

Corruption and Political Mobilization: Evidence from a Natural Experiment*

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Abstract

How do voters react to news of political corruption? Information about corruption may mobilize citizens to demand political and institutional reform, but existing empirical evidence is mixed. We argue that the effects of information about corruption on citizen attitudes and voting behavior is mediated by political efficacy (perceived influence of activism on political outcomes), which varies considerably across and within emerging democracies. To test the argument, we draw on survey data from Afghanistan collected during the 2010 Kabul Bank crisis, which revealed corruption in the formal banking system. The unanticipated scandal unfolded midway through the collection of the survey, allowing us to adopt a novel quasi-experimental approach. The scandal led to an increase in perceived corruption in government and heterogeneous effects on voting via a political efficacy mechanism. Our argument and results clarify an important puzzle in the cross national literature on corruption and voter mobilization.

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The damaging effects of official corruption on economic growth and democratic governance are well-documented, both at the macro-level (Rose-Ackerman, 1999; Mauro, 1995; Emerson, 2006; Ahlin and Pang, 2008) and the micro-level (Ferraz, Finan and Moreira, 2012; Olken, 2007; Reinikka and Svensson, 2005; Fisman and Svensson, 2007). To combat corruption, elections theoretically act as institutions of accountability (Ferejohn, 1986; Besley, 2006; Billger and Goel, 2009), disciplining corrupt politicians and improving governance. Why, then, does government corruption persist?

One of the most oft-cited answers to this question is that citizens lack the requisite information to vote out corrupt actors (Pande, 2011). If this information asymmetry were resolved, citizens would punish corruption electorally. Perhaps the most compelling evidence of this theory comes from Brazil, where news of political corruption mobilized voters to punish incumbents at the polls (Ferraz and Finan, 2008). Yet other studies of information's effects on turnout return either little or no evidence of a mobilizing effect on turnout (Chang, Golden and Hill, 2010; Humphreys and Weinstein, 2012), or indicate that information can actually *depress* turnout (Chong et al., 2015; Stockemer, LaMontagne and Scruggs, 2013). What explains this mixed empirical evidence?

We examine this puzzle in the context of the 2010 Kabul Bank crisis, one of the largest banking failures in the world, which revealed corrupt links between high-ranking Afghanistan public officials and the largest Afghan private lender (McLeod, 2016; Kos, 2012). Within days, the scandal triggered widespread bank runs and the largest government bailout in the country's history. The unanticipated scandal unfolded three weeks before the 2010 parliamentary election. The scandal also unexpectedly occurred midway through the collection of a nationwide survey, which included questions about corruption in government, voter preferences, and the efficacy of government institutions. Since the sequence of survey sampling was fixed months prior to the survey and enumeration was randomized within districts, we are able to adopt a novel quasi-experimental approach successfully utilized in other contexts

(e.g., Balcells and Torrats-Espinosa (2018); Mikulaschek, Pant and Tesfaye (forthcoming)).

Overall, we find that while the scandal made people more likely to think corruption was a problem in the government, it did not have either an overall mobilizing or demobilizing effect on turnout. These aggregate results, however, may mask offsetting heterogeneous effects anticipated by prior work. Motivated by a theoretical and empirical literature that argues voter mobilization is influenced by citizens' beliefs that their votes substantively impact political outcomes (Burlacu, 2018; Campbell, Gurin and Miller, 1954; Norris, 2004, 2011), we examine whether the scandal had heterogeneous impacts on citizens' attitudes and intention to vote. In areas with low efficacy, where citizens are skeptical of their influence on political reform, we anticipate voter demobilization. On the other hand, in regions with high efficacy, where citizens firmly believe their votes can affect policy change, we anticipate voter mobilization. Indeed, we find that in areas with relatively low levels of political efficacy (as measured by a separate nationwide survey), news of the scandal caused citizens whose material interests would be affected—those most likely to be using the formal banking system—to be *more* likely to see corruption as a serious problem in government but *less* likely to intend to vote in the parliamentary election several weeks later. In contrast, in areas with relatively high levels of self-reported political efficacy, we observe evidence consistent with studies from other contexts that find a mobilizing effect of information about corruption on voter turnout among those citizens whose material interests would be affected: the unfolding bank scandal had a sizeable, positive, and highly statistically significant effect on respondents' intention to vote. We show these results are robust to a number of alternative model and sample specifications, including a range of pre/post event windows, alternative thresholds for classifying political efficacy, and estimation over different subsets of administrative districts based on exogenous patterns of survey enumeration.

This article makes several important contributions. First, our research contributes to our understanding of processes of democratization where corruption threatens growth and

political institutions. Dating at least to Tocqueville and Mill, scholars of democracy have theorized that democratic participation makes people better citizens and strengthens society’s democratic norms and institutions (Mansbridge, 1998; Pateman, 1976). As Jane Mansbridge (1998, 319) observes: “As we look back over the history of small democracies—from the politically engaged daily life of the Athenian citizen, through the competition for office in the far less democratic Florentine republic, to the direct democracy of a New England town hall meeting—we find considerable evidence that engagement in political life and public responsibility go hand in hand.” Our paper speaks to this rich literature and emphasizes the importance of these theoretical and philosophical insights for citizen engagement with democracy, particularly in the wake of scandals that can undermine or mobilize voter participation.

Second, we increase confidence in our results by taking advantage of an unanticipated political event that “as if” randomly transmitted new information to some citizens about political corruption during the course of a single survey just prior to a major national election. Some of the uncertainty in the literature derives in no small part from the paucity of opportunities available for scholars to exploit naturally occurring information “shocks” and study effects on turnout, as in Ferraz and Finan (2008). This financial scandal provides an excellent test of the informational theory in that it was unexpected, it was politically relevant because it occurred roughly two weeks prior to a national election of considerable importance, it involved corruption associated with the highest profile political actor in the country (President Karzai), and it was economically destabilizing. We combine the strengths of experimental and observational approaches, estimating causal effects of an actual financial scandal on citizen perceptions of government corruption and intention-to-vote.

Third, we study corruption in a highly relevant policy context. The recent declassification of the Afghanistan Papers, a compilation of retrospective interviews conducted by the Special Inspector General for Afghan Reconstruction, make this point clear: tens of billions of dollars

were siphoned from official projects to enrich political elites, warlords, and the Taliban. 129 of the roughly 400 interviews explicitly mention concerns about the role of corruption in undermining economic growth, political stability, and security provision in Afghanistan. With the future of international operations in Afghanistan hanging in the balance, it is important to better understand how, if at all, citizens responded to an unexpected, high profile corruption scandal on the eve of a national election.

Finally, while the evidence we present is from one important case of corruption, our theoretical argument has broad applicability to other contexts. By emphasizing the scope of citizens' exposure to unfolding corruption and their level of political efficacy, the argument generates predictions for a broad range of cases and helps to make sense of otherwise puzzling empirical patterns of citizen reaction to corruption.

In the next section, we provide relevant background information on the Kabul Bank scandal. The third section explains the research design and describes the data used in the analysis. In the fourth section, we present the main results and then provide robustness checks on them, as well as supplemental results. We conclude by discussing these results in the broader context of research on the persistence of political corruption in the developing world.

Context

To examine the political consequences of financial scandals, we exploit the unexpected onset of a major Afghan financial crisis: the 2010 Kabul Bank corruption scandal and subsequent government bailout. Potentially fraudulent activity was reported to the Afghan Central Bank, Da Afghanistan Bank (DAF), in 2009. Despite these reports, banking transactions remained stable, large-scale withdrawals (runs) by individuals did not occur, and external advisers from Deloitte did not follow up on fraud allegations. By late August 2010, however, DAF mandated the removal of Chairman Farnood as well as Chief Executive Officer

Khalilullah Ferozi. Kabul Bank was put into conservatorship on August 28. After the government announced its seizure of the bank, panic spread and a large-scale run on the bank's deposits occurred.¹ Within days, 180 million USD in savings were withdrawn, jeopardizing the bank's solvency.

The Kabul Bank scandal revealed serious flaws in Afghanistan's weak banking institutions (Kos, 2012; Rosenberg, 2012). From 2004 to 2010, the bank issued roughly 74 million USD in legitimate loans and more than 860 million USD in fraudulent loans. The Bank's illegal activities included speculative investments in Dubai real estate, the commercial airline Pamir Airways, and the reelection campaign of then-president Hamid Karzai. The Bank also loaned Mahmoud Karzai, the president's brother, 22 million USD which he used to purchase enough Kabul Bank shares to make himself the third largest shareholder. The DAF intervention kept the Afghan banking system afloat, forestalling a potential economic collapse. The effectiveness of this bailout, however, remained highly uncertain during the short window we study after the scandal emerged.

We study the effect that the scandal had on citizens' attitudes about corruption in government and their intention to vote several weeks later in the parliamentary election. We argue that this measure of political engagement is useful and informative for at least two reasons. First, scholars in other contexts have carefully studied intention to vote as a meaningful measure of electoral participation, such as in Spanish elections after terrorist attacks (Balcells and Torrats-Espinosa, 2018). Second, there might be concern that respondents'

¹It is possible that some subset of our sample was aware of the fraudulent lending prior to the removal of Chairman Farnood and CEO Ferozi. We anticipate that survey respondents with prior knowledge about the bank's vulnerabilities are most likely to be among the group of high socioeconomic status persons that we classify as "exposed" to effects of the crisis. Given our research design (which leverages changes in attitudes pre versus post), this potential concern would cause a downward bias in our estimate.

intention to vote is systematically higher than actual turnout, as has been documented elsewhere (Achen and Blais, 2016). Given the data available in our case, this is an empirical question which we can investigate. We gather official turnout data by province and calculate a comparable intent-to-vote by province.²

In Figure 1, we plot province-level averages from the intent-to-vote survey question we use in the main analysis below (X axis) against official turnout in the province (weighted by voting age population) (Y axis). We overlay the line of best fit on the scatterplot. While there are a few provinces off the regression line, the correlation coefficient is 0.624 and intention-to-vote explains just over half of the variation in actual turnout in the subsequent election. Although we cannot make inferences about turnout at the individual level from these aggregate statistics, this exercise gives us more confidence that our measure of intent-to-vote is likely to map on to actual voting behavior.

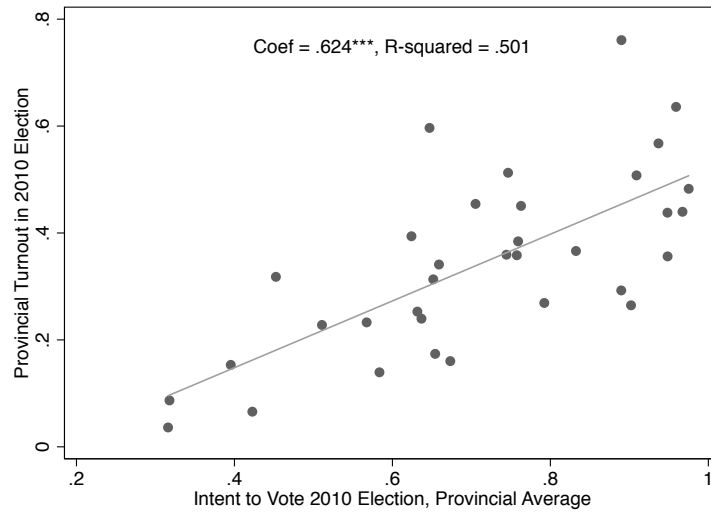
Data and Design

To estimate the effects of the Kabul Bank scandal, we study Wave 9 of the Afghanistan Nationwide Quarterly Research (ANQAR) survey collected from August 25, 2010 to September 3, 2010.³ The North Atlantic Treaty Organization (NATO) contracted the Afghan Center for Socio-Economic and Opinion Research (ACSOR) to design and implement the survey. ACSOR selected enumerators from the sampled regions and trained them in proper household and respondent selection, recording of responses, culturally appropriate interview techniques, and secure use of respondent information. The administrative district is the primary sampling unit and districts are selected via probability proportional to size systematic sampling.

²As we detail below, our survey is stratified by province and yields a representative sample at this level, which enables us to confidently calculate aggregate statistics by province.

³Figure SI-2 shows that the sample collected in Wave 9 appears to be consistent with demographic information collected across 13 years of data available from the Asia Foundation.

Figure 1: Intention-to-Vote against Turnout in 2010 Afghan Election



Notes: Figure displays province-level averages of pre-election intent-to-vote (from Afghanistan Nationwide Quarterly Research survey data) and official turnout (from publicly available Independent Election Commission of Afghanistan data), weighted by voting age population (from Afghanistan’s Central Statistics Organization population estimates).

We rectify the sampling frame used by ACSOR with the administrative map produced by the Empirical Studies of Conflict (ESOC) group. Among sampled districts, secondary sampling units (villages/settlements) are randomly selected from a sampling frame based on records from the Afghan Central Statistics Organization. A random walk method is used to identify target households and a Kish grid technique is used to randomize the respondent within each target household. ACSOR is able to secure access to sampled settlements by establishing ties with local elders.⁴

⁴See Figure SI-1 for data on refusal rates, non-contact rates, and overall cooperation rates across ACSOR-enumerated waves of ANQAR for which data are available (Waves 16-38). This means we cannot produce these statistics for our study wave despite it also being conducted by ACSOR. Importantly, the survey collection critiqued in Blair, Imai and Lyall (2014) was conducted by Eureka Research, not ACSOR. Overall, the refusal rates observed by ACSOR are lower (~3.6%) than those reported in a comparable survey (~15%) conducted

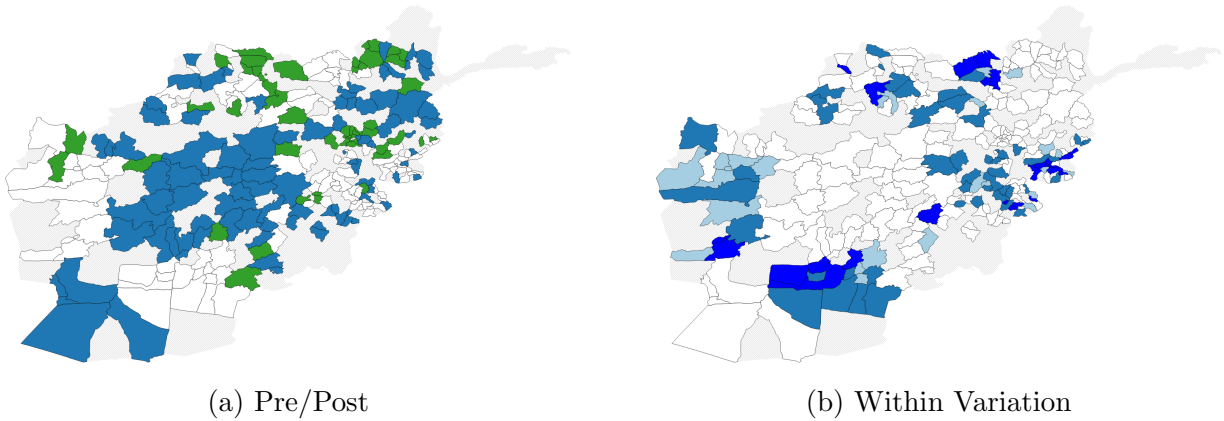
Importantly for our research design, the survey sampling sequence was set ahead of, and unaltered by, the financial scandal. We visualize the percentage of households sampled within each district after the scandal broke in Figure 2a and 2b. Several features of the spatial allocation of enumeration are apparent. A large number of districts sampled entirely before or after the scandal are clustered geographically and are contiguous to districts of the opposite status (i.e., entirely before or after) (Panel A). To address concerns about potentially imbalanced probability of sampling (by date) across districts (due to, for example, ease of access to a given district by road), we introduce a district fixed effect in our main specification. Our estimation therefore relies on within-district variation in the sequencing of enumeration. To visualize the spatial distribution of districts with variation in sampling before and after the scandal, focus on Panel B. Notice that these districts are effectively scattered in a quasi-random fashion within provinces and across the country. Because there still may be residual imbalances in observable characteristics sampled within these districts, we incorporate a number of demographic characteristics of surveyed respondents (see balance tests below for additional discussion).

We leverage the unexpected onset of the Kabul Bank crisis during the course of our survey to study the impact of the financial scandal on perceived corruption and intention-to-vote. We develop a timeline of the scandal using unclassified documents provided by the U.S. Agency for International Development, as well as secondary sources. The timeline is shown in Figure 3.

The fourth day of our survey, August 28, is when the government puts Kabul Bank into conservatorship. Bank runs begin on the next day. By August 30, the crisis expands, with the bank listing 300 million USD in investment losses. Notice that the daily total of surveys collected reaches its peak during the second day of the scandal. Because it may take time for information about the scandal to spread, we classify day 6 and later as the “post” period

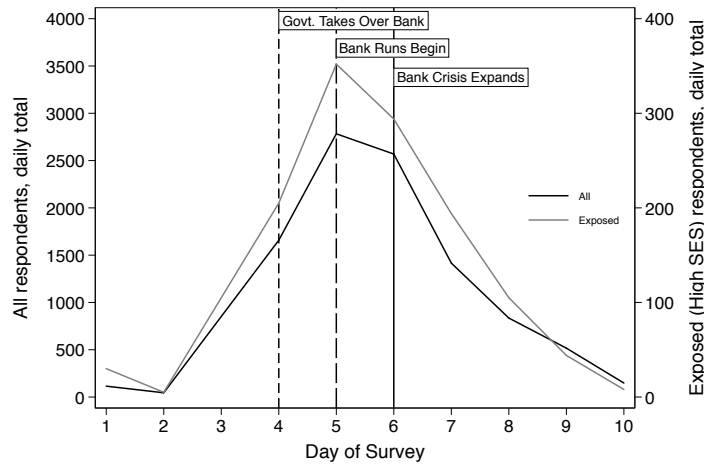
in Afghanistan in 2011 (Lyall, Shiraito and Imai, 2015).

Figure 2: Percentage of households by district enumerated after the scandal emerged



Notes: Figure displays binned classifications of the percentage enumerated on or after day 6 of sample. In Panel A, all survey respondents were enumerated either before (green) *or* after (blue) the scandal. In Panel B, we plot districts with variation in the timing of sampling before *and* after the scandal. Light blue indicates 1-40% enumerated during the post period; medium blue indicates 40-60%; dark blue indicates 61-99%. Districts that were not sampled are noted with grey diagonal lines.

Figure 3: Timeline of Kabul Bank crisis and survey collection



Notes: Dark (bottom) line indicates the daily total of respondents enumerated by day of the sample (left axis). Grey (top) line indicates the daily total of high SES respondents sampled (right axis). See text for descriptive total figures before and after day 6. The timeline is reconstructed from USAID Report No. F-306-11-003-S (see <https://tinyurl.com/y2pn2q12>) and secondary sources (see, e.g., <https://tinyurl.com/ya9thog2>).

of our analysis.⁵ If some survey respondents in fact receive information about the scandal

⁵In the analysis below, we show the results are robust to varying this cutoff.

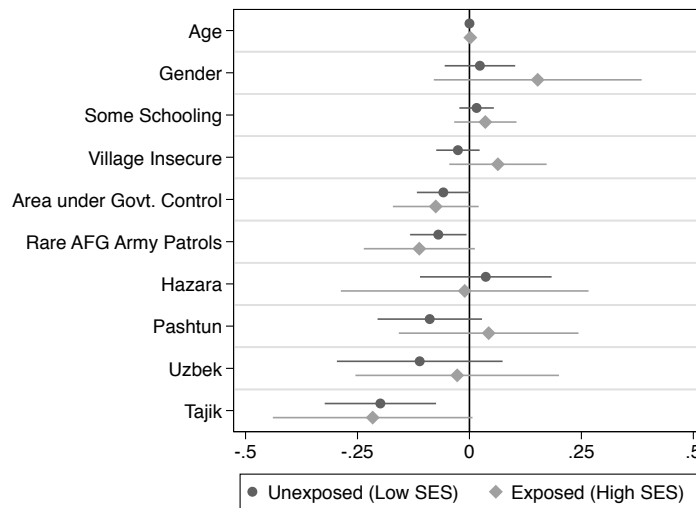
on day 5, we expect that this will bias our estimated effects downward (attenuation towards zero), since treated units are classified as controls. In our sample, 4,601 households are surveyed before day 6 (pre), while 5,485 are surveyed on or after day 6 (post). Since banking use in Afghanistan is limited to affluent individuals, we split our sample into high and low socioeconomic status (SES) respondents. This allows us to more narrowly focus on the impact of the scandal on individuals whose material interests (cash deposits) are far more likely to be directly affected by the scandal, and which our theory predicts should be most likely to react to the information. In this sense, our measure of scandal exposure is imperfect and should be thought of as an intention-to-treat effect with potential non-compliance in information relevance and exposure. Reassuringly, roughly 12% of our sample is classified as high SES, in line with the roughly 10% of Afghans estimated to have accounts in the formal banking system (Demirguc et al., 2011). 592 high SES households were enumerated prior to day 6 (pre) and 645 high SES units were surveyed on or after that date (post). Kabul Bank held the assets of roughly one-third of all Afghans with formal deposits. We expect these individuals to be differentially exposed to the financial consequences of the scandal. Reassuringly, the daily trends in survey collection for all respondents track very closely with responses gathered from high SES respondents.

Although the spatial allocation of timing around the unexpected scandal outbreak is plausibly random, it is still possible that imbalances are present across households across the groups with differential material exposure to the information (low and high SES). Practically, this is the quasi-experimental equivalent of failed randomization within a randomized control trial. To investigate this, we use multivariate regressions to produce point estimates for our main and supplemental demographic and household characteristics, separately for our two groups, where the outcome is being sampled in the post period.⁶ We present these point

⁶One of the core demographics of interest is gender, which likely correlated with exposure to and tolerance for corruption (Swamy et al., 2001).

estimates in Figure 4. Of the 20 coefficient estimates, we find 2 (10%) are statistically significant at the 10% level, which is what we would anticipate in expectation. Importantly, none of the coefficient estimates are statistically significantly different *across* low and high SES groups at the 10% level. This evidence strongly suggests that the sequence of enumeration was “as-good-as-randomized” around the Kabul Bank scandal.

Figure 4: Regression-Based Balance Tests across Pre/Post and Low/High SES



Notes: Outcome of interest is an indicator variable for post (equals 1 if a respondent is sampled in Day 6 or later). Ethnicity is split into four dummy variables for ethnic groups with at least 500 individuals sampled. Samples are split by exposure (low SES plotted above; high SES plotted below). Multivariate regressions are used to estimate coefficients and standard errors for samples split by low and high SES. Following the main regressions, standard errors are clustered by administrative district.

Our empirical design follows the logic of a difference-in-difference estimator: we compare the preferences of high SES respondents before and after the scandal occurs to the preferences of low SES respondents across the two periods. The scandal was not anticipated during survey preparation and no explicit questions about bank use were included. Instead, we rely on SES to identify individuals who were relatively more likely to utilize the formal banking system and have material exposure to the crisis (“exposed”). Naturally, affluent respondents may be systematically different from poorer subjects and thus may have different

beliefs about the extent (and importance) of government corruption⁷ and latent preferences for voting.⁸ Our design allows us to hold these general characteristics fixed as they are differenced out during estimation. That is, any systematic differences between high and low SES respondents are taken into account and held fixed by the research design. However, because our survey is not longitudinal (sampled respondents are only sampled once), some individual characteristics may vary among the surveyed populations before and after the scandal. We address this concern above by demonstrating balance across the quasi-experimental groups before and after the scandal. To partial out any residual variation correlated with their characteristics and improve precision, however, we incorporate demographic characteristics in our baseline specification. We begin by studying equation (1):

$$y_i = \alpha + \beta_1 Post_i + \beta_2 Exposed_i + \beta_3 Post_i \times Exposed_i + \lambda D_i + \gamma X_i + \epsilon \quad (1)$$

where y_i is the respondent’s beliefs about corruption and intention-to-vote. Shifting beliefs about the severity of corruption provides evidence consistent with respondents being aware of, and affected by, the financial scandal. Changes in intention-to-vote reveal whether the scandal affected voters ahead of the parliamentary election several weeks later. $Post_i$ takes the value of 1 if the respondent is surveyed after the scandal expands (August 30 or later). $Exposed_i$ indicates that the respondent is high SES. $Post_i \times Exposed_i$ captures the difference-in-difference estimator of the change in y_i of the exposed (high SES) after treatment (the scandal). D_i indicates district-level fixed effects and X_i is a vector of control

⁷“Do you strongly agree, agree somewhat, disagree somewhat or strongly disagree with the following statement: ‘Corruption is a serious problem in the government.’” Coded 1 if response was “strongly agree”. See Table SI-1 for questions and coding decisions.

⁸“Do you plan to vote in the upcoming Wolesa Jirga election?” Coded 1 if response was “Yes.”

variables. All models include age, age squared, gender, education, and ethnicity as demographic controls. We parameterize instrument non-response using a set of indicator variables. Robust standard errors are clustered by district to account for potential spatial clustering in high SES households, exposure to insecurity, and the sampling design (i.e., correlation of survey timing within the primary sampling unit). All models are adjusted using population sampling weights.

Other conditions might influence respondent’s preferences, including local security dynamics, government control of the respondent’s area, and security force patrol frequency. We incorporate these parameters in our robustness checks. In addition, surveys relying on direct questions may yield biased estimates if respondents do not truthfully reveal their preferences or beliefs. In the absence of indirect questions or a list experiment, these concerns are difficult to rule out definitively. We attempt to address these concerns in several ways. First, enumerators were asked to identify (and record) the respondent’s level of comfort and understanding of the survey. Second, enumerators recorded the number of people present during the interview. Respondents who are unfamiliar or uncomfortable with responding to questions from non-family members may give unreliable answers. Responses collected in the presence of a large number of people might also be less reliable. We incorporate these measures as additional parameters in our robustness checks.

Results

Main Results

We examine the impact of the scandal on public sentiment regarding government corruption and willingness to vote and present main results in Table 1. Columns 1 and 2 are from our baseline model with district and period fixed effects (pre/post), as well as demographic control variables. Our difference-in-difference estimates (row three) suggest perceptions of

government corruption increase by approximately 12%. This effect is precisely estimated. The impact on willingness to vote is equivalent to a 5.4% reduction, but is statistically imprecise. This suggests that the upward shift in perceived corruption does not correspond to a disenchantment (or mobilization) effect.

Table 1: Estimates of Financial Scandal Exposure on Perceived Corruption and Intent-to-Vote

	Benchmark Model		Benchmark - Security		Benchmark - Survey Bias	
	(1)	(2)	(3)	(4)	(5)	(6)
	Corruption Severe	Vote (Intent)	Corruption Severe	Vote (Intent)	Corruption Severe	Vote (Intent)
Post	-0.00668 (0.0321)	-0.0986** (0.0393)	-0.00566 (0.0312)	-0.0970** (0.0376)	-0.00540 (0.0313)	-0.0954** (0.0384)
Exposed	-0.123*** (0.0418)	0.0318 (0.0232)	-0.116*** (0.0405)	0.0180 (0.0233)	-0.116*** (0.0405)	0.0157 (0.0233)
Post \times Exposed	0.122** (0.0476)	-0.0540 (0.0491)	0.120** (0.0471)	-0.0494 (0.0447)	0.120** (0.0472)	-0.0503 (0.0441)
SUMMARY STATISTICS						
Outcome Mean	0.627	0.699	0.627	0.699	0.627	0.699
Outcome SD	0.484	0.459	0.484	0.459	0.484	0.459
PARAMETERS						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS						
Security	No	No	Yes	Yes	Yes	Yes
Govt. Control	No	No	Yes	Yes	Yes	Yes
Govt. Patrols	No	No	Yes	Yes	Yes	Yes
Survey Bias Controls	No	No	No	No	Yes	Yes
MODEL STATISTICS						
N	9944	9803	9944	9803	9943	9802
Clusters	241	240	241	240	241	240

Notes: Outcomes in Table 1 vary by column (see column heading). In sequence, the outcomes are: (1) Is corruption a serious problem in government? (2) Do you plan to vote in the upcoming election? Columns are staggered. Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as baseline demographic controls (age, age squared, education, gender, ethnicity). Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We next turn our attention to several robustness checks. It is possible that the scandal affected corruption and voting behavior differently in places with poor security provision, weak government control, and limited efforts by the government to thwart insurgent activity.

To address these concerns, we add a vector of control variables to our main specification in Columns 3 and 4 in Table 1. In particular, we account for self-reported village security, extent of government control over the respondent’s area, and the patrol frequency of security forces. Our balance tests suggest these factors are balanced, so our point estimates should not vary substantially, but these additional parameters may increase the precision of the main effect. Indeed, our point estimates are indistinguishable from the baseline model. In Columns 5 and 6 of Table 1, we account for whether the survey respondent was comfortable with the survey and understood most of the survey instruments (separate parameters). We also address potential concerns about household size and the number of people present during the interview, both of which may increase the likelihood the subject did not answer questions truthfully. The core results on attitudes about corruption and intent-to-vote are unaffected.

Overall, these results suggest that revelation of corruption increased perceived corruption in government but did not significantly influence intent-to-vote. Why might voting preferences not shift? One explanation is the presence of offsetting effects in the data. Our difference-in-differences estimate is a weighted average of individuals with high and low political efficacy. If low efficacy leads to demobilization and high efficacy leads to mobilization, we would expect a null result if the number of respondents in each subset were sufficiently balanced in number. We explore this next.

Heterogeneous Effects of Political Efficacy

Political efficacy has been defined as “the feeling that individual political action does have, or can have, an impact upon the political process, i. e. , that it is worth while to perform one’s civic duties. It is the feeling that political and social change is possible, and that the individual citizen can play a part in bringing about this change” (Campbell, Gurin and Miller, 1954, 187). In her review of the expansive literature on democratic participation, Pippa Norris (2011, 16) writes, “Ever since Almond and Verba, an extensive body of evidence has

examined how social psychological attitudes influence why and how citizens choose to engage in public affairs. Hence, positive feelings of political trust, internal efficacy, and institutional confidence in parties, legislatures, and the government are widely assumed to strengthen conventional activism such as voting participation, party membership, and belonging to voluntary associations.” Indeed, there is evidence that efficacy increases the probability of voting (Norris, 2004).

We might expect, then, that it is in places with relatively high political efficacy that “[v]oters punish corrupt politicians if they have the opportunity to do so” (De Vries and Solaz, 2017, 395). Information about corruption mobilizes turnout to hold politicians accountable, either through a sanctioning (Ferejohn, 1986) or selection (Fearon, 2000) model of retrospective voting. There is evidence from a variety of contexts and using different empirical approaches that corruption information can mobilize voter turnout (Banerjee et al., 2011; Cruz, Keefer and Labonne, 2019; Kostadinova, 2009; Krause and Méndez, 2009; Stockemer, LaMontagne and Scruggs, 2013; Vicente and Wantchekon, 2009; Wantchekon, 2003; Winters and Weitz-Shapiro, 2013).

In contrast, where political efficacy is low, it will be difficult for information about political corruption to motivate citizens to turn out, since they already have little confidence that voting can make a difference. Knowledge of corruption delegitimizes the state in citizens’ eyes (Seligson, 2002; della Porta, 2000), reduces their confidence in government’s ability and responsiveness (Caillier, 2010), and may make citizens less willing to participate in political activities like voting. Evidence from a series of papers supports this logic. Chang, Golden and Hill (2010) find almost no evidence that the revelation of corruption-related information about members of the lower house of Italy’s parliament affected voting behavior. Official charges of malfeasance had no effect on the probability that a candidate ran in the next election; in only one of 11 legislative sessions between 1948 and 1994 did a charge of malfeasance have any effect on the probability of reelection. Even field experiments from other settings

that deliver actual information on incumbents' performance often fail to find strong effects on voter behavior. Random revelation of information about Ugandan MPs' performance did not affect the probability of their reelection (Humphreys and Weinstein, 2012). In an innovative field experiment that delivered information about mayoral corruption to voters just prior to local elections in Mexico, Chong et al. (2015) found that learning about corruption actually *reduced* turnout slightly in treated precincts. This discussion motivates our analysis here because the logic of how political efficacy could mediate information's effects on turnout could plausibly explain the mixed empirical results in the literature. We turn now to a series of tests along these lines.

The ANQAR survey does not provide any information on respondents' political efficacy, so we cannot examine how this might have mediated any information effect of the scandal on attitudes about corruption and intent-to-vote at the individual level. Instead, we take advantage of another high quality survey that the Asia Foundation conducted across Afghanistan between June 18 and July 5, 2010, only weeks *before* the ANQAR survey was enumerated and the Kabul Bank scandal unfolded. The Asia Foundation survey asked respondents "How much influence do you think someone like you can have over government decisions?", an ideal measure of personal efficacy in the political process. Across 6,259 respondents, roughly 12% answered "a lot", which we classify as high efficacy. The remaining responses are classified as low efficacy, including "some", "very little", and "none at all." (This classification is in line with the Asia Foundation's own evaluation of self-reported political influence in the survey (Tariq, Ayoubi and Haqbeen, 2010, 96-97).) We note that one advantage of using these separate data to construct a measure of political efficacy is that subjects in the ANQAR survey could not have tried to calibrate their responses about political attitudes and behavior to any self-reported sense of political efficacy because no such question was asked of them. Moreover, the Asia Foundation survey was also stratified by province, allowing us to study heterogeneous effects across provinces with high or low efficacy.

Based on these data, we code Afghan provinces as either high or low on political efficacy, using the mean level of efficacy observed in the distribution. This allows us to modify the original estimating equation using a triple interaction approach. Importantly, the original difference-in-difference (post \times exposed) becomes the effect at the baseline when high efficacy equals 0 (i.e., the effect of information about corruption in low efficacy provinces). The triple interaction becomes the marginal effect of “turning on” efficacy relative to the baseline condition.⁹ It is possible that provinces with high and low efficacy differ on important observable factors. To assess this we present a balance test in Figure SI-3. Across the baseline demographics we evaluated earlier in our individual-level balance tests, we observe a difference (at the 10% level) for only the Uzbek ethnic classification. This indicates that, on average, high efficacy provinces have a relatively larger Uzbek population. That said, because our estimating equation includes a location fixed effect below the province (administrative districts), the cross sectional variation in efficacy is conditioned out. This means that any residual imbalances at the provincial level are partialled out during estimation (including the imbalance in Uzbek representation). It is also worth noting that the full set of interaction terms in our model absorb any pre/post changes in respondent attitudes across provinces with varying levels of political efficacy. These two features of our research design address potential concerns that low and high efficacy provinces are systematically different on average in ways that are either fixed or time-varying with the scandal. Although we advise caution in giving the heterogeneous effects a causal interpretation, these features give us more confidence in the magnitude of the coefficients we estimate.

We introduce the triple difference in Table 2. Notice in Column 1, the baseline effect is large, positive and statistically precise. The shift in perceived corruption is large,

⁹Note: the marginal effects reported are distinct from total effects, which are difficult to calculate since one of the base terms in the triple difference is absorbed by our location fixed effects.

roughly 12%, among respondents living in areas where average feelings of political efficacy are relatively low, with no statistically precise marginal effects estimated with the efficacy interaction. This result suggests that respondents' shifting beliefs about corruption do not systematically differ across high and low efficacy provinces. In Column 2, the differences across low and high efficacy provinces are substantial. For voters in low efficacy provinces, the scandal depresses the willingness to vote by a statistically precise 8.6%. For voters in high efficacy provinces, the effect reverses, with willingness to vote increasing by 17% relative to the baseline estimate. These results suggest that voters have heterogeneous responses to corruption driven, at least in part, by varying levels of perceived political influence (efficacy). As in previous specifications (Table 1), in Columns 3-6 we progressively add controls to account for the security situation and elements of survey enumeration. The results are unaffected.

Supplemental Results

We next report on several additional sensitivity tests of the main results, as well as supplemental results. Figures 5a and 5b show the robustness of the estimated effects on perceived corruption and intent-to-vote, respectively, across a range of pre/post cutoff windows. In the left panel of (a) and (b), we plot the baseline difference-in-difference (when political efficacy is low). In the right panel of each subfigure, we plot the marginal effects (as noted in the main tables). Each subsequent estimate eliminates one day before and after the scandal event, starting at six days pre/post and ending with two days pre/post. Even narrowing the cutoff to two days before and after the scandal broke, results are largely unaffected. Similarly, in Figures 6a and 6b, we demonstrate the main heterogeneous effects in Table 2 are also largely insensitive to alternative cutoffs for classifying respondents' level of political efficacy by province.

Next, we present additional results to address several potential concerns about the main

Table 2: Estimates of Financial Scandal Exposure on Perceived Corruption and Intent-to-Vote: Accounting for Heterogeneity by Political Efficacy

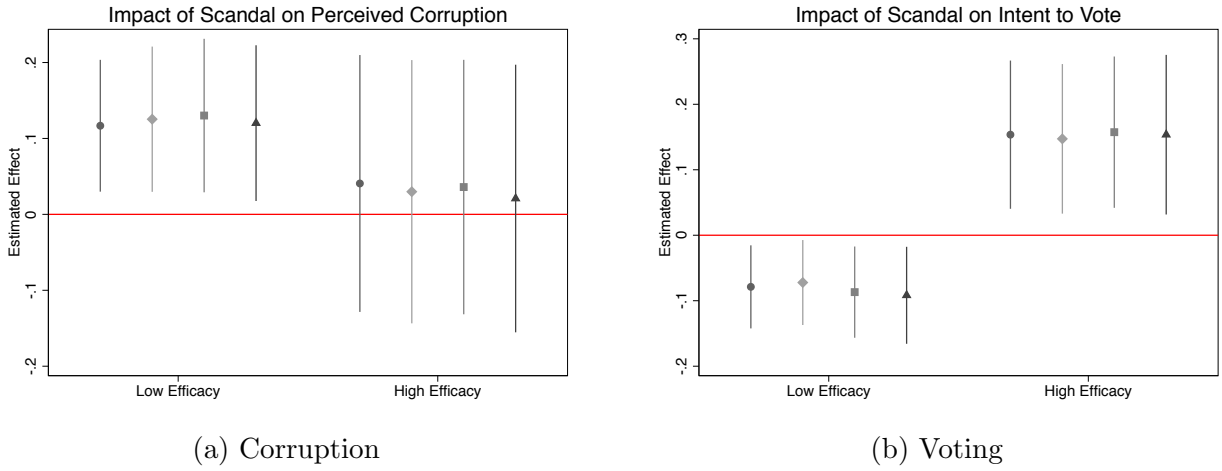
	Benchmark Model		Benchmark - Security		Benchmark - Survey Bias	
	(1) Corruption Severe	(2) Vote (Intent)	(3) Corruption Severe	(4) Vote (Intent)	(5) Corruption Severe	(6) Vote (Intent)
Post	-0.0319 (0.0361)	-0.136*** (0.0423)	-0.0306 (0.0353)	-0.134*** (0.0400)	-0.0311 (0.0355)	-0.135*** (0.0406)
Exposed	-0.107** (0.0467)	0.0546** (0.0232)	-0.0998** (0.0448)	0.0378 (0.0236)	-0.100** (0.0447)	0.0345 (0.0234)
Post × Exposed	0.119** (0.0535)	-0.0860** (0.0436)	0.117** (0.0525)	-0.0771* (0.0397)	0.117** (0.0526)	-0.0788** (0.0384)
Post × High Efficacy	0.0901 (0.0653)	0.130** (0.0538)	0.0885 (0.0643)	0.129** (0.0540)	0.0913 (0.0644)	0.136** (0.0530)
Exposed × High Efficacy	-0.0861 (0.0729)	-0.131** (0.0505)	-0.0900 (0.0732)	-0.115** (0.0524)	-0.0898 (0.0728)	-0.112** (0.0513)
Post × Exposed × High Efficacy	0.0392 (0.101)	0.170** (0.0739)	0.0393 (0.102)	0.150** (0.0705)	0.0407 (0.102)	0.154** (0.0685)
SUMMARY STATISTICS						
Outcome Mean	0.627	0.699	0.627	0.699	0.627	0.699
Outcome SD	0.484	0.459	0.484	0.459	0.484	0.459
PARAMETERS						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS						
Security	No	No	Yes	Yes	Yes	Yes
Govt. Control	No	No	Yes	Yes	Yes	Yes
Govt. Patrols	No	No	Yes	Yes	Yes	Yes
Survey Bias Controls	No	No	No	No	Yes	Yes
MODEL STATISTICS						
N	9944	9803	9944	9803	9943	9802
Clusters	241	240	241	240	241	240

Notes: Outcome in Table 2 vary by column (see column heading). In sequence, the outcomes are: (1) Is corruption a serious problem in government? (2) Do you plan to vote in the upcoming election? Columns are staggered. Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as baseline demographic controls (age, age squared, education, gender, ethnicity). Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

findings. First, it might be the case that our results regarding corruption are sensitive to our binary classification of attitudes towards corruption. If we use the original continuous scale of the variable, however, our results are consistent. These results are reported in Table 3.

Second, given that only a subset of districts are enumerated both before and after news

Figure 5: Sensitivity of Heterogeneous Effects to Alternative Pre/Post Windows

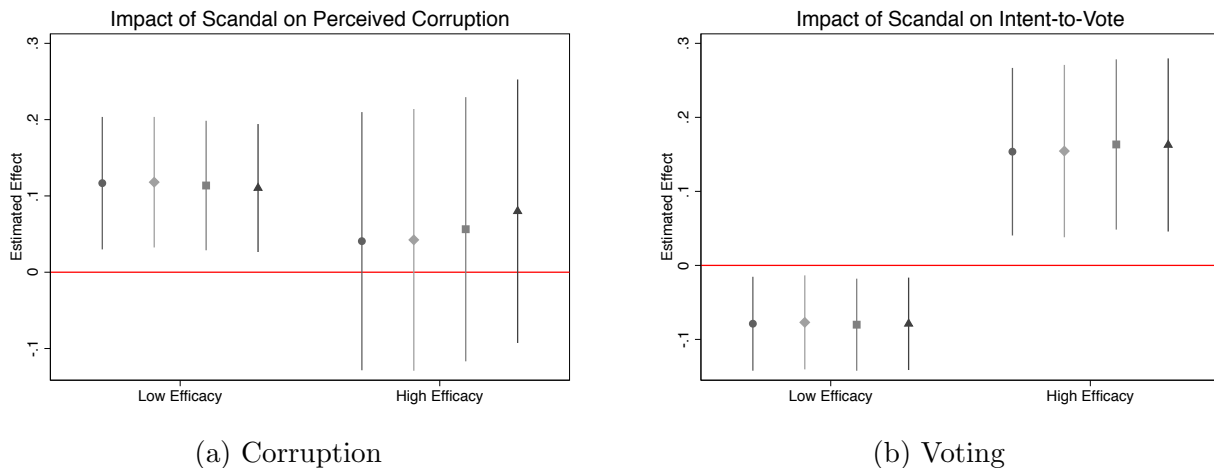


Notes: Figure displays regression coefficients from benchmark models in Table 2 Columns 5 and 6, respectively. Each point estimate in tandem represents the estimated heterogeneous effects if the pre/post cutoff is decreased by 1 day, starting at 6 and ending at 2 days. Each set of reported coefficients replicates the benchmark specification in the main analysis with varying sample cutoffs. All other model parameters and specifications (standard errors, weights) remain consistent with benchmark specification.

of the scandal broke, it could be the case that readers are cautious about the use of district fixed effects (which reduce the variation available for the difference-in-difference estimator). It might also be the case that our main specification is biased by disproportionate weights being assigned to districts with no within-case variation (that is, districts with complete enumeration either before or after the scandal). To address these two concerns, we can replicate the benchmark specifications from Table 2 under two separate conditions: dropping the fixed effects entirely and excluding any districts that do not have within-units variation (while retaining the fixed effects). These results are presented in Table 4. Because the estimator in both cases has changed, we would expect the coefficient magnitudes to vary but the direction of the main effects to be consistent. This is precisely what we find.

Third, in Figure SI-2 we present evidence comparing the sample we study in ANQAR and demographic data from the Asia Foundation’s 13 years of fieldwork. Although there is a high level of consistency across ethnic groups and age cohorts, we observe some un-

Figure 6: Sensitivity of Heterogeneous Effects to Alternative Cutoffs for Political Efficacy Classification



Notes: Figure displays regression coefficients from benchmark models in Table 2 Columns 1 and 2, respectively. Each point estimate in tandem represents the estimated heterogeneous effects if the threshold of efficacy is increased by the amount necessary to move the next province from “high efficacy” to “low efficacy” categorization. Each set of reported coefficients replicates the benchmark specification in the main analysis with varying province-wise definitions of efficacy. All other model parameters and specifications (standard errors, weights) remain consistent with benchmark specification.

derrepresentation of female respondents in several provinces in ANQAR. The balance tests presented earlier indicate this undersampling of female subjects was consistent across high and low SES subjects as well as pre- and post-scandal. Undersampling therefore does not cause bias in our estimate but it could suggest that our point estimate is not representative of the population parameter estimate that could be recovered if we had more complete inclusion of female subjects. Although we cannot retroactively survey female subjects, we can examine whether the estimated effect varies if we exclude provinces where females are underrepresented in our sample. If the effect does not vary substantially, it suggests our main estimate is likely consistent with what would be recovered if we could retroactively survey more female subjects. These results are presented in Table 5. Notice that the main effects are highly consistent with Table 2, indicating that the uneven gender balance likely should not change our interpretation of the estimated effects.

Table 3: Estimates of Financial Scandal Exposure on Perceived Corruption (continuous scale) and Intent-to-Vote

	Benchmark Model		Benchmark - Security		Benchmark - Survey Bias	
	(1)	(2)	(3)	(4)	(5)	(6)
	Corruption Severity	Vote (Intent)	Corruption Severity	Vote (Intent)	Corruption Severity	Vote (Intent)
Post	-0.0692 (0.0614)	-0.136*** (0.0423)	-0.0657 (0.0574)	-0.134*** (0.0400)	-0.0656 (0.0572)	-0.135*** (0.0406)
Exposed	-0.126* (0.0685)	0.0546** (0.0232)	-0.118* (0.0664)	0.0378 (0.0236)	-0.118* (0.0659)	0.0345 (0.0234)
Post × Exposed	0.165** (0.0750)	-0.0860** (0.0436)	0.163** (0.0738)	-0.0771* (0.0397)	0.163** (0.0737)	-0.0788** (0.0384)
Post × High Efficacy	0.192 (0.125)	0.130** (0.0538)	0.186 (0.124)	0.129** (0.0540)	0.188 (0.123)	0.136** (0.0530)
Exposed × High Efficacy	-0.110 (0.108)	-0.131** (0.0505)	-0.108 (0.109)	-0.115** (0.0524)	-0.111 (0.108)	-0.112** (0.0513)
Post × Exposed × High Efficacy	-0.00157 (0.140)	0.170** (0.0739)	-0.00418 (0.143)	0.150** (0.0705)	0.00145 (0.143)	0.154** (0.0685)
SUMMARY STATISTICS						
Outcome Mean	3.502	0.699	3.502	0.699	3.502	0.699
Outcome SD	0.740	0.459	0.740	0.459	0.740	0.459
PARAMETERS						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS						
Security	No	No	Yes	Yes	Yes	Yes
Govt. Control	No	No	Yes	Yes	Yes	Yes
Govt. Patrols	No	No	Yes	Yes	Yes	Yes
Survey Bias Controls	No	No	No	No	Yes	Yes
MODEL STATISTICS						
N	9944	9803	9944	9803	9943	9802
Clusters	241	240	241	240	241	240

Notes: Outcomes in Table 3 vary by column (see column heading). In sequence, the outcomes are: (1) Is corruption a serious problem in government? (Continuous scale = severity of corruption) (2) Do you plan to vote in the upcoming election? Columns are staggered. Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as baseline demographic controls (age, age squared, education, gender, ethnicity). Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Finally, we provide a simple placebo test following the intuition of Ferraz and Finan (2008), which suggested that information is most likely to be transmitted through media sources. Splitting the sample into individuals with and without media access,¹⁰ we re-

¹⁰Respondents are coded as having media access if they report television or radio as their

Table 4: Estimates of Financial Scandal Exposure on Perceived Corruption and Intent-to-Vote (no district fixed effects & only districts w/ within variation)

	Benchmark - Survey Bias		Benchmark - Survey Bias	
	(1)	(2)	(3)	(4)
	Corruption Severe No Dist FE	Vote (Intent) No Dist FE	Corruption Severe Within Var	Vote (Intent) Within Var
Post	-0.0137 (0.0313)	-0.135*** (0.0384)	-0.0357 (0.0361)	-0.127*** (0.0418)
Exposed	-0.0903** (0.0357)	0.0454* (0.0270)	-0.0665 (0.0457)	0.0370 (0.0246)
Post × Exposed	0.128*** (0.0492)	-0.114** (0.0505)	0.104** (0.0520)	-0.134*** (0.0350)
Post × High Efficacy	0.00207 (0.0533)	0.167*** (0.0486)	0.109* (0.0650)	0.120** (0.0527)
Exposed × High Efficacy	-0.0672 (0.0593)	-0.0841 (0.0544)	-0.127 (0.0984)	-0.171*** (0.0485)
Post × Exposed × High Efficacy	0.109 (0.104)	0.147* (0.0787)	0.0496 (0.119)	0.253*** (0.0678)
SUMMARY STATISTICS				
Outcome Mean	0.627	0.699	0.604	0.685
Outcome SD	0.484	0.459	0.489	0.465
PARAMETERS				
District FE	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS				
Security	Yes	Yes	Yes	Yes
Govt. Control	Yes	Yes	Yes	Yes
Govt. Patrols	Yes	Yes	Yes	Yes
Survey Bias Controls	Yes	Yes	Yes	Yes
MODEL STATISTICS				
N	9943	9802	6455	6348
Clusters	241	240	98	97

Notes: Outcomes in Table 4 vary by column (see column heading). In sequence, the outcomes are: (1) Is corruption a serious problem in government? (2) Do you plan to vote in the upcoming election? Columns are staggered. Unit of analysis is individual survey respondent. Models include baseline demographic controls (age, age squared, education, gender, ethnicity). Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

estimate specifications in Table 2, and report them in Table 6. Notice that Columns 5 and 6, relying solely on subjects with media access, yield the statistically precise base terms from most frequent source of news and information, and having no media access otherwise.

Table 5: Estimates of Financial Scandal Exposure on Perceived Corruption and Intent-to-Vote: Accounting for Heterogeneity by Political Efficacy and Undersampling of Female Subjects

	Benchmark Model		Benchmark - Security		Benchmark - Survey Bias	
	(1) Corruption Severe	(2) Vote (Intent)	(3) Corruption Severe	(4) Vote (Intent)	(5) Corruption Severe	(6) Vote (Intent)
Post	-0.0256 (0.0371)	-0.137*** (0.0414)	-0.0237 (0.0362)	-0.136*** (0.0401)	-0.0244 (0.0363)	-0.137*** (0.0406)
Exposed	-0.0983** (0.0472)	0.0586** (0.0233)	-0.0916** (0.0451)	0.0451* (0.0235)	-0.0911** (0.0449)	0.0419* (0.0233)
Post × Exposed	0.0995* (0.0560)	-0.0923** (0.0429)	0.0986* (0.0542)	-0.0854** (0.0394)	0.0982* (0.0542)	-0.0858** (0.0386)
Post × High Efficacy	0.0811 (0.0683)	0.136** (0.0545)	0.0809 (0.0674)	0.131** (0.0553)	0.0832 (0.0675)	0.137** (0.0543)
Exposed × High Efficacy	-0.0930 (0.0799)	-0.124** (0.0544)	-0.0953 (0.0800)	-0.116** (0.0563)	-0.0954 (0.0796)	-0.114** (0.0553)
Post × Exposed × High Efficacy	0.0572 (0.108)	0.167** (0.0776)	0.0545 (0.109)	0.156** (0.0747)	0.0556 (0.108)	0.159** (0.0731)
SUMMARY STATISTICS						
Outcome Mean	0.630	0.716	0.630	0.716	0.630	0.716
Outcome SD	0.483	0.451	0.483	0.451	0.483	0.451
PARAMETERS						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS						
Security	No	No	Yes	Yes	Yes	Yes
Govt. Control	No	No	Yes	Yes	Yes	Yes
Govt. Patrols	No	No	Yes	Yes	Yes	Yes
Survey Bias Controls	No	No	No	No	Yes	Yes
MODEL STATISTICS						
N	8677	8542	8677	8542	8676	8541
Clusters	199	198	199	198	199	198

Notes: Outcome in Table 5 vary by column (see column heading). In sequence, the outcomes are: (1) Is corruption a serious problem in government? (2) Do you plan to vote in the upcoming election? Columns are staggered. Unit of analysis is individual survey respondent. Provinces with undersampling of female subjects (relative Asia Foundation data) are excluded from the sample. All models include administrative district fixed effects (using ESOC boundaries), as well as baseline demographic controls (age, age squared, education, gender, ethnicity). Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

our main analysis, while the effect sizes and precision in Columns 3 and 4, where subjects do not have media access, are inconsistent. Although the marginal effect in Column 6 is not precise at conventional levels, it is much closer to statistical significance than the comparable

coefficient estimated using the sample without media access. Overall, this simple test reveals evidence consistent with the mechanism articulated in Ferraz and Finan (2008) and gives us more confidence in the plausibility of the information channel.

Table 6: Estimates of Financial Scandal Exposure on Perceived Corruption and Intent-to-Vote: Accounting for Heterogeneity by Political Efficacy and Media (information) Access

	Full Sample		No Media Access		With Media Access	
	(1) Corruption Severe	(2) Vote (Intent)	(3) Corruption Severe	(4) Vote (Intent)	(5) Corruption Severe	(6) Vote (Intent)
Post	-0.0311 (0.0355)	-0.135*** (0.0406)	-0.0740 (0.0671)	-0.0792* (0.0433)	-0.0378 (0.0372)	-0.154*** (0.0435)
Exposed	-0.100** (0.0447)	0.0345 (0.0234)	-0.104 (0.0767)	0.121** (0.0514)	-0.105** (0.0459)	0.0127 (0.0279)
Post × Exposed	0.117** (0.0526)	-0.0788** (0.0384)	0.0576 (0.122)	-0.122 (0.105)	0.132** (0.0531)	-0.0782** (0.0355)
Post × High Efficacy	0.0913 (0.0644)	0.136** (0.0530)	0.176 (0.107)	0.0740 (0.0608)	0.108 (0.0719)	0.157*** (0.0573)
Exposed × High Efficacy	-0.0898 (0.0728)	-0.112** (0.0513)	-0.103 (0.121)	-0.165 (0.105)	-0.0636 (0.0774)	-0.0963 (0.0639)
Post × Exposed × High Efficacy	0.0407 (0.102)	0.154** (0.0685)	0.163 (0.174)	0.175 (0.150)	-0.0631 (0.114)	0.125 (0.0827)
SUMMARY STATISTICS						
Outcome Mean	0.627	0.699	0.594	0.668	0.641	0.719
Outcome SD	0.484	0.459	0.491	0.471	0.480	0.450
PARAMETERS						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS						
Security	Yes	Yes	Yes	Yes	Yes	Yes
Govt. Control	Yes	Yes	Yes	Yes	Yes	Yes
Govt. Patrols	Yes	Yes	Yes	Yes	Yes	Yes
Survey Bias Controls	Yes	Yes	Yes	Yes	Yes	Yes
MODEL STATISTICS						
N	9943	9802	3585	3538	5962	5871
Clusters	241	240	225	221	235	233

Notes: Outcome in Table 6 vary by column (see column heading). In sequence, the outcomes are: (1) Is corruption a serious problem in government? (2) Do you plan to vote in the upcoming election? Columns are staggered. Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as baseline demographic controls (age, age squared, education, gender, ethnicity). Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Conclusion

How do voters react to news of large-scale political corruption? Competing theoretical logics and mixed empirical evidence motivate our study of Afghan citizens' reactions to the Kabul Bank financial scandal of 2010. We focus on a real and economically damaging political scandal to study this question. We leverage the fact that the scandal broke while a survey was being enumerated across Afghanistan, which provides the opportunity to identify the causal effects that news of the scandal had on political attitudes and intent-to-vote in the parliamentary election scheduled several weeks later. To explain reactions in this case and others, we draw on a theoretical literature that emphasizes how political efficacy might mediate the effect of information revealing corruption on citizens' reactions and behavior. This is an important contribution, as scholars have argued that investigation into the conditions under which information about corruption affects political behavior represents the next generation of research on accountability (Pande, 2011).

Overall, the scandal changed people's minds about corruption, increasing the likelihood that they perceived corruption as a serious problem for the country. But we do not observe a statistically significant effect on citizens' intention-to-vote. We reason that this is reflective of the fact that, like many democracies where corruption is a pervasive problem, political efficacy on average is low in Afghanistan.

The evidence we find suggests that political efficacy likely plays an influential role in shaping how voters mobilize in the wake of an unexpected corruption scandal. However, it is important to highlight that we cannot necessarily lend a causal interpretation to the heterogeneous effects we find. Political efficacy is not randomly assigned in this setting and could be a function of all sorts of factors that also contribute to citizens' willingness to vote. Although our research design helps us account for any systematic or time-varying differences across provinces with high and low efficacy, there may still be sources of bias that remain.

Our main point is that, no matter what explains variation in the ebb and flow of political efficacy across and within countries, we should expect that citizens will react differently to information about corruption because of it. The sub-national results from this case provide strong support for this argument. While there are important cross-national differences other than political efficacy that undoubtedly contribute to variance in the way in which citizens do or do not react to political corruption, we submit that these results provide reason to further test empirical implications of the theoretical argument in other contexts that vary in the level of citizens' political efficacy. Given the pervasiveness of political corruption across a range of democracies, this theoretical argument is relevant to the places we most care about understanding the dynamics of corruption and political behavior.

Important questions remain about the broader consequences of the Kabul Bank crisis. Our identification strategy leverages the “as if” random sampling of survey respondents before and after the scandal emerged to estimate the short-term political consequences of the crisis. The quasi-experimental approach we take lends credibility to our estimates. Yet it is difficult to assess the medium-run effects of the scandal on actual voting patterns in the parliamentary election two weeks after our survey was completed. This is a weakness that our natural experiment does not allow us to address. It is possible that voter behavior differs from self-reported willingness to vote in a manner that systematically overstates the willingness of high SES citizens to vote when they did not in fact cast ballots. That is, voter turnout may have been substantially affected by the scandal in ways that our survey does not allow us to identify due to the narrow time window around which we can plausibly claim to make causal inferences. Perhaps more importantly, it is possible that citizens did not fully recognize the extent of the criminal acts at the Kabul Bank or the network of political actors implicated by the scandal until long after the election.

These findings highlight a problem common in developing contexts: the extent and intensity of corruption may normalize misconduct by political actors (Pande, 2008; Fisman

and Miguel, 2007). Even a large-scale financial scandal may be insufficient to trigger unconditional political mobilization and reform if it reflects “government as usual.” Corruption remains a prominent impediment to economic development and political consolidation. Understanding how the revelation of political corruption affects public opinion and voter preferences is critical to identifying policy interventions that can meaningfully deter or constrain corrupt political actors.

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SUPPORTING INFORMATION

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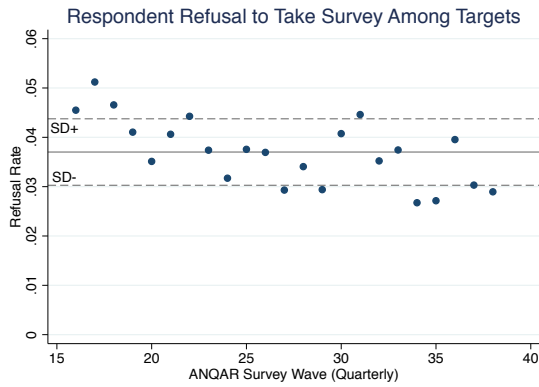
Supplemental Figures

SI-1 ANQAR diagnostics during later waves (16-38) conducted by firm collecting Wave 9 survey data (ACSOR)	SI-2
SI-2 Comparison of ANQAR Wave 9 and Asia Foundation Demographic Data . .	SI-3
SI-3 Regression-Based Balance Tests across Low/High Efficacy provinces	SI-4

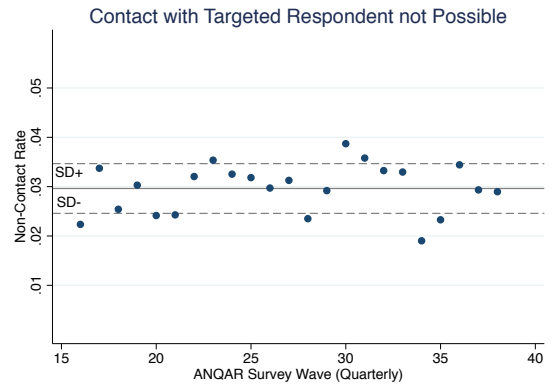
Supplemental Tables

SI-1 Survey Instruments Overview	SI-5
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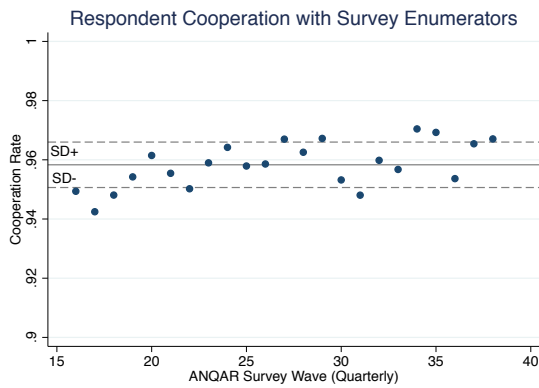
Figure SI-1: ANQAR diagnostics during later waves (16-38) conducted by firm collecting Wave 9 survey data (ACSOR)



(a) Refusal rate



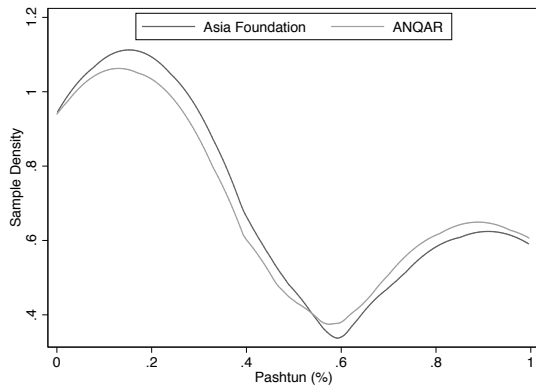
(b) Non-contact rate



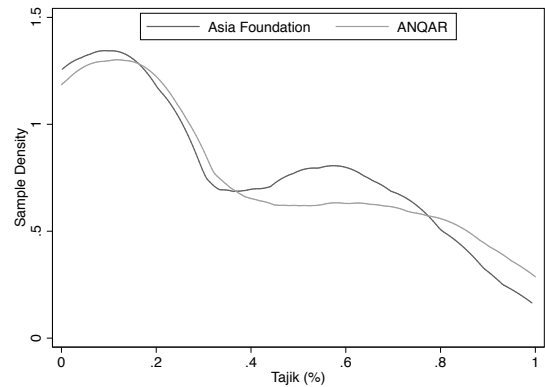
(c) Cooperation rate

Notes: Data on refusal, non-contact, and overall cooperation were shared with the authors by NATO. This data is only available for the waves presented (not available for Wave 9). Author's own calculations.

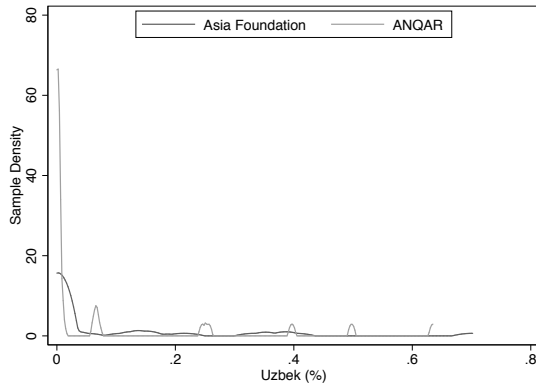
Figure SI-2: Comparison of ANQAR Wave 9 and Asia Foundation Demographic Data



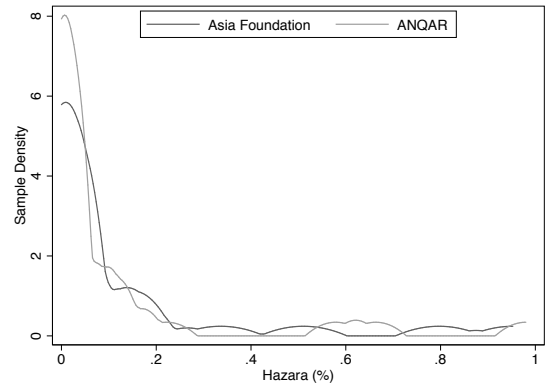
(a) Pashtun (%)



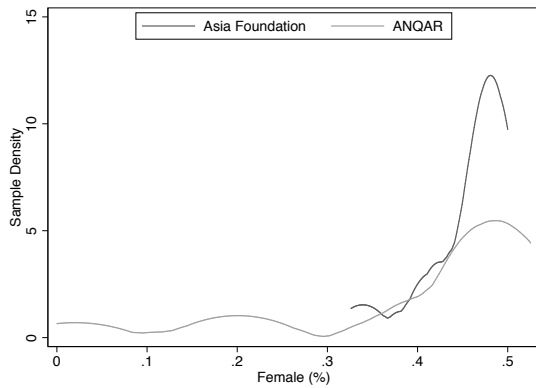
(b) Tajik (%)



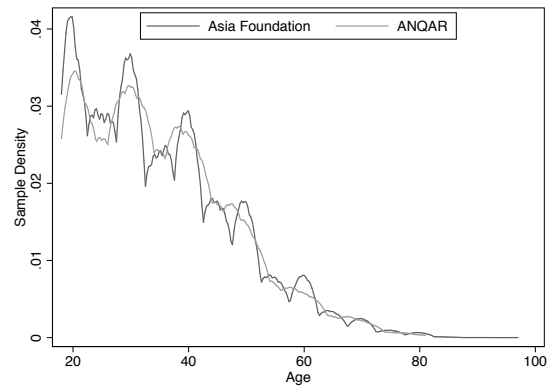
(c) Uzbek (%)



(d) Hazara (%)



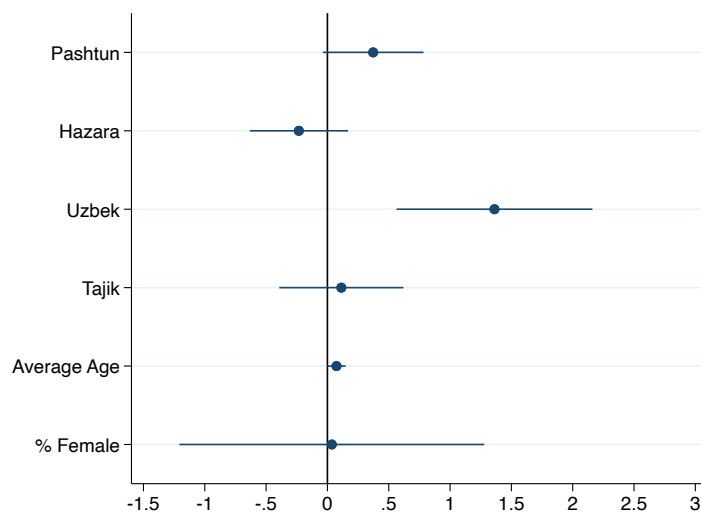
(e) Female (%)



(f) Age

Notes: Sub-figures a-e are province averages of binary demographics; sub-figure f uses individual-level age data (continuous). Asia Foundation data includes information from 2006 to 2018 and is plotted in black; ANQAR indicates Wave 9 from 2010 and is plotted in gray. Demographics are highly consistent across the two data sources with the exception of underrepresentation of female respondents in several provinces in the ANQAR sample. The main results are robust to excluding provinces with no females sampled in ANQAR Wave 9 (see Table 5).

Figure SI-3: Regression-Based Balance Tests across Low/High Efficacy provinces



Notes: Data drawn from Wave 5 of the Survey of the Afghan People, conducted for the Asia Foundation by ACSOR. Outcome of interest is an indicator variable for high efficacy (see main text for additional details). Ethnicity is split into four dummy variables for ethnic groups with at least 500 individuals sampled, following the main analysis. Overall, Pashtuns make up roughly 45%, Tajiks 35%, Hazaras 11%, and Ubeks 8% of the provincial samples. Age indicates the mean age of the sampled population. Female indicates the percentage female in each provincial sample. Multivariate regressions are used to estimate coefficients and standard errors.

Table SI-1: Survey Instruments Overview

Variable	Question	Coding (= 1 if)
Government Corruption is Problem	Do you strongly agree, agree somewhat, disagree somewhat or strongly disagree with the following statement: "Corruption is a serious problem in the government"	Strongly Agree
Plan to Vote	Do you plan to vote in the upcoming Wolesa Jirga election?	Yes
Government Misuses Power	Do you believe the following persons misuse their power? Do you think they misuse their power most of the time, sometimes or never? Government of Afghanistan	Most of the Time
Trust in Government	Even if you haven't seen or heard information or video, audio and print materials communicated by the following institutions and groups, how trustworthy do you think the messaging is of each of them? Messaging of the Government	Totally Trustworthy
Govt. going Wrong Direction	Generally speaking, do you believe the Government of Afghanistan is going in the right direction, the wrong direction, or is in the same place, not going anywhere?	Wrong Direction
Village Insecure	How is the security situation in your mantaqa? Good, fair, bad?	Bad
Govt. Control	Between the two, the Anti-Government Elements and the Government, who has more influence in your mantaqa now?	Government
Govt. Patrols	How often do you see the Police in your mantaqa?	Less than Monthly