

Corruption and Political Mobilization: Evidence from a Natural Experiment*

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Abstract

How do voters react to news of political corruption? Theory and evidence suggest that information about corruption may mobilize citizens to demand political and institutional reform but also may demobilize in some contexts. 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 quasi-experimental approach. The scandal led to a significant decrease in citizens' reported intent-to-vote in the parliamentary election several weeks later. We argue that the effects of information are moderated by political efficacy (perceived influence of activism on political outcomes), which varies considerably across and within emerging democracies. We find evidence of 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 (Mauro, 1995; Rose-Ackerman, 1999) and the micro-level (Olken, 2007; Reinikka and Svensson, 2005). To combat corruption, elections theoretically act as institutions of accountability (Ferejohn, 1986), disciplining corrupt politicians and improving governance. Why, then, does government corruption persist?

One of the most often-cited answers to this question is that citizens lack the requisite information to vote out corrupt actors (Olken, 2009; 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 offer 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). 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 scandal 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 quasi-experimental approach successfully utilized in other contexts (e.g., Balcells and Torrats-Espinosa 2018; Mikulaschek, Pant and Tesfaye 2020).

Overall, we find that the informational shock of the scandal caused a statistically and substantively significant decrease in citizens' intention to vote in the parliamentary election

scheduled two weeks after the survey was enumerated. We show that these results are robust to a number of alternative model and sample specifications, including a range of pre/post event windows, and estimation over different subsets of administrative districts based on exogenous patterns of survey enumeration. This is the main result and contributes to the empirical literature relating corruption to voter turnout.

Motivated by a theoretical and empirical literature that argues voter mobilization is influenced by citizens' beliefs that their votes substantively impact political outcomes (Campbell, Gurin and Miller, 1954; Norris, 2004, 2011), we reason that individuals who believe they do not have agency to influence outcomes through political engagement will be demobilized by the emergence of a corruption scandal. However, as beliefs about efficacy rise, we expect demobilizing effect to wane and even mobilize turnout in some contexts. We examine whether the scandal had heterogeneous effects on citizens' intention to vote.

We find that in areas with relatively low levels of political efficacy (as measured by a separate nationwide survey), news of the scandal made individuals *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 a marginal effect that is large and positive. This suggests that potential voters in high efficacy areas were significantly less demobilized by news of the scandal. Considering the overall effect on intentions to vote, our estimates indicate that these respondents were not affected by news of the Kabul Bank crisis. That is, they were neither demobilized nor mobilized by the event. At minimum, this suggests that increasing levels of political efficacy may provide citizens some "immunity" against the demobilizing effect of corruption scandals in places like Afghanistan where citizens see official corruption as a severe problem in society.

This research note makes several contributions. First, we increase confidence in the causal nature of our results by taking advantage of an unanticipated political event that "as if" randomly transmitted new information to citizens about political corruption during the

course of a single survey just prior to a major national election. Features of this particular event and the survey data collection process accord with best practices laid out for estimating causal effects within this research design (Muñoz, Falcó-Gimeno and Hernández, 2020). Some of the uncertainty in the literature on citizen responsiveness to political corruption derives in no small part from the paucity of opportunities available for scholars to exploit naturally occurring information shocks and study effects on turnout (e.g., 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. Indeed, we provide an explicit test of the informational mechanism’s logic, the results of which should strengthen confidence in the plausibility of the information channel in this case.

Second, we speak to how information can both mobilize *and* demobilize voters, depending on pre-existing political attitudes. Much of the experimental work on this question comes from contexts where political efficacy is relatively high (e.g., Brazil), and in such places information has been shown to encourage electoral mobilization and accountability. But if we consider that in most places where corruption and elite capture are a first-order issue in politics (Pande, 2008), individuals are characterized by relatively low political efficacy, responsiveness to information may follow quite different dynamics as a result. To be sure, like any single case, Afghanistan is unique in many respects and probably an outlier on some dimensions that affect turnout decisions, all of which makes generalizing from these results difficult. Our main theoretical point is that information about corruption might have different effects on turnout depending on levels of pre-existing political efficacy, which might help explain variation in existing results across countries and suggest a fruitful avenue of future research.

Third, we study corruption in a highly relevant policy context and our results have implications for anti-corruption information campaigns in certain settings. Since at least as far back as 2006, a vast majority of Afghans consistently report that corruption is a major problem in the country (Akseer et al., 2019, 22,142-44). 129 of the roughly 400 interviews in the recently declassified Afghanistan Papers explicitly mention concerns about the role of corruption in undermining economic growth, political stability, and security provision in Afghanistan, and revealed that tens of billions of dollars were siphoned from official projects to enrich political elites, warlords, and the Taliban. As in other developing democracies that struggle with widespread corruption, information interventions would seem to hold promise as tools for increasing democratic participation and rooting out malfeasance. But the results here urge caution in expecting too much from increasing transparency and the availability of corruption-related information. For electoral sanctioning to work, voters must be made aware of corruption. But they must also be willing to denounce official corruption by turning out and voting against it (Boas, Hidalgo and Melo, 2019). The Kabul Bank scandal depressed intention to vote in this case and our results are consistent with the argument that in places where political efficacy is relatively low, information may not be enough to mobilize turnout and punish corruption. In fact, it may even demobilize political participation in the short term.

Motivation

How does information revealing official corruption affect voter attitudes and behavior? We situate our inquiry within a burgeoning, but inconclusive, theoretical and empirical literature on this question (De Vries and Solaz, 2017). Existing theories imply opposing effects of information on turnout and empirical evidence from studies testing implications of those theories is mixed.¹ Empirically, features of our quasi-experimental design combine some

¹For example, Ferraz and Finan (2008) show that publishing information about local government corruption had an appreciable effect on Brazilians' vote choice in the 2004 election. More recently, in their

of the strengths from observational and experimental studies, yielding results we can have high confidence in about a real world corruption event of considerable importance. While observational studies have the virtue of analyzing the effects of real information on actual behavior, it is difficult to rule out possible confounds in the analysis. As one recent review of this literature observes, “[t]he scant evidence from observational studies about the effect of exposing corruption on electoral turnout is inconclusive” (Chong et al., 2015, 56). To more cleanly estimate effects of interest in this area, researchers have used survey experiments to examine the informational mechanism. But since the informational “shock” in the surveys is hypothetical in nature, how relevant such results are for understanding real world dynamics is unclear. Incerti (2020, 761) finds that results from field and survey experiments in this literature diverge significantly, and that while “survey experiments may provide point estimates that are not representative of real-world behavior”, “field experimental estimates may also not recover the ‘true’ effects due to design decisions and limitations.” Substantiated “as if” random assignment in natural experiments obviates confounding, and since the intervention is not externally designed, the data recovered “are often the product of [naturally occurring] social and political forces” (Dunning, 2012, 16). In this paper, we make use of what others have described as an unexpected-event-during-survey-design (Muñoz, Falcó-Gimeno and Hernández, 2020) to reliably estimate causal effects. In the Data and Research Design section, we detail the ways in which we guard against threats to identification and robustness checks we perform to lend credibility to our claims to recover causal estimates.

The theoretical motivation for the research note is the competing logics and associated supporting evidence for how information about political corruption should affect electoral mobilization and attitudes. On the one hand, making citizens aware of government corrup-

innovative field experimental study in Benin, Adida et al. (2019) find no statistically significant effects of information on incumbent vote share. Similarly, while survey experiments indicate Brazilians strongly condemn corruption in the abstract (Winters and Weitz-Shapiro, 2013), Boas, Hidalgo and Melo (2019) find null effects of information about political corruption on voting behavior in their experimental work and argue that this is because when it comes to real elections in Brazil, corruption is a relatively low salience issue.

tion increases their likelihood of turning out to vote because they want to punish corruption in the only way possible: at the polls. Indeed, there is some evidence that corruption information mobilizes voters and causes them to punish incumbents (Banerjee et al., 2011; Cruz, Keefer and Labonne, 2021; Winters and Weitz-Shapiro, 2013). On the other hand, we might expect revelation of official corruption to lead to lower turnout since knowledge of corruption delegitimizes the state in citizens’ eyes (Seligson, 2002) and reduces their confidence in government’s ability and responsiveness. Evidence from a series of observational and experimental studies 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. Random revelation of information about Ugandan MPs’ performance did not affect the probability of their reelection (Humphreys and Weinstein, 2012). Chong et al. (2015) found that learning about corruption actually *reduced* turnout slightly in treated precincts just prior to local elections in Mexico. Most recently, a series of pre-registered, randomized control trials was conducted in multiple countries (Dunning and et al., 2019*b*) and found that overall, neither positive nor negative information (“good” or “bad” news) about politicians had any discernible effect on vote choice or on turnout, either in the pooled samples or in the individual country samples (Dunning and et al., 2019*a*).

To address this puzzle, we draw from scholarship on political efficacy in an effort to explain why information about corruption seems to have such divergent effects on citizen responsiveness across space and time. 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, “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).

But we note that much of our highest quality evidence on this question comes from a relative outlier in terms of citizens’ level of political efficacy. Consider that out of 113 countries whose citizens were surveyed in 2016 by The Quality of Government Institute, Brazilians reported the *highest* level of political efficacy specifically with respect to corruption. 83% of Brazilians agreed that they could make a difference in the fight against corruption; only 5% disagreed (Teorell et al., 2018).² To more systematically assess cross-national differences and their implications, we gather supplemental data from the Comparative Study of Electoral Systems (CSES, 2015), which conducted a survey prior to the Brazilian election cycle studied by Ferraz and Finan (2008). We draw on two items from this survey: who people vote for makes a difference, and who is in power can make a difference. (We have re-scaled the variables such that stronger agreement with each statement takes the value 5 and strong disagreement takes the value 1.) We then plot the mean response for each survey item by country in Figure 1a and 1b. Notice that Brazil is in the bottom right quadrant, with high levels of corruption (lower values on Y) and high political efficacy (higher values on X). With respect to the broader cross-national distribution, a simple linear regression is a very poor fit for the data when Brazil and three other outliers are included ($R^2=.02$). When these outliers are excluded, political efficacy and levels of corruption are more strongly correlated ($R^2=.21$).

In these respects, Brazil represents cases in which we should expect that information mobilizes turnout to hold politicians accountable, since citizens are especially receptive to

²Survey question: “Would you agree or disagree with the following statement: Ordinary people can make a difference in the fight against corruption.” Mean country-level agreement in the dataset was 54%.

revelations of political corruption. In principle, even small revelations of corruption can shift the attitudes and behaviors of individuals who overestimate their “pivotal” status in political reform. To better understand how information revelation influences electoral accountability more generally, we need evidence from other cases, especially where citizens report low political efficacy.

We reason that individuals with low efficacy—those who believe they do not have agency to influence outcomes through political engagement—will be demobilized by the emergence of a corruption scandal. However, as individuals’ sense of their own efficacy strengthens, we expect the relative demobilizing effect of information to attenuate. At some point, when political efficacy is high enough, information should begin to *mobilize* voters to turn out, which is in fact what others have observed in some places, using a variety of research designs.

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. Within days, 180 million USD in savings were withdrawn, jeopardizing the bank’s solvency.

Since “the precise time at which a corruption scandal breaks in the news...although, unexpected for most citizens, might be actually planned by politically motivated actors”

(Muñoz, Falcó-Gimeno and Hernández, 2020, 190-91), an important aspect of this timeline for our study is that the revelation to US authorities that set in motion the events of July and August 2010 (including the scandal becoming public knowledge) does not seem to have been timed strategically to coincide with the parliamentary election. Nor did elites paying attention to these issues at the time anticipate it. Observers explain Farnood’s revelation of information to US authorities as not only unexpected in its timing, but self-serving in purpose (Higgins, 2010): “Quite unexpectedly that month [July 2010], Kabul Bank Chairman Sherkhan Farnood visited the U.S. Embassy in Kabul and blew the whistle on Kabul Bank, telling the U.S. embassy staff ‘everything.’ Farnood told all because he and CEO Ferozi had a falling out that summer and, consequently, Farnood was reported to have feared losing his influence at Kabul Bank” (*The Kabul Bank Scandal and the Crisis that Followed*, N.d., 9). This contextual information about the breaking of the scandal is useful for increasing confidence that in this respect, the Kabul Bank scandal can be treated as an unexpected event during survey.

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.³ 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 a useful and informative measure of political

³See Strand (2014) for background on the Bank’s links with political officials.

engagement: citizens' intention to vote several weeks later in the parliamentary election. 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) and as part of efforts to study the effects of information campaigns on voting behavior (Lierl and Holmlund, 2019; Platas and Raffler, 2019). One might be concerned that respondents' 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.⁴

In Figure 2, 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, 2010.⁵ The North Atlantic Treaty Organization (NATO) contracted the Afghan Center for Socio-Economic and Opinion Research (ACSOR) to design and implement the survey. AC-

⁴As 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.

⁵Figure SI-5 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.

SOR 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. 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.⁶

Importantly for our research design, the survey sampling sequence was set ahead of, and unaltered by, the financial scandal. Above, we provided a substantive examination of the event in an effort to validate excludability assumptions inherent in a research design of the kind we employ here (Muñoz, Falcó-Gimeno and Hernández, 2020, 194). Here, we provide evidence for a crucial element of the research design, that any differences we observe between respondents surveyed before and after the event are a consequence of that event (Muñoz, Falcó-Gimeno and Hernández, 2020, 189). We plot a visualization of households sampled within each district after the scandal broke in Figure 3a and 3b. 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

⁶See Figure SI-4 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 ($\sim 3.6\%$) than those reported in a comparable survey ($\sim 15\%$) conducted in Afghanistan in 2011 (Lyall, Shiraito and Imai, 2015).

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 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 4.

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 of our analysis.⁷ If some survey respondents in fact receive information about the scandal 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).

Although the spatial allocation of timing around the unexpected scandal outbreak is plausibly random, it is still possible that imbalances are present across households enumerated before and after the scandal broke. Practically, this is the quasi-experimental equiv-

⁷In the analysis below, we show the results are robust to varying this cutoff.

alent of failed randomization within a randomized control trial. To investigate this, and as recommended by Muñoz, Falcó-Gimeno and Hernández (2020) to check for imbalances on observables, we use multivariate regressions to produce point estimates for our main and supplemental demographic and household characteristics, where the outcome is being sampled in the post period. We present these point estimates in Figure 5. Of the 10 coefficient estimates, we find none statistically significant at the 5% level, and 2 statistically significant at the 10% level. This evidence suggests that the sequence of enumeration was “as-good-as-randomized” around the Kabul Bank scandal.

Our design allows us to hold general characteristics associated with voter engagement and voting preference fixed as they are differenced out during estimation. That is, any systematic differences between demographic groups 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 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):

$$y_i = \alpha + \beta Post_i + \lambda D_i + \gamma X_i + \epsilon \tag{1}$$

where y_i is the respondent’s intention-to-vote. 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). D_i indicates district-level fixed effects and X_i is a vector of control variables. All models include age, age squared, gender, education, and ethnicity as demographic controls. We also include a set of indicator variables to control for individual non-responsiveness to

relevant questions. Robust standard errors are clustered by district to account for potential spatial clustering in 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 intention to vote and present main results in Table 1. Column 1 is from our baseline model with district fixed effects and demographic controls as described above. Overall, we estimate a sizeable (10%) decrease in intent-to-vote.

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 2 and 3 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 Column 3 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 intent-to-vote are unaffected.

These results suggest that revelation of corruption significantly and negatively affected citizens’ intention to vote in the parliamentary election two weeks later. 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. 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.

But why did information about the scandal demobilize voters in this case? As we argued above, we expect that this is due, in part, to Afghan citizens generally not feeling that political activity can make much of a difference in the political process. We turn next to an

empirical exploration of this argument.

Heterogeneous Effects of Political Efficacy

The ANQAR survey does not provide any information on respondents' political efficacy, so we cannot examine how this might have moderated any information effect of the scandal on 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."⁸ 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 by adding an interaction term, $\text{post} \times \text{efficacy}$. It is possible that provinces with high and low efficacy differ on important observable factors. Figure SI-1 presents a balance test on observables across the pre/post periods for high efficacy provinces, following the approach in Figure 5. Because our estimating equation includes a location fixed

⁸This 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).

effect below the province (i.e., 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. It is also worth noting that the full set of interaction terms in our model absorbs 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 interaction of high efficacy and post-scandal enumeration in Table 2. We observe a significant reduction in intention-to-vote among individuals living in low efficacy provinces, which is driving the observed average effect in the full sample (Table 1). This is what we would expect based on the logic connecting low levels of personal efficacy to political disengagement. Moreover, while we do not observe a precisely estimated overall mobilization effect of information in high efficacy provinces, the marginal effect of moving to a high efficacy province is positive and statistically significant, which also is consistent with the theoretical logic. The demobilization effect of information about the corruption scandal attenuates as efficacy increases. As in previous specifications (Table 1), in Columns 2-3 we progressively add controls to account for the security situation and elements of survey enumeration. The results are unaffected.

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 or others 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 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.

Information Mechanism Test

In their seminal paper testing the informational theory of voter behavior, Ferraz and Finan (2008) show that the release of audit information to the public about local government corruption significantly reduced incumbents' chance of reelection. Further in line with the informational theory of voter behavior, this effect was particularly pronounced in places where local radio was present, which facilitated the dissemination of information to the public.

Following this intuition, to increase confidence in our argument, we provide a simple placebo population test (Eggers, Tuñón and Dafoe, 2021) of the purported mechanism that information is most likely to be transmitted through media sources. Splitting the sample into individuals with and without media access,⁹ we re-estimate specifications in Table 2 that include the full set of control variables, and report them in Table 3. Notice that Column 3, relying solely on subjects with media access, yields the statistically precise base term from our main analysis, while the effect sizes and precision in Column 2, where subjects do not

⁹Respondents are coded as having media access if they report television or radio as their most frequent source of news and information, and having no media access otherwise.

have media access, are inconsistent. 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 in this case.

Supplemental Results

In Supporting Information, we provide and discuss in more detail a series of supplemental results to address potential concerns about the main findings. First, we show that results in Table 2 are robust to a range of pre/post cutoff windows (Figure SI-2). Second, the results are insensitive to alternative cutoffs for classifying province-level political efficacy (Figure SI-3). Third, dropping the district fixed effects from our models or excluding districts without within-unit variation in timing of enumeration (i.e., pre/post) does not affect results (Table SI-1). Finally, we show the comparability of demographic characteristics across the ANQAR and Asia Foundation survey samples (Figure SI-5).

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 real and economically damaging Kabul Bank financial scandal of 2010. 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 intent-to-vote in the parliamentary election scheduled several weeks later. Overall, we observe a statistically significant and negative effect of the scandal on citizens' intent-to-vote, which is consistent with other work that documents information's demobilizing effects in some contexts. Various features of our quasi-experimental research design lend strength to the causal nature of our claims.

To explain variation in reactions in this case and potentially others, we draw on a theoret-

ical literature that emphasizes how political efficacy might moderate the effect of corruption-related information on citizens' political mobilization. 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).

The policy implications of an informational theory of accountability and sanctioning have motivated anti-corruption efforts by governments and international organizations worldwide, including recently in Nigeria, the Dominican Republic, and Afghanistan. But Olken and Pande (2012) observe that the same programs that might “work” to discourage political corruption in some countries via an electoral sanctioning mechanism may have a much more muted effect in other countries. Our argument emphasizing variation in political efficacy (both within and across countries) provides one reason why this might be the case. In the context studied here, we find evidence consistent with the argument that pre-existing political efficacy moderates information's effects: information had a demobilizing effect in low efficacy provinces but this effect was attenuated in high efficacy provinces. Thus, the analysis suggests that where efficacy is low, information about corruption actually harms accountability by reducing likely turnout.

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) and even citizens themselves (Corbacho et al., 2016). 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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TABLES

Table 1: Estimates of Financial Scandal on Intent-to-Vote

	(1)	(2)	(3)
	Benchmark	Benchmark - Security	Benchmark - Survey Bias
Post	-0.108** (0.0459)	-0.106** (0.0435)	-0.105** (0.0442)
SUMMARY STATISTICS			
Outcome Mean	0.699	0.699	0.699
Outcome SD	0.459	0.459	0.459
PARAMETERS			
District FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
ADDITIONAL PARAMETERS			
Security	No	Yes	Yes
Govt. Control	No	Yes	Yes
Govt. Patrols	No	Yes	Yes
Survey Bias Controls	No	No	Yes
MODEL STATISTICS			
N	9803	9803	9802
Clusters	240	240	240

Notes: Outcome in Table 1 is: “Do you plan to vote in the upcoming election?” 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 *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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

	(1)	(2)	(3)
	Benchmark	Benchmark - Security	Benchmark - Survey Bias
Post	-0.153*** (0.0479)	-0.150*** (0.0450)	-0.150*** (0.0454)
Post \times High Efficacy	0.160*** (0.0594)	0.155*** (0.0581)	0.163*** (0.0571)
SUMMARY STATISTICS			
Outcome Mean	0.699	0.699	0.699
Outcome SD	0.459	0.459	0.459
PARAMETERS			
District FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
ADDITIONAL PARAMETERS			
Security	No	Yes	Yes
Govt. Control	No	Yes	Yes
Govt. Patrols	No	Yes	Yes
Survey Bias Controls	No	No	Yes
MODEL STATISTICS			
N	9803	9803	9802
Clusters	240	240	240

Notes: Outcome in Table 2 is: “Do you plan to vote in the upcoming election?” 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 *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

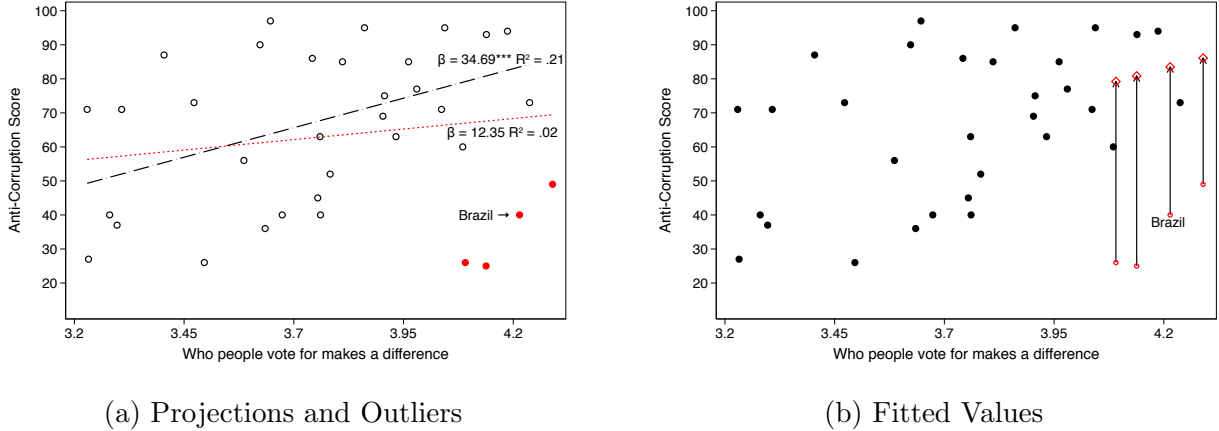
Table 3: Estimates of Financial Scandal on Intent-to-Vote: Accounting for Heterogeneity by Political Efficacy and Media (information) Access

	(1)	(2)	(3)
	Full Sample	No Media Access	With Media Access
Post	-0.150*** (0.0454)	-0.0899* (0.0477)	-0.171*** (0.0447)
Post × High Efficacy	0.163*** (0.0571)	0.0919 (0.0618)	0.183*** (0.0593)
SUMMARY STATISTICS			
Outcome Mean	0.699	0.668	0.719
Outcome SD	0.459	0.471	0.450
PARAMETERS			
District FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
ADDITIONAL PARAMETERS			
Security	Yes	Yes	Yes
Govt. Control	Yes	Yes	Yes
Govt. Patrols	Yes	Yes	Yes
Survey Bias Controls	Yes	Yes	Yes
MODEL STATISTICS			
N	9802	3538	5871
Clusters	240	221	233

Notes: Outcome in Table 3 is: “Do you plan to vote in the upcoming election?” 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 *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

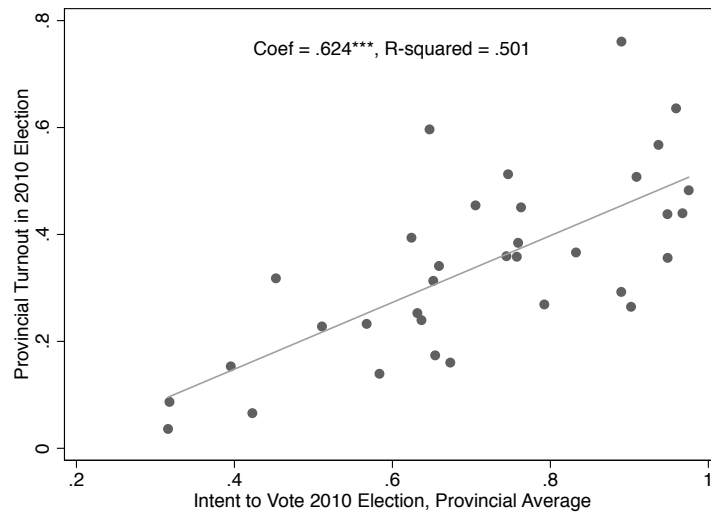
FIGURES

Figure 1: Cross-national Evidence Linking Voter Beliefs about Efficacy and Corruption Scores



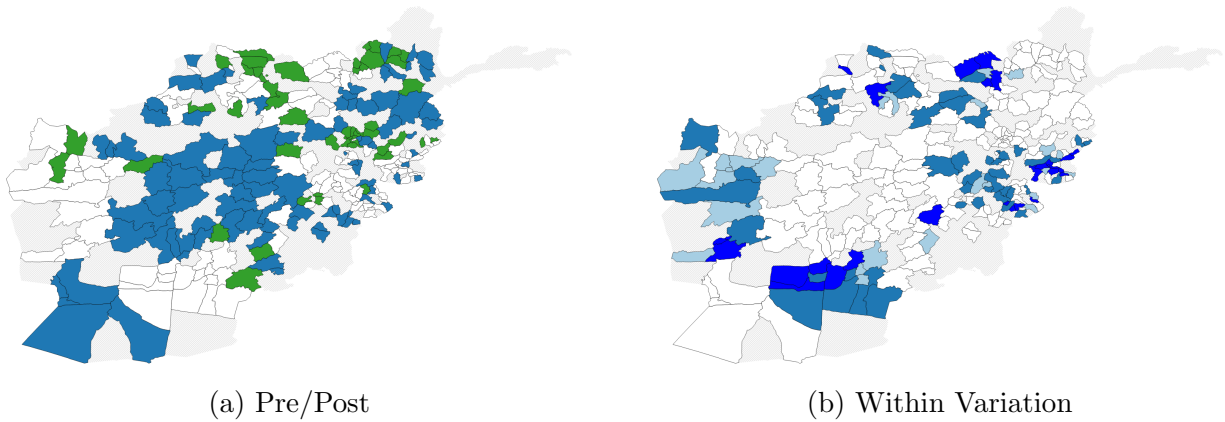
Notes: Higher anti-corruption scores indicate lower corruption; higher scores on voter dimension indicate stronger belief voting makes a difference. In Panel A, we plot data from CSES and CPI. Red dots indicate outliers. Red (dotted) line is fit through all data points. Black (dashed) line is fit through all data points except outliers (Albania, Brazil, Hungary, Romania). Slope coefficients, statistical precision, and model fit statistics noted in plot. In Panel B, we plot outliers with red (circle) dots and out-of-sample predictions about values of outliers on corruption score (outcome) with red diamonds. Vertical shift in projection noted with arrows.

Figure 2: Intention-to-Vote against Turnout in 2010 Afghan Election



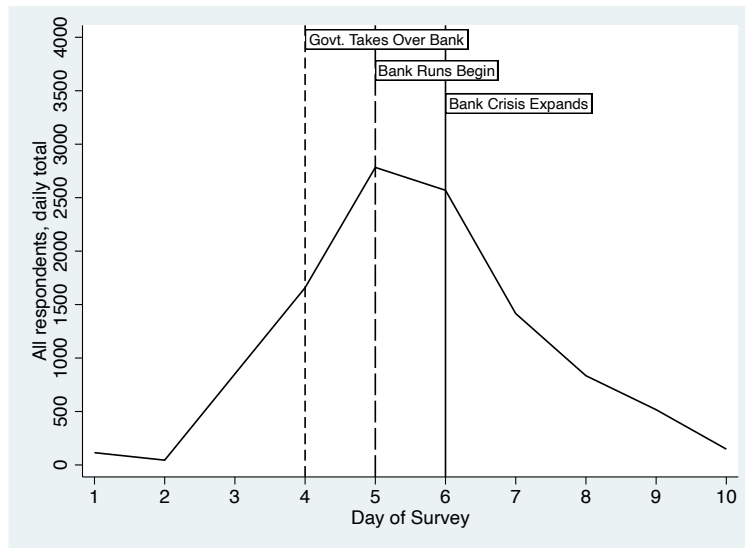
Notes: Figure displays province-level averages of pre-election intent-to-vote (from Afghanistan Nationwide Quarterly Research survey data) and official turnout (from publicly available Independent Election Commission of Afghanistan data), weighted by voting age population (from Afghanistan’s Central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

Figure 3: Percentage of households by district enumerated after the scandal emerged



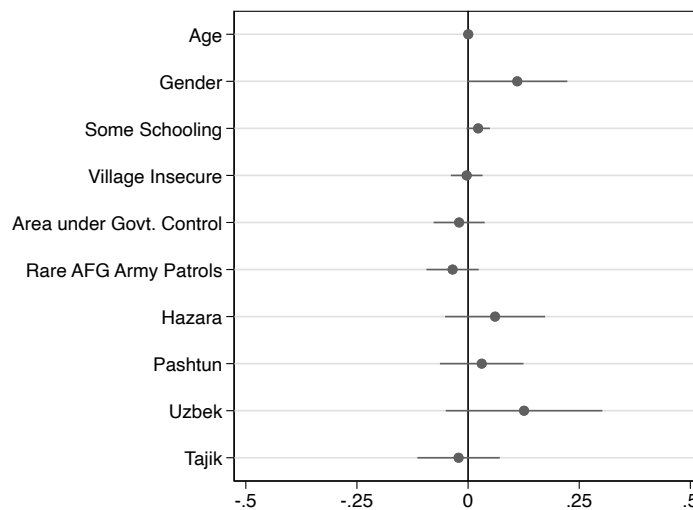
Notes: Figure displays binned classifications of the percentage enumerated on or after day 6 of sample. In Panel A, all survey respondents were enumerated either before (green) *or* after (blue) the scandal. In Panel B, we plot districts with variation in the timing of sampling before *and* after the 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 4: Timeline of Kabul Bank crisis and survey collection



Notes: Dark line indicates the daily total of respondents enumerated by day of the sample. 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>).

Figure 5: Regression-Based Balance Tests across Pre/Post



Notes: Coefficient plots from regression where outcome of interest is an indicator variable for post (equals 1 if a respondent is sampled in Day 6 or later), 95% confidence intervals shown. Ethnicity is split into four dummy variables for ethnic groups with at least 500 individuals sampled. Following the main regressions, district fixed effects are included and standard errors are clustered by administrative district.

SUPPORTING INFORMATION

— For Online Publication Only —

Supplemental Figures

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SI-3	Sensitivity of Heterogeneous Effects to Alternative Cutoffs for Political Efficacy Classification	SI-4
SI-4	ANQAR diagnostics during later waves (16-38) conducted by firm collecting Wave 9 survey data (ACSOR)	SI-7
SI-5	Comparison of ANQAR Wave 9 and Asia Foundation Demographic Data . .	SI-8

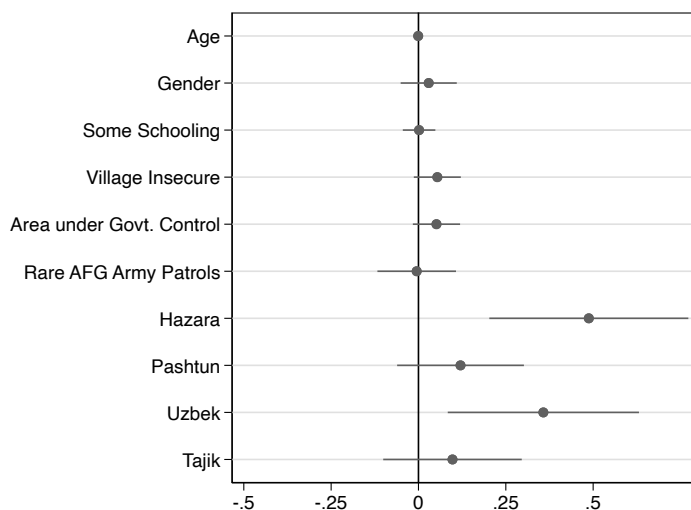
Supplemental Tables

SI-1	Estimates of Financial Scandal Exposure on Intent-to-Vote (no district fixed effects & only districts with within variation)	SI-5
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SI-3	Survey Instruments Overview	SI-9

A Supplemental Results

In this appendix section we report on several additional sensitivity tests of the main results, as well as supplemental results. As in Figure 5, in Figure SI-1 we check for imbalances on observables across the pre/post periods, but only for those coded as living in high efficacy provinces. There is some imbalance in the sample, specifically for Hazaras and Uzbeks. We note that ethnicity is a control variable in all regressions.

Figure SI-1: Regression-Based Balance Tests across Pre/Post (High Efficacy only)

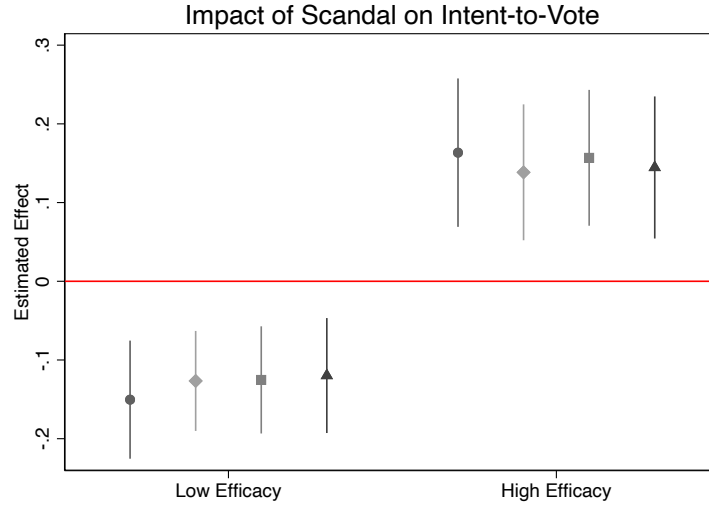


Notes: Coefficient plots from regression where outcome of interest is an indicator variable for post (equals 1 if a respondent is sampled in Day 6 or later), 95% confidence intervals shown. Ethnicity is split into four dummy variables for ethnic groups with at least 500 individuals sampled. Following the main regressions, district fixed effects are included and standard errors are clustered by administrative district. Sample includes only individuals in high efficacy provinces.

Figure SI-2 shows the robustness of the estimated effects on intent-to-vote, across a range of pre/post cutoff windows. In the left panel, we plot the baseline difference-in-difference (when political efficacy is low). In the right panel, 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 Figure SI-3, 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 findings. First, given that only a subset of districts is enumerated both before and after news 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).

Figure SI-2: Sensitivity of Heterogeneous Effects to Alternative Pre/Post Windows

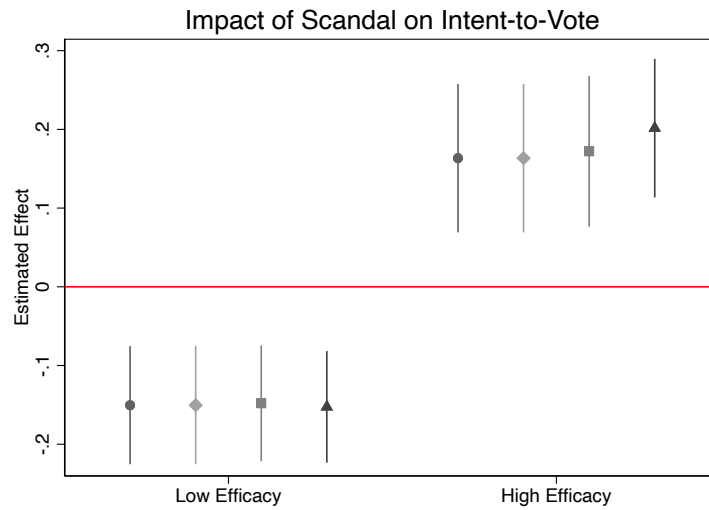


Notes: Figure displays regression coefficients from benchmark models in Table 2 Column 3. Each point estimate in tandem represents the estimated heterogeneous effects if the pre/post cutoff is decreased by 1 day, starting at 6 and ending at 2 days. Each set of reported coefficients replicates the benchmark specification in the main analysis with varying sample cutoffs. All other model parameters and specifications (standard errors, weights) remain consistent with benchmark specification.

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 SI-1. 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.

Second, in Figure SI-5 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 underrepresentation of female respondents in several provinces in ANQAR. The balance tests presented earlier indicate this undersampling of female subjects was consistent across pre- and post-scandal groups. 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

Figure SI-3: Sensitivity of Heterogeneous Effects to Alternative Cutoffs for Political Efficacy Classification



Notes: Figure displays regression coefficients from benchmark models in Table 2 Column 3. 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.

presented in Table SI-2. 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 SI-1: Estimates of Financial Scandal Exposure on Intent-to-Vote (no district fixed effects & only districts with within variation)

	(1)	(2)
	Benchmark - Survey Bias No Dist FE	Benchmark - Survey Bias Within Var
Post	-0.155*** (0.0423)	-0.153*** (0.0436)
Post × High Efficacy	0.190*** (0.0501)	0.165*** (0.0552)
SUMMARY STATISTICS		
Outcome Mean	0.699	0.685
Outcome SD	0.459	0.465
PARAMETERS		
District FE	No	Yes
Demographic Controls	Yes	Yes
ADDITIONAL PARAMETERS		
Security	Yes	Yes
Govt. Control	Yes	Yes
Govt. Patrols	Yes	Yes
Survey Bias Controls	Yes	Yes
MODEL STATISTICS		
N	9802	6348
Clusters	240	97

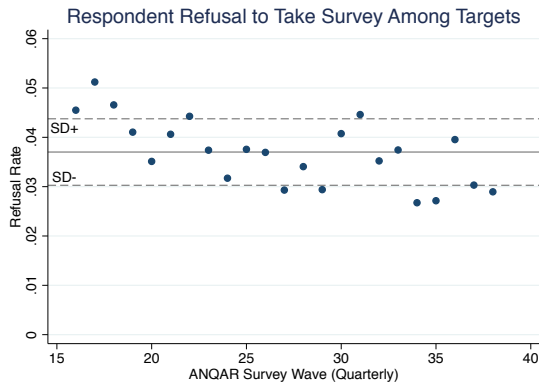
Notes: Table replicates model in Table 2 Column 3. Outcome in Table SI-1 is “Do you plan to vote in the upcoming election?” Model in Column 1 does not include district fixed effects and model in Column 2 includes only districts with within variation. Unit of analysis is individual survey respondent. Models include baseline demographic controls (age, age squared, education, gender, ethnicity) as well as additional parameters indicated. Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table SI-2: Estimates of Financial Scandal on Intent-to-Vote: Accounting for Heterogeneity by Political Efficacy and Undersampling of Female Subjects

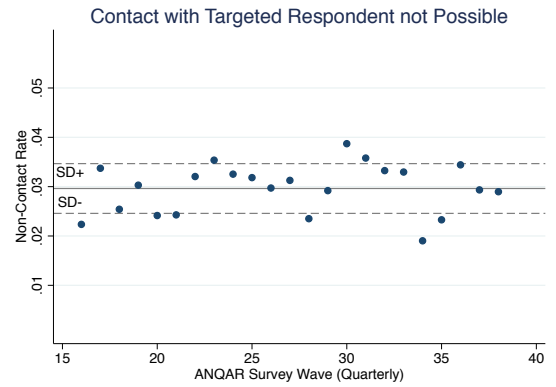
	(1)	(2)	(3)
	Benchmark	Benchmark - Security	Benchmark - Survey Bias
Post	-0.155*** (0.0467)	-0.152*** (0.0448)	-0.153*** (0.0451)
Post × High Efficacy	0.166*** (0.0595)	0.159*** (0.0591)	0.167*** (0.0581)
SUMMARY STATISTICS			
Outcome Mean	0.716	0.716	0.716
Outcome SD	0.451	0.451	0.451
PARAMETERS			
District FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
ADDITIONAL PARAMETERS			
Security	No	Yes	Yes
Govt. Control	No	Yes	Yes
Govt. Patrols	No	Yes	Yes
Survey Bias Controls	No	No	Yes
MODEL STATISTICS			
N	8542	8542	8541
Clusters	198	198	198

Notes: Table replicates models in Table 2. Outcome in Table SI-2 is: “Do you plan to vote in the upcoming election?” Unit of analysis is individual survey respondent. Provinces with undersampling of female subjects (relative to 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 *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

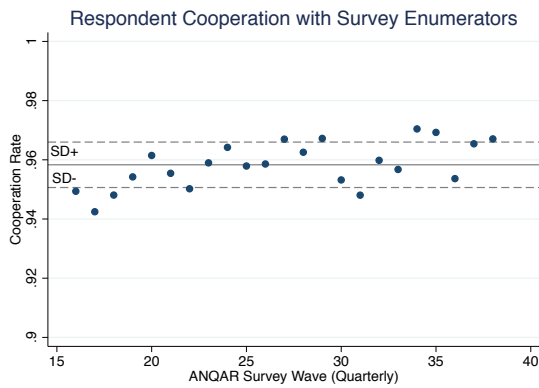
Figure SI-4: ANQAR diagnostics during later waves (16-38) conducted by firm collecting Wave 9 survey data (ACSOR)



(a) Refusal rate



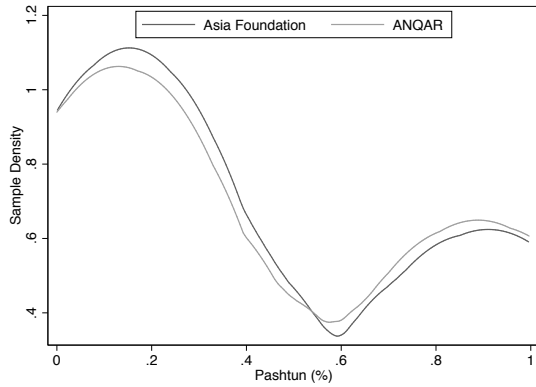
(b) Non-contact rate



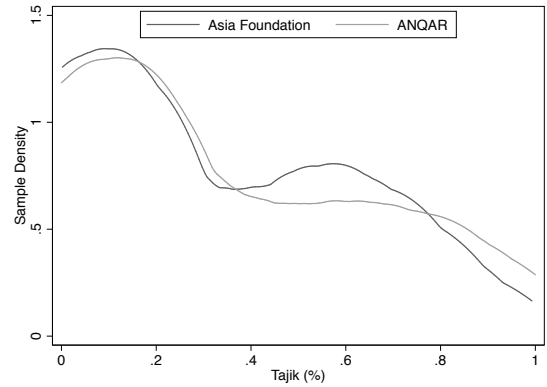
(c) Cooperation rate

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.

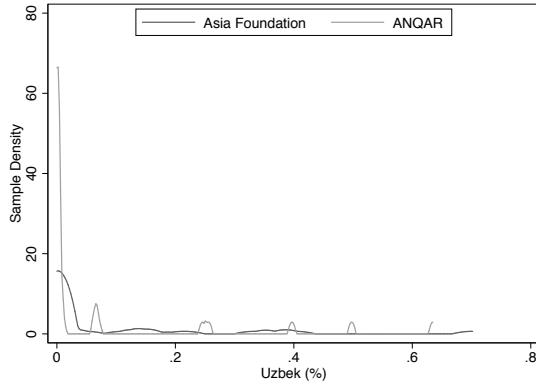
Figure SI-5: Comparison of ANQAR Wave 9 and Asia Foundation Demographic Data



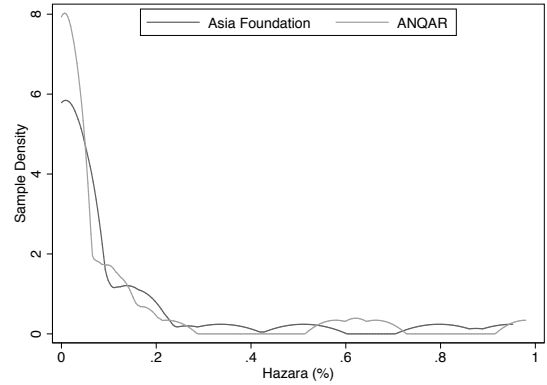
(a) Pashtun (%)



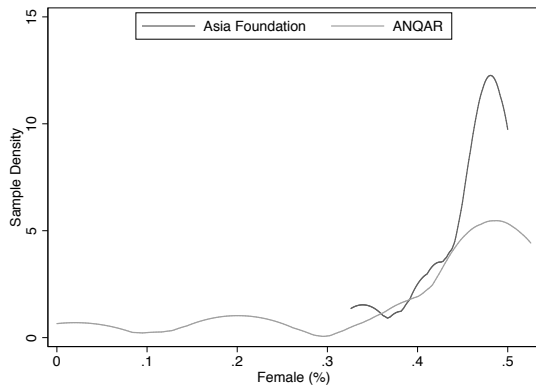
(b) Tajik (%)



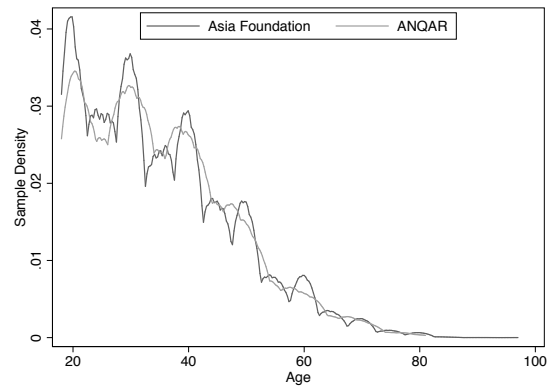
(c) Uzbek (%)



(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 SI-2).

Table SI-3: Survey Instruments Overview

Variable	Question	Coding (= 1 if)
Plan to Vote	Do you plan to vote in the upcoming Wolesa Jirga election?	Yes
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