willing to pay a sizable financial cost solely to express their ideological views. Furthermore, they suggest that individuals' political expression in private may significantly differ from—and be much more extreme than—views expressed publicly. This suggests that bringing extremist groups into the light of day, rather than forcing members to interact in the shadows, could affect political rhetoric, and perhaps policy positions as well.²⁰

A comparison between our approach to eliciting attitudes and other methods is perhaps best made with a particular application in mind. Suppose a researcher conducts a randomized control trial that allocates foreign aid to a treatment group, with the aim of determining whether aid is able to moderate extremist political views, and more generally can influence people's "hearts and minds".²¹ The researcher will naturally be concerned about subjects' willingness to reveal their true attitudes in response to direct questions: individuals may fear some sanction for revealing particular attitudes, or may be ashamed to admit certain views—especially when those views are explicitly being evaluated by a research team. While privacy and anonymity (of the sort provided in our study) can certainly help, direct questions seem likely to be distorted by experimenter demand effects that *interact* with treatments such as the receipt of foreign aid. For example, receiving aid might lead to more positive *reported* attitudes toward the aid provider because subjects feel a need to reciprocate to the experimenter, rather than because subjects' attitudes toward the funder truly changed. Indeed, we find evidence suggesting that our experimental intervention—the U.S. bonus payment offer—may have distorted responses to direct survey questions in our study. Eliciting attitudes in an indirect, natural manner can make a need to reciprocate (or other effects of perceived experimenter demand) less salient.

A large literature in sociology, statistics, and political science describes experimental survey techniques used to address biases that can arise when respondents prefer not to confide their true

¹⁹For example, Clingingsmith et al. (2009) study the impact of the Hajj pilgrimage on a broad range of attitudes among Pakistanis. Beath et al. (2012) study the impact of foreign aid on Afghans' views on security and on the Afghan government, NGO's and foreign military forces. See Gentzkow and Shapiro (2004) for an overview of anti-Americanism in the Islamic world.

²⁰Of course, one must keep in mind the caveat that the external validity of our findings may be limited; we discuss this further below.

²¹A substantial body of military doctrine and scholarship is based on the theory that humanitarian aid can reduce violence by inducing civilians to share tactically useful information such as the identity of key leaders or pending attacks (Galula, 1963, Galula, 1964, Popkin, 1979, Kalyvas, 2006, and United States Army, 2006). More recent empirical results provide support for the view that aid can reduce violence (Berman et al., 2011, 2013) and can change civilian attitudes (Beath et al., 2012; Berman et al., 2014).

views to the interviewer.²² Our methodology offers three advantages over these other approaches. First, experimental survey techniques require a large sample in order to estimate a quantity of interest (say the share of individuals with particular political attitudes) *for a population*. These methods are limited in their ability to measure the impact of experimental interventions on *individual* attitudes. Second, while these other methods should reduce social desirability bias, they still require respondents to volunteer information that may be socially stigmatized, and respondents have no incentive to truthfully reveal their attitudes—beyond a desire to honestly respond to the surveyor. Our methodology makes truthful revelation more likely both by measuring subjects’ attitudes in an indirect way (so subjects are less conscious of the elicitation process itself) and by incentivizing subjects by imposing a financial cost on particular choices.²³ Third, by eliciting subjects’ attitudes indirectly, our method mitigates concerns about experimenter demand effects interacting with experimental treatments.

We next describe the design and implementation of our experiment in Section 2. We then present our empirical results in Section 3, and discuss these results and rule out alternative explanations for our findings in Section 4. We discuss our methodology and compare it to other attitude elicitation methods in Section 5. Finally, we offer concluding thoughts in Section 6.

2 Experimental Design and Implementation

Our experiment was implemented in two stages: first, a set of pilot studies that served as a “proof of concept” that our design could be implemented safely and successfully; then, the main study.

2.1 Piloting

We developed our protocol in a series of pilots. First, in November 2012, we ran a small pilot and focus group discussion with 20 undergraduate students at the Lahore University of Management Sciences (LUMS). Next, before running the full experiment, we ran a larger pilot study in the field with 143 subjects. The exercise comprised 6 separate sessions, with approximately 24 subjects per session. 71 subjects participated on June 24th, 2013, in Islamabad and 72 subjects participated on June 25th, 2013, in Peshawar. Anticipating the necessity of having Pakistanis conduct the main experiment, we used the larger pilot to train our lab coordinators, allowing us to avoid the direct involvement of any foreigners in the implementation of the main experiment.²⁴

²²Warner (1965) introduced the “randomized response technique”, Raghavarao and Federer (1979) formalized the “list experiment” (also called the “unmatched count” and the “item count technique”), and Sniderman and Piazza (1993) provide, to our knowledge, the first example of an endorsement experiment. We discuss experimental survey techniques in further detail in Section 5, below.

²³In our study, costs are imposed asymmetrically: the expression of anti-American views is costly; however, subjects’ acceptance of the bonus payment is *not* a revealed preference of pro-American views.

²⁴Our concern was that the elicitation of anti-American attitudes by a team including Americans would compromise the validity of our findings.

Data from the pilot allowed us to refine our experimental design and to establish that we could carry out the activity safely with minimal risk to enumerators or participants. We committed in advance to using data from the pilot studies only for these purposes, and do not include them in our main analysis.²⁵

2.2 Timeline and Site Selection

We implemented our experiment simultaneously in three cities, Peshawar, Islamabad, and Dera Ghazi Khan, between July 7th and July 16th, 2013. We selected these dates so that half of our sessions would be completed prior to Ramadan and half would be completed during Ramadan, which began on July 11, 2013.

One objective of our project was to measure the degree of anti-Americanism among populations directly affected by the war on terror—this is where anti-American views are likely to be of greatest importance.²⁶ To access these populations, we ran our experiment in areas either directly affected by the United States-led invasion of Afghanistan (Peshawar) or in cities that have substantial numbers of refugees from conflict-affected areas (Islamabad and Dera Ghazi Khan).²⁷

Peshawar and Islamabad have large Pashtun populations and Dera Ghazi Khan has a large Balochi population, which make them especially interesting locations for the study of anti-American attitudes. Pashtuns are an ethnic majority in Southern and Eastern Afghanistan and in Northern in Western Pakistan. Both the Afghan and the Pakistani Taliban draw their support primarily from Pashtuns in this region and the vast majority of the fighting related to the U.S.-led invasion of Afghanistan has happened in predominately Pashtun areas. At the time of the study, Balochistan was home to a very active secessionist movement, and the capital, Quetta, is home to the Quetta Shura which is the primary faction of the Afghan Taliban. In scouting locations for our initial pilot, we determined that direct implementation of the experiment either in rural Khyber Pakhtunhwa or in the Federally Administered Tribal Areas (FATA) involved too much risk to respondents and to enumerators, so we opted to work in urban areas with large migrant populations, which are generally safer.

2.3 Subject Recruitment and Screening

We contracted with local survey firms to recruit men aged between 18 and 35 from neighborhoods with large migrant populations in Islamabad and Peshawar. In both cities, we asked the recruiters to target migrants from the Federally Administered Tribal Areas (FATA), Khyber Pakhtunhwa

²⁵Results were qualitatively similar (available from the authors upon request).

²⁶Those individuals affected by the war on terror may in fact be less anti-American than other Pakistanis because they may have fled from regions influenced or controlled by the Pakistani Taliban.

²⁷Peshawar lies between Kabul, Afghanistan, and Islamabad on the Khyber pass and is the capital of Khyber Pakhtunhwa Province (formerly Northwestern Frontier Province). Dera Ghazi Khan and Islamabad both lie close to the provincial border of Khyber Pakhtunhwa and have large migrant populations.

(KP), and Balochistan.²⁸ In Dera Ghazi Khan, we first selected a *tehsil* randomly, then selected a union council randomly, and then used a simple right-hand sampling rule to contact potential participants. We ran 22 sessions in Peshawar, 10 sessions in Islamabad, and 16 sessions in Dera Ghazi Khan (Appendix A1, Figure A.1, presents a map of the laboratory locations).

Upon contacting a potential subject, recruiters asked him to read aloud a short script in order to verify literacy, and an additional literacy test of comparable difficulty was administered when a subject reached the study site. Potential subjects who failed either test were not allowed to participate. Subject literacy was crucial for our experimental design, as the entire study required subjects to comprehend printed text. Appendix A1, Figure A.2, provides Urdu translations of the two literacy screening tasks and English translations of both literacy test scripts are reproduced in Appendix A2.

2.4 Enrollment

After subjects arrived at the study site, they were directed to a waiting room, provided with an informed consent form to read, and asked to wait until they were called to participate. We relied on *verbal* informed consent to assure subjects that personally-identifiable information on their participation and choices was not being collected. The study coordinator called subjects to enroll one at a time; subjects then received a chit with a randomly assigned subject number, between 1 and 24, from a research assistant.²⁹ After receiving their number, subjects then went to the enrollment desk outside of the laboratory (Appendix A1, Figure A.3, provides a picture of the enrollment desk). At the desk, subjects read the second literacy script aloud, and received a payment envelope with their subject number printed on it.³⁰ After completing the enrollment procedure, a research assistant led subjects into the laboratory and seated them at the individual lab station corresponding to their subject number.

Lab stations consisted of a chair with a clipboard; laboratory materials were placed on the chairs, which were positioned approximately four feet apart to prevent subjects from observing each other’s choices (in Appendix A1, Figure A.4 provides a picture of the experiment site in Islamabad and Figure A.5 provides a picture of the experiment site in Peshawar). We randomly assigned survey versions to lab station numbers using a simple computer program (Appendix A1,

²⁸While we did not record the birth place of subjects to preserve anonymity, in these cities our recruiters drew subjects from neighborhoods primarily populated by migrants from the Swat and Malakand agencies (agencies are administrative units in FATA). Both of these agencies, located in FATA, have seen substantial levels of insurgent conflict in recent years.

²⁹Individual stations were ordered sequentially by subject number inside the lab. Subject numbers were provided in random order to reduce the chance that subjects would be acquainted with the person sitting next to them—a concern if acquainted subjects entered the study site together, and station assignments were made in a non-random order. In practice, a research assistant handed each subject a chit, numbered from 1 to 24, from a shuffled deck. The number on the chit became a subject’s participant identification number.

³⁰Only one potential subject passed the first reading comprehension test but failed the second; this subject was replaced from the pool of recruits.

Figure A.6, provides the mapping between survey versions and lab stations). All sessions involved exactly 24 subjects, resulting in a total of 1,152 men participating in the main study. After a session, research assistants ensured that subjects exited the building; they were bussed off site immediately and were not allowed to interact with other subjects waiting to participate in the study.

2.5 The Experiment

At the beginning of a session, the lab director read a set of instructions aloud. After explaining the laboratory protocol, the instructor took the subjects through three specific example questions. Each subject had a printed version of these questions, which were intended to familiarize subjects with the kinds of multiple choice questions that they would have to answer in activity 1 (a personality survey). Importantly, these instructions included no content related to politics or ideology. After completing the instructions, the lab director took questions. The director then indicated that no questions would be answered during the experiment, allowing subjects one final opportunity to ask questions before the experiment commenced.³¹ It is important to emphasize that no details were provided by the lab director regarding the payment process; research assistants were told to reveal no more than that payment for completing the study would occur at the end of the session. To increase subjects' confidence that they would be paid, subjects were provided their show-up fee of 300 Rupees when they began the first activity in the study.

The experiment involved four separate activities, each of which required completing a form contained in a separate envelope, numbered in order. These materials are reproduced completely in Appendix A2. Upon completion of an activity, subjects were instructed to close their envelope and place it below their chair before proceeding. Furthermore, they were told not to return to previously completed activities, and that subjects who did not comply would be asked to leave. The primary purpose of strictly disallowing participants from returning to previous activities was to ensure that they could not change their responses in the revealed preference activity (activity 2) after completing the stated preference activity (activity 4).

When all subjects had completed the four activities, the lab director and research assistant collected all laboratory materials except for subjects' chits, which subjects had received upon enrollment. The envelopes (on which were written subjects' participant identification numbers) were taken into a separate room with the laboratory materials for the calculation of payments for each subject. Payments were placed in envelopes, which were then exchanged for the chits handed out to subjects at the beginning of the session.

³¹We disallowed questions because we did not want subjects' inquiries to contaminate the research design.

Activity 1

The experiment began as a standard personality survey which was contained in envelope 1 along with subjects' "show up" payment. In addition to a few demographic questions, participants completed a Big 5 personality assessment. The version of the Big 5 assessment used by our team was adapted to use in Pakistan and validated by psychologists at the National Institute of Psychology at Quaid-i-Azam University.

Activity 2

After completing the survey, subjects opened the second envelope.³² This envelope contained an offer of an *additional* payment (above that for showing up) in return for checking a box on a letter to the funding agency. Checking the box indicated receipt of the payment and gratitude to the agency for providing funding. This activity measures whether respondents are differentially willing to forgo a payment, depending on the funder (U.S. government or the Lahore University of Management Sciences), the amount of the payment (100 Rupees or 500 Rupees), and respondents' perception of whether the payment would be public or private. Specifically, envelope 2 contained a piece of paper with the following information printed on it (*italics indicate text that varied depending on the experimental condition, with slashes dividing the conditions*):

"Thank you for completing the survey. In return for completing the survey, we can offer you a bonus payment. Funding for the bonus payment comes from {*Lahore University of Management Sciences / the U.S. government*}. We can pay you {*100 Rupees / 500 Rupees*} for completing the survey, but in order to receive the bonus payment you are required to acknowledge receipt of the funds provided by {*Lahore University of Management Sciences / the U.S. government*} and thank the funder. If you choose not to accept the payment, you will forgo the bonus payment of {*100 Rupees / 500 Rupees*}, but not the payment of 300 Rs. for your participation."

After this passage, in half of the forms, the language emphasized that subjects' choices would be private, while in the other half, the language implied that subjects' choices to accept the bonus payment would be observed by other subjects, as follows:

"If you choose to accept the bonus payment, in order to receive this additional payment, {*you will be asked to turn the letter in to the survey coordinator in the front of the room, so other participants will see you turn in the letter / **your decision will be completely private**; you will simply replace the letter in envelope 2 and submit it with your other survey materials at the end of the study, so no other participants will know your choice*}. Once you have made your decision

³²The language used in envelope 2 was of particular concern in designing the study, and was a focus of our pilot study focus group discussions.

on the next page, please place the letter into envelope 2, whether or not you chose to accept the bonus payment.”

In addition to the sheet of paper with instructions, envelope 2 contained the bonus payment acceptance/rejection letter, with the following options (the funding agency in the letter matched the agency mentioned in the instructional form):

☐ I gratefully thank {*Lahore University of Management Sciences / the U.S. government*} for its generosity and I accept the bonus payment offer.

☐ I choose not to accept the bonus payment offer.

It is worth noting that to ensure the safety of participants, in practice, no subject’s choice of whether to accept the bonus payment was actually public. All participants turned their acceptance/rejection letter in to the survey coordinator at the front of the room, having replaced their letter into envelope 2, and submitting it with the other survey materials (note that we did not actually provide false information about what would be required of participants, as the language in both “public” and “private” conditions was literally true). The language in the “public” treatment arm was designed to suggest (without providing false information) that the decision to accept the bonus payment would not be private, but subjects in the “public” condition still may have expected their decision to be private because they knew that the letter would be enclosed in an envelope. Because we can only imperfectly manipulate expectations of privacy, we view this exercise as providing a lower bound estimate of the effect of making the decision to accept the bonus payment public.

Activity 3

In activity 3, participants filled out a self-response survey that began by asking subjects to guess how many of the other participants were willing to accept the bonus payment. This question was incentivized: subjects were informed that the three individuals who guessed closest to the actual number would receive an additional 300 Rupees. Next, the survey collected information on the number of other participants the respondent knew.

We then ran a “list experiment,” a method used to measure attitudes toward sensitive topics. List experiments provide individual respondents with some degree of plausible deniability (“cover”) for their expression of an unpopular, embarrassing, or stigmatized view, and thus increase the likelihood that such expression will occur (though truthful expression is not incentivized).

The list experiment works as follows: first, respondents are (randomly) assigned either into a control group or to one or more treatment groups. Subjects in all conditions are asked to indicate the *number* of policy positions they support from a list of positions on several issues. Support for any particular policy position is never indicated, only the total number of positions articulated on the list that a subject supports. In the control condition, the list includes a set of

contentious, but *not* stigmatized, policy positions. In the treatment condition, the list includes the contentious policy positions from the control list, but also adds the policy position of interest, which *is* stigmatized. The degree of support for the stigmatized position *at the population level* is determined by comparing the average number of issues supported in the treatment and control conditions.

In our study, we randomly assigned our subjects to a control group or to one of two treatment groups, with each group containing 384 subjects. In the control condition, we asked respondents:

The following are four policies some government officials express support for. Please report HOW MANY of the four you support. You do not need to indicate which ones you support, just how many.

- Providing the poor with free electricity generators
- Establishing an independent state in Kashmir that is not part of India and not part of Pakistan
- Ensuring that civilians (President or Prime Minister) control the military
- Reducing the number of people eligible for the Benazir Income Support Program, but increasing payments to those eligible.³³

In the treatment conditions, subjects were asked a question that is identical other than the inclusion of an additional stigmatized item. In the first treatment group (the “U.S. aid list”), we added the policy position:

- refusing humanitarian aid from the U.S. government.

In the second treatment group (the “PTI list”), we added the position:

- supporting the activities of Pakistan Tehreek-e-Insaf (PTI).³⁴

Activity 4

Envelope 4 contained another survey, which asked subjects direct questions to elicit their stated preference support for: (i) aid provided by the Japanese government to Pakistan; (ii) the Japanese government overall; (iii) aid provided by the United States; and (iv) the United States government overall. We also asked a question regarding willingness to take risk using a simple Likert scale approach; we asked about subjects’ political awareness; and, about their support for Japan and the U.S. *relative* to other subjects in the room.

³³The Benazir Income Support Program is a popular targeted, unconditional cash transfer program.

³⁴PTI is the most anti-American of the major parties in Pakistan. Support for a party known for its anti-American stance was a natural policy position reflecting anti-American attitudes for us to include in the list experiment.

Payments

When all subjects had completed the survey, they were asked to come, in order of their subject number, to the front of the room. They gave their payment envelope and materials packet to the session coordinator and were asked to return to their seat to await payment. After collecting all 24 packets, two research assistants went into a separate room and calculated total subject payments. The payments were sealed in an envelope, with the cash payments wrapped in a thick debriefing handout so that subjects could not tell how much each had been paid. This was important to ensure that subjects could not be identified as having accepted a bonus payment offer based on the thickness of the payment envelope.

Subjects were then called to the front of the room, were paid by providing their chit with the subject number on it in exchange for the payment, and were sent out of the lab into a waiting bus—there were no opportunities for subjects who had completed the study to communicate with subjects who had not yet participated. As soon as all subjects were paid and had exited, the subsequent session began immediately.

3 Empirical Analysis of Political Expression

This section presents our core empirical results. We first present the experimental subject’s decision viewed through the framework of a simple utility expression; this will help structure and clarify our analysis of ideology and political expression. We then present descriptive data on our sample and study individuals’ private ideological expression. Next, we explore the role of social pressure by examining differences in rejection rates between subjects in the private and public conditions. Then, we study the sensitivity of private ideological expression to the size of the payment offer. Finally, we use the experimental variation in the financial cost of political expression to estimate the cost, in monetary terms, of expressing anti-American attitudes in public, and to estimate the share of subjects who would reject very small bonus payment offers.

3.1 A Simple Framework

Suppose that individual i derives utility from expressing attitude j through three channels. First, expressing attitude j may provide an individual with utility for *instrumental* reasons; that is, because expression changes the world (for individual i or for others) in ways that bring individual i utility. Second, individual i might derive utility for *intrinsic* reasons—that is, for reasons of ideology—directly from the act of expressing attitude j . Finally, expression might provide utility for *social* reasons when it is observed by others; individual i may derive additional utility or disutility from the public expression of attitude j , compared to its private expression (because public expression allows an individual to send a signal to others, because group expression may

result in a different experience, etc.). These components of the utility function of individual i can be expressed as follows:

$$\begin{aligned}
u_i = & \alpha \times \Pr(\textit{express } j \textit{ consequential}) \times \mathbb{1}[\textit{express } j] \\
& + \beta \times \mathbb{1}[\textit{express } j] \\
& + \gamma \times \mathbb{1}[\textit{express } j] \times \mathbb{1}[\textit{public expression}] \\
& + \varepsilon_i,
\end{aligned} \tag{1}$$

where $\Pr(\textit{express } j \textit{ consequential})$ is the probability that expressing j changes the world in a way that brings utility to individual i , and ε_i is an individual-specific preference parameter. We provide a context in which $\Pr(\textit{express } j \textit{ consequential}) \approx 0$: ticking the box to reject the bonus payment was likely to have been viewed by subjects as having negligible real world consequences that might affect utility for *instrumental* reasons.

In our setting, the attitude j of interest is anti-Americanism; individual i will choose to express attitude j (by rejecting the bonus payment) if the utility derived from expressing the attitude exceeds the cost of expression, which we denote c . The mere act of ticking the box imposed essentially zero cost on subjects, making c simply the forgone bonus payment. We experimentally vary c and turn on and off the public expression indicator function, $\mathbb{1}[\textit{public expression}]$. To study intrinsic motivation for political expression, for each experimental financial cost $c \in \{100Rs., 500Rs.\}$, we estimate the fraction of individuals i such that $\beta + \varepsilon_i > c$. To determine how public expression differs from private, we estimate the fraction of individuals such that $\beta + \gamma + \varepsilon_i > c$.

3.2 Sample Characteristics and Balance Across Conditions

Table 1, column 1, presents the characteristics of our experimental sample. One can see that all of our participants were men, which was by design. In addition, participants were, on average, young and relatively well-educated. The latter is again by design, as literacy was required to implement our study. Around one half of the subjects were engaged in some economic activity at the time of the study. Around two-thirds of subjects were Pashtun, 10 percent Punjabi, and another 10 percent Baloch. The bottom row of Table 1 displays the sample sizes in each treatment cell, and columns 2–9 of Table 1 present the characteristics of subjects across experimental conditions. We find that respondent characteristics, including demographics, education levels, and Big 5 personality traits are balanced across conditions (see Table 1, column 10).

3.3 Measuring Private Attitudes

We begin by considering rejection rates among subjects offered the low payment option (100 Rupees) in the “private” condition. Subjects in this group will provide us with an indication of whether individuals are willing to pay a significant cost simply to privately express their ideological position: viewed through the framework outlined in Section 3.1, we have tried to eliminate any instrumentally- or socially-derived utility from rejecting payment. Table 2, row 1, column 1, presents the raw rejection rate for the U.S. government offer among subjects offered 100 Rupee bonus payments in the private condition: in this group, 25.2% of subjects chose to *reject* the offer.

Of course, it might be the case that some of these subjects would have rejected money from any funding agency, not only from the U.S. government. In order to account for this possibility, we present in Table 2, row 1, column 2, the rejection rate among subjects offered 100 Rupees from LUMS, in the private condition: in this group, only 8.4% of individuals chose to reject the payment. Individuals who rejected the LUMS offer represent an estimate of the proportion of individuals who rejected the U.S. government offer not because of anti-Americanism, but because they would reject a bonus payment even from a relatively neutral entity. We subtract this fraction from the overall rate of rejection of the U.S. government offer to estimate that the proportion of subjects who rejected the U.S. offer, but would have accepted an offer from LUMS, is 16.8% (see Table 2, row 1, column 3; the p-value from a test that this difference equals zero is <0.001).³⁵

In Appendix A1, Table A.1, we report regressions reflecting the difference in rejection rates for U.S. government and LUMS offers controlling for session fixed effects and a set of subject covariates.³⁶ The estimated treatment effects and standard errors remain virtually unchanged, suggesting the implementation of the laboratory protocol across rounds and experimental sites was successful.³⁷

3.4 The Role of Social Context

We next investigate a second dimension of randomization incorporated into our design: variation in subjects’ perceptions of whether their choices to accept the bonus payment offer would be publicly

³⁵Note that 16.8% may represent a lower bound for the fraction of people who are anti-American, as some of those who rejected the LUMS offer might be anti-American as well. Indeed, LUMS has an international orientation, and is patterned after universities in the United States. Given this, subjects may associate LUMS with the United States, biasing our results toward finding no anti-Americanism when we compare U.S. government offer rejection rates to LUMS offer rejection rates. Of course, if subjects would have rejected payment from any *government*, then this would also result in higher rejection rates for the U.S. government offer than the LUMS offer. We explore whether attitudes toward foreign governments in general might drive our results, along with other alternative hypotheses, in Section 4.2, below.

³⁶We have also estimated all of the specifications presented in the paper and appendix, but with standard errors clustered at the level of the experimental session. Results are extremely similar and are available from the authors upon request.

³⁷Implementation is of special concern in our study: as outsiders (including the co-author from Eastern Pakistan), our presence could have affected subjects’ behavior, preventing us from directly monitoring the experiment.

observed by other participants at the end of the session. We present the effects of (anticipated) public expression on subjects’ willingness to reject the bonus payment in the second row of Table 2. Column 1 presents the difference between the public and private conditions in rejection rates of the 100 Rs. offer from the U.S. government. The proportion of subjects who rejected the U.S. government offer in the public condition was 8.2 percentage points *lower* than in the private condition (the p-value from a test that rejection rates in the public and private conditions are the same is 0.093).

Subjects’ decisions of whether to accept the bonus payment offer might differ between the public and private conditions even in the absence of any effect of social pressure on the expression of political ideology, *per se*. For example, one may be less likely to reject the bonus payment offer in the public condition out of concern that one will appear ungrateful or foolish. One might also be less likely to reject payment in public if one worried about family members’ displeasure if they discovered that a financial payment was forgone. On the other hand, one may be more likely to reject the payment offer in the public condition if one were concerned about being publicly identified as having just received a large payment. These effects of the public condition in our study would exist irrespective of the identity of the funding agency.

We study these effects of the public condition on rejection rates by considering the same public versus private difference in rejection rates for subjects who received a 100 Rs. offer from LUMS. In Table 2, row 2, column 2, one can see that the difference between the public and private rejection rates of the 100 Rs. LUMS offer was quite small—an increase in rejection of 2.7 percentage points—and not statistically significant ($p=0.439$). The *higher* rejection rates in public for the LUMS offer suggests that the *lower* public rejection rates we found for the U.S. offer were not a result of a general reduction in rejection rates when choices are made publicly.

In Table 2, row 2, column 3, we show the public versus private difference in rejection rates of the U.S. offer, after differencing out the rejection rates for the LUMS offer. We now estimate a 10.9 percentage point lower rejection rate for the U.S. government offer in the public condition ($p=0.069$). These results indicate that social context affects the expression of ideological positions. Moreover, the direction of the effect of anticipated social pressure, in the context of our study, is toward *moderation*: fewer subjects rejected the U.S. offer when they believed their choice would be made public to other participants.³⁸

An important consideration when evaluating our estimated effects of social pressure is whether these effects are consistent with subjects’ beliefs about the views of the other subjects around them. For example, if anti-American subjects moderated the public expression of their political views out of a desire to conform to the (perceived) majority attitude, then it should be the case that these subjects correctly perceived that they were in the minority.

³⁸In Appendix A1, Table A.2, we present regression results estimating the effect of the public condition on rejection rates controlling for session fixed effects and a set of subject covariates, and continue to find a statistically significant reduction in the rejection of the U.S. government offer in the public condition.

To measure subjects' beliefs about other subjects' willingness to accept the bonus payment, we included additional components in the study *after* the decision of whether to accept the bonus payment offer. The third envelope in the experiment (immediately after the bonus payment offer) included an incentivized elicitation of individuals' beliefs about the number of other participants in the room (from 0 to 23) who accepted the bonus payment offer (all sessions included exactly 24 participants). Among respondents who received the 100 Rs. offer from the U.S. government, in the private condition, the average guess was that 80% (median 95.6%) of other participants in the room accepted the payment offer. Thus, subjects correctly believed that the majority of others would choose *in private* to accept the money from the U.S. government. Importantly, respondents who rejected the U.S. government offer correctly viewed themselves as belonging to a minority: among respondents who rejected the 100 Rs. U.S. government offer in private, the average guess was that 62.3% (median 87%) of other respondents accepted the offer.

We also directly elicited subjects' views of the individuals around them: in the fourth (and final) envelope, subjects were directly asked to compare their views to those of others in the room regarding: (i) the U.S. government; and (ii) accepting U.S. aid. Among those who accepted the "100 Rs.-private-U.S. donor" payment, 17% of subjects viewed themselves as more anti-U.S. government than the other respondents in the room; among those who rejected that offer, that number rose to 57.2%.³⁹ Moreover, only 14.3% of respondents rejecting the offer report viewing others in the room as more anti-American than themselves.⁴⁰

The results we find in our analysis of the exercises contained in envelopes 3 and 4 paint a consistent picture: rejectors of the U.S. government bonus payment offer believed that a majority of the other subjects would accept the payment, and also self-identified as belonging to an anti-American minority. Our results are consistent with anti-American individuals anticipating a social cost when expressing their ideology publicly. Of course, *ex ante*, one might have hypothesized that a minority of extremists might have pressured moderate individuals to express *more* anti-American attitudes in public. While this might occur in some settings, our findings of moderating effects of public expression are of interest given the theoretical ambiguity.

Finally, we consider the (non-random) variation in social context arising from respondents' familiarity with each other from previous interactions outside the study. In the third envelope, we included a question asking subjects how many people they knew in the room.⁴¹ Nearly 60% of respondents reported knowing at least one other person, suggesting that although the study

³⁹ Admittedly, one worries that this elicitation is affected by subjects' decisions regarding the bonus payment, so it is best viewed as a suggestive complement to the incentivized estimates of other subjects' behavior.

⁴⁰ When we look at views on accepting U.S. aid, the numbers are very similar: among those who accepted the offer, 18.2% view themselves as more likely to refuse U.S. aid, whereas the percentage is 58.3% among those who reject the money offer (and only 16.6% of those rejecting the offer view themselves as less likely to refuse U.S. aid than others in the room).

⁴¹ Subjects were asked to pick from 5 categories: no other participant; between 1 and 6 other participants; between 7 and 12; between 13 and 18; and, between 19 and 23. This was asked just after subjects estimated the number of other subjects who accepted the bonus payment.

occurred in an artificial setting, some of the social connections in the room were natural. We find that the impact of social pressure on an individual’s ideological expression positively varies with that individual’s familiarity with others in the room, and that the moderating effect of the public condition on ideological expression is largest among individuals knowing most of the participants in the session.

3.5 Sensitivity of Political Expression to Payment Size

In Section 3.3, we showed that a significant minority of individuals in our sample were willing to reject an offer of 100 Rs. rather than express gratitude to the U.S. government. We view this as evidence that some respondents were willing to pay a positive financial cost to privately express their ideological position. Next, in Section 3.4, we found that this expression was shaped by the social context: a significant fraction of anti-American individuals converged in their behavior toward the moderate majority when they believed their choice would be publicly observed. We next ask: how sensitive is the expression of ideology to the financial cost of that expression?

To answer this question, we exploit the random assignment of bonus payments of 500 Rs., rather than 100 Rs., to half of the study’s subjects. Table 2, row 3, column 1, reports the difference in rejection rates of the U.S. government offer when the bonus payment is 500 Rs., relative to 100 Rs., in the private condition. One sees that an increase in the offer to 500 Rs. decreased the rejection rate by 15.5 percentage points, from 25.2% to 9.7% ($p=0.001$). For the LUMS payment, there is only a 2.8 percentage point reduction in rejection rates comparing the (private) 100 Rs. and 500 Rs. offers (column 2). The reduction in rejection of the U.S. government offer, after subtracting rejection rates for the LUMS offer, is a significant 12.7 percentage points ($p=0.0128$; see Table 2, row 3, column 3).⁴²

3.6 Additional Estimates

We can exploit the experimental variation in prices to estimate several additional parameters of interest. First, one might wish to estimate the financial cost equivalent to the social cost of public expression found above. To do so, we assume a functional form for the relationship between the size of the private, U.S. government bonus payment offer and the rejection rate. Then, we estimate the financial cost of rejection that would produce the rejection rate we observed above in the 100 Rs., *public* U.S. government, condition. The difference between this amount and 100 Rs. is our estimate of the financial equivalent to the social cost faced by individuals offered the bonus payment from the U.S. government in the public condition.

We first assume that the relationship between rejection and the cost of rejection (i.e., the bonus payment size) is linear. In this case, we estimate that the social cost of rejecting the U.S. government

⁴²In Appendix A1, Table A.3, we present regression results for the effect of higher payments on rejection rates controlling for session fixed effects and a set of subject covariates, and results are very similar.

offer is 211 Rs. If we instead assume that there is a logistic relationship between rejection and the size of the bonus payment, we find estimate that the social cost of rejecting the bonus payment publicly is around 180 Rs.

One might also be interested in individuals' private expressions of anti-American attitudes when the cost of expression is arbitrarily low. This will allow for a more direct comparison with stated views and the list experiment responses, which come at no cost (we turn to this below). We thus predict private rejection rates of the U.S. offer at price zero, exploiting the experimental variation we observe between 100 Rs. and 500 Rs. Assuming a linear relationship between expression and cost, the private rejection rate of the U.S. offer would be 29% at price zero. Assuming a logistic functional form, the estimated rejection rate at price zero is 31%.

4 Discussion

In this section, we first present evidence that our revealed preference method of eliciting subjects' political attitudes correlates with subjects' stated preference views and with behavior indicating religious extremism. Next, we explore alternative mechanisms that may have driven our results. Finally, we discuss the external validity of our findings.

4.1 Correlations with Stated Preferences and Behavior

As mentioned above, following subjects' decisions of whether to accept the bonus payment, they were asked to answer a number of direct survey questions, which included elicitation of their stated views on: (i) aid provided by the U.S. government, (ii) the U.S. government overall, (iii) aid provided by the Japanese government, and (iv) the Japanese government overall (Japan was picked as a plausibly neutral, but still rich and foreign, funding nation). For each of these questions, respondents were asked to express their views by picking a number from 1 to 5, with 1 corresponding to very negative views, and 5 to very positive views. We convert responses into a "negative views" dummy variable equal to 1 if subjects responses were either "1" or "2". Subjects were also asked to compare their views on the four aforementioned topics *relative* to the other participants in the room, also on a scale from 1 to 5; we converted these into analogous "negative relative views" dummy variables. Responses to the direct questions on stated views about U.S. aid and the U.S. government suggest anti-American sentiment among a significant minority of the sample: 26.4% of respondents report having a negative view of U.S. aid (i.e., picked either 1 or 2 as their answer to the corresponding question) and 29.8% of respondents have a negative view of the U.S. government overall.

We can use the answers to these direct survey questions to provide suggestive evidence "validating" our revealed preference ideology measure, keeping in mind the typical caveats regarding the interpretation of responses to direct survey questions (which we take up further below). To do so,

we examine the correlation between individuals’ decisions to reject the U.S. government payment in the 100 Rs., private, U.S. offer, condition, and their responses to the direct survey questions.⁴³

We first regress the “negative views on U.S. aid” dummy variable on a dummy variable indicating whether subjects rejected the bonus payment. In Table 3, column 1, one can see that individuals who rejected the U.S. payment were around 63 percentage points more likely to express negative views on U.S. aid in response to a direct question (the coefficient is significant at the 1% level). In Table 3, column 2, we present results from an analogous regression, but using negative views of the U.S. government as the outcome. Again, one sees economically and statistically significantly higher rates of expressing negative views among subjects who rejected the U.S. government bonus payment.

In Table 3, columns 3 and 4, we present results analogous to columns 1 and 2, but based on questions asking subjects about their views relative to others in the room. One can see that subjects who rejected the U.S. bonus payment offer view themselves as *relatively* more anti-American. Finally, as a falsification exercise, in Table 3, columns 5–8, we repeated the regressions from columns 1–4, but study subjects’ views on aid from Japan, and on the Japanese government more generally. One can see that rejection of the U.S. payment is associated with very small, statistically insignificant differences in views on Japan.

It is important to emphasize that we view the correlation between revealed and stated preferences as merely suggestive: in addition to standard concerns regarding stated preference responses, the stated preferences we examine may have been directly affected by subjects’ previous receipt of a payment offer from the U.S. government. Indeed, as we discuss in Section 5, we find evidence that participants report being more pro-American after receiving the U.S. offer.⁴⁴

An alternative validation exercise is to check whether rejection of the U.S. payment offer is correlated with some behavior of interest. In the context of our study, an important behavioral choice that some subjects make, and that we were able to observe, is dressing in a manner that overtly signals their religious devotion.⁴⁵ Importantly, in this context, a signal of religious dress is often associated with political antipathy toward the United States government.⁴⁶

We regress a dummy variable indicating that a subject was dressed religiously on the interaction of a dummy variable indicating that an individual rejected the payment offer and a dummy indi-

⁴³Using alternative samples of subjects yields very similar results (available from the authors upon request).

⁴⁴Note that the correlation between receiving a U.S. offer and stating positive views about the U.S. would only be problematic for our validation exercise if it also interacted with whether or not the participant accepted the U.S. offer.

⁴⁵Berman (2011) provides an excellent overview of the literature on religious codes. In each experimental session, the lab coordinator was asked to fill out a scoring sheet indicating whether subjects’ clothing contained items that indicated their religiosity. The sheet included several items associated with religiosity, for example, a religious cap, a *mehraab* on the forehead, a *kara* (religious bracelet), or a *taaveez* (amulet); subjects are coded as “religious” if experimenters observed at least one item. Around 24% of subjects in the sample were coded as “religious.”

⁴⁶Of course, the relationship between religiosity and political ideology is complex. However, several religious sects in Pakistan have publicly anti-American views and require members to adhere to dress codes. For example, Deobandi Sunnis, a sect common in all three experimental sites, are known for antagonism toward the United States.

cating that the offer was from the U.S. government (plus the lower-order terms). We first present results estimated using individuals who received offers of 100 Rs. (from the U.S. or LUMS), in the private condition. The coefficient on the interaction indicates whether visible religiosity is associated with rejecting the U.S. offer, rather than accepting it, after differencing out the propensity to reject the LUMS offer.

In Table 4, column 1, one can see that we find a large, positive coefficient on the interaction, though it is not statistically significant. Next, we run the same regression, but include individuals who made their choices in the “public” condition. Including individuals in the public condition is of particular interest here because one would expect that individuals who publicly signal their religiosity should be less affected by the public pressure to express moderate views, found above. Thus, one would expect rejection of the U.S. government offer to be even more strongly correlated with visible religiosity when rejection occurred in the public condition. In Table 4, column 2, one can see that when we examine the pooled public and private condition subjects, we find a large, positive, and statistically significant coefficient on the interaction term. Thus, both stated preferences and behavioral outcomes associated with anti-American attitudes are correlated with rejection of the U.S. government offer.

4.2 Evaluating Alternative Mechanisms

We next address a set of potential confounding factors and alternative interpretations of our results.

Distaste for accepting money offer. Subjects might have privately rejected the U.S. offer not because they disliked the U.S., but rather because they felt uncomfortable accepting an additional monetary payment. As discussed above, we address this possibility by differencing out the private rejection rates from the LUMS offer. This procedure likely generates a lower bound for our estimates since it assumes that no subject who rejected the LUMS offer would have rejected the U.S. offer for being anti-American. Moreover, we also difference out the LUMS public rejection rates from the U.S. public rejection rates to deal with factors other than anti-American sentiment that might specifically affect the public decision (e.g., embarrassment to publicly accept money). As discussed above, our results are robust to subtracting LUMS rejection rates.

Does rejecting the U.S. offer imply anti-American views? One might believe that individuals who dislike the U.S. would actually prefer to take its money—this might be for consequentialist reasons (less money in U.S. government hands can reduce any perceived harm the U.S. might cause) or because it feels good to benefit at the expense of an adversary. Similarly, one might wonder if pro-U.S. individuals might want to leave more money in U.S. government hands to support perceived good that might be done. We believe this is unlikely: not only would the consequences of taking the U.S. government’s money be trivial, but we also find that individuals who reject the U.S. bonus payment offer are strikingly more anti-American in their stated views (and are more likely to be visibly religious).

Rejecting payment might express anti-foreign or anti-government views. The U.S. government offer differed from the LUMS offer both in the foreignness of the entity offering the payment, and in the fact that the entity was a government. One might be concerned that the difference in rejection rates between the U.S. payment and the LUMS payment conditions arose from anti-foreign or anti-government views, rather than specifically anti-American views. However, in examining the correlation between rejection of the U.S. offer and stated preferences, one sees that while individuals who rejected the offer expressed very anti-American views, their views were *not* differentially negative regarding the Japanese government. This suggests that rejection was specifically an expression of anti-American views.

Subjects might feel “insulted” by the offer. Another possible concern with our results is that subjects may have felt insulted by the bonus payment amounts, thinking that they were too small, especially the 100 Rs. payment offer from the U.S. government. Moreover, the correlation between rejection rates of the U.S. offer and stated views on the U.S. are consistent with subjects feeling insulted, since the stated opinions were elicited after the payment intervention. However, we do not believe that this is likely to drive our findings. First, the show-up fee to participate in the experiment was 300 Rs.: participants were willing to take a bus and participate in the survey for that amount. It thus seems unlikely that they found 100 Rs. unreasonably small as a payment for completing the personality survey. Second, as a benchmark for the offer, the 100 Rs. payment was roughly a fifth of a day’s wage, far from trivial. In fact, we believe that it would have been unnatural to offer bonus payment amounts any larger than those we offered, given that the survey subjects completed was not particularly long or challenging.

Time cost to read instructions and check the box. One might wonder if our results might arise from subjects not being willing to make the effort to read the the instructions or to check the box indicating that they accepted the bonus payment offer. We do not believe this is likely for several reasons: first, the payment amounts were sufficiently large that one would expect subjects to find it worthwhile to make the effort. Second, rejecting the money offer also required checking a box. A small number of subjects (less than 3% of our sample) did not check any box and were not paid any bonus payment; our results are robust to dropping these observations (or assigning them to either the acceptance or rejection categories, the latter being our baseline specification). Importantly, not checking any box does not correlate with receiving a U.S. offer, suggesting that a lack of effort or understanding was not specific to the U.S. offer conditions (the p-value of the correlation is 0.6). Finally, if subjects did not read the instructions and thus rejected the bonus payment offer without any ideological component of this choice, one would expect similar rejection rates between the U.S. government and LUMS. One also would not expect rejection to be correlated with stated preferences. Our pattern of results suggests that subjects’ choices to reject payment were a reflection of their attitudes.

Were subjects conscious of the elicitation of their attitudes? Our study aims to

improve upon asking direct survey questions by eliciting subjects’ political attitudes without their being aware of the elicitation. However, the choice that subjects made regarding accepting the bonus payment intentionally, *crucially*, had an ideological component. Thus, it is important to consider whether this ideological component led subjects to think consciously about whether the experimenter was engaged in measuring their ideological positions.

We believe that this is unlikely to be a major concern: subjects had just completed a survey that was entirely non-ideological.⁴⁷ Having completed the survey, subjects simply made a natural choice about payment. Making the acceptance or rejection of payment (and acknowledgement of receipt of funds) as natural and not-explicitly-ideological as possible was a priority in the design of the study. It is thus unlikely that subjects would think about this decision as they would a direct survey question that was explicitly ideological, and was explicitly asked for the purposes of being recorded as part of a research design. Because the payment decision appeared to be merely ancillary to the main study, subjects would have been less conscious of either public stigma or private embarrassment that might arise when providing responses to direct questions.

Were subjects’ choices distorted by fear or risk aversion? Related to concerns about subjects being conscious of the measurement of their attitudes, a natural concern about our design is that subjects’ choices may have been distorted by fear or risk aversion, perhaps arising simply from mentioning the U.S. government. Our motivation in developing a method for eliciting subjects’ attitudes without their being aware of it was that we did not want subjects’ choices to be affected by concerns about what the experimenter (or funder) wished to hear, or what the experimenter (or funder) recorded. We thus specifically designed our study to minimize subjects’ concerns about the anonymity of their choices (with the exception of the “public expression” condition). For example, no signature or identifying information was ever collected from subjects; individual choices therefore could not be matched to the subjects who made them (this is true even at the payment stage).

Still, one might be concerned that subjects would choose whether to accept the bonus payment while thinking about the signal that accepting or rejecting payment would send. In particular, one might worry that rejection of the U.S. government offer was artificially low because risk averse individuals accepted payment *despite* their ideologies, conforming to perceived pressure from the experimenter. As a check of whether subjects’ choices were likely to have been affected by concerns about sanctions for expressing particular attitudes, we can examine whether patterns of behavior were similar for subjects with differing levels of risk aversion.

In envelope 4, we measured subjects’ risk preferences using a five-point Likert scale. We create a “risk averse” dummy that is equal to one if individuals reported to be either “very unwilling” or “unwilling” to take risks (around 56% of the sample are thus categorized as risk averse). First, we note that there is no effect of receiving a U.S. offer on reported risk preferences (results available

⁴⁷As noted above, the experimental instructions prior to the study were focused entirely on the Big 5 personality survey that subjects completed before the intervention of interest, and had nothing to do with political attitudes.

upon request). Second, individuals who are risk averse according to this definition do not show significantly different rates of rejection of the U.S. offer in our baseline condition (100 Rs. payment, private condition) or in the pooled sample; rejection rates are actually (statistically insignificantly) *higher* among the more risk averse.

4.3 External Validity

Our sample consists of literate, young men, and is therefore not representative of Pakistan’s population as a whole. However, our sample includes a broad representation from across Pakistani ethnic groups, drawn from three distinct study sites, and we find the same patterns of results across all main ethnic groups represented and across all three sites (results available upon request). Therefore, although our sample is not representative, our results may broadly hold across a range of literate, young men.

Perhaps our most surprising finding is that moderation of political expression can be achieved via social pressure. While public expression may not always be more moderate than private expression, we find some evidence that this result seems to arise in our context from *natural* social ties: social pressure effects toward moderation are particularly strong in our study for the subjects who reported knowing most other participants in their session. This suggests that our moderation effects were most likely not a consequence of social networks artificially created in the lab.

5 Comparisons with Other Methods of Eliciting Attitudes

5.1 Estimating the share of subjects with anti-American views.

As discussed above, we estimate that if there were zero financial cost of expressing one’s ideology, the private rejection rates of the U.S. offer would be around 30% in our sample. We can compare this number to other estimates of the share of individuals with anti-American views in our sample, coming from direct survey questions, and from list experiments, both of which elicit attitudes at zero financial cost (with varying degrees of “cover” for stigmatized views). It is important to emphasize that in our study, these elicitations were conducted after our intervention of interest. Because simply *receiving* the U.S. government bonus payment offer may have directly affected stated attitudes toward the U.S. (we explore this below), in our analysis of responses to the direct survey questions and to the list experiments we focus on individuals who received the LUMS bonus payment offer.

First, we consider stated attitudes in response to direct survey questions. As described above, we can construct a “negative views” dummy variable based on responses to questions about views on U.S. aid and on the U.S. government more generally. We find that the share of participants who received the LUMS offer who report having a negative view on U.S. aid is 33.3%; the share of those

reporting a negative view of the U.S. government in general is 36.3%. Both of these estimates are quite similar to estimates derived from our revealed preference methodology.

Second, we can estimate the fraction of experimental subjects who are anti-American using the list experiments we conducted. Among subjects receiving the LUMS offer, we estimate that 22% of subjects indicate support for, “refusing humanitarian aid from the U.S. government,” and that 55.1% indicate support for, “supporting the activities of Pakistan Tehreek-e-Insaf (PTI),” the most anti-American of the major parties in Pakistan. Thus, using our revealed preference methodology, we estimate a share of individuals expressing anti-American attitudes at zero financial cost that lies between the estimates coming from the list experiments.

5.2 Does the experimental intervention affect attitudes?

We can also examine whether our experimental intervention, the bonus payment offer, affected subjects’ reported attitudes. Receiving a bonus payment offer from the U.S. government (whether or not it is accepted) may affect subjects’ reported attitudes both because the offer can affect *true* attitudes, and because it can increase the salience of experimenter demand effects, resulting in more positive reported attitudes even when true attitudes don’t change.

In Table 5, we assess the impact of randomly receiving a U.S. government bonus payment offer, rather than a LUMS payment offer, on stated views of U.S. aid and of the U.S. government. In this analysis we regress our “negative views” dummy variables on an indicator that an individual received a U.S. bonus payment offer. We also examine the effect of receiving a U.S. bonus payment offer on the number of statements supported across control and experimental lists in our list experiment. To do so, we regress the number of statements supported in a given list on an indicator that an individual received a U.S. bonus payment offer. If the U.S. payment offer affected true attitudes, one would expect subjects who received the U.S. offer to be less likely to express negative views of the U.S. government and U.S. aid, and also to be less likely to support the anti-American statements in the list experiment.

In Table 5, column 1, one can see that receiving the U.S. bonus payment offer significantly reduced subjects’ likelihood of reporting a negative view of U.S. aid, in response to a direct question. We also find (in column 3) that receiving the U.S. payment offer is associated with a large and significant reduction in the expression of negative views of the U.S. government in general.

Our findings when examining the number of statements supported in the list experiment are far more ambiguous. We first present evidence that randomly receiving the U.S. offer did not affect the number of statements supported in the control list (see Table 5, column 5). Interestingly, when we examine the effect of the U.S. offer on support for a statement indicating opposition to U.S. aid, we find *no effect* (see column 6). The sign of the coefficient is the opposite of what one would expect if the U.S. offer affected actual attitudes toward U.S. aid. We do find evidence that receiving the U.S. offer reduces support for the activities of the anti-American PTI party, though the effect is

not quite statistically significant (see column 7; the p-value is 0.105).

It is natural to wonder whether the difference in statistical significance between the estimated effects of the U.S. offer on stated preferences and preferences revealed using the list experiment were merely a result of the smaller samples available in our examination of the list experiment outcomes. Thus, we estimate the specifications in Table 5, columns 1 and 3, but only using the subsample of individuals who received the control list. We continue to find highly significant effects of the U.S. offer on stated preferences even in this subsample (see Table 5, columns 2 and 4).

These results indicate that subjects who received a bonus payment offer from the U.S. government, rather than LUMS, report more positive attitudes toward the U.S. in response to direct survey questions. However, the experimental intervention may have affected stated attitudes because of experimenter demand effects, rather than actual changes in attitudes. Consider the effect of the U.S. payment offer on stated views of U.S. aid, where our direct survey question and list experiment statement were most directly comparable. Subjects who received the U.S. offer may have reasonably believed that the experimenters preferred that they express positive attitudes toward U.S. aid. When asked directly, this belief about experimenters' preferences may have affected subjects' responses; offering subjects plausible deniability for the expression of negative attitudes (using a list experiment) undid the measured effects of the U.S. offer on reported attitudes.

Our results suggest that list experiments, by providing plausible deniability, may be less affected by experimenter demand effects. However, list experiments suffer from some important drawbacks: most importantly, list experiments are informative about attitudes at the *group*-level; making inferences about any *individual's* attitudes from a list experiment requires making very strong assumptions (and one can see in Table 5 that regressions examining list experiment responses can produce extremely imprecise estimates). List experiments also are not incentivized, and they still require respondents to volunteer (with noise) sensitive information.⁴⁸ Our approach not only reduces concerns regarding experimenter demand effects and other biases by making individuals' preference-revealing actions occur in a state of greater unawareness, but also has the benefits of providing incentivized, individual-level measurements.

5.3 Limitations of our Methodology

Having discussed the virtues of our methodology, it is important to mention its disadvantages as well. Most obviously, some "machinery" is involved, which will not be appropriate for measuring many attitudes or political preferences of interest. Depending on funding availability and willingness to use deception, the approach may be very difficult to apply for certain political entities of interest. The privacy of actions in our study was crucial, and required individuals to be literate to complete the study; this limits our method to use on literate populations or forces the experimenter to

⁴⁸Many of the drawbacks of list experiments also apply to the use of randomized response techniques (see, for example, Warner, 1965) and endorsement experiments (see Bullock et al., 2011b; Blair et al., 2013a).

increase costs by using audio recordings. Finally, one could not measure multiple attitudes for the same individual using this method. All of these caveats should be kept in mind in considering the application of our method in other settings.

6 Conclusion

Understanding the expression of anti-American ideology is a challenging task, not least because the measurement of a potentially sensitive attitude is fraught with difficulties. We have presented a novel methodology, an “offer experiment,” for eliciting individuals’ ideological positions. Our method provides advantages over both direct survey questions and survey experimental techniques used to encourage truthful revelation. Relative to direct survey questions, our method mitigates concerns about untruthful response, experimenter demand, or Hawthorne effects by eliciting subjects’ ideological views without directly asking about them, thus reducing subjects’ awareness of the elicitation. While other methods, such as list experiments, can provide estimates of sensitive attitudes for a *sample*, they are very limited in their ability to do so for an individual. They also do not incentivize truthful reporting. In contrast, our method allows us to observe *revealed preference* expressions of *individuals’* political attitudes.

Using our method, we show that a significant minority of Pakistani men in our sample are willing to forgo a sizable payment simply to avoid checking a box that affirms gratitude toward the U.S. government for providing the funds. This behavior is private, and is unlikely to be of “real world” consequence, suggesting that rejection of payment is an expression of anti-American ideology. Experimental variation in the financial cost of expressing anti-American attitudes allows us to estimate individuals’ willingness to pay to express their views; we find that expression is, indeed, sensitive to price. In addition, experimental variation in perceptions of privacy points to an important social component of political expression, with public expression in our setting appearing much more moderate than private expression. These results contribute to our understanding of an important ideological current in a pivotal part of the world, suggesting that even individuals with extreme views might suppress those views, depending on the economic cost of expression, and on the social environment in which expression occurs.

In addition to providing evidence on the determinants of political expression, our analysis underscores the importance of thinking carefully about biases that may arise in the measurement of attitudes as outcome variables of interest. Along with concerns about eliciting stigmatized attitudes, we point to the particular threat of experimenter demand effects that *interact* with treatments that are being evaluated. Indeed, we find evidence that stated preference measures of attitudes in our study may have been distorted by the experimental intervention (the bonus payment offer) that preceded elicitation.

Our methodology could be applied to study the effects of interventions by governments, political

parties, non-governmental organizations, and corporations. We believe that researchers and policy-makers conducting impact evaluations in which attitudes are an important outcome should consider methods of eliciting attitudes, such as the “offer experiment” studied here, which can mitigate these concerns. We hope that future research will both directly compare the performance of alternative attitude elicitation methods, and adapt the offer experiment methodology for implementation in other settings.

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Table 1: Summary Statistics and Covariates Balance

	Full Sample (1)	LUMS				U.S. government				p-value (10)
		Low payment		High payment		Low payment		High payment		
		Pri (2)	Pub (3)	Pri (4)	Pub (5)	Pri (6)	Pub (7)	Pri (8)	Pub (9)	
Currently engaged in economic activity?	0.504 (0.500)	0.468 [0.042]	0.489 [0.042]	0.518 [0.043]	0.489 [0.043]	0.500 [0.042]	0.529 [0.042]	0.521 [0.042]	0.518 [0.042]	0.97
Age	23.7 (5.0)	23.2 [0.4]	23.6 [0.5]	23.6 [0.4]	24.2 [0.4]	23.3 [0.4]	23.8 [0.5]	24.2 [0.5]	23.6 [0.4]	0.63
Gender (male=1)	1 -	1 -	1 -	1 -	1 -	1 -	1 -	1 -	1 -	-
Single	0.692 (0.462)	0.696 [0.039]	0.691 [0.040]	0.691 [0.039]	0.683 [0.040]	0.748 [0.038]	0.669 [0.040]	0.674 [0.040]	0.684 [0.040]	0.90
Years of education	11.9 (2.8)	12.1 [0.2]	11.9 [0.3]	11.8 [0.2]	11.7 [0.2]	12.1 [0.2]	11.5 [0.3]	11.7 [0.3]	12.0 [0.2]	0.55
Visibly religious	0.239 (0.426)	0.208 [0.034]	0.257 [0.037]	0.264 [0.037]	0.229 [0.035]	0.229 [0.035]	0.160 [0.031]	0.306 [0.039]	0.257 [0.037]	0.12
<i>Ethnic groups</i>										
Punjabi	0.101 (0.301)	0.090 [0.025]	0.098 [0.026]	0.096 [0.025]	0.104 [0.027]	0.101 [0.027]	0.093 [0.026]	0.119 [0.028]	0.105 [0.027]	1.00
Pashtun	0.641 (0.480)	0.634 [0.042]	0.632 [0.042]	0.640 [0.041]	0.634 [0.042]	0.643 [0.042]	0.667 [0.042]	0.622 [0.042]	0.654 [0.041]	1.00
Baluchi	0.091 (0.288)	0.082 [0.024]	0.120 [0.028]	0.103 [0.026]	0.067 [0.022]	0.093 [0.026]	0.093 [0.026]	0.096 [0.025]	0.075 [0.023]	0.88
<i>Religion</i>										
Shia	0.053 (0.224)	0.037 [0.016]	0.045 [0.018]	0.083 [0.024]	0.060 [0.021]	0.040 [0.017]	0.076 [0.023]	0.045 [0.018]	0.038 [0.017]	0.66
Sunni	0.853 (0.354)	0.844 [0.031]	0.841 [0.032]	0.812 [0.034]	0.851 [0.031]	0.849 [0.032]	0.855 [0.031]	0.895 [0.027]	0.880 [0.028]	0.67
Muslim	0.076 (0.265)	0.096 [0.025]	0.091 [0.025]	0.090 [0.025]	0.075 [0.023]	0.095 [0.026]	0.061 [0.021]	0.038 [0.017]	0.060 [0.021]	0.37
<i>Big 5</i>										
Openness to experience	3.020 (0.424)	3.062 [0.032]	3.072 [0.039]	2.998 [0.036]	3.014 [0.035]	3.021 [0.038]	3.015 [0.036]	2.990 [0.035]	2.985 [0.032]	0.56
Conscientiousness	4.110 (0.563)	4.110 [0.047]	4.101 [0.048]	4.110 [0.044]	4.121 [0.044]	4.095 [0.058]	4.124 [0.045]	4.064 [0.043]	4.157 [0.047]	0.93
Extraversion	3.590 (0.512)	3.655 [0.044]	3.586 [0.045]	3.572 [0.038]	3.564 [0.042]	3.543 [0.049]	3.543 [0.041]	3.566 [0.040]	3.689 [0.042]	0.14
Agreeableness	3.805 (0.566)	3.812 [0.047]	3.848 [0.051]	3.792 [0.047]	3.740 [0.048]	3.785 [0.046]	3.835 [0.044]	3.797 [0.048]	3.829 [0.047]	0.82
Neuroticism	2.901 (0.530)	2.902 [0.047]	2.919 [0.041]	2.952 [0.038]	2.869 [0.046]	2.898 [0.046]	2.911 [0.040]	2.880 [0.046]	2.876 [0.049]	0.89
Number of observations	1152	144	144	144	144	144	144	144	144	

Notes: Column 1 presents the mean for each variable based on our sample of 1,152 subjects. The Big 5 characteristics were recorded on a 1 to 5 Likert scale. Standard deviations are in parentheses. Columns 2 to 10 report the mean level of each variable, with standard errors in brackets, for each treatment cell. For each variable, column 10 reports the p-value of a joint test that the mean levels are the same for all treatment cells (columns 2–9). The last row presents the number of observations in each treatment condition. Some calculations used a smaller sample size due to missing information. The proportion of subjects with missing information for each variable is never greater than 8%. The ethnic group categories do not sum to one because of a few small omitted categories (e.g., subjects identifying as Seraiki speakers) and non-response to this question.

Table 2: Rejection Rates, Social Pressure Effects, and Price Effects

	U.S. government (1)	LUMS (2)	U.S. – LUMS (3)
Baseline (private, low payment)	0.252*** [0.036]	0.084*** [0.023]	0.168*** [0.043]
Effects of social pressure (public, low payment) – (baseline)	-0.082* [0.048]	0.027 [0.035]	-0.109* [0.060]
Effect of high payment (private, high payment) – (baseline)	-0.155*** [0.044]	-0.028 [0.030]	-0.127** [0.053]

Notes: The first row of column 1 presents the rejection rate for subjects who received the 100 Rs. U.S. government offer in the private condition. The second row of column 1 presents the difference in rejection rates for those who received the 100 Rs. offer from the U.S. government in the public versus the private condition. The third row of column 1 presents the difference in rejection rates for those who received 500 Rs. versus 100 Rs. offers from the U.S. government in the private condition. Column 2 replicates column 1 for subjects who received an offer from LUMS rather than the U.S. government. Column 3 presents the differences between columns 1 and 2. Robust standard errors in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Revealed and Stated Preferences

	Negative views about U.S. aid (1)	Negative views about U.S. government (2)	Relatively more negative views about U.S. aid (3)	Relatively more negative views about U.S. government (4)	Negative views about Japan aid (5)	Negative views about Japan government (6)	Relatively more negative views about Japan aid (7)	Relatively more negative views about Japan government (8)
Rejected	0.627*** [0.081]	0.542*** [0.085]	0.401*** [0.092]	0.402*** [0.092]	0.020 [0.076]	0.042 [0.071]	-0.019 [0.079]	0.030 [0.086]
Mean (accepted U.S. offer)	0.115*** [0.032]	0.152*** [0.035]	0.170*** [0.038]	0.182*** [0.039]	0.175*** [0.038]	0.125*** [0.033]	0.219*** [0.041]	0.248*** [0.042]
Observations	139	141	135	135	139	140	140	141

Notes: This table reports differences in stated preference views between subjects who rejected and those who accepted the U.S. 100 Rs. private offer. In Column (1), subjects were asked about their views toward aid provided by the U.S. government to Pakistan; possible responses were “very negative”, “negative”, “neither negative nor positive”, “positive”, or “very positive”. We coded a “negative” views about U.S. aid” dummy variable equal to 1 for subjects who answered “very negative” or “negative”. Column (2) uses a question about subjects’ views toward U.S. government in general. Columns (3) and (4) are based on questions asking subjects how their views of U.S. aid and the U.S. government in general compare to those of others in the room. Columns (5) to (8) replicate columns (1) to (4) using views on Japan instead of the U.S. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Revealed Preferences and Behavioral Outcomes

Dependent variable	Visibly religious dummy	
	(1)	(2)
Rejected \times U.S. donor	0.138 (0.137)	0.203** (0.089)
Rejected offer	-0.093 (0.115)	-0.136** (0.064)
U.S. donor	-0.110** (0.052)	-0.060 (0.037)
Constant	0.260*** (0.039)	0.243*** (0.027)
Observations	286	571
R-squared	0.016	0.009
Share visibly religious	0.206	0.214
Sample	Low, private	Low

Notes: This table shows coefficients of regressions of the visibly religious dummy on an interaction of the rejection dummy and the U.S. donor dummy, and main effects of the rejection dummy, and the U.S. donor dummy. The first regression restricts the sample to low payment in private, while the second regression pools subjects who received the low payment offer in private and public conditions. Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Effects of U.S. Offer on Stated Preferences and List Experiments

	Stated preferences				List experiments		
	Negative views about:				Number of supported statements		
	U.S. aid		the U.S. government				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
U.S. donor	-0.138*** [0.026]	-0.125*** [0.026]	-0.133*** [0.027]	-0.098** [0.044]	-0.001 [0.121]	0.011 [0.134]	-0.238 [0.147]
Constant	0.333*** [0.020]	0.293*** [0.033]	0.363*** [0.020]	0.297*** [0.033]	2.262*** [0.083]	2.481*** [0.096]	2.813*** [0.106]
Observations	1,120	375	1,126	378	379	380	375
R-squared	0.025	0.022	0.021	0.013	0.000	0.000	0.007
Sample	Full	Control list	Full	Control list	Control list	U.S. aid list	PTI list

Notes: Columns 1-4 present regressions of whether the subject stated negative views about U.S. aid or about the U.S. government on a U.S. offer dummy for both the full sample and the control list subsample. Column 5 presents a regression of the number of statements the subject indicated agreeing with on a U.S. offer dummy for the control list subsample. Column 6 reports the same specification as column 5, but estimated on the subsample where the item added to the control list indicates opposition to U.S. aid. Column 7 reports the same specification as column 5, but estimated on the subsample where the item added to the control list indicates support for the PTI party. Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Online Appendix: Not for publication

A1 Appendix Figures and Tables

Table A.1: Measuring Private Political Attitudes

Dependent Variable:	Rejected (=1)		
	(1)	(2)	(3)
U.S. government	0.168*** [0.043]	0.169*** [0.043]	0.175*** [0.046]
Mean LUMS offer (low, private)	0.084 [0.023]	0.084 [0.023]	0.084 [0.023]
Session FE	No	Yes	Yes
Covariates	No	No	Yes
Observations	286	286	243

Notes: This table reports the coefficients of regressions of rejection on a U.S. government donor dummy. Column 1 reports coefficients of a regression with no controls. Column 2 reports coefficients of a regression using session fixed effects. Column 3 reports coefficients of a regression including session fixed effects and a set of subject covariates. The sample in these regressions includes subjects who received the 100 Rs. offer in the private condition. All of the variables presented in Table 1 are included as covariates in column 3. The sample size in the regression presented in column 3 is smaller due to missing values for some covariates. Robust standard errors are in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.2: The Effect of the Public Treatment

Dependent Variable:	Rejected (=1)		
	(1)	(2)	(3)
Public \times U.S. government	-0.109* [0.060]	-0.107* [0.060]	-0.141** [0.064]
Public	0.027 [0.035]	0.028 [0.036]	0.066* [0.038]
U.S. government	0.168*** [0.043]	0.169*** [0.043]	0.179*** [0.045]
Mean LUMS offer (low, private)	0.084 [0.023]	0.084 [0.023]	0.084 [0.023]
Session FE	No	Yes	Yes
Covariates	No	No	Yes
Observations	571	571	488

Notes: This table reports the coefficients of regressions of rejection on the interaction of a public condition dummy and a U.S. government donor dummy, and main effects of the public condition dummy and the U.S. government donor dummy. Column 1 reports coefficients of a regression with no controls. Column 2 reports coefficients of a regression using session fixed effects. Column 3 reports coefficients of a regression including session fixed effects and a set of subject covariates. The sample in these regressions includes all subjects who received the 100 Rs. offer. All of the variables presented in Table 1 are included as covariates in column 3. The sample size in the regression presented in column 3 is smaller due to missing values for some covariates. Robust standard errors are in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.3: Price Effects

Dependent Variable:	Rejected (=1)		
	(1)	(2)	(3)
High payment \times U.S. government	-0.127** [0.053]	-0.127** [0.053]	-0.129** [0.056]
High payment	-0.028 [0.030]	-0.027 [0.033]	-0.016 [0.033]
U.S. government	0.168*** [0.043]	0.169*** [0.042]	0.181*** [0.044]
Mean LUMS offer (low, private)	0.084 [0.023]	0.084 [0.023]	0.084 [0.023]
Session FE	No	Yes	Yes
Covariates	No	No	Yes
Observations	572	572	499

Notes: This table reports the coefficients of regressions of rejection on the interaction of a high payment dummy and a U.S. government donor dummy, and main effects of the high payment dummy and the U.S. government donor dummy. Column 1 reports coefficients of a regression with no controls. Column 2 reports coefficients of a regression using session fixed effects. Column 3 reports coefficients of a regression including session fixed effects and a set of subject covariates. The sample in these regressions includes subjects who received an offer in the private condition. All of the variables presented in Table 1 are included as covariates in column 3. The sample size in the regression presented in column 3 is smaller due to missing values for some covariates. Robust standard errors are in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

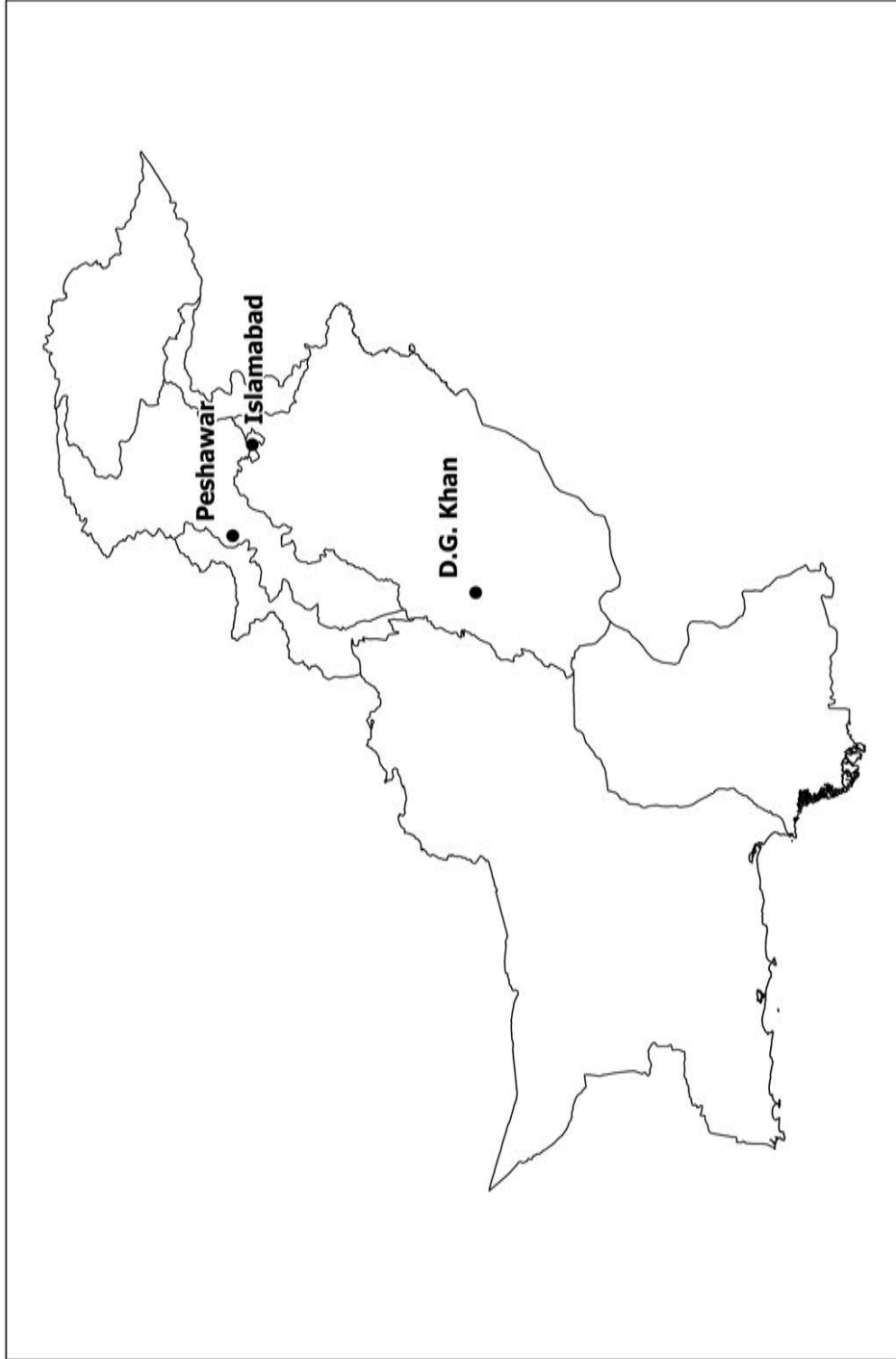


Figure A.1: Map of Laboratory Locations in Pakistan

Panel A: First Screening Test

ایسا کبھی نہیں ہوتا

”دنیا بھر کی سستی، کام چوری اور کاہلی میری لڑکی پر ختم ہے۔“

امی کی ایونٹ ٹرانسمیشن کا آغاز خلاف توقع آج جلدی ہو گیا تھا۔ اس نے ڈھنائی کی اعلیٰ روایات قائم کرتے ہوئے انہیں نظر انداز کر کے لینے رہنے کی کوشش کی مگر آج امی فارم میں تھیں اور مسلسل اس کی مدح سرائی فرما رہی تھیں اسے اٹھنا ہی پڑا مگر یہ اٹھنا عام اٹھنا نہیں تھا۔ اپنے کمرے کے دروازے کو اچھی طرح پٹخ کر وہ باہر آئی تھی۔

”چار گھنٹے پہلے تو آپ کا فرمان تھا کہ دنیا بھر کی سستی، کام چوری اور کاہلی تجھ سے شروع ہوتی ہے اور چار گھنٹے کے اندر اندر یہ مجھ پر ختم ہوتا شروع ہو گئیں بندے کو اپنی زبان پر تو قائم رہنا چاہئے۔“

اس نے صحن میں آتے ہی بیان داغا تھا اور پھر برآمدے کے واش بیسن کے سامنے کھڑے ہو کر چہرے پر پانی کے چھینے مارنے لگی امی صحن میں تخت پر بیٹھی ہنسی بنا رہی تھیں۔

”زبان دیکھی ہے قہنجی کی طرح چلتی ہے۔“

انہوں نے اس کی بات پر آگ بگولہ ہوتے ہوئے کہا تھا۔

Panel B: On-site Screening Test

ترکیب

مرغ کو صاف کر کے دھو لیں۔ ایک دہیچی میں آدھا کپ پانی ڈالیں، اس میں لوہنگ اور بسن ڈال کر مرغ ہلکا گلائیں۔ بلینڈر میں بادام، پستہ، انجیر، کھوپرا، ادراک، پنے، خشخاش، دہی ڈال کر موٹا موٹا پیس لیں۔ ایک دہیچی میں کوکنگ آئل گرم کر کے اس میں پیاز ڈال کر ہلکا سبز ہونے دیں اس کے بعد اس میں پیسا ہوا مصالحہ، نمک اور سرخ مرچ پاؤڈر شامل کر کے بھون لیں۔ اس کے بعد بھنا ہوا مصالحہ مرغ کے پیس میں بھر دیں۔ اوپر بھی اچھی طرح لگا دیں۔ بیکنگ ڈش میں کوکنگ آئل لگا کر اسے چکنا کر لیں مرغ اس میں رکھ کر پہلے سے گرم اوون میں 200 ڈگری سینٹی گریڈ پر رکھ کر پینتیس سے چالیس منٹ تک بیک کریں۔ اس کے بعد مرغ کے اوپر کوکنگ آئل لگا کر مزید دس منٹ تک بیک کریں۔ گوشت کی رنگت سرخی مائل ہونے پر اسے اوون سے نکال لیں۔ لذیز مرغ میوہ دار تیار ہے۔

سلاد کے ساتھ پیش کریں۔

Figure A.2: Urdu Versions of Literacy Screening Tests

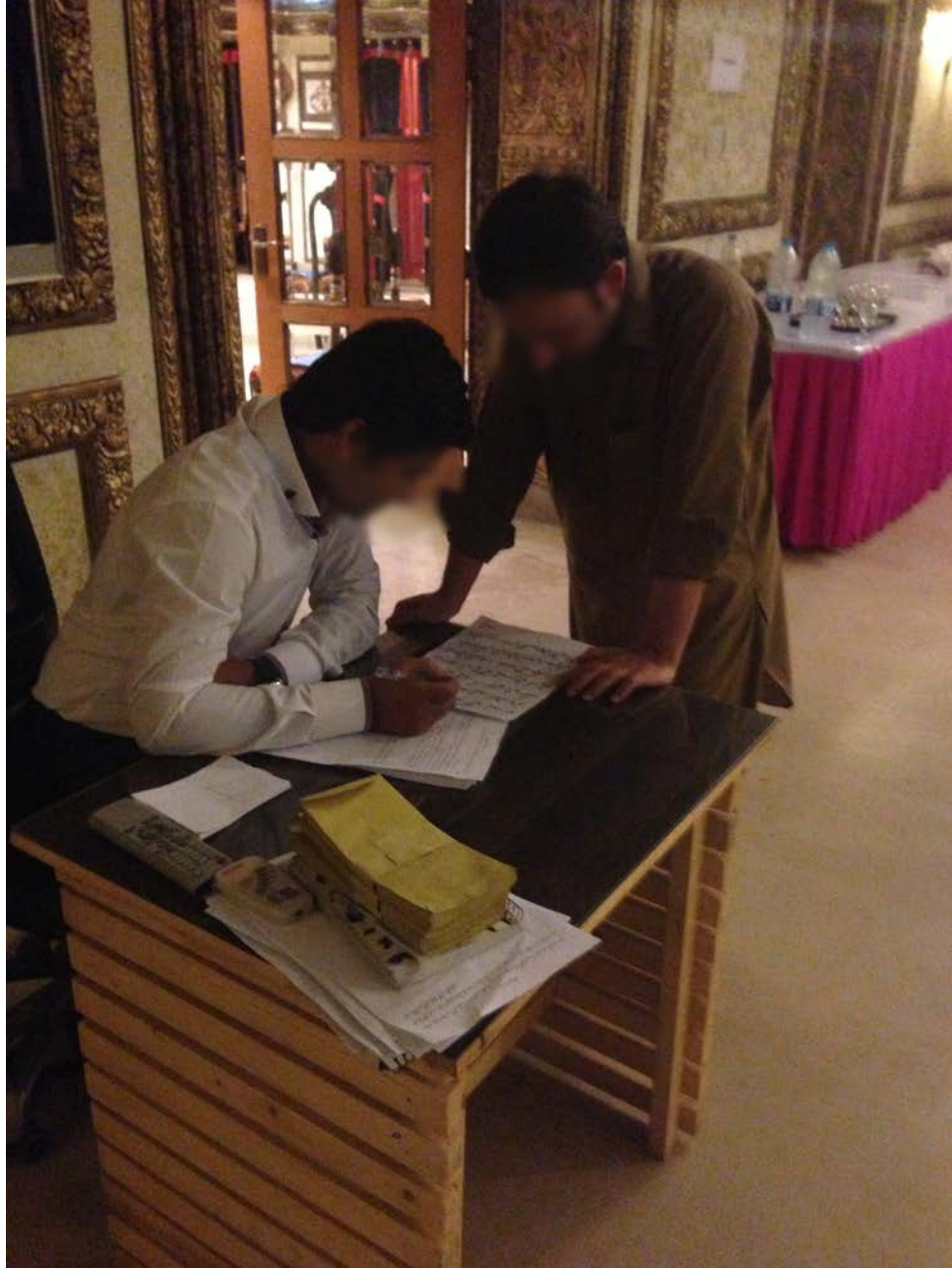


Figure A.3: Enrollment Desk Outside of the Lab in Islamabad



Figure A.4: Experiment Session in Islamabad



Figure A.5: Experiment Session in Peshawar

A2 Experimental Protocols

On-site literacy screening script

Clean the chicken and then wash it. Add half a cup of water. Add cloves and garlic and cook the chicken until it is slightly tender. Blend together almonds, pistachios, fig, coconut, ginger, chick peas, poppy seeds and yogurt in a blender. Put some cooking oil in a pot and warm it. Add some onion to it and allow it to become green. Then add to it crushed spices, salt, and red chili powder and cook. Then stuff the cooked spices in the chicken's stomach. Cover the outside of the chicken with this preparation as well. Cover the baking dish with cooking oil and put the chicken in the dish. Then put this dish in an oven pre-heated to 200 degrees centigrade, and let it bake for 35 to 40 minutes. Then put some cooking oil on the chicken and bake it for another 10 minutes. When the chicken starts turning red, take it out. Your delicious, sweet chicken is ready! Serve with salads.¹ (See the Urdu version of our screening tests in Appendix Figure A.2.)

Experimental Instructions

Activity/Envelope 1

Standard Big 5 survey adapted to use in Pakistan. This is used in all survey versions (versions A-X).

¹This text was taken from a free online repository of recipes in Urdu (<http://www.lawaonline.com/blog/murg-mewa-dar-recipes-pakistani-cooking-urdu-recipes/>), accessed July 7, 2013.

Activity/Envelope1

Instructions for filling out the questionnaire:

1. Read every statement carefully and encircle the response you agree with.
 - a. If you completely disagree with the statement, encircle (1).
 - b. If you mostly disagree with the statement, encircle (2).
 - c. If you are indifferent to the statement, encircle (3).
 - d. If you mostly agree with the statement, encircle (4).
 - e. If you completely agree with the statement, encircle (5).
2. This test has no concept of right or wrong, nor do you have to be an expert to solve it. Respond as sincerely as possible. Write your opinion as carefully and honestly as possible. Answer every question and ensure that for every response, you have encircled the right option. During the test, if you encircle the wrong option by mistake or if you change your mind after encircling a response, do not erase it. Instead, mark the wrong response with a cross and encircle your correct one.

Statements:

1. I am not depressed 1 2 3 4 5
2. I like to be amongst lots of people 1 2 3 4 5
3. I don't like to waste time day-dreaming 1 2 3 4 5
4. I try to be polite to everyone I meet 1 2 3 4 5
5. I keep all my things clean and tidy 1 2 3 4 5
6. I often feel inferior to other people 1 2 3 4 5
7. I laugh easily 1 2 3 4 5
8. When I find out the right way to do something, I stick with it 1 2 3 4 5
9. I often get into quarrels with my family members and coworkers 1 2 3 4 5
10. I pace my work such that I am able to complete everything on time 1 2 3 4 5
11. Sometimes when I am under intense psychological pressure, I feel as if I am about to fall to pieces 1 2 3 4 5
12. I don't consider myself to be a jolly person 1 2 3 4 5
13. Art and wonders of nature fascinate me 1 2 3 4 5
14. Some people think that I am selfish and egoistic 1 2 3 4 5
15. I am not a very organized person 1 2 3 4 5
16. I rarely feel lonely or sad 1 2 3 4 5
17. I really enjoy talking to people 1 2 3 4 5
18. I think that listening to controversial speakers can confuse students and lead them astray 1 2 3 4 5
19. I prefer cooperation over conflict 1 2 3 4 5
20. I try to complete all tasks entrusted to me according to my conscience 1 2 3 4 5
21. I often feel mentally stressed and anxious 1 2 3 4 5
22. I often long for thrilling situations 1 2 3 4 5
23. Poetry has very little or no influence on me 1 2 3 4 5
24. I am mistrustful and skeptical about the intentions of others 1 2 3 4 5

25. My objectives are very clear and I work to achieve them in a very organized way 1 2 3 4 5
26. Sometimes I feel completely worthless 1 2 3 4 5
27. I usually prefer to work alone 1 2 3 4 5
28. I often try new and exotic dishes 1 2 3 4 5
29. I believe that if you give them the chance, people will always exploit you 1 2 3 4 5
30. I waste a lot of time before starting to work 1 2 3 4 5
31. I rarely feel scared or depressed 1 2 3 4 5
32. I often feel full of energy 1 2 3 4 5
33. I don't pay much attention to the moods and feelings evoked by my surroundings and circumstances 1 2 3 4 5
34. People who know me usually like me 1 2 3 4 5
35. I work very hard to achieve my goals 1 2 3 4 5
36. I often get frustrated by the way people treat me 1 2 3 4 5
37. I am a jolly and optimistic person 1 2 3 4 5
38. I believe that we should consult religious leaders for making decisions involving moral affairs 1 2 3 4 5
39. Some people think I am cold-hearted and selfish 1 2 3 4 5
40. When I start something, I don't rest until I finish it 1 2 3 4 5
41. Often when things start taking a turn for the worse, I give up and abandon my work 1 2 3 4 5
42. I am not a jolly and optimistic person 1 2 3 4 5
43. Sometimes while studying poetry or looking at masterpieces of art, I feel chills of thrill and excitement 1 2 3 4 5
44. I am strict and stubborn in my attitude 1 2 3 4 5
45. Sometimes I am not as trustworthy as I ought to be 1 2 3 4 5
46. I am rarely sad or depressed 1 2 3 4 5
47. Fast pace is a highlight of my life 1 2 3 4 5
48. I have little interest in pondering over the working of the universe or the human condition 1 2 3 4 5
49. I usually try to be concerned and care about others 1 2 3 4 5
50. I am a useful person and always do my work 1 2 3 4 5
51. I often feel helpless and wish someone else would resolve my problems 1 2 3 4 5
52. I am a very active person 1 2 3 4 5
53. I have a lot of intellectual curiosity in me 1 2 3 4 5
54. If I don't like someone I let him/her know about it 1 2 3 4 5
55. I feel that I can never keep myself organized 1 2 3 4 5
56. Sometimes I want to hide myself due to shame 1 2 3 4 5
57. I would prefer to live on my own terms as opposed to being a leader for others 1 2 3 4 5
58. I often enjoy abstract ideas and theories 1 2 3 4 5
59. If need be, I am ready to use people to get my own work done 1 2 3 4 5
60. I try to do everything perfectly 1 2 3 4 5

Please give us answers to the following questions.

- 1.1 Are you currently engaged in any economic activity from which you earn income?
1. Yes
 2. No
- 1.2 Apart from your main economic activity, are you engaged in any other economic activity?
1. Yes
 2. No
- 1.3 Which of these best describes your secondary economic activity? (S.A.)
1. Employee receiving wages / salary
 2. Daily paid / casual worker / in temporary employment
 3. Agricultural crops or livestock related self employment
 4. Other self employment
 5. Other (describe _____)
- 1.4 Which of the following types of agricultural crop/livestock activities are you involved in? (mark all)
1. Rice
 2. Wheat
 3. Cotton
 4. Other grains (corn, maize, etc.)
 5. Tobacco
 6. Other (specify: _____)
- 1.5 How often do you receive income from these agricultural crop/livestock self employment activities? (mark all)
1. At least weekly
 2. At least every two weeks
 3. At least monthly
 4. Less frequently than monthly

Personal Information

- 1.6 What is your age and year of birth?
- Years _____ Months _____ Year of Birth _____
- 1.7 Gender
1. Male
 2. Female
- 1.8 Marital Status
1. Single/Never Married
 2. Married
 3. Widowed
 4. Divorced
 5. Separated

1.9 What ethnic group do you belong to ?

1. Punjabi
2. Pashtun
3. Tajik
4. Hazara
5. Baluchi
6. Other (Specify.....)

1.10 What religious group do you belong to?

1. Shia
2. Sunni
3. Christian
4. Hindu
5. Sikh
6. Other (Specify.....)

1.11 Which languages do you speak at home?

	1. Yes	2. No
a. Pashto	1	2
b. Persian/Dari	1	2
c. Balochi	1	2
d. Punjabi	1	2
e. Urdu	1	2

1.12 What is the highest level of education that you have completed? _____

- 1=Completed Grade 1,
- 2=Completed Grade 2,
- 3=Completed Grade 3,
- 4=Completed Grade 4,
- 5=Completed Grade 5,
- 6=Completed Grade 6,
- 7=Completed Grade 7,
- 8=Completed Grade 8,
- 9=Completed Grade 9,
- 10=Completed Grade 10 (Matric/ O Levels),
- 11=Completed Grade 11,
- 12=Completed Grade 12 (FSC/A Levels),
- 13=Completed some years at university,
- 15=Completed university degree,
- 16=Masters,
- 17=Technical Training,
- 18=Not gone to school

1.13 Then how many years of formal education have you had?

Years _____

Please place the completed survey back in envelope 1.

After that, you should close and seal envelope 1, and place it in the larger envelope.

After that, you may open envelope 2.

Activity/Envelope 2

Thank you for completing the survey. In return for completing the survey, we can offer you a bonus payment. Funding for the bonus payment comes from {*Survey versions A-L*: Lahore University of Management Sciences; *Survey versions M-X*: the U.S. government}. We can pay you {*Survey versions A, B, C, G, H, I, M, N, O, S, T, U*: 100 Rs.; *Survey versions D, E, F, J, K, L, P, Q, R, V, W, X*: 500Rs} for completing the survey, but in order to receive the bonus payment you are required to acknowledge receipt of the funds provided by {*Survey versions A-L*: Lahore University of Management Sciences; *Survey versions M-X*: the U.S. government} and thank the funder. If you choose not to accept the payment, you will forgo the bonus payment of {*Survey versions A, B, C, G, H, I, M, N, O, S, T, U*: 100 Rs; *Survey versions D, E, F, J, K, L, P, Q, R, V, W, X*: 500 Rs}, but not the payment of 300 Rs for your participation.

The letter of acknowledgment and thanks can be found on the next page.

{*Survey versions G-L, S-X*: If you choose to accept the bonus payment, in order to receive this additional payment, you will be asked to turn the letter in to the survey coordinator in the front of the room, so other participants will see you turn in the letter. Once you have made your decision on the next page, please place the letter into envelope 2, whether or not you chose to accept the bonus payment.}

{*Survey versions A-F, M-R*: If you choose to accept the bonus payment, in order to receive this additional payment, **your decision will be completely private**; you will simply replace the letter in envelope 2 and submit it with your other survey materials at the end of the study, so no other participants will know your choice. Once you have made your decision on the next page, please place the letter into envelope 2, whether or not you chose to accept the bonus payment.}

After that, you should close and seal envelope 2, and place it in the larger envelope.

After that, you may open envelope 3.

Letter of acknowledgment and thanks

Letter of acknowledgment and thanks

☐ I gratefully thank Lahore University of Management Sciences for its generosity and I accept the bonus payment offer.

☐ I choose not to accept the bonus payment offer.

Envelope 3

1) [Survey versions A-X]

In the previous section, you were asked to check a box to indicate that you thanked the funder for their generosity. There are 24 people in this room. How many people in this group, excluding yourself, do you believe were willing to accept the additional payment by checking the box?

If your guess is one of the three closest to the true number (among participants in this group), you will receive an extra 300 rupees.

Please indicate your belief about how many checked the box here _____

2) [Survey versions A-X]

There are 24 participants in this session. Approximately, how many people in this room are you acquainted with?

1 – No one

2 - Between 1 and 6

3 - Between 7 and 12

4 – Between 13 and 18

5 - Between 19 and 23

6 – Everyone

Envelope 3

LIST EXPERIMENTS: [DO NOT TRANSLATE THIS LINE]

Control: [Survey versions A, D, G, J, M, P, S, V]

The following are four policies some government officials express support for. Please report HOW MANY of the four you support. You do not need to indicate which ones you support, just how many.

- a. Providing the poor with free electricity generators
- b. Establishing an independent state in Kashmir that is not part of India and not part of Pakistan
- c. Ensuring that civilians (President or Prime Minister) control the military
- d. Reducing number of people eligible for the Benazir Income Support Program, but increasing payments to those eligible

TOTAL THAT YOU SUPPORT (CIRCLE ONE) 0 1 2 3 4

Treatment 1: [Survey versions B, E, H, K, N, Q, T, W]

The following are five policies some government officials express support for. Please report HOW MANY of the five you support. You do not need to indicate which ones you support, just how many.

- a. Providing the poor with free electricity generators
- b. Establishing an independent state in Kashmir that is not part of India and not part of Pakistan
- c. Ensuring that civilians (President or Prime Minister) control the military
- d. Reducing number of people eligible for the Benazir Income Support Program, but increasing payments to those eligible
- e. Refusing humanitarian aid from the US government

TOTAL THAT YOU SUPPORT (CIRCLE ONE) 0 1 2 3 4 5

Treatment 2: [Survey versions C, F, I, L, O, R, U, X]

The following are five policies some government officials express support for. Please report HOW MANY of the five you support. You do not need to indicate which ones you support, just how many.

- a. Providing the poor with free electricity generators
- b. Establishing an independent state in Kashmir that is not part of India and not part of Pakistan
- c. Ensuring that civilians (President or Prime Minister) control the military
- d. Reducing number of people eligible for the Benazir Income Support Program, but increasing payments to those eligible
- e. Supporting the activities of Pakistan Tehreek-e-Insaf (PTI)

TOTAL THAT YOU SUPPORT (CIRCLE ONE) 0 1 2 3 4 5

Envelope 3

[Survey versions A-X]

Please place the completed survey back in envelope 3.

After that, you should close and seal envelope 3, and place it in the larger envelope.

After that, you may open envelope 4.

Activity/Envelope 4

SURVEY VERSIONS A-X

This is the final section. Please complete the questions below and then place this document back in the envelope.

1. How do you view aid provided by the Japanese government to Pakistan? Very negatively (1), very positively (5), or something in between?

Circle one of the following: 1 2 3 4 5

2. How do you view the Japanese government overall? Very negatively (1), very positively (5), or something in between?

Circle one of the following: 1 2 3 4 5

3. How do you view aid provided by the United States government to Pakistan? Very negatively (1), very positively (5), or something in between?

Circle one of the following: 1 2 3 4 5

4. How do you view the United States government overall? Very negatively (1), very positively (5), or something in between?

Circle one of the following: 1 2 3 4 5

5. How willing are you to take risks? Are you very unwilling to take risks (1)? Are you very willing to take risks (5)? Or, something in between?

Circle one of the following: 1 2 3 4 5

6. Do you know the name of the chief minister of your province? Please write the name below:

7. How do you think your political views on Japan compare to other individuals in the room? More anti-Japanese (1), more pro-Japanese (5), or something between?

Circle one of the following: 1 2 3 4 5

8. How do you think your political views on receiving aid from Japan differ relative to other individuals in the room? Less willing to accept aid (1), more willing to accept aid (5), or something in between?

Circle one of the following: 1 2 3 4 5

9. How do you think your political views on the United States compare to other individuals in the room? More anti-American (1), more pro-American (5), or something between?

Circle one of the following: 1 2 3 4 5

10. How do you think your political views on receiving aid differ from the United States relative to other individuals in the room? Less willing to accept aid (1), more willing to accept aid (5), or something in between?

Circle one of the following: 1 2 3 4 5

11. Would your decision of whether to take the additional payment by checking the box have changed if the payment amount was increased by 100 rupees?

1 – Yes

2 – No

12. Would your decision of whether to take the additional payment by checking the box have changed if the payment amount was increased by 300 rupees?

1 – Yes

2 – No

13. Would your decision of whether to take the additional payment by checking the box have changed if the payment amount were offered by the government of Japan?

1 – Yes

2 – No

14. Would your decision of whether to take the additional payment by checking the box have changed if the payment amount were offered by the University of California (an American university unaffiliated with the government)?

1 – Yes

2 – No

Please place this completed survey back in envelope 4, seal the envelope, and place envelope 4 in the large envelope. Then, raise your hand to indicate that you have completed the survey.