

The impact of terrorism on civil servants: Longitudinal evidence from the July 22, 2011 attack in Norway

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Abstract

Building on a growing literature assessing the societal impact of terrorism, this article analyzes whether and how a terror attack targeting public institutions affects civil servants in their day-to-day work. This is an important question to enhance our understanding of how terrorism can (or cannot) affect the operation of core government functions. Theoretically, the study contributes to a broader account of the political consequences of terrorism by combining insights from social identity and organization theory. Empirically, we exploit a two-wave survey fielded before and after the 2011 terror attack in Norway, which allows us to study the *same* civil servants ($N = 186$) before and after this event. While terrorists wish to disrupt public institutions, our findings indicate that a terror attack targeting core government institutions strengthens internal cohesion and increases attention to political signals in work tasks. We discuss implications of these effects for the functioning of democratic government.

Practitioner points/Evidence for practice

- A terror attack directed against the government apparatus can have important implications for the civil servants located at the epicenter of the attack.
- Civil servants' sense of belonging to their organization and a tightening of political authority are two mechanisms through which terrorism might impact public governance.
- Political leadership may gain influence over civil servants' decision-making in the aftermath of an external threat.
- This contraction of public authority can potentially reduce civil servants' attention to bureaucratic leaders and professional principles in their day-to-day work.

INTRODUCTION

How – if at all – does terrorism targeted at public organizations affect the operation of core government functions? This is an important question from a public administration and democratic politics perspective since public organizations and the staff employed within them play a fundamental role in the governing of modern societies (Huber, 2000; Orren & Skowronek, 2017; Putnam, 1973). As such, it is critical to understand whether and how “acute extra-organizational stressors” (Byron & Peterson, 2002, p. 895) such as terrorist attacks on public organizations affect civil servants during the

execution of their work. Building on a growing literature assessing the societal impact of terrorism (Berrebi & Klor, 2008; Breton & Eady, 2022; Huddy et al., 2002; Huddy et al., 2005), this study combines unique individual-level longitudinal data with a quasi-experimental empirical approach to address this question.

There is no single definition of terrorism that commands full international approval (Schmid, 2021). Extant definitions include objective as well as subjective elements (Shanahan, 2016) and cover legal, moral, and behavioral perspectives (Ruby, 2002). In this article, we study the responses of the victims of acts of terrorism, such that a behavioral-perceptual understanding of

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terrorism is most suitable. Thus, terrorism is understood as a strategically indiscriminate act “harming members of a target group in order to influence [their] psychological states (...) in ways that perpetrators anticipate may be beneficial to the advancement of their agenda” (Shanahan, 2016, p. 110). This understanding of terrorism includes the “deliberate use of violence and intimidation directed at a large audience to coerce a community (government) into conceding politically or ideologically motivated demands” (Krieger & Meierrieks, 2011, p. 4), which was the case for the event in our analysis.

Extant research shows that terror attacks may spur reforms in the government apparatus, disrupt public service provision and interrupt the short-term operation of governmental functions (Kettl, 2004; May et al., 2008). A 2002 Symposium Issue of *Public Administration Review* dedicated to the 9/11 attacks, for instance, extensively discussed how these events invoked a reassessment and restructuring of US administrative systems (Ricucci, 2002; Sloan, 2002; Terry & Stivers, 2002). Similarly, considerable work studies the impact on, and implications for, public organizations as the target of terrorist attacks: e.g., IRA bombings of police stations and army barracks, lone wolf school and hospital shootings, or attacks on government buildings including embassies (Lewis 2002; Sandler & Enders, 2004; Fischbacher-Smith & Fischbacher-Smith, 2013). Yet, these studies rarely focus on the impact of terror attacks on individual civil servants.¹ This is an important knowledge gap since any impact at the level of individual civil servants may constitute indirect effects of terrorism on core government functions. Civil servants are, after all, “responsible for the vast majority of policy initiatives taken by governments” (Putnam, 1973, p. 257) and they “work to formulate and implement public policy” (Huber, 2000, p. 397). Terrorism thus may not only coerce governments into granting policy concessions by affecting voters’ preferences (Berrebi & Klor, 2008; Lewis, 2000), but could be achieving the same goal through its impact on *civil servants*. The main contribution of our analysis lies in evaluating this alternative route toward the highly disputed (in)effectiveness of terrorism (Abrahms, 2012; Pape, 2003), and thus further develops our understanding of the democratic implications of terror events.

Our empirical analysis thereby investigates how the large-scale terror attack on the central government complex in Oslo on July 22, 2011 affected the civil servants who were directly under attack. Reminiscent of the 1995 bombing of the Alfred P. Murrah Federal Building in Oklahoma City, the 22 July attack involved a massive car bomb that killed eight people, injured dozens, and damaged the buildings of seven ministries to such an extent that they were forced to relocate permanently (Norwegian Government, 2012). We analyze this event by combining two waves of a survey conducted among staff of all Norwegian ministries in 2006 and 2016. A critical feature of our dataset is that it allows a comparison of the

same staff members before and after the terror attack ($N = 186$), and study *within-individual* changes over time for staff at ministries directly affected by the attack relative to staff at less proximate ministries.

The next section outlines our theoretical framework and derives testable propositions. The subsequent section presents our empirical strategy and data. The final parts of the paper outline the empirical results, robustness and validity checks, and a concluding discussion.

THEORETICAL FRAMEWORK AND EXPECTATIONS

A terror attack directed at a workplace can involve anything from loss of lives and the destruction of offices to a need for organizational restructuring and physical relocation. In addition to causing material damage, such events may also have a major psychological dimension as they are devised to create fear and anxiety (Krieger & Meierrieks, 2011; Shanahan, 2016). As such, workplace terror events have at least the potential to (re)shape a wide range of staff attitudes within organizations. In the remainder of this section, we develop propositions regarding such effects along two dimensions.

Before doing so, it is important to clarify two key points. First, workplace terror events need not originate from outside the organization. Research in crisis management, for instance, shows that workplace violence and insider threats from employees can also constitute acts of terrorism and represent a growing problem in some areas of public service delivery (BaMaung et al., 2018; Caputo, 2021; White, 2021). Our analysis, however, focuses on “lone actor terrorism” from outside the organization (Gill, 2021), which likewise poses a serious collective threat to the organization and its staff (Birkeland et al., 2017; Byron & Peterson, 2002). Second, one must distinguish between *exposure* and *proximity* to a terror attack. In many cases, most people will be *exposed* to the occurrence of a terror event (e.g., via news or social media). Yet, *proximity* to such events – in a geographic (i.e. close in space) or personal sense (e.g., being or knowing a victim) – may affect how the event is experienced. Proximity implies that people receive direct auditory, visual, or even olfactory information, which is unfiltered by media or other intermediaries. Psychological research maintains that such personal, vivid information is particularly meaningful to individuals (Agerberg & Sohlberg, 2021). As a result, proximity to dramatic events is expected to lead to stronger effects than ‘mere’ exposure (Agerberg & Sohlberg, 2021; Gartner et al., 1997; Newman & Hartman, 2019).² This distinction is central to our empirical strategy since all our respondents were *exposed* to the event under analysis, but only a subset was in close *proximity* to the event location.

Terror and in-group sense of belonging

A long-standing and widely held proposition across the social sciences is that external threats can strengthen in-group bonding and cohesion (Bastian et al., 2014; Hogg & Terry, 2000; Stein, 1976). One reason is that threats from the outside activate us-versus-them thinking, and reinforce the salience and perceived value of (being part of) the in-group (Hogg & Terry, 2000). In an excellent review of early research dealing with this proposition in sociology, psychology, anthropology and political science, Stein (1976, p. 165) concludes that there exists a “clear convergence (...) that external conflict does increase internal cohesion.” Subsequent social-psychological research has by and large confirmed these early conclusions across diverse contexts and empirical approaches, while also providing further clarification with respect to the underlying processes and mechanisms (Bastian et al., 2014; Greenaway & Cruwys, 2019; Summers et al., 1988).

This line of argument suggests that terror attacks targeting public organizations can strengthen affected civil servants’ bond with their immediate colleagues and work organization. Further to specify this general prediction, however, we need to account for the exact nature of the threat. As argued by Gibler et al. (2012, p.1658), “the social context provided by external stimuli determines which group identities are relevant to the individual.” In our empirical setting, we study a politically motivated attack targeting the governing institutions of a country. Hence, we expect that this event influences how affected civil servants perceive their relation to the institutions under attack (of which they are an inherent part): i.e. their ministry as well as the civil service as a whole.

From a job embeddedness perspective, any such strengthened in-group bond is likely to have important side effects in terms of individuals’ intention to remain with their organization. The reason is that “the extent to which people have links with other people and activities” constitutes one of three critical factors determining turnover intentions and decisions (Mitchell et al., 2001, p. 1104). More specifically, as argued by Moynihan & Pandey (2008, p. 206), “employees who perceive that they have a high level of support from their fellow coworkers (...) are less likely to express an intention to quit.” Any terrorism-induced boost in individuals’ sense of in-group belonging thus may create a type of “social glue” (Bastian et al., 2014) that works to “constrain people from leaving their current employment” (Mitchell et al., 2001, p. 1115). As such, it may create ties that bind employees to their organization in spite of a major negative shock (Moynihan & Pandey, 2008). This leads to our first hypothesis:

H1a. Terror attacks targeting government ministries strengthen civil servants’ sense of belonging to their ministry and the civil

service as a whole, and reduce turnover intentions.

A direct corollary to H1a is that this increasing sense of belonging to one’s ministry or the civil service as a whole may invoke a relative reduction in the same feeling toward one’s *other* social identities. Social-psychological research indeed maintains that people hold multiple (social) identities in a hierarchy of salience (Ashforth & Johnson, 2001; Stryker, 1968). Moreover, these distinct identities “tend to be cognitively segmented and buffered, suggesting that individuals are capable of invoking only one identity at a time” (Ashforth & Johnson, 2001, p. 46). Hence, when an external threat heightens the relevance of specific group identities – in our case, ministries and the civil service – it leads other group identities to fall down the salience hierarchy (Stryker, 1968). This would suggest that other group identities relevant to civil servants – such as their profession or organizational unit – may decline in the aftermath of a terror event targeting the governing institutions of a country. We therefore expect:

H1b. Terror attacks targeting government ministries undermine civil servants’ sense of belonging to group identities outside the nature of the threat (e.g., their organizational unit or profession).³

Terror and work routines

The organizational theory approach to public administration maintains that organizational structures create systematic bias in human behavior and decision-making processes by directing the attention of staff members toward certain problems and solutions (Egeberg & Trondal, 2018; May et al., 2008; Olsen, 2010). This is important since organizations, when confronted with a threat, often rely on increased formalization and centralization to reduce uncertainty and ambiguity. For instance, Stoker et al. (2019) show that “directive leadership” – which relies on “structuring subordinates’ work through providing clear directions and expectations regarding compliance with instructions” (Lorinkova et al., 2013, p. 573) – became more prevalent after the 2008 financial crisis. Similarly, public administration and management scholarship has argued – and empirically demonstrated – that the decisions and actions of leaders play a critical role when guiding organizations through turbulent times (Boin & ‘t Hart, 2003; Selznick, 1957).

To the extent that threatening events induce a tightening of authority and control, we expect that government employees pay increased attention to guidelines and directives from their political and/or administrative leadership. One reason is that organizational members display a heightened tendency to look for guidance from

leaders during crises, because “they expect them to be prepared to take charge” (Birkeland et al., 2017, p. 660; Byron & Peterson, 2002; Boin & ‘t Hart, 2003). Moreover, the organizational theory approach to public administration highlights that this search for guidance is underpinned by organizational capacities for executive steering, which would direct public employees’ attention toward signals from political and/or administrative leaders. Consistent with this argument, May et al. (2008, p. 528) observe that federal agency preparedness agendas became more attentive to presidential and congressional signals “following the 1995 bombing of the Oklahoma City federal building.” As an individual-level analog to such agency-level effects, we hypothesize that civil servants directly exposed to a terror attack become more attentive to the signals of their leaders – possibly at the cost of reduced attention to other signals (such as their professional principles):

H2. Terror attacks targeting public institutions are associated with increased attention to signals from political (and administrative) leaders.

METHOD AND DATA

Research context

Two politically motivated terror attacks struck Norway on July 22, 2011. First, a massive car bomb was detonated in the center of Oslo outside the main building of the government complex housing several ministries including the Prime Minister’s Office and the Ministry of Justice. Several other buildings in the government quarter were likewise damaged beyond repair (see Appendix Table B.1). The bomb killed eight people and injured scores more. Second, a few hours after the car bomb was detonated, 69 people attending the Labour Party’s youth organization camp on Utøya Island were shot and killed by the same perpetrator; 33 more were injured. Both attacks were carried out by an ethnic Norwegian citizen, who was arrested the same evening and immediately admitted responsibility. One year later, the trial demonstrated that he operated on his own.

These attacks came as a terrible shock. Until July 22, 2011, Norway had little experience with terrorism and political violence. It is generally regarded as a peaceful, open democracy, and a high-trust society (Delhey & Newton, 2005). The attacks struck at the very heart of the country’s democratic system, and the perpetrator explicitly viewed them as targeting the Norwegian political regime (Oslo Tingrett, 2011). This made all government employees “targets of the assailant’s operation” (Birkeland et al., 2017, p. 662), as was clearly reflected in a speech 1 month after the event by the Secretary General of the Ministry of Transport: “... and then we [who

worked in the government complex] were attacked” (Hildrum, 2011). Even though the immediate reaction of the Norwegian Prime Minister advocated “more openness, more democracy and more humanity, but never naivety,” a government inquiry 1 year after the attacks severely criticized the country’s emergency preparedness and crisis management (Norwegian Government, 2012). This report initiated an intense debate about preventive security and counterterror measures that were new to Norway – though remaining nowhere near as invasive as in the United States.

Sample

Our dataset derives from the Norwegian Administration Surveys fielded in 2006 and 2016 by the Norwegian Centre for Research Data (NSD) in collaboration with a project group encompassing several Norwegian universities. Both survey waves covered all case officers and managers with at least 1 year of service in a Norwegian ministry. The 2006 survey obtained 1874 responses, while the 2016 survey obtained 2322 responses. Response rates were 66 percent in 2006 and 60 percent in 2016 (Christensen et al., 2018), which implies that the representativeness of the surveys is very good.

Since each survey is an independent cross-sectional study, the data are not immediately linkable at the individual level over time. To overcome this, we follow the methodology pioneered in Murdoch et al. (2019) to create individual-level identifiers using the broad set of socio-demographic characteristics available in the data. Specifically, we match respondents across both survey waves when they have the same sex, education (in terms of main study field and having studied abroad), language, education of parents, occupation of parents, and municipality of birth. We also verify that respondents’ (self-reported) employment history is consistent across both surveys, and that their age and length of employment increased with 10 years between both surveys (more details on this methodology are collected in Appendix D; see also Geys, 2023). This procedure reveals 186 unique respondents that appear in both survey samples (roughly 10 percent of respondents in the 2006 wave).⁴ These form the dataset for our analysis, and Appendix Table A.1 shows the descriptive statistics. The final column in this table also presents the results of balancing tests verifying the representativeness of our estimation sample relative to respondents in the 2006 survey that we do not observe again in 2016. This illustrates that entry into our panel dataset is – with minor exceptions for sex and study field – orthogonal to civil servants’ observed characteristics.

Naturally, one might worry that employees were more likely to exit the most strongly affected ministries in the aftermath of 22 July. If so, this could bias our sample. Fortunately, official data from Statistics Norway (<https://>

www.ssb.no/en/statbank/list/regsyst) provide no evidence of differential exit across affected and unaffected ministries in the period 2012–2017. Exits to another ministry accounted for 3.05 percent (3.62 percent) of staff on an annual basis in affected (unaffected) ministries ($t = 1.06$; $p > .10$). The equivalent numbers for exits to the private sector were 2.10 and 1.80 percent on an annual basis, respectively ($t = 0.53$; $p > .10$). The same finding persists for the 2 years immediately following the 2011 attack.⁵ Appendix Table C.1 furthermore shows that the share of respondents in strongly affected, weakly affected, and unaffected ministries is very similar for our panel dataset ($N = 186$) and all respondents of the 2006 survey ($N = 1871$).

Measures

Dependent variables

Our first dependent variable aims to capture individuals' sense of belonging to specific in-groups (H1a,b), which constitutes one of the three key dimensions of social and organizational identification (Abrams & Hogg, 2006; Cameron, 2004). This approach follows, for instance, Huddy and Khatib (2007, p. 65, our italics), who define and operationalize “national identity as a subjective or internalized *sense of belonging* to the nation.” Similarly, Trondal (2001, p. 18) defines identities as “feelings of belonging to different parts of (...) governmental organizations.” In line with Bright (2008) and Tummers and Knies (2013), among others, we operationalize this using the following survey question: “How strong or weak is your sense of belonging to [your unit] [your department as a whole] [the civil service in general] [your profession]?”. Answers are on a scale from 1 (‘Very strong’) to 5 (‘Very weak’).

Our second dependent variable relates to the factors that civil servants perceive to guide their behavior and decision-making processes during the execution of their work tasks (H2). Following Wynen et al. (2020), we operationalize this via the level of attention civil servants perceive themselves as giving to certain stakeholders and interests using the question: “What is the level of attention you give to [signals from the political leadership] [loyalty to your immediate superior][professional standards][openness and transparency] when performing your work tasks?” (coded from 1 ‘Very important’ to 5 ‘Very unimportant’).

While reliance on single-item measurements was a common approach at the time the surveys we use were initially fielded (i.e. starting in 1976), one might worry that one single statement to measure any concept risks a reduced psychometric quality relative to more extensive and carefully validated scales. Recent evidence suggests, however, that single-item measures can achieve acceptable reliability for sufficiently homogeneous

concepts – such as individuals' social identification, job satisfaction, turnover intention (Bergkvist, 2015; Postmes et al., 2013). Even so, this data limitation should be kept in mind when interpreting our findings.

Independent variable

The central independent variable is an indicator variable equal to 1 for respondents working in ministries directly affected by the terror attack on July 22, 2011, 0 otherwise. For seven ministries, the building was damaged to such an extent that the entire ministry was forced to relocate permanently to a different location. Other ministerial buildings suffered less extensive damage or triggered only partial staff relocation (eight ministries), while some ministries were not damaged at all due to their location in another part of Oslo (three ministries). Appendix Table B.1 provides a full overview. The impact of the attack on a ministry thus is operationalized via its proximity to the event location and the extent of damage to its building.

It is important to highlight three aspects with respect to this operationalization. First, we do not observe whether individuals were at work on July 22, 2011. We thus effectively pool employees of affected ministries who were and were not present into one group of “treated” respondents. If those present during the attack would be (much) more severely impacted than those who were not, this is likely to bias our estimates toward 0. Second, using Danish population-wide registry data, Hansen et al. (2017) show that the incidence of trauma- and stressor-related disorders in Denmark increased by 16 percent following the 22 July attack in Norway. As similar effects might arise even more strongly within Norway, one can argue that our setting strictly speaking does not include unaffected individuals. While this is true, our empirical approach can nevertheless identify *differences* in effects across staff members of more and less proximate ministries (see below). Finally, a binary operationalization of our terror “treatment” may not fully capture that some respondents were more proximate to the event location. As a robustness check, we therefore also use a continuous measure of proximity: i.e. the physical distance between a ministerial building and the event location.

While we unfortunately lack detailed information on background characteristics in the target population (i.e. all employees in Norwegian ministries), comparing the information in Appendix Tables A.1, C.1, and C.2 offers a sense of the representativeness of the control and treatment groups relative to the target population. This indicates that the control and treatment groups are a close match to the total respondent sample on our outcome variables of interest, but representativeness appears less close on gender and experience. Appendix Table C.1 likewise shows that the distribution of respondents over the

control and treatment groups matches that of the total respondent sample of the surveys.

Empirical strategy

Our empirical strategy relies on the comparison over time (pre-event versus post-event) of employees in ministries who were or were not directly impacted by the terror attack on July 22, 2011. More specifically, with i representing individual respondents and t designating time, the model can be written as:

$$Y_{it} = \alpha_i + \beta_1 \text{Wave2016}_t + \beta_2 \text{Wave2016}_t * \text{Error treatment}_t + \varepsilon_{it} \quad (1)$$

Where Y_{it} reflects the dependent variables discussed previously. Wave2016_t is 1 for responses in the 2016 survey (0 for responses in 2006), and Error treatment_t is as defined in the previous section. The interaction between these two variables allows us to study whether ministerial staff directly exposed to the terror event ($\beta_1 + \beta_2$) develop differently between both survey waves compared with all other ministerial staff (β_1). We extend the model with a full set of respondent fixed effects (α_i), which control for all aspects of respondents that do not change over time (such as their gender or sociability).⁶ We also control for general time trends via Wave2016_t , which captures any changes over time that affect all respondents equally. Finally, we cluster standard errors at the ministry level (where the terror “treatment” takes place) to account for the lack of independence of observations from respondents within the same ministry.

Since we only observe two time points, it is not possible to capture detailed temporal dynamics. For instance, responses to terror events might arise quickly or incrementally, can develop immediately or only after some delay, and so on (Kettl, 2004; Murdoch et al., 2022). Given the data available, our analysis by construction assesses the long-run impact of the 22 July attacks observed 5 years after the event, and a more in-depth study of the temporal dynamics leading to this long-run “equilibrium” remains an important avenue for further research. One might also worry that (many) other things happened to each Norwegian ministry in the period between our two observation points. The most important events are the merger of the ministries of trade and fisheries in 2014, and the integration of the ministry of government administration and reform into the ministry of local government that same year (Christensen et al., 2018). We exclude respondents from these merger-affected ministries throughout the analysis (though including them does not affect our findings). Yet, crucially, 22 July is the *only* thing that happened to all affected ministries *as a group*. This is what we exploit

to identify the long-run effect of 22 July on the civil servants in our sample.

Note, finally, that equation (1) captures the combined impact of *all* multifarious factors related to the attack – such as the disruption of moving office, loss of colleagues, introduction of new crisis-management and training procedures, provision of remembrance ceremonies and support services, and so on (i.e. a compound treatment effect). Although we are not aware of any differential response to the event across ministries in terms of, for instance, crisis-management procedures, remembrance ceremonies, or support services (which was organized centrally), this should be kept in mind while interpreting our findings.

RESULTS

Terror and in-group sense of belonging

We start by looking at the impact of the 22 July attacks on civil servants’ sense of belonging to their organizational context (H1a/b). Panel I in Table 1 differentiates between the two main groups of “affected” and “unaffected” ministries, while Panel II further separates the latter into “weakly affected” and “unaffected” ministries (see Appendix Table B.1). Given our coding, negative values in Table 1 imply a strengthening sense of belonging to one’s unit (Column (1)), one’s ministry (Column (2)), the civil service (Column (3)), and one’s profession (Column (4)). For ease of interpretation, Table 1 reports results from estimating equation (1) using linear regression models despite our categorical dependent variables. However, estimation of fixed-effects ordered logistic regression models provides closely equivalent findings (Appendix Table A.2).

The statistically significant coefficient estimates for the interaction terms in Columns (2) and (3) of Panel I confirm that the attack is linked to a *strengthened* sense of belonging to one’s department and the civil service among civil servants in affected ministries – relative to those in unaffected ministries. In terms of effect size, affected respondents are located approximately 0.24–0.45 lower on a five-point scale, which reflects 30–51 percent of the standard deviation of the outcome variable. As such, this is a substantively meaningful result in line with H1a, suggesting that shared terror experiences are associated with civil servants pulling together and “rallying around” their attacked organization (Bastian et al., 2014; Stein, 1976).

Although we do not observe significant results for the sense of belonging to civil servants’ organizational unit and profession in Panel I, this changes in Panel II when separating between strongly, weakly, and unaffected ministries. Column I in Panel II indeed shows significantly *stronger* sense of belonging to the unit over time among civil servants in unaffected ministries, compared with a

TABLE 1 Main results for sense of belonging.

| | Unit (1) | Department (2) | Civil service (3) | Profession (4) |
|------------------------------------------------------------------|-------------------|-------------------|-------------------|----------------|
| Panel I: Treated versus untreated ministries | | | | |
| Wave 2016 | 0.040 (0.39) | −0.030 (−0.37) | −0.070 (−1.04) | 0.021 (0.22) |
| Terror treatment * Wave2016 | 0.108 (0.68) | −0.210* (−1.78) | −0.383*** (−3.41) | 0.036 (0.22) |
| <i>N</i> | 313 | 311 | 311 | 306 |
| <i>R</i> ² (within) | 0.008 | 0.029 | 0.077 | 0.001 |
| <i>F</i> -test | 1.45 | 8.18*** | 25.39*** | 0.17 |
| Panel II: Separating strongly, weakly, and unaffected ministries | | | | |
| Wave2016 | −0.200*** (−3.01) | 0.114*** (3.39) | −0.133 (−1.49) | −0.073 (−0.59) |
| Weak terror treatment * Wave2016 | 0.436*** (3.34) | −0.259** (−2.11) | 0.115 (0.97) | 0.162 (0.88) |
| Strong terror treatment * Wave2016 | 0.348*** (2.48) | −0.354*** (−3.91) | −0.319*** (−2.52) | 0.130 (0.70) |
| <i>N</i> | 313 | 311 | 311 | 306 |
| <i>R</i> ² (within) | 0.038 | 0.043 | 0.080 | 0.016 |
| <i>F</i> -test (weak treatment) | 4.41* | 1.51 | 0.05 | 0.44 |
| <i>F</i> -test (strong treatment) | 1.44 | 8.16** | 25.31*** | 0.17 |

Note: The table displays the result from linear regression models with the dependent variable reflecting respondents' answers to the question: "How strong or weak is your sense of belonging to [your unit] [your department as a whole] [the civil service in general] [your profession]?" Answers coded from 1 ("Very strong") to 5 ("Very weak"). Panel I differentiates between the two main groups of "treated" and "untreated" ministries, while Panel II separates three groups of strongly affected, weakly affected, and unaffected ministries (see Appendix Tables B.1). All models include a full set of individual fixed effects. *t*-values based on standard errors clustered at the ministry level between parentheses. *F*-test evaluates the significance of the sum of the coefficients of *Wave2016* and *Terror treatment * Wave2016* (i.e. the effect of the terror treatment on treated individuals). ****p* < .01; ***p* < .05; **p* < .1.

weakening sense of belonging among civil servants in both weakly and strongly affected ministries. A similar, but substantively and statistically weaker, pattern arises for respondents' professional sense of belonging. These findings are in line with H1b, suggesting that the increased sense of belonging to one's ministry and the civil service following the terror event pushes other group identities down the "hierarchy of salience" (Stryker, 1968, p. 560).

Our data also allow analyzing whether this strengthened sense of belonging is linked to a reduction in individuals' intention to leave the ministry or civil service (H1a; Mitchell et al., 2001; Moynihan & Pandey, 2008). This is operationalized using the question: "Do you, today, have any plans or intentions to leave the ministry for another job?" (coded as "Yes" (1) or "No" (0)). If the answer is "yes," respondents are asked whether they are considering a move within the same ministry (or its underlying agencies), to other public sector organizations, or to the private sector (again coded as "Yes" (1) or "No" (0)). The results are summarized in Online Appendix Table A.3. Respondents in unaffected ministries display significantly decreasing intentions to leave over time (−0.200; *p* < .01), whereas this is *not* the case in affected ministries (−0.200 + 0.176 = −0.024; *p* > .10). This finding is driven largely by intentions for cross-ministerial moves, suggesting, at odds with H1a, that affected civil servants remain at least somewhat unsettled in their workplace even 5 years after the event. Any increase in job embeddedness from strengthened in-group cohesion (Mitchell et al., 2001; Moynihan & Pandey, 2008) thus appears to be (partly) balanced out by persistent unease

about having a ministry targeted by terrorism as one's workplace (Hansen et al., 2019; Huddy et al., 2005).

Terror and work routines

In Table 2, we turn to the impact of 22 July on civil servants' self-perception of their day-to-day work routines (H2). Column (1) in Table 2 indicates that civil servants in (strongly) affected ministries claim to award increasing attention to political signals after the event. The coefficient estimate suggests that respondents in these ministries move approximately 0.33 on a five-point scale (*p* < .10 in both cases), which is equivalent to 41 percent of the standard deviation in the outcome variable. This provides substantively meaningful support for H2. It also offers one potential *individual-level* mechanism behind the observation that the policy profile of US federal agencies became more attentive to "higher-level signals concerning the domestic security agenda" following the 1995 Oklahoma City bombing (May et al., 2008, p. 528). Interestingly, Column (4) in Table 2 suggests that civil servants in affected ministries also claim to give more attention to transparency and openness after the event (−0.204−0.160 = −0.364; *p* < .05) compared with those in unaffected ministries (−0.204; *p* < .05). This is in line with the Norwegian Prime Minister's stress on the need for "more openness" – among other things – in his initial reactions to the event (see above). As such, this result aligns closely with the observation that employees in affected ministries declare increasing attention to signals from political leaders after the event (Column (1)).

TABLE 2 Main results for work routines.

| | Political signals (1) | Loyalty to leadership (2) | Professional standards (3) | Openness and transparency (4) |
|------------------------------------------------------------------|-----------------------|---------------------------|----------------------------|-------------------------------|
| Panel I: Treated versus untreated ministries | | | | |
| Wave2016 | 0.000 (0.00) | 0.049 (0.60) | -0.192*** (-2.65) | -0.204** (-2.02) |
| Terror treatment * Wave2016 | -0.333*** (-2.41) | 0.006 (0.04) | 0.138 (1.30) | -0.160 (-0.87) |
| <i>N</i> | 317 | 317 | 318 | 317 |
| <i>R</i> ² (within) | 0.481 | 0.004 | 0.062 | 0.055 |
| <i>F</i> -test | 7.27** | 0.14 | 0.50 | 5.54 ** |
| Panel II: Separating strongly, weakly, and unaffected ministries | | | | |
| Wave2016 | -0.063 (-0.84) | -0.083*** (-2.82) | -0.208*** (-3.62) | -0.208 (-1.51) |
| Weak terror treatment * Wave2016 | 0.116 (0.96) | 0.247** (2.25) | 0.030 (0.22) | 0.008 (0.04) |
| Strong terror treatment * Wave2016 | -0.271* (-1.88) | 0.138 (0.94) | 0.154 (1.60) | -0.155(-0.75) |
| <i>N</i> | 317 | 317 | 318 | 317 |
| <i>R</i> ² (within) | 0.050 | 0.018 | 0.062 | 0.055 |
| <i>F</i> -test (weak treatment) | 0.32 | 2.39 | 2.11 | 1.84 |
| <i>F</i> -test (strong treatment) | 7.25* | 0.14 | 0.50 | 5.53** |

Note: The table displays the result from linear regression models with the dependent variable reflecting respondents' answers to the question: "What is the level of attention you give to [signals from the political leadership][loyalty to your direct superior][professional standards][openness and transparency] when performing your work tasks?" Answers coded from 1 ("Very important") to 5 ("Very unimportant"). Panel I differentiates between the two main groups of "treated" and "untreated" ministries, while Panel II separates three groups of strongly affected, weakly affected, and unaffected ministries (see Appendix Tables B.1). All models include a full set of individual fixed effects. *t*-values based on standard errors clustered at the ministry level between parentheses. *F*-test evaluates the significance of the sum of the coefficients of *Wave2016* and *Terror treatment * Wave2016* (i.e. the effect of the terror treatment on treated individuals). ****p* < .01; ***p* < .05; **p* < .1.

Column (2) of Table 2 shows no similar association between terror events and civil servants' professed loyalty toward their immediate supervisor. If anything, panel II suggests that only civil servants in *unaffected* ministries claim to increase attention to their immediate superior after the event (-0.083; *p* < .01) – compared with no significant change for respondents in weakly or strongly affected ministries (-0.083 + 0.247 = 0.164; -0.083 + 0.138 = 0.055; *p* < .10 in both cases). In similar vein, both panels in Column (3) show that self-professed attention to professional standards increases significantly between both survey waves for civil servants in *unaffected* ministries (*p* < .01), but *not* for respondents in (strongly) affected ministries (*p* > .10). Taken together, these findings suggest that civil servants' heightened attention to political signals (see above) may come with considerable "costs" in terms of giving less attention to bureaucratic leaders and professional principles. This apparent tradeoff or rebalancing would, however, require further substantiation in future research.⁷

Robustness and validity checks

A key assumption underlying our analysis is that any differences in the changes over time between the affected and unaffected ministries are due to the terror event, rather than other unobserved elements that put both groups on diverging tracks already prior to the event (parallel pre-trends assumption). With only two time periods in our dataset, this assumption cannot be tested directly.

Although this is the key limitation of our two-period, two-group research design, many studies across multiple disciplines continue to rely on a similar research design. Recent examples in political science and public administration include, for instance, Murdoch et al. (2019), Jilke and Baekgaard (2020), and Zhang et al. (2022). In line with the remainder of this section, such studies generally offer alternative ways to assess the internal validity of the research design in the specific setting at hand.

First, absent the ability to test for parallel trends, one important check is to compare the characteristics of respondents in affected and unaffected ministries on our main outcome variables at the initial point of measurement. If the ministries affected by the terror event were truly "random," we would not expect any differences between both groups in 2006. Appendix Table C.2 shows that this is the case, which makes sense given that our outcome variables were of no relevance to the perpetrator's motives (Oslo Tingrett, 2011).

Second, we exploit an earlier survey wave (fielded in 1996) to assess the "effect" of a non-existing event between the 1996 and 2006 survey waves (*N* = 142 panel respondents). The "treatment" in this case is set to 1 for respondents in ministries affected by the 2011 terror event, 0 otherwise. Although this "placebo" test studies a different sample, it can still be informative about potential differences in pre-treatment changes among employees of affected and unaffected ministries. Appendix Table A.4 shows no significant effects when looking at individuals' sense of belonging, which implies there was *no* differential development across "treatment" groups in the

1996–2006 period. This strengthens our inference that the 2011 attack drives the effects observed in Table 1, rather than representing a statistical artifact. Appendix Table A.5 displays no differential shifts across “treatment” groups in terms of respondents’ professed attention to administrative leaders, professional standards, as well as transparency and openness. There is a significant interaction term for political signals, but this has the *opposite* sign to that in Table 2. While this indicates that our findings in Table 2 are *not* due to a longer-term “trend” toward strengthening political signals in affected ministries – but rather suggests that the 2011 attack may have reversed an earlier movement in the opposite direction – it does caution against a strong causal interpretation of this result.

Finally, we also considered extending our model to a triple-difference estimation further to strengthen the analysis. Unfortunately, our small sample size poses a critical constraint here. Splitting both the treated and non-treated groups into small(er) subsets along a second criterion (beyond the terror treatment) would lead to very low power and imprecise point estimates. Even so, we consider this an important avenue for further research when larger samples are available, particularly in settings where specific subsets of employees were targeted during the terrorist event (e.g., a school shooter targeting teachers of a certain race, gender or age cohort).⁸

CONCLUDING DISCUSSION

Public administration serves as a critical infrastructure to the functioning of democratic government (Huber, 2000; Orren & Skowronek, 2017; Putnam, 1973). If terrorism has a measurable impact upon the civil servants exposed to such events, it may have important – but previously unrecognized – democratic implications by affecting the operation of core government functions. Building on a two-wave survey that allows studying the *same* civil servants before and after the July 22, 2011 terror attack in Norway, our analysis is the first to explore the impact of terrorism on civil servants in their operation of core government functions. Our main findings suggest that a terror attack targeting civil servants’ workplace is associated with increased in-group cohesion within the targeted organization and higher (self-)perceived attention to the signals of political leaders. Given the potential policy implications of such effects, this provides novel insights for the persistent debate about the (in)effectiveness of terrorism (e.g., Pape, 2003 versus Abrahms, 2012). More specifically, our analysis indicates that civil servants’ sense of belonging to their organization and a tightening of political authority constitute two potential mechanisms through which terrorism might impact public governance.

Our findings have practical as well as theoretical implications. From a practical perspective, we learn that opinion dynamics following terror events manifest themselves

in several important dimensions among civil servants. This extends previous work into the influence of terrorism on public administrations using organization-level data (e.g., May et al., 2008) or descriptive evidence (Christensen et al., 2019; Terry & Stivers, 2002). Public sector managers thus hold considerable responsibility to cope with these attitudinal shifts and avoid undue biases in policy outcomes after terror events. From a theoretical perspective, our results suggest that “off-the-job” shocks may have countervailing impacts upon individuals’ job embeddedness. That is, while such events can increase job embeddedness by stimulating a heightened sense of in-group belonging (Mitchell et al., 2001; Moynihan & Pandey, 2008), they may simultaneously reduce embeddedness by creating discomfort and anxiety about one’s workplace (Hansen et al., 2019; Huddy et al., 2005). These opposing forces are important to acknowledge in future research on job embeddedness (as a precursor of turnover intentions), where the impact of external shocks has received at best limited attention.

Naturally, our Norwegian case has some characteristics – including high trust in government, a low-corruption civil service, and a (very) low frequency of terror events – that could limit the generalizability of our findings. From this perspective, it is interesting that our results align well with those of May et al. (2008) on the 1995 Oklahoma City bombing. They show that this event increased the responsiveness of federal *agencies* to their political leaders, which is consistent with – and may be explained by – our finding that *individual civil servants* profess increased attention to signals by their political leaders after a terror event. Such correspondence suggests that our findings may well generalize beyond our Norwegian setting to other advanced Western democracies. That being said, whether and how our findings apply to contexts with high political instability and violence remains an open question. While one might argue that the observed effects could be cumulative when additional events take place, an alternative prediction could be that the impact of repeated terrorist attacks becomes abated.

Further avenues toward the generalization of our work may include the direct comparison of government employees and other groups of employees. At present, our analysis does not allow any indication of whether the findings observed among civil servants translate to employees more generally. In similar vein, future work should engage in comparative analyses of, for instance, terror attacks and natural disasters (e.g., earthquakes, floods), man-made disasters (e.g., wildfires, oil spills), or pandemics (e.g., Covid-19). It would also be important to assess whether our results hold similarly across lone-wolf terror attacks (as in our setting) versus attacks perpetrated by a terrorist network. Closely related, we suspect that the broader security and political context may be an important moderating factor. From this perspective, it would be interesting to compare, for instance, the impact on employees of the Pentagon after 9/11, the US Capitol

after the January 6 riots (Zulli et al., 2023), and diplomats exposed to terror attacks on embassies (Sandler & Enders, 2004).

Finally, our study suggests that exposure to an isolated terrorist attack can harness the robustness of the civil service. Although the concept of robustness is far from new, there is growing interest in attempts to balance stability and change in scientific disciplines as varied as engineering, eco-systems theory, and the social sciences (Ansell et al., 2022; Jen, 2005). The conceptual attraction of robustness is its insistence that in a threatening environment some features of a system must be changed for others to remain stable. This raises key questions about which core functions, purposes, and values to maintain (and change) in the face of disruptive perturbations. Answering such questions would not only call for large-T longitudinal research designs, but also for theorizing the temporal dimension of public governance robustness.

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ENDNOTES

- ¹ To the best of our knowledge, the few studies analyzing the impact of terror events on public employees focus mainly on health-related outcomes such as psychological distress, symptom-defined PTSD, and sick leave (e.g., Hansen et al., 2019; Jordan et al., 2004; North et al., 1999). In fact, none of the 20 contributions to the 2002 Symposium Issue of Public Administration Review addressed how 9/11 affected public employees.
- ² Empirical evidence is inconclusive. Some studies show a differential impact of 'local' war casualties (Gartner et al., 1997), residence in areas around a mass shooting (Newman & Hartman, 2019) or living in New York City at the time of 9/11 (Huddy et al., 2002). Yet, these studies are restricted to the broad vicinity of an event rather than people at the exact event location. This is important since 'secondary victims' – i.e. "those not directly affected by an event but residing in the

community in which an event occurs" (Ryan et al., 2003, p. 647) – may still be less affected than primary victims (Agerberg & Sohlberg, 2021).

- ³ Observe that H1a and H1b concern distinct social identities. Our key point is that some of these gain in salience due to the terrorist attack under analysis (as reflected in H1a), while others are – as a direct result – pushed down the hierarchy of salience (as reflected in H1b).
- ⁴ The fact that our sample covers only a relatively small subset of the original survey respondents predominantly restricts our ability to generalize to the broader population of Norwegian ministerial employees. Furthermore, the small sample size precludes the detailed exploration of heterogeneous treatment effects across respondent groups. Crucially, however, it does not affect the internal validity of our quasi-experimental research design (Geys, 2023).
- ⁵ We always correct for the relocation of several departments between ministries over the period under analysis, and exclude the Prime Minister's Office due to a change from a left-leaning to a right-leaning government in Norway in 2013. Note also that fairly low annual turnover rates still imply a considerable turnover when aggregated over a longer period. The 10-year period between both survey waves thus explains in large part why our panel dataset only covers circa 10 percent of the 2006 sample.
- ⁶ This is the reason *Terror treatment_i* is not included as a separate variable in equation (1), since it is perfectly collinear with the respondent fixed effects. On a more technical note, observe also that our two-way fixed effects specification is numerically equivalent to a difference-in-differences (DiD) approach in our two-group, two-period set-up (Imai & Kim, 2021; Wooldridge, 2021).
- ⁷ Remember that our analysis is based on survey evidence and thus is limited to the individuals' attitudes and perceptions rather than actual behavioral changes. We explored the possibility of using, for instance, newspaper archives to assess behavioral change in politician-bureaucrat interactions in the aftermath of the event. Unfortunately, this proved impossible in part because any such change would be expected to arise predominantly in affected – rather than unaffected – ministries. We nonetheless consider this an important avenue for further research, and are grateful to an anonymous referee for pointing this out. Even so, shifts in staff attitudes are important to study in their own right because they systematically determine subsequent staff behavior (Egeberg & Trondal, 2018).
- ⁸ Note also that adding an extra control for a change in the political affiliation of ministers to the far-right Progress party during the 2013 change in government leaves our inferences unaffected (Appendix Table A.6). Similarly, replacing our main independent variables with a continuous measure of "proximity" to the event location – i.e. the physical distance between the ministerial buildings and the event location – leaves our main inferences unaffected (Appendix Table A.7). For instance, respondents at a distance of 0 m develop a significantly stronger sense of belonging to their department and the civil service after the event, while increasing distance from the event location weakens this effect (the point estimate loses statistical significance at distances exceeding circa 80 m). Furthermore, only respondents within a 50 m distance from the event location report becoming significantly more likely to give increased attention to political signals after the event. When distance to the event location increases beyond roughly 100 m, this relationship reverses.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.