

# To Blame or to Support? Large-scale Insurgent Attacks on Civilians and Public Trust in State Institutions

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While there is a substantial body of literature on the consequences of terror attacks on public attitudes toward state institutions in Western democracies, little is known about the impact that such events have in the context of armed conflict. We address this gap by exploring the attitudinal effects of a 2012 Taliban attack on civilians in Kabul City, Afghanistan. We test two competing hypotheses: the “rally-effect” hypothesis according to which individuals increase their trust in incumbent institutions in the aftermath of violent attacks and the “accountability” hypothesis according to which individuals punish state institutions for their inability to provide security by withdrawing trust. Leveraging a quasi-experiment that compares individuals interviewed before the attack to individuals interviewed thereafter, we find that the attack—in line with the rally-effect hypothesis—increased trust in several state institutions among residents of Kabul City.

Si bien existe una importante bibliografía sobre las consecuencias de los atentados terroristas en las actitudes de la población frente a las instituciones estatales en las democracias occidentales, se sabe poco sobre el impacto que tienen dichos acontecimientos en el contexto de los conflictos armados. Para abordar este vacío, exploramos los efectos actitudinales de un ataque talibán de 2012 contra civiles en la ciudad de Kabul, Afganistán. Probamos dos hipótesis que compiten entre sí: la hipótesis del “efecto rally”, según la cual las personas aumentan su confianza en las instituciones en el poder después de los ataques violentos; y la hipótesis de la “responsabilidad”, según la cual las personas les retiran la confianza a las instituciones estatales a fin de castigarlas por su incapacidad para brindar seguridad. Gracias a un cuasiexperimento que compara a las personas que se entrevistaron antes del atentado con las que se entrevistaron después, descubrimos que el atentado (en consonancia con la hipótesis del “efecto rally”) aumentó la confianza en varias instituciones estatales entre los residentes de la ciudad de Kabul.

Bien qu’il existe un corpus substantiel de littérature sur les conséquences des attaques terroristes sur les attitudes publiques envers les institutions d’État dans les démocraties occidentales, nous ne savons que peu de choses sur l’impact que de tels événements ont dans le contexte d’un conflit armé. Nous abordons cette lacune en explorant les effets sur les attitudes face à une attaque que les Talibans ont perpétré en 2012 contre des civils de Kaboul, Afghanistan. Nous mettons les deux hypothèses concurrentes suivantes: l’hypothèse “d’effet de ralliement” selon laquelle les individus accordent davantage leur confiance aux institutions en place suite à des attaques violentes, et l’hypothèse de “responsabilité” selon laquelle les individus sanctionnent les institutions d’État pour leur incapacité à assurer leur sécurité en leur retirant leur confiance. Nous avons mis à profit une quasi-expérience comparant les réponses d’individus interrogés avant l’attaque à celles d’individus interrogés après, et nous avons constaté que—conformément à l’hypothèse d’effet de ralliement—l’attaque avait accru la confiance des habitants de Kaboul en plusieurs institutions d’État.

## Introduction

Between 1989 and 2018, insurgent groups around the globe deliberately killed over 200,000 civilians.<sup>1</sup> Perhaps one of the most sinister ways in which civilians die in the context of armed conflict is through attacks that aim to produce large numbers of civilian casualties. That violence against civilians constitutes a prominent feature of the violent repertoires of insurgent groups to pressure governments into concessions and to undermine public support for the state is well known (e.g., Crenshaw 1981; Hultman 2009;

Stanton 2013; Thomas 2014; Fortna 2015; Polo and Gleditsch 2016; Butcher 2017; Polo and González 2020). Yet, we know little about the capacity of large-scale attacks to shape public attitudes toward the state and its institutions in the context of ongoing armed conflict.

Understanding how such attacks shape citizens’ perceptions of formal state institutions is important, as the democratic and effective functioning of these require some levels of institutional trust to be present (cf. Deglow 2018, 12). How much trust the public places in its institutions affects levels of compliance with the rules and regulations these institutions produce (Levi and Stoker 2000). This is doubly important in societies affected by war, as post-war stability is likely to depend on participation in economic and political processes, as well as respect for government decision-making (De Juan and Pierskalla 2016).

Previous research on large-scale attacks on civilians outside of conflict contexts points to such attacks causing a “rally ’round-the-flag” effect, whereby the public comes together and boosts its support of and confidence in incumbent government institutions (e.g., Wollebaek et al. 2012; Dinesen and Jaeger 2013; Coupe 2017). As the underlying mechanisms triggering rally effects are often theorized to

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<sup>1</sup>Authors’ own assessment based on the UCDP One-sided violence data set, version 19.1

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be related to psychological mechanisms that allow individuals to cope with the stress and uncertainty stemming from violent shocks, it might be that large-scale attacks on civilians during armed conflict trigger the same psychological responses and, therefore, outcomes. Yet, research on the political legacies of civil war indicates that violence predominantly seems to reduce trust in state institutions (Grosjean 2014; Voors and Bulte 2014; Wig and Tollefsen 2015; Deglow and Sundberg 2020 (early view); De Juan and Pierskalla 2016). Civil war contexts differ from more stable political environments in ways that might make rally effects less likely to occur (Gates and Justesen 2020). State and government legitimacy is by definition contested; state institutions are less likely to display the properties commonly known to trigger confidence among the citizenry, and baseline levels of trust in the state and its institutions are often low. In contexts of armed conflicts, does the populace blame the government for its failure to provide security when faced with large-scale attacks on civilians, or does it rally around the state and its institutions?

To assess this puzzle, we leverage a quasi-experiment arising from the coincidental co-occurrence of a Taliban attack with one of Afghanistan's largest public opinion surveys, the Survey of the Afghan People (hereafter SoAP). We focus on an attack by Taliban insurgents on the Spozhmai Hotel in Kabul City on June 21, 2012, and analyze the effect it had on individual-level trust in three major state institutions: the local government (here, the provincial government), parliament (National Assembly), and the police (Afghan National Police, ANP). Specifically, we use the attack as an exogenous source of variation to compare individuals interviewed before the attack to individuals interviewed after. This causal identification strategy has successfully been used in previous studies on the consequences of terrorist attacks for a range of political attitudes (e.g., Wollebaek et al. 2012; Dinesen and Jaeger 2013; Legewie 2013; Balcells and Torrats-Espinosa 2018; Gates and Justesen 2020; Muñoz, Falcó-Gimeno, and Hernández 2020; Nussio 2020).

Afghanistan is a particularly relevant case to study such attacks for two reasons. First, insurgents have repeatedly killed large numbers of civilians in urban spaces through deliberate attacks in recent years. 2017 and early 2018 saw Afghanistan—and in particular Kabul—experience multiple high-casualty attacks aimed at ‘soft’ targets: in May 2017, a car bomb in Kabul’s diplomatic quarter killed at least 150 people and in January 2018 an ambulance laden with explosives killed more than 100 people in the government office quarter. Civilian casualties in these kinds of attacks reached an unprecedented high in Afghanistan in 2018, causing “disproportionate and extreme harm to Kabul residents” who experienced almost half of all attacks that year (UNAMA 2019). Although government officials and commentators noted that insurgents groups were using such attacks as a strategy aimed at eroding public trust in state institutions in general (Rashid 2017) and security forces in particular (Merharaban 2018), no academic study has yet analyzed how these attacks affect institutional trust. Second, Afghanistan represents a class of cases where state-building runs parallel to a high-intensity armed conflict. Our findings should therefore be informative beyond Afghanistan and shed light on the challenges of building trusted state institutions during and after armed conflicts.

We find that the attack increased trust in all three state institutions among residents of Kabul City. Conducting several robustness tests and exploring observable implications of the mechanisms commonly argued to trigger rally effects, we conclude that large-scale attacks on civilians seem capable

of boosting trust in state institutions—even in the context of violent conflict. We make three main contributions: First, we add to the literature on the effects of terrorism by providing evidence on the consequences such attacks might have in the context of civil wars. Second, we show that rally effects might be triggered by a much more diverse set of attacks than commonly anticipated, and demonstrate tentative evidence of the psychological basis of the hypothesis. Finally, we contribute to the literature on the institutional consequences of civil wars by providing insights into the immediate effects of organized political violence. Taken together, these findings demonstrate that insurgents’ strategies to target the unarmed are not only violations of the laws of war, but might very well achieve the opposite of their intent.

In the next section, we review the two relevant strands of literature that speak to the puzzle that we address: the literature on the relationship between terrorist attacks and institutional trust and the literature on civil war and institutional trust. We deduce two competing hypotheses before providing a background to the armed conflict in Afghanistan, the 2012 Taliban attack and our causal identification strategy. We then present and discuss the results, and end by drawing conclusions and discussing avenues for future research.

### Previous Research and Hypotheses

The majority of previous research on the effects of large-scale attacks on civilians has been conducted in the context of Western democracies. Overall, these studies have identified one main effect: small to large short-term increases in trust and confidence in incumbent leaders and various types of state institutions. For instance, Dinesen and Jaeger (2013) find significant increases in trust in the national government, parliament, the media, and the police as a consequence of the 3/11 Madrid terror attacks. Wollebaek et al. (2012) find that trust in the police, the court system, government, and parliament was higher after the Utøya attacks of 2011 in Norway than previously. Coupe (2017) finds that the November 2015 attacks in Paris boosted trust in the central government. And a range of studies identify increased approval of the presidency and government institutions, as well as increasing patriotism, following the 9/11 attacks in New York City (e.g., Chanley 2002; Gaines 2002; Hetherington and Nelson 2003; Bonanno and Jost 2006). These findings are commonly explained by the “rally ‘round-the-flag’” effect, whereby individuals increase their trust and confidence in political leaders and state institutions in the immediate aftermath of large-scale attacks. While the rally effect was originally theorized in regard to leadership approval (Mueller 1970), the findings reviewed above indicate that such effects seem to be applicable to trust in state institutions more broadly. Individuals might not exclusively rally around political leadership figures in response to large-scale violent attacks, but around an entire political system and its institutions (Parker 1995). Increases in trust in state institutions can therefore be seen as a version of the rally effect that spans domains of public opinion beyond leadership approval (Gaines 2002; Perrin and Smolek 2009; Dinesen and Jaeger 2013).

If we assume that large-scale attacks on civilians in contexts of internal armed conflict are comparable events to attacks in more stable political contexts, then we would expect the rally effect found in previous research to also apply during civil wars. The basis of comparability may be found in psychological theory. The underlying mechanism of the rally effect is commonly theorized to be related to

psychological processes of coping with stressful events, rather than cognitive reflection on democratic accountability. For instance, studies have demonstrated how perceptions of threat—both real-world, and primed in experimental settings—lead individuals to prefer conservative over liberal and progressive ideology, to shift toward authoritarian attitudes, and to focus on the preservation of existing social institutions (Jost et al. 2003; Landau et al. 2004; Bonanno and Jost 2006; Echebarria-Echabe and Fernández-Guede 2006; Dyrstad 2013). This is also in line with several studies that find that terrorism increases the vote share for right-wing political parties that commonly campaign on a more authoritarian platform (e.g., Berrebi and Klor 2008; Kibris 2011; Getmansky and Zeitzoff 2014).

In terms of mechanisms to explain this threat-to-change relationship, several interrelated explanations exist. The Mortality Salience hypothesis (Landau et al. 2004) proposes that when faced with cues of one's own death individuals boost self-esteem and increase adherence to their worldview: often increasing their support for incumbent leaders. Yet, some studies also show how this extends to the government as well as other institutions, such as the army (Chatard et al. 2011). In the perspective of System Justification Theory, threats—such as terrorist attacks—to the 'system' in which people live their lives tend to make individuals seek out familiarity and certainty, shifting attitudes to become more conservative and more supportive of incumbent leaders and institutions (Bonanno and Jost 2006). Finally, Canetti-Nisim et al. (2013) propose a stress-based model, where the existential insecurity and psychological distress caused by terrorist attacks plays an important role in attitude change: also in contexts of conflict. Although somewhat different in focus and specification of causal processes, these approaches share a theoretical basis: to counter the stress and uncertainty that stems from large-scale attacks on civilians, people become more supportive of institutions that promise stability.

From a theoretical perspective it is plausible that such psychological coping mechanisms are universally applicable to large-scale attacks on civilians independent of the context in which these occur. A vast amount of studies within political psychology have shown that violence in conflict settings can shape individual-level attitudes—even when individuals are repeatedly exposed (for an overview, see Canetti-Nisim et al. 2013). It is therefore possible that large-scale attacks on civilians represent shocks even if they occur during armed conflict and consequently trigger similar psychological responses as they do in more peaceful environments. This leads us to our first hypothesis:

**H1:** *Large-scale insurgent attacks on civilians are associated with an immediate increase in trust in state institutions.*

Yet, there are also reasons to assume that large-scale attacks on civilians during armed conflict are not comparable to attacks in stable political contexts and that we therefore should expect such attacks to have different effects. A burgeoning literature on the effects of violent conflict finds that violence appears to undermine trust and confidence in a range of state institutions (e.g., Hutchison and Hutchison 2011; Grosjean 2014; Voors and Bulte 2014; Wig and Tollefsen 2015; Deglow and Sundberg 2020 (early view); De Juan and Pierskalla 2016; Gates and Justesen 2020). This negative correlation is commonly explained by referring to an "accountability" argument (cf. Gates and Justesen 2020). When states are not able to provide security to their citizenry they signal low competence, which, in turn, leads

to a loss of legitimacy and trust (Lake 2010; Hutchison and Hutchison 2011; Bakke et al. 2014). Put simply, government performance matters for perceptions of trust (Keele 2007), and poorly performing governments are held accountable and punished with trust withdrawal. In contexts of civil wars, each large-scale attack might therefore be seen as an additional sign that the state is not able to provide security, leading to a "jading" rather than a "rally" effect "in which repeated terrorist attacks produce frustration with authorities for their failure to prevent attacks, rather than unity against those who carry them out" (Tallman 2007, 77).

This withdrawal of trust can apply to a range of state institutions, which may also include the three studied here. For instance, individuals might lose trust in the police as a consequence of insurgent attacks because this is the key institution that is by law mandated to protect citizens and prevent such attacks (Deglow and Sundberg 2020 (early view); Jonathan-Zamir, Weisburd and Hasisi 2014). Beyond the police, a diverse set of state institutions might further be blamed for violent attacks. While the particularities of which state institution is responsible for providing security—and might therefore be punished by the citizenry—is likely to depend on both the political system and country context, it may be that citizens generalize blame as they do not always have the information necessary to assign it to a specific institution or political leader (Gates and Justesen 2020).

That the rally-effect logic might not be applicable to violent contexts has also been noted by Gates and Justesen (2020), who suggest that attacks in such environments do not display the characteristics commonly assumed to trigger boosts in support of, and trust in, state institutions. Rally effects are expected to materialize in the aftermath of events that are characterized by three main features (cf. Tallman 2007): First, they are international in scope; second, there is an external threat; and third, the event poses a specific shock to attract public attention (Mueller 1970). As Dinesen and Jaeger (2013, 918) summarize, a rally effect often occurs after a "traumatizing event like a large-scale terrorist attack" that "leads to a collective loss of sense of security and, in the face of a national public threat, citizens tend to rally around and thus place more trust in national political institutions." Consequently, in the original definition of the rally effect, domestic crisis situations were not considered to lead to such an effect, as they "are at least as likely to exacerbate internal divisions as to soothe" ((Tallman 2007, 66), quoting Mueller (1970)). With regard to large-scale attacks on civilians in contexts of internal armed conflict, it could therefore be argued that a rally effect is unlikely to occur, as these three main features are absent: attacks during internal armed conflicts are unlikely to be international in scope; they are internal threats, rather than external ones; and they might not represent the kind of shock commonly associated with rally effects, as they occur more frequently and within a setting of an elevated security risk (cf. Gates and Justesen 2020, 6).

Civil war contexts also differ from more stable political environments in other important ways that make a rally effect less likely to occur. State and government legitimacy is by definition contested, state institutions are less likely to display the properties commonly known to trigger confidence among the citizenry, and baseline levels of trust in the state and its institutions are therefore likely to be low. This implies that violent shocks, such as large-scale attacks on civilians, should be unlikely to trigger increases in trust in state institutions, because citizens lack perceptions of political legitimacy that they can base their evaluation of state institutions on. This is in line with some of the findings

from studies on how crises, such as natural disasters (Carlin, Love, and Zechmeister 2014) or high levels of public insecurity (Pérez 2003), may further decrease citizens' perception of political legitimacy in contexts where political legitimacy is weak to begin with.

It is thus plausible that rather than increasing trust, large-scale attacks on civilians in contexts of civil war might trigger a reduction in institutional trust due to perceptions of government incompetence (cf. Arvanitidis, Economou, and Kollias 2016, 233) and a baseline lack of legitimacy. Thus, our second and rivaling hypothesis reads:

**H2:** *Large-scale insurgent attacks on civilians are associated with an immediate decrease in trust in state institutions.*

Three aspects are relevant to emphasize with regard to our hypotheses: First, since rally effects are expected to occur in the immediate aftermath of attacks we limit our theoretical expectations to short-term effects, with no predictions for the long term. Second, our two hypotheses are institution-agnostic. Concerning the rally effect (hypothesis 1), previous studies have not found such effects only for certain institutions, but that those rallied around differ in what appears to be a relatively random manner. To enhance comparability with previous findings, we therefore do not develop institution-specific expectations. The same agnosticism is applied to the “accountability” effect (hypothesis 2) as it is possible that citizens both generalize blame to a wide set of state institutions and that the particularities of a country's political system determine which state institutions individuals deem responsible for preventing and dealing with attacks and which institutions citizens blame for failing to do so (cf. Gates and Justesen 2020). For the same reason, we do not theorize about differences in the extent to which state institutions might be supported or punished as a consequence of insurgent attacks, but leave discussions about possible differences in effect sizes for the empirical part. Third, note that the null hypothesis for both Hypothesis 1 and Hypothesis 2 is that the attack will have no discernible effects on trust in institutions. Such results would correspond to findings by Nussio (2020), who notes that when individuals anticipate terrorist attacks they might become desensitized and political attitudes might therefore not change in the aftermath of an attack. In a violent context as Afghanistan, the anticipation of violence might be a desensitizing factor contributing to a null effect.

### The Spozhmai Hotel Attack of 2012

The attack that serves as the focal point for our quasi-experimental approach occurred late in the evening of June 21, 2012, when Taliban forces carried out a widely publicized attack on the well-known Spozhmai Lakeside Resort.<sup>2</sup> Located approximately 10 miles from Kabul City at Lake Qargha, the hotel was popular with Afghan officials and businessmen and was a venue for family-weekend get-aways for Kabul's elite, as well as foreigners. The hotel—with some 300 guests at the time—was attacked during the night. At least fifty hostages were taken and fifteen civilians killed. At dawn on June 22, Afghan soldiers and police, supported by NATO troops and US helicopters, surrounded the hotel. By midday on June 22, the five attackers had been killed and the siege of the hotel ended. The Taliban later claimed responsibility, stating that the attack had been carried out because the resort was “usually used for immoral and un-

ethical purposes both for the foreigners and their puppet colleagues” (Zahori and Sukhanyar 2012).

Apart from the fact that the attack occurred while a nationwide public opinion survey was being conducted, it features two additional characteristics that make it suitable for our research. First, the attack was widely publicized both nationally and internationally. This high level of publicity means that the impact of the attack on public opinion—if any—should be measurable in contrast to less well-known attacks in rural areas of the country (see the online appendix for a more in-depth description of news coverage both within, and outside of Afghanistan). Second, while large-scale attacks of this kind have now become a common feature of warfare in Afghanistan, by 2012 the attack was one of the largest and first of its kind in terms of the number of civilians killed and taken hostage in a direct attack on a ‘soft target’ in Kabul City.<sup>3</sup> To a higher degree than when the security forces are killed in the line of duty, attacks on such soft targets are likely to engender public reaction—be it in the form of outrage or support toward state institutions.

## Empirical Strategy, Data, and Measurement

### Causal Identification

Our causal identification strategy is similar to those employed in other studies on the consequences of violent attacks for political outcomes (e.g., Wollebaek et al. 2012; Dinesen and Jaeger 2013; Balcells and Torrats-Espinosa 2018; Gates and Justesen 2020; Muñoz, Falcó-Gimeno, and Hernández 2020; Nussio 2020): we make use of the coincidental co-occurrence of a large-scale attack with a public opinion survey. In our case, the Spozhmai Hotel attack occurred while survey interviewing for Afghanistan's leading public opinion survey, the Survey of the Afghan People (SoAP, Asia Foundation), was ongoing.<sup>4</sup> The SoAP is a nationally representative annual survey executed by the *Afghan Center for Socio-Economic and Opinion Research (ACSOR)*, a renowned research firm that is frequently contracted by national and international NGOs, as well as by government institutions. The survey gathers information on public opinion, including attitudes toward state institutions. Survey interviewing is conducted during the summer months and, in 2012, interviews in Kabul City were conducted between June 17 and 28. A more detailed description of the SoAP's general sampling procedure is provided in the online appendix. Our design exploits the fact that the Spozhmai Hotel attack takes place during this period of interviewing, therefore creating an exogenous source of variation that allows us to compare trust in state institutions between a group surveyed before the attack and a group surveyed after the attack. We will refer to these two groups as “control group” and “treatment group” throughout this study to facilitate communication. We thus assume that the attack divides survey respondents in an as-if random manner and consequently produces two groups of comparable individuals in which any difference in the outcome should be attributable to the attack (Dunning 2012; Muñoz, Falcó-Gimeno and Hernández 2020).

<sup>3</sup>Authors' own assessment of violent events in Afghanistan, based on the Uppsala Conflict Data Program's Georeferenced Dataset (UCDP GED) (Sundberg and Melander 2013)

<sup>4</sup>Studying several attacks in Kabul with the same methodological setup would have been preferable to gain more statistical power as well as possibilities for generalization. Perusing the data did not, however, reveal any other attacks in Kabul of a similar type and magnitude during other survey periods.

<sup>2</sup>The following description builds on Zahori and Sukhanyar (2012).

Our sample includes a total of 719 respondents within Kabul City ( $N_{treatment} = 401$ ,  $N_{control} = 318$ ). We exclude thirty-five individuals due to missing values on either the dependent variables ( $N = 31$ ), or on any of the pre-treatment characteristics ( $N = 13$ ). We show in the section on threats to causal inference that this list-wise deletion procedure is unlikely to introduce bias. For both theoretical and methodological reasons, we deliberately study residents of Kabul City only, and do not include residents from the rest of Afghanistan. From a theoretical perspective the focus on Kabul City makes sense. The Spozhmai Hotel is located at the outskirts of the City and frequently visited by Kabul's financial and political elite. As such, in terms of geographic and sociocultural proximity, it is closest to individuals residing in Kabul City and an effect of the attack likely to be observable among a sample of Kabul City residents. More importantly, the attack does not function as a source of exogenous variation outside of Kabul City. Interview periods for the rest of Afghanistan differ from district to district, introducing stark imbalances on pre-treatment characteristics between treatment and control group. As such, the ignorability assumption is violated in the sample including respondents from outside of Kabul City. As we will show below, threats to causal inference are much less of a concern with regard to Kabul City.

#### Treatment Assignment

To assess the effect of the attack on institutional trust, we construct a dichotomous independent variable indicating whether an individual has been interviewed before the attack or afterward. Individuals are assigned a value of 0 if they have been interviewed between June 17 and 21, and a value of 1 if they have been interviewed between June 22 and 28 (see figure 1). That is, we cover the whole period of survey interviewing for Kabul City. Since the attack happened late at night on June 21, we include individuals interviewed on this day in the control group, as it is extremely unlikely that interviews were conducted so late in the evening that individuals in the control group received the treatment. In turn, it is unlikely that individuals interviewed on June 22 ( $N = 81$ ) did not receive the treatment, as 83 percent stated they had access to a radio and therefore to news. Even if individuals were interviewed early in the morning on June 22, the chances are high that news reporting of the attack had already started in the evening and continued throughout the night. Robustness tests in table A.15 in the online appendix further show that excluding individuals interviewed on June 22 does not affect the results. We are therefore confident that this division into treatment and control group reflects the actual treatment assignment.<sup>5</sup>

To ensure that the treatment and control group are comparable, we conduct a set of balance tests. For instance, it could be that poor neighborhoods are surveyed before the attack and affluent ones after. If these characteristics correlate with trust our results might be biased. One observable implication of such a situation is that interviews conducted before the attack should cluster in certain areas of Kabul City, while interviews conducted after the attack should cluster in other areas. Figure 2 shows the distribution of interviews conducted before the attack (light grey) and after the attack (dark grey) across Kabul subdistricts, the smallest unit to which we can disaggregate the interview locations. While the distribution of interviews across these

subdistricts is not identical, no systematic pattern emerges that would indicate bias due to roll-out method.

We corroborate this visualization with a more stringent test by regressing the treatment score of the respondents as the dependent variable (0 for individuals interviewed before the attack, 1 for after) on a set of six socio-economic and demographic variables that could be confounders. Any statistically significant effect of these indicators on the treatment score should be taken as evidence that the treatment and control group are unbalanced concerning this characteristic (Gerber and Green 2012). We use the variables *age* (continuous), *sex* (dichotomous), *income* (categorical), *education* (categorical) and *ethnicity* (dichotomous, whether or not respondent is Pashtun). We also include *radio ownership* (dichotomous) to assess whether the likelihood of having heard about the attack is balanced across groups, or in other words that individuals interviewed after the attack have received the treatment. Figure 3 shows the results of the balance tests in a coefficient plot.<sup>6</sup>

We use logistic regression models and report coefficients in log-odds. In line with conventional statistical significance levels ( $p < .05$ ), the only characteristic on which the sample is not balanced is *sex*. Given the imbalance on this variable, we cannot compare the means of our dependent variables between control and treatment group, but rely on multivariate regression as our main method. We further provide results using a matched sample as an alternative approach to reduce imbalances in table A.8 in the online appendix.

#### Dependent Variables

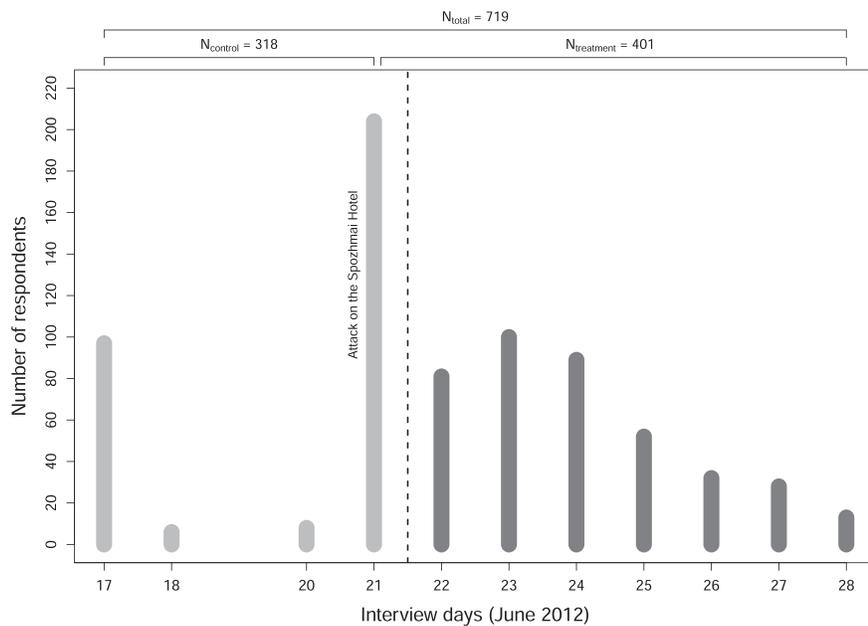
Our dependent variables are individual-level trust in three major state institutions: the local government (provincial government), parliament (National Assembly) and the police (Afghan National Police, ANP).<sup>7</sup> While there is no agreed upon definition of trust, most conceptualizations include reference to one (or both) of the following aspects. First, that trust refers to "belief that others will not deliberately or knowingly do us harm, if they can avoid it, and look after our interest, if this is possible" (Newton 2007, 343). Second, that it entails an evaluation "of whether or not political authorities and institutions are performing in accordance with normative expectations held by the public" (Miller and Listhaug 1990, 358).

Since we focus on trust in state institutions, we are concerned with what is commonly labeled 'institutional', 'political', or 'vertical trust' (e.g., Newton 2007). This differs from what is referred to as 'social', 'interpersonal', or 'horizontal' trust in that it describes the extent to which individuals place trust in formal institutions of the state rather than in other individuals or groups (Mishler and Rose 2001; Newton 2007).

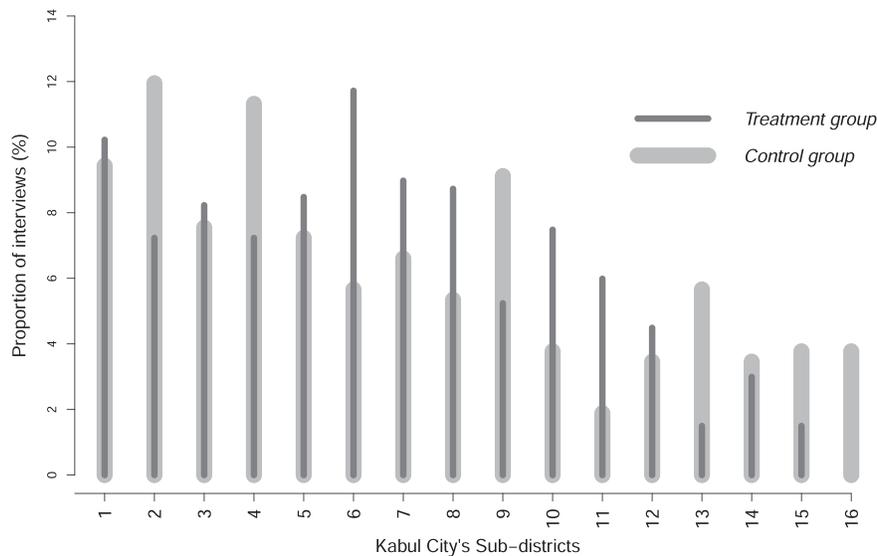
<sup>6</sup>Balance tests using difference-in-means and chi-squared tests yield identical results (table A.7 in the online appendix).

<sup>7</sup>We would also have liked to assess the impact on trust in the president, national government, and armed forces to cover the most relevant state institutions. There are no survey items on the president, and for the national government only items on government effectiveness/performance are available. Given that we cannot directly compare this item to the items about trust/confidence for the other state institutions, we do not include it in our analysis. We do not include trust in the armed forces, as this variable is skewed to the extent that analyzing it would provide no meaningful information. Survey respondents report on average extremely high levels of trust, and there is little space for movement on this variable. The effect of the attack on trust in the armed forces, as well as on perceptions of the national government's performance, does not reach conventional levels of statistical significance.

<sup>5</sup>According to data from the Uppsala Conflict Data Program, no other attacks on civilians took place in Kabul City or Kabul province during the survey period.



**Figure 1.** Treatment and control group for the Kabul City sample. The graph shows the number of respondents interviewed per day. The light grey bars indicate interviews conducted before the attack and the dark grey bars interviews conducted after the attack. The dashed black line indicates the timing of the attack (late evening on June 21). Respondents interviewed on June 21, are included in the control group. No interviews were conducted on June 19. The total number of respondents for control and treatment group, as well as for the pooled sample, is stated in the upper margins of the figure.

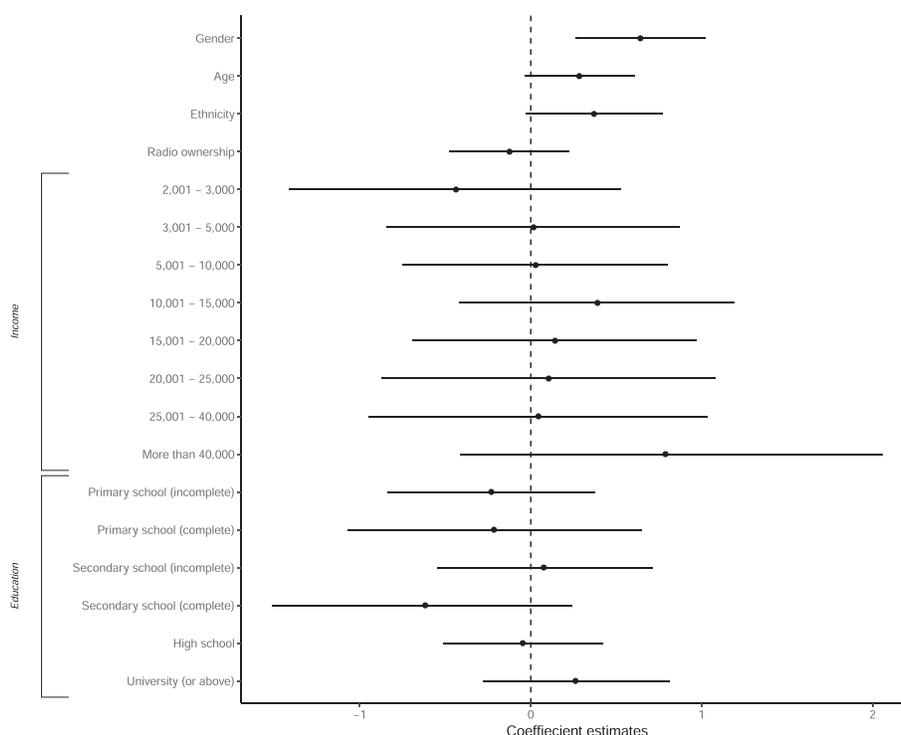


**Figure 2.** Distribution of interviews by treatment and control group and sub-districts. The figure shows the distribution of interviews across Kabul City's sub-districts for the whole interview period by treatment (gray thick bar) and control group (black thin bar).

Having a broader set of what are arguably some of the most important state institutions rather than focusing on a particular one is relevant to ensure that our study is comparable to previous work that commonly focuses on a range of outcomes. For each state institution, we construct a separate dependent variable based on survey items from the 2012 wave of the SoAP. Each of these variables are ordinal categorical variables in the form of a 4-point Likert scale, with answer categories “no trust at all,” “not very much

trust,” “a fair amount of trust,” and “a great deal of trust.”<sup>8</sup> Table A.1 in the online appendix gives a detailed overview of the survey items used for each state institution, as well as our recoding. While the wording of the survey items differs slightly across institutions—asking about *confidence* in the local government and parliament and about *trust*

<sup>8</sup>We reverse code the original scale of the items. We also provide results when using an additive index instead of separate dependent variables (table A.5 in the online appendix).



**Figure 3.** Balance tests. Coefficient plot for pre-treatment characteristics (dependent variable is treatment, i.e., whether individuals were exposed to the attack or not). Displayed are 95 percent confidence intervals.

in the police—these items can be seen as measuring the same underlying concept. Trust and confidence are theoretical concepts that are commonly used interchangeably with regard to individual-level attitudes toward political institutions (cf. Miller and Listhaug 1990; Levi and Stoker 2000), and institutional trust is often measured with survey questions asking about confidence (e.g., Newton 2007; Newton and Zmerli 2011). In addition, these survey items are otherwise worded identically, further reducing concerns about concept mismatch: “I would like to ask you about some officials, institutions and organizations in our country. I will read these out to you. As I read out each, please tell me how much *confidence/trust* you have in each of the institutions and organizations and officials to perform their jobs.” We therefore refer to our dependent variables using the overarching concept “trust.” Table A.2 in the online appendix provides descriptive statistics for all three dependent variables, as well as any other variables used in the main analysis.

## Results

To evaluate our two rivaling hypotheses, we regress our dependent variables (trust in state institutions) on our independent variable (interviewed before/after the attack), while controlling for a set of pre-treatment characteristics to increase the precision of our estimates and account for imbalances (Gerber and Green 2012). These are the same that we used for the balance test, i.e., age, sex, income, education, ethnicity, and radio ownership. Due to the ordinal nature of our dependent variables, we use ordered logistic regressions. Table 1 displays the results with coefficients reported in log-odds. We first provide a bivariate regression model for each dependent variable that only includes our treatment and outcome variable (M1, M3, M5). We then

provide for each dependent variable the co-variate-adjusted model (M2, M4, M6). Across all these models, the coefficients for all three state institutions are positive and reach conventional statistical significance levels,<sup>9</sup> indicating that individuals interviewed after the attack (treatment group) are more likely to report that they have trust in these particular institutions compared to those individuals interviewed before the attack (control group).

These effects are quite considerable with regard to their substantial impact. Figure 4 shows the effects of exposure to the attack on the predicted probability of reporting the four levels of trust that form our outcome measure: no trust at all, not very much trust, a fair amount of trust, and a great deal of trust. The predicted probabilities are based on the covariate-adjusted models. For both the provincial government and the parliament, the predicted probability to report “a fair amount” or “a great deal of trust” is higher for individuals exposed to the attack than for those not exposed. For instance, the predicted probability to report the highest level of trust in the provincial government increases for individuals that have experienced the attack by 31.65 percent from 4.96 percent to 6.53 percent (1.57 percentage points). Vice versa, the predicted probability to report “not very much” or “no trust at all” is lower for individuals that were exposed to the attack than for those not being exposed. For the lowest outcome category, i.e. “no trust at all” it decreases by 22.16 percent from 18.28 percent to 14.32 percent (3.96 percentage points).

For the parliament, these differences are even more pronounced with a 65.09 percent increase to report the

<sup>9</sup>Because we test the same hypothesis on three outcomes, we provide *p*-values adjusted for multiple comparison using several methods in table A.6 in the online appendix. *p*-values remain below .05 for the parliament, independent of the correction used. For the provincial government and police there is more variation across correction methods, with some below and some above a *p*-value of .05

**Table 1.** The effect of being exposed to the attack on institutional trust

	<i>Local government</i>		<i>Parliament</i>		<i>Police</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.319* (0.141)	0.292* (0.144)	0.464*** (0.137)	0.550*** (0.141)	0.397** (0.144)	0.320* (0.150)
Gender		-0.322 (0.177)		-0.798*** (0.175)		0.617*** (0.185)
Age		0.013* (0.005)		0.004 (0.005)		-0.009 (0.005)
Radio ownership		0.152 (0.163)		0.176 (0.160)		-0.091 (0.170)
Ethnicity		0.448* (0.188)		0.025 (0.181)		0.327 (0.194)
Cut 1	-1.656*** (0.130)	-1.298** (0.448)	-1.005*** (0.113)	-1.122* (0.438)	-3.253*** (0.228)	-5.024*** (0.571)
Cut 2	-0.099 (0.107)	0.310 (0.444)	0.272** (0.104)	0.212 (0.437)	-1.776*** (0.135)	-3.510*** (0.539)
Cut 3	2.658*** (0.163)	3.151*** (0.466)	2.334*** (0.144)	2.344*** (0.447)	0.287** (0.110)	-1.310* (0.526)
Observations	719	719	719	719	719	719
AIC	1703.467	1705.663	1874.119	1865.723	1498.899	1472.987
BIC	1721.779	1806.376	1892.430	1966.436	1517.211	1573.700

Standard errors in parentheses; \*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$ .

Note: Ordered logistic regressions with coefficients reported in ordered log-odds.

Categorical control variables with more than two categories (income, education) are omitted, and coefficients are reported in table A.3 in the online appendix. Reference categories for binary variables are: gender=men, radio ownership=yes, ethnicity=other than Pashtun.

highest level of trust (from 6.76 percent to 11.16 percent, 4.4 percentage points), and a 33.87 percent decrease to report the lowest level of trust (from 30.09 percent to 19 percent, 10.19 percentage points). The differences in predicted probabilities between treatment and control group reach statistical significance levels across all outcome categories for the parliament (i.e.,  $p < .05$ ). While the differences border statistical significance at the 95 percent confidence level for the lowest (no trust at all) and highest outcome category (a great deal of trust) for the provincial government, the overall picture is in line with hypothesis 1: individuals that have experienced the attack are more likely to report higher levels of trust.

For the police, the picture is similar, with the difference that exposure to the attack has only a positive effect on the predicted probability to report very high levels of trust: it increases by 12.61 percent from 59.15 percent to 66.61 percent (7.46 percentage points). For any lower level of trust, individuals in the treatment group are less likely to report it. Apart from the lowest trust level, these differences reach conventional statistical significance levels, or are just above. Overall, these substantial effects are in line with the hypothesis that exposure to the attack increases trust in state institutions.

As Brant tests indicate that the parallel regression/proportional odds assumption underlying ordered logistic regression models is violated for Model 2 (parliament) and Model 3 (police), we further replicate these but relax the assumption for those variables that display diverging effects on the categories of our ordinal variable (table A.4 in the online appendix). We thus run partial proportional odds models that allow the coefficient of treatment to vary for Model 4 (parliament), while we allow the coefficient of gender to vary for Model 6 (Police). The

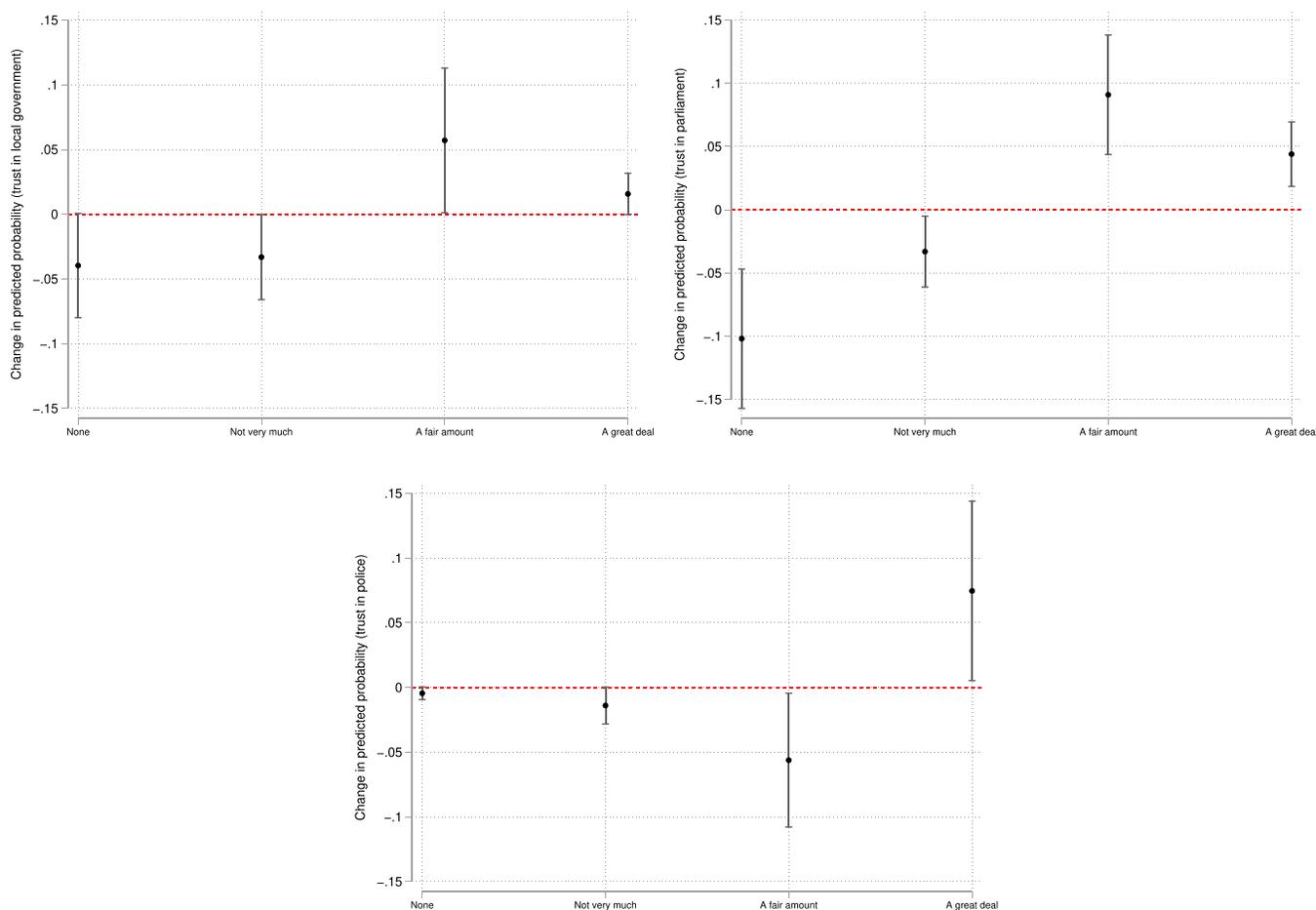
results are in line with the ordered logistic models, meaning that the effect of being exposed to the attack is overall statistically significant and positive.

*Threats to Causal Inference*

Two key assumptions need to hold for the above identified effects to be unbiased estimates: ignorability and excludability. We follow the best practices outlined in Muñoz, Falcó-Gimeno, and Hernández (2020) to assess violations of these assumptions. These tests are described in detail in the online appendix.

The *ignorability* assumption implies that the attack needs to be exogenous to any characteristics of the survey respondents that might affect both the independent (i.e., treatment assignment) and dependent variable (institutional trust), meaning that it needs to be as-if random (see also Dunning 2012). There are several threats to this assumption that are relevant to consider for our study.

First, there is the possibility that individuals self-select into the treatment and control group. Yet, this seems highly implausible as it would entail a high proportion of respondents having the capacity and/or information to elect when they would be surveyed prior to the occurrence of the attack (Dunning 2012). This seems unlikely for two reasons: First, it is hard to imagine that a large number of individuals had the *information* needed to predict the exact timing of the attack. Second, even if individuals had such information, it is unlikely that they would have the *capacity* to select into treatment or control group. The SoAP’s survey sampling procedure is based on a multi-stage random stratification process, whereby respondent selection in the final stages is based on (1) a random walk through a randomly selected neighborhood, so-called “nahias”, and



**Figure 4.** The change in predicted probability for each outcome category of trust in state institutions due to exposure to the attack, with 95 percent confidence intervals. Continuous control variables are set to their mean, while categorical control variables are set to their mode value. The estimations are based on the covariate-adjusted models (M2, M4, M6) from table 1.

(2) on random selection of household participants using Kish grids (Tariq, Haqbeen, and Kakar 2012). Individuals thus have no influence over interview timing.

Second, there is the possibility that the attack shapes the willingness of individuals to take part in the survey and/or to respond to questions about institutional trust. Survey questions on trust in state institutions measure political preferences. It is possible that individuals interviewed in the aftermath of the attack are less likely to reveal these because of a heightened awareness of the possibility of retaliation (García-Ponce and Pasquale 2015). To assess whether this introduces bias, we check whether item non-responses for the dependent variables are balanced across control and treatment group. If the attack did lead to a lower willingness to respond, we would expect to detect more item non-responses in the treatment group vis-a-vis the control group. To test this we calculate balance measures in table A.9 in the online appendix and find that there is no worrisome imbalance. Thus, self-censorship appears unlikely to be biasing our results.<sup>10</sup> We also tested whether there is a systematic difference in the number of attempts to contact a selected respondent between the treatment and control

<sup>10</sup>Note that item non-response refers to “don’t know” answers. No respondent opted to refuse to answer any of the survey items measuring our dependent variables. We also wanted to test for outright refusals to partake in the survey, i.e., unit non-response. However, such data were, according to our contact at the Asia Foundation, not collected in 2012.

group, which could indicate hesitation to participate. All respondents in our sample participated in the survey at the first attempt, alleviating concerns related to systematic differences in reachability (Muñoz, Falcó-Gimeno, and Hernández 2020). While figure 1 may raise suspicions that a change in the distribution of respondents before and after the attack could be due to changes in sampling in the sense that the attack increased incentives of enumerators to replace respondents instead of re-contacting them, we do not find any information in line with this. The SoAP has a protocol of two callbacks before replacing respondents, and while the SoAP acknowledges that this may not be feasible in rural areas out of security concerns (Tariq, Haqbeen, and Kakar 2012, 187), there is no indication that this is an issue for large cities such as Kabul. While we therefore cannot completely exclude that sampling procedures have changed as a consequence of the attack, we assume that this is unlikely given the information that we have.

Third, there is the possibility that the timing of the attack in relation to the survey roll-out method divides respondents into unbalanced groups. We provided evidence in the section on Treatment assignment that there is no reason for major concern.

Finally, there is the possibility that unobserved confounders that are not included in the balance tests drive our results. To this end, we run our analysis on a subsample including only individuals interviewed the day before and

after the attack. Detecting an effect in such a short temporal bandwidth would indicate that it is very unlikely that anything but the attack itself affects institutional trust. We can replicate our findings for the parliament and police. While the effect remains positive for the provincial government, it is estimated with less precision and does not reach conventional statistical significance levels.

Next, we assess violations of the *excludability* assumption, i.e., that any difference in institutional trust between individuals exposed and not exposed to the attack is only due to the attack itself. There are two threats to this assumption relevant for our study.

First, the possibility that increases in trust are due to a time trend that already exists prior to the attack. To this end, we conduct two tests. We include a time trend variable in the form of days since the beginning of the field work and interact it with the treatment to assess whether institutional trust is temporally stable in the control group. We find no evidence of a pre-existing time trend in these tests, alleviating concerns that the effect we observe is caused by a temporal dynamic unrelated to the attack. Second, we run our analysis using several different temporal bandwidths, i.e., subsamples of +/- 1 to 4 days around the attack. In both tests, we find some evidence for a substantially weak positive time trend set in motion after the attack for one of the three institutions. Trust in the provincial government seems to slightly increase as days pass after the attack. For this particular state institution, we can therefore not fully exclude that we are measuring a composite effect of the attack and dynamics set in motion by it.<sup>11</sup> Yet, because this time trend is substantially weak, we do not believe that it is much of a concern.

Second, there is the possibility that the observed effect is caused by an annual timetrend. Therefore, we conduct a placebo test where we test for a similar effect as the one identified above where none should logically occur (Dunning 2012; Muñoz, Falcó-Gimeno, and Hernández 2020). Running such tests for two previous survey waves (2010 and 2011) and using the same strategy as in our main analysis, we find no statistically significant effects of our placebo treatment (tables A.12 and A.13 in the online appendix).

Third, we conduct a placebo test using the control group only, and dividing it into a placebo treatment and control group. We find no worrisome systematic differences between these groups that would indicate a pre-existing time trend in regard to institutional trust (table A.14 in the online appendix).

In sum, although some of our effects are less precisely estimated, we deem our tests of the ignorability and excludability assumptions to identify no causes for great concern regarding causal identification.

*Probing Theory: Observable Implications*

Our analysis provides support for hypothesis 1—individuals interviewed after the attack were more likely to report trust in state institutions. Although results are in line with the expectations of the “rally ’round the flag” hypothesis, it is also possible that they are produced by other processes with similar or identical empirical manifestations. We consequently deduce and test three observable implications that either speak for or against the theoretical underpinnings of hypothesis 1.

We begin by investigating the possibility that our results stem not from rallying around the flag, but from preference falsification, i.e., untruthfully portraying oneself as support-

**Table 2.** Observable implications of mechanisms

	<i>Preference falsification</i> <i>Ordered logit</i> (1)	<i>Threat perception</i> <i>Ordered logit</i> (2)	<i>Authoritarian views</i> <i>Logit</i> (3)
Treatment	0.079 (0.182)	-0.021 (0.141)	0.756*** (0.162)
Gender	0.383 (0.222)	0.360* (0.173)	-0.617** (0.197)
Age	-0.000 (0.006)	-0.003 (0.005)	0.001 (0.006)
Radio ownership	-0.092 (0.206)	-0.015 (0.159)	-0.040 (0.180)
Ethnicity	0.016 (0.231)	-0.356* (0.179)	0.269 (0.206)
Constant			-0.132 (0.489)
Cut 1	-1.209* (0.551)	-2.276*** (0.457)	
Cut 2	-0.820 (0.550)	-1.384** (0.453)	
Cut 3		0.396 (0.449)	
Observations	682	717	709
AIC	989.215	1910.660	971.203
BIC	1084.241	2011.312	1062.481

Notes: Standard errors in parentheses; \*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$ . Ordered logistic regression models assume proportional odds. Coefficients in log-odds.

ive of the government. Hiding one’s true opinions due to fears of repercussions is a known problem in countries with repressive governments when sensitive questions are asked about government support (Kuran 1997; García-Ponce and Pasquale 2015). The attack might increase citizens’ awareness of a state’s intention to crack down on insurgent supporters and therefore decrease their willingness to report anti-government sentiment. Since we rely on available survey data and could not make use of data collection methods to handle preference falsification, we assess this post-hoc. We analyze the effect of our treatment on another potentially sensitive question, but one that should not be directly affected by the attack: “If you think about your family, would you say that today your family is more prosperous, less prosperous, or about as prosperous as under the Taliban government?” The question is sensitive as stating that having been more prosperous under Taliban rule can be seen as siding with that party. Seeing how it was the Taliban who conducted the attack under study, some might wish to hide sympathies for the group by presenting oneself as pro-government. Past living standards cannot, however, be shaped by exposure to the attack. If we find that individuals in the treatment group were more likely to report that their living standards are better today than compared to Taliban rule, this could signify that respondents in the treatment group are falsifying their preferences. Results using an ordered logistic regression do not, however, support preference falsification. The coefficient for our treatment does not reach conventional statistical significance levels, therefore indicating that individuals exposed to the attack do not systematically differ from individuals not exposed to the attack in their likelihood to report being more prosperous during Taliban rule (see Model 1, table 2).

Turning to our first theoretically derived implication, one individual-level explanation for rallying around the flag is

<sup>11</sup>See Muñoz and Anduiza (2019) for a similar argument.

that terror attacks increase peoples' perceptions of threat. In a psychological attempt to mitigate threat, individuals rally around those institutions and leaders that are familiar and which offer protection (Landau et al. 2004; Bonanno and Jost 2006; Canetti-Nisim et al. 2009). A possible observable implication is that individuals should have a heightened sense of fear for their safety in the attack's aftermath. To test this we compared answers before and after the attack on a survey question which reads: "How often do you fear for your personal safety or security of your family these days?", with available responses being "never," "rarely," "sometimes," or "often". The results did not support this implication, as the treatment variable has no statistically significant effect on levels of perceived threat (see Model 2, table 2).

A second related theoretical approach holds that rallying around incumbent leaders and institutions stems from events such as terror attacks sparking psychological shifts toward authoritarianism, leading people to support incumbent institutions as these seem to offer security and stability (Huddy et al. 2002; Bonanno and Jost 2006; Echebarria-Echabe and Fernández-Guede 2006). An observable implication of this is that individuals in the treatment groups should report more authoritarian attitudes than those in the control group. We test this assertion by using the following two statements: "A: Since the president was elected to lead the country, he should not be bound by laws or court decisions" and "B: The President must always obey the laws and the courts". We assume that agreeing with statement A over B captures a comparatively more authoritarian leaning. Results support the assertion on increased authoritarianism: respondents in the treatment group were significantly more likely to agree with statement A than were those in the control group, as is indicated by the positive and statistically significant coefficient of the treatment in Model 3, table 2.

### Discussion

In the previous section, we provided evidence that strengthened our confidence in a causal relationship between the attack and institutional trust, as well as this relationship being driven by a rally effect. We showed that preference falsification is unlikely to account for the observed relationship and demonstrated that—in line with the rally-effect hypothesis—individuals in the treatment group reported on average stronger authoritarian leanings than individuals in the control group. We were, however, not able to confirm the observable implication that individuals in the treatment group should report higher levels of threat. If we presume that the observed relationship is due to a rally effect, what might explain this discrepancy in the empirical results? A possible explanation lies in the type of threat perceptions that attacks such as the one we study trigger. For instance, Huddy et al. (2002) have demonstrated differences in attitude correlations dependent on whether a threat is perceived as personal or national. Though speculative, it might be that the attack in Kabul did not spark higher perceptions of personal threat, but instead higher perceptions of national threat, which are subsequently linked to rallying around the flag. In situations of ongoing armed conflict of the Afghan magnitude, it is not implausible that national or group-level perspectives attain a prominent psychological place. While we are not able to test this proposition empirically due to the absence of survey items capturing individual perceptions of national threats, we encourage future research to investigate the different components of the "rally 'round-the-flag" argument in more detail.

A second aspect that warrants discussion is the possibility of post-treatment contamination. Our design does not allow us to fully exclude that differences in institutional trust between treatment and control group are caused by reactions and processes triggered by the event, rather than the attack itself, so-called 'collateral events' (Muñoz, Falcó-Gimeno, and Hernández 2020). Although the empirical tests that we have conducted alleviate concerns about such collateral events being a major concern, we cannot completely exclude that it is not the response of the security forces, the government, or the media that produces an outcome in line with hypothesis 1. Baker and Oneal (2001), for instance, show how rally effects are contingent on the type of media coverage an event receives, how governments "spin" an event, and on bi-partisan unity. While collateral events is a relevant caveat, it is also a common challenge when relying on quasi-experimental designs (e.g., Legewie 2013; Muñoz and Anduiza 2019). It is, however, pertinent to keep in mind that the collateral events that would be the most likely to produce our findings would have been an outstanding performance on the part of the security forces or in President Karzai's government addressing the event. The few evaluations of security force performance that we have come across are, however, mixed at best. We could, additionally, not identify anything in reporting or in statements reminiscent of an extraordinary "spin" by either Karzai or the government at large.

Finally, to what extent do we believe our findings to be generalizable beyond Kabul City? On the one hand, the particularities of each attack, each country, and each civil war might translate into different outcomes across contexts. On the other hand, such empirical heterogeneities do not necessarily imply inconsistent or contradictory findings. Seeing to how the "rally around the flag" effect has been identified across several non-civil war contexts around the globe—thus accounting for empirical heterogeneities—and how we replicate it also in this setting of civil war, it seems reasonable to expect the effect to be generalizable. This expectation can be seen as being additionally strengthened when taking into account the universal nature of the proposed—and demonstrated—mechanism of an authoritarian shift. The scope of this mechanism is not restricted to specific contexts, but rather claims to capture a basic element of human psychology. Thus, in other civil war situations where civilians have at least some basic trust in their government and its institutions, attacks seem likely to cause a rallying around these institutions. Consequently, we would expect our identified effect to be generalizable to similar attacks in other civil war contexts.

### Conclusion

We began this article by asking whether large-scale insurgent attacks on civilians during ongoing civil wars indeed undermine trust in state institutions as commonly anticipated or whether they are able to boost support in incumbent institutions. Leveraging a quasi-experiment that allowed us to compare individuals interviewed before the attack to those interviewed after, we found support for a rally effect among a Kabul City sample. That is, we found statistically and substantially significant increases in trust in the local government, the national parliament, and the police force. Further probing the proposed "rally 'round-the-flag" argument with additional tests also identified some—though not all—derived observable implications of the theory's components.

Our study on the attitudinal effects of large-scale insurgent attacks on civilians in Kabul City contributes to

advancing our knowledge on the topic in at least three ways. First, we show that large-scale attacks on civilians have relatively strong effects on institutional trust, even during ongoing armed conflict. In other words, our study indicates that attacks that target civilians and cause extensive fatalities have attitudinal consequences even in circumstances in which violence is an everyday occurrence. We therefore add to previous work that has analyzed the effects of large-scale insurgent attacks on military targets during armed conflict (Gates and Justesen 2020), as well as work focusing on the impacts of attacks on civilians on preferences and social attitudes more generally (Huddy et al. 2002; Canetti-Nisim, Ariely, and Halperin 2008; Wollebaek et al. 2012; Canetti-Nisim et al. 2013; Dinesen and Jaeger 2013, e.g.).

Second, our findings indicate that rally effects might be triggered by a more diverse set of attacks than commonly anticipated. Our study illustrates that attacks by domestic actors can trigger very similar responses to attacks by external actors and that they appear to do so even in the context of an active civil war. Although we expect our results to be generalizable to other contexts of civil war, further assessment is necessary to assuage fears that our findings are context-specific. Future research should therefore attempt to replicate our findings in other settings of armed conflict, and, preferably, also attempt to single out what variables may mitigate or enhance possible rally and accountability effects. Related to this, we were not able to extensively test the causal mechanism linking the attack to increases in trust. While our test of observable implications of potential mechanisms highlights a shift in authoritarian attitudes as a possible link, it may be that different mechanisms account for increases in trust across institutions. That is, while the effect might be the same in terms of direction, the reason for why an attack triggers such an effect might differ. We therefore encourage future research to study whether rally effects might be produced by different mechanisms across state institutions.

Third, we contribute to the literature on the effects of internal armed conflict on institutional trust by showing how the short-term effects of a single violent shock might differ from long-term effects of cumulative violence. While previous research has studied how exposure to civil war violence more generally leads to a reduction in institutional trust (Hutchison and Hutchison 2011; Grosjean 2014; Voors and Bulte 2014; Wig and Tollefsen 2015; De Juan and Pierskalla 2016; Gates and Justesen 2020), our study indicates that single large-scale attacks on civilians might trigger short-term increases in trust and confidence in a range of state institutions. The findings of this study therefore show that insurgent attacks on civilians are not only violations of the laws of war. They also do not seem to produce the outcomes anticipated by insurgents. This is particularly relevant in the context of Afghanistan, which has in recent years seen a steady increase in insurgent-perpetrated violence in general and large-scale attacks on urban populations in particular.

### Supplementary Information

Supplementary information is available at the *International Studies Quarterly* data archive.

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