**Is Commitment Getting Infected Too?
How COVID-19 Stay-Home Orders Influence Workgroup Commitment**

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Abstract

The COVID-19 pandemic and associated “Stay-Home” restrictions in the United States have disrupted employees’ lives. We leverage the change brought on by the Stay-Home restrictions to examine corresponding changes in employees’ commitment to their workgroup. Specifically, we advance and test a model predicting that the Stay-Home restrictions prevented workgroups from engaging in rigidly performed, meaningful workplace activities (i.e., *ritualistic workplace activities*), which subsequently made members feel that the group was less cohesive and ultimately reduced members’ workgroup commitment. We also compare changes in workgroup commitment to changes in workgroup identification, hypothesizing that commitment to one’s group erodes more than identification when workgroups are perceived to be less cohesive. We test our model in a four-wave panel survey of 772 U.S. employees at the onset of the Stay-Home restrictions, which allows us to examine within-person changes to commitment over time. Consistent with our hypotheses, commitment decreased as the duration of Stay-Home restrictions increased, which was mediated by corresponding declines in engaging in ritualistic workplace activities and perceptions of the workgroup’s cohesiveness. Further, commitment to one’s workgroup declined more than did identification with the workgroup, due to the stronger relationship between perceived group cohesion and commitment (vs. identification). We replicated these results in a separate, preregistered cross-sectional survey. Our findings shed light on the mechanisms underlying workgroup commitment, demonstrating that engagement in ritualistic activities, which enhance workgroup cohesion, is linked to stronger commitment—more so than identification—over time.

Keywords: *commitment; identification; group; ritual; COVID-19*

Is Commitment Getting Infected Too? How COVID-19 Stay-Home Orders Influence Workgroup Commitment

The coronavirus (COVID-19) pandemic has brought fast-moving and vast changes to the employee experience. By April 2020, more than 95% of the U.S. population was urged to stay home via government issued “Stay-Home” orders.¹ We investigate how this unprecedented disruption influenced employees’ commitment to their workgroups—that is, the degree that employees have a volitional psychological bond that reflects their dedication to and responsibility for a workgroup (Klein et al., 2012).² Understanding how the Stay-Home orders—which were designed to slow the spread of COVID-19—influenced employees’ commitment is important because commitment is linked to a range of positive employee outcomes including performance, retention, and well-being (e.g., Becker, 1992; O’Reilly & Chatman, 1986).

We advance a model to understand how the Stay-Home orders influenced employee workgroup commitment and test the model in a longitudinal survey of working Americans during the early stages of the Stay-Home orders in the U.S. (April to June 2020). We theorize that meaningful behavioral investments that employees make at work, which we operationalize as workplace activities that are performed rigidly and are perceived as meaningful (i.e., *ritualistic workplace activities*; Hobson et al., 2018), influence perceptions that the workgroup is cohesive, and that group cohesion subsequently affects commitment. As a result, we propose that, because the Stay-Home orders often prevented workgroups from engaging in the ritualistic workplace activities that existed before these restrictions were in place, employees will perceive their workgroups as less cohesive and consequently feel less committed to their workgroups as

¹ <https://web.archive.org/web/20200406005340/https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html>

² We used Klein et al.’s (2012) unidimensional commitment conceptualization because it was designed to be applicable to multiple foci (including workgroup commitment) and it supported our focus on commitment as a psychological bond rather than a purely instrumental one (e.g., Caldwell et al., 1990).

the duration of Stay-Home orders lengthened. We further hypothesize that commitment, compared to workgroup identification—the extent to which an individual defines themselves in terms of their membership in a particular workgroup (Ashforth & Mael, 1989)—will be more contingent on ongoing behavioral demonstrations such as the degree to which members are behaviorally linked (i.e., perceived workgroup cohesiveness), rather than on how a person defines his or her own self-concept and relates it to the group. As such, commitment will erode more than identification will at the onset of the Stay-Home orders, because (1) the Stay-Home orders hindered the ability of workgroups to engage in ritualistic workplace activities,³ resulting in lower levels of perceived cohesiveness, and (2) the perceived cohesiveness of the workgroup influences commitment to—more than identification with—one’s workgroup, because identification is less dependent on continuing behavioral investments, such as engagement in ritualistic activities and the ensuing group cohesion.

This research makes four key contributions. First, we advance theories of workgroup commitment, suggesting that commitment may be less stable than previously theorized (e.g., Solinger et al., 2008), and contingent on members’ ongoing behavioral investments (Becker, 1960). To derive this insight, our study contains four waves of data collection, investigating within-person changes in commitment rather than the more typical focus on between-person differences using cross-sectional designs (Bergman & Jean, 2016). Second, our research leverages a catastrophic event, the COVID-19 pandemic, as a useful empirical referent (Smith, 1977) enabling us to surface the essential features of commitment and sharpen the distinction between workgroup commitment and identification (van Dick, 2016). Specifically, we show that commitment and identification erode at different rates and are, as such, likely fueled by different

³ Although rituals can develop virtually and not just in-person, many organizations had in-person rituals that the Stay-Home orders disrupted. We seek to study the disruption of those rituals and the relevant consequences.

mechanisms. Third, by drawing from sociological and psychological theory, we consider ritualistic workplace activities and resulting group cohesion as novel mechanisms that explain why workgroup commitment may decline after a significant change in work conditions brought on by an exogenous shock (Stay-Home orders). Finally, we identify a key managerial implication, which is that workgroups aspiring to maintain high levels of commitment should develop rituals that can be meaningfully accomplished.

COVID-19 Orders, Workplace Activities, Workgroup Cohesion, and Commitment

Due to the rapid spread and virulence of COVID-19, local and state governments in the U.S. limited the number of people who could gather together in-person starting in late March 2020. The resulting “Stay-Home” orders—which were designed to slow the spread of the virus—had the additional effect of preventing workgroups from engaging in typical activities together and, in many cases, required them to conduct work remotely (e.g., Kniffin et al. 2021; Van Bavel et al., 2020). Building on prior theorizing that situational factors influence the development of commitment through cognitive and affective evaluations (Klein et al., 2012; O’Reilly & Chatman, 1986), we examine how Stay-Home orders altered the types of activities in which workgroups engaged and how this changed members’ perceptions of their workgroup, and, ultimately, their commitment. We examine workgroups as the focus of commitment because employees are likely to interact with workgroup members on a daily basis, increasing the immediacy and salience of the changes in workgroup interaction induced by Stay-Home orders (Riketta & van Dick, 2005).

The Stay-Home orders, which required most employees to socially distance, disrupted certain types of group activities more than others. In particular, activities that occur more routinely and with fixed, rigid procedures that are seen as meaningful to employees, were

especially likely to be disrupted. Consider, for instance, social events like weekly “happy hours” among workgroups, certain ways of enacting meetings to make them more structured and enjoyable, or specific celebrations for workplace achievements like promotion ceremonies; all are all activities that needed to be significantly adapted in the context of Stay-Home orders. Prior research has operationalized the structure and meaningfulness of activities as the degree to which the activity is *ritualistic* (e.g., Norton & Gino, 2014; Stein et al., 2021).

We build on previous research to conceptualize workplace activities as being more ritualistic when they have specific physical features (rigid, repetitive actions) as well as psychological features (a sense of meaning; Hobson et al., 2018). Activities can range in how ritualistic they are. For example, even a simple work meeting can be made more ritualistic if it has the physical features of a ritual—such as saying a company motto at the start of a meeting—and the psychological features of a ritual—such as affirming different values of the group.⁴ Ritualistic activities rely on rigid adherence to performing sequenced actions (Boyer & Liénard, 2020). Thus, when major exogenous shocks impact contact, it disrupts the group’s ability to adhere to each of the actions in the sequence. We therefore predict that employees’ propensity to engage in workgroup rituals will decline at the early stages of Stay-Home orders.

Hypothesis 1: Employees’ engagement in ritualistic workplace activities will decrease as the COVID-19 Stay-Home orders continue to be in effect over time.

In considering the potential consequences of disrupting ritualistic workplace activities, we propose that engaging in ritualistic activities may be linked to people’s perceptions that their workgroups are cohesive, or bonded together as a group (Hamilton et al., 2002). The more groups engage intimately with one another, share common values, goals, and rules, and feel

⁴ One example is the Walmart Cheer. Walmart employees sing the structured chant three times a day and the chant ends with an affirmation of one of Walmart’s core values (“Who’s number one? The Customer!”; Bergdahl, 2004).

emotionally close to one another, the more cohesive members perceive the group (e.g., Campbell, 1958; Yzerbyt et al., 2004). Engaging in ritualistic activities may promote perceptions of group cohesion because ritualistic activities foster salient, visible manifestations of the group's values. In this way, ritualistic activities serve to promote, protect, and perpetuate group members' shared values (Stein et al., 2021). These shared values foster feelings of similarity and intimacy, enabling group members to engage in similar behaviors and feel close (Chatman & O'Reilly, 2016). Because ritualistic activities promote a shared value system, we propose that engaging in more ritualistic workplace activities will enhance members' perceptions that the workgroup is cohesive. Consistent with this reasoning, external observers view groups with more established rituals as more cohesive (e.g., Lakens, 2010; Watson-Jones & Legare, 2016), although prior research has not examined whether engaging in ritualistic activities influences members' own perceptions of their group's cohesiveness, as we suggest here.

Hypothesis 2: Engaging in ritualistic workplace activities is positively associated with workgroup members' perception that their workgroup is more cohesive.

We propose that perceptions of workgroup cohesion can positively influence workgroup commitment because cohesive groups are more effectively able to mobilize resources to influence individual outcomes. Thus, more cohesive groups are able to reward committed group members, promoting commitment through social exchange (e.g., Zaccaro & Dobbins, 1989).

Hypothesis 3: Employees who view their workgroup as more cohesive will be more likely to demonstrate stronger workgroup commitment.

Integrating H1-H3, we propose that the Stay-Home orders will have a negative effect on workgroup commitment by reducing members' engagement in ritualistic workplace activities and consequently, by lowering perceptions that the workgroup is cohesive.

Hypothesis 4a: Workgroup commitment will decrease as COVID-19 Stay-Home orders continue to be in effect over time.

Hypothesis 4b: The negative relationship between the Stay-Home orders and workgroup commitment will be sequentially mediated by the reduced engagement in ritualistic workplace activities and, consequently, lowered workgroup cohesion.⁵

Differentiating Workgroup Commitment from Workgroup Identification

The psychological relationship between employees and their workgroup has been conceptualized not just in terms of how committed employees are, but also how much they *identify* with their workgroups—that is, how much employees integrate their workgroup into their self-concept (Dutton et al., 1994; Mael & Ashforth, 1992). Commitment and identification refer to a person's attachment to a group (e.g., Meyer et al., 2006; Riketta & van Dick, 2005) and are closely related (e.g., $r = .78$ in Riketta's [2005] meta-analysis). Researchers have thus called for studies to distinguish the two constructs from one another (e.g., van Rosenberg et al., 2018).

Although both commitment and identification can be dynamic (Bednar et al., 2020; Klein et al., 2012), we suggest that changes in commitment to and identification with one's workgroup may show different trajectories over time in response to the dramatic workplace changes brought on by the Stay-Home orders. Commitment has been conceptualized as more behaviorally based than identification (Becker, 1960; Staw, 1981). Specifically, commitment answers the question, "*How much do I want to invest in and provide benefits to the group?*", highlighting a behavioral orientation to contribute to the group's welfare. In contrast, identification addresses the degree to which a person integrates their workgroup into their self-concept, answering the question, "*How much do I view the group as a part of who I am?*" (e.g., Haslam, 2004; Hogg & Terry, 2000). Accordingly, demonstrated pro-group behaviors tend to be more characteristic of commitment than of identification (e.g., Bergami & Bagozzi, 2000; Marique & Stinglhamber, 2011).

⁵ A sequential (serial) multiple mediator model has two or more mediators, with one of the mediators being a cause of the other mediator (Hayes, 2015).

Because groups that are more cohesive are also more cooperative (e.g., supporting one another via explicit behaviors; Mullen & Copper, 1994; Chatman et al., 2019)—and groups perceived to be more cohesive are likewise inferred to engage in more cooperative behaviors (Gammage et al., 2001)—we further hypothesize that perceived group cohesion will influence commitment, which is driven by behavioral investments, more than it will influence identification. Identification is focused on whether the workgroup can contribute to group members' self-definition (e.g., Ashforth & Schinoff, 2016) and thus, may be based on more stable properties of the group (such as the workgroup's external reputation) rather than more dynamic changes in members' perceived behavioral investments. Putting together the prediction that perceived cohesion will influence members' workgroup commitment more so than their workgroup identification with our prior hypotheses leads us to also expect that the Stay-Home orders will be associated with a larger decrease in workgroup commitment than in workgroup identification. See Figure 1 for our theoretical model.

Hypothesis 5a: Workgroup commitment will decrease more than workgroup identification as COVID-19 Stay-Home orders continue to be in effect over time.⁶

Hypothesis 5b: Reduced engagement in ritualistic workplace activities and lower perceived workgroup cohesion will influence the relation between Stay-Home orders and declining workgroup commitment more than it will influence the relation between Stay-Home orders and declining workgroup identification (moderated sequential mediation).

Method

We tested our hypotheses using a longitudinal design, collecting time-varying assessments of data from the same individuals to examine the stability of commitment during the onset of the COVID-19 Stay-Home orders. We also preregistered our measures, and several

⁶ We are hypothesizing workgroup identification will change *less* than workgroup commitment, not that workgroup identification will exhibit no change. Since the self-concept can change over time, workgroup identification—as a component of the self-concept—can also change (e.g., Ashforth et al., 2008; Bednar et al., 2020; Schultz, 2016).

research questions, on the Open Science Framework (OSF). We posted a preliminary preregistration after collecting wave 0 (<https://tinyurl.com/covidprereg1>) and an updated preregistration after collecting wave 2 (<https://tinyurl.com/covidprereg2>).⁷

Participants. We administered four surveys to adult U.S. workers at the onset of the Stay-Home orders. At wave 0, which occurred shortly after the Stay-Home orders were announced, we recruited $N = 1,000$ U.S. participants from Amazon Mechanical Turk who had been employed full-time prior to the COVID-19 pandemic. Excluding those who failed to follow instructions ($N = 40$; see Appendix A), we invited all participants recruited in wave 0 to participate in waves 1-3; no one was excluded for nonresponse, and we retained partial responses in our analyses. To distinguish our results from the effects of job termination on commitment (e.g., Porter et al., 1974), we excluded participants who reported that they were not employed at the same organization for the duration of the study ($N = 188$), resulting in a final sample of 772 participants (see Table 1 for demographics).⁸ This exclusion, while not preregistered, provides a more conservative test of our hypothesis; we compare the full sample ($N = 960$) to the final sample ($N = 772$) in Appendix B. The response rate for the final sample ($N = 772$) was 80.96% in wave 1 (625/772), 73.70% in wave 2 (569/772), and 69.04% in wave 3 (533/772).

Procedure. Across all four waves of data collection, we asked participants to “*think about a workgroup with whom you’re close. A workgroup is defined as an intact group of people who all know each other and work together.*” In wave 0 (collected on April 10), participants

⁷ Notably, the survey was intended to capture a much broader set of research questions, which are reported in the preregistration. All survey items are also available on the OSF Repository for this project (https://osf.io/g6nua/?view_only=f79a6b5037ad49378ca986675a1bbd1b).

⁸ In waves 1-3, we measured current employment status with the following item, “Which best describes your employment situation at the present time?” Participants ($N = 772$) who indicated they were “working for the same company as prior to the COVID-19 pandemic” in all the waves they participated were retained in the sample.

retrospectively recalled their work experiences before the Stay-Home orders.⁹ In waves 1-3, participants reported their experiences with their workgroup “*at the present time*”: wave 1 (April 18–22), wave 2 (May 19–24), and wave 3 (June 22–28). To ensure participants wrote about the same workgroup, in waves 2 and 3, we piped into the survey each participants’ nickname for the workgroup. Participants completed the same measures in each survey wave (but survey items were phrased in the past tense for wave 0 only).

Materials (Survey)

Workgroup commitment. We measured workgroup commitment using four items: (1) “Are you strongly committed to your work group?”; (2) “Are you glad to contribute to your work group?”; (3) “Do you feel strongly affiliated with your work group?”; and (4) “Do you like to help your work group?” (1 = *not at all*, 5 = *extremely*; adapted from Klein et al., 2014, Roccas et al., 2008; $\alpha = .92$).

Workgroup identification. We measured workgroup identification with Bergami and Bagozzi’s (2000) Venn diagram method. Participants viewed seven pairs of circles that ranged from just touching to almost completely overlapping. One circle in each pair was labeled “You,” and the second circle was labeled “Work Group” (see Figure 2 for the full scale). Participants selected the option that described their relationship with their workgroup, where “1” showed non-overlapping circles, and “7” showed almost entirely overlapping circles. Higher numbers indicate the circles are closer to overlapping, indicating higher workgroup identification.

Ritualistic workplace activities. To measure the degree to which individuals engaged in ritualistic workplace activities, we asked participants to:

“Please think about some specific instances of meaningful activities your work group did together in the past few weeks. List at least one and up to three activities below. For

⁹ We asked participants to “*think about a time that was just prior to the COVID-19 pandemic lockdown, such as in early January of this year.*” The wave 0 data collection thus established baseline levels of our measures.

instance, maybe your work group played a game together, performed a warm-up chant together, or ate a meal together. The activities can be remote/virtual.”

For each reported activity (up to three, in order to keep the survey brief), we asked participants to rate the degree to which it was ritualistic by answering the following question: “To what extent do you consider this group activity to be a tradition or ritual in your group? A tradition/ritual is an activity that symbolizes group values and tends to be done multiple times in the same specific way each time” (1 = *not at all*, 5 = *very much*; adapted from Garcia-Rada et al., 2019). Table 2 presents illustrative workgroup activities that participants reported to be high or low in ritualism. To compute our ritual index, for each survey wave we averaged participants’ ratings of the workgroup activities’ ritualism ($\alpha = .71$). Because participants could list zero to three activities ($M = 2.25$ activities, $SD = 0.95$), we substituted missing ritual ratings with 0 if a participant listed less than three activities. The results show an identical pattern when operationalizing ritual as simply the number of activities (i.e., 0, 1, 2, or 3 activities listed; see Appendix B).

Perceived workgroup cohesion. To measure workgroup cohesion, we asked participants to indicate how much their workgroup members: (1) “Share knowledge”; (2) “Have common goals”; (3) “Have strong interpersonal bonds”; (4) “Have shared rules (formal and informal) that you follow together”; (5) “Influence each other”; and (6) “Interact with each other” (1 = *not at all*, 5 = *extremely*; Denson et al., 2006; $\alpha = .87$).

Control variables. To control for variables that past work has shown to correlate with commitment (e.g., Meyer et al., 2002), we report robustness tests controlling for demographic variables (gender, race/ethnicity, age, education, income), individual differences (personality, loneliness, perceived COVID-19 threat, individual COVID-19 infection), and group-level characteristics (in-person communication time, total communication time, intimacy of interactions, group member COVID-19 infection). All survey items are available in Appendix A.

Results

We provide descriptive statistics and zero-order relationships for the study variables and selected demographic variables (across waves) in Table 1.

Analytic strategy. Our longitudinal design allows us to test within-person changes over time. To account for the nested data structure and to model the within-person effects for testing H1-H3, H4a, and H5a, we analyzed the data using multilevel modeling with observations nested within participant and a random slope to allow intra-individual predictors to vary between participants (random intercept and slope model; Aguinis et al., 2013). We centered all intra-individual predictors around the mean for each individual and controlled for the (aggregated) person-means of the intra-individual predictors to isolate within-person (rather than between-person) effects (“within-between formulation”; Bell & Jones, 2015). This within-between approach allows us to remove average variation between persons and focus on within-person variation, which corrects for unobserved heterogeneity bias.

To test H4b and H5b, we employed a path modeling approach which, consistent with our theory, allows us to examine multiple dependent variables and allows for variables to be both independent and dependent in a single analysis (Kline, 2015).¹⁰ To account for the nested data structure, we again employed a within-between formulation and used cluster-robust standard errors. In all analyses, we operationalize the duration of COVID-19 Stay-Home orders as survey waves—0, 1, 2, 3—which represent the length of time since the Stay-Home orders were enacted.

Main Results. Consistent with our theoretical model, we found a negative relationship between the duration of Stay-Home orders and ritualistic workplace activities such that, as the duration of Stay-Home orders increased, ritualistic workplace activities declined (H1), $\beta = -0.25$,

¹⁰ For instance, in H4b, ritualistic activities serve as both a dependent variable (predicted by Stay-Home orders) and an independent variable (predicts perceived cohesion and workgroup commitment).

$SE(\beta) = 0.015$, $t(1150.35) = -16.40$, $p < .001$. We also found, as predicted, a positive relationship between ritualistic workplace activities and perceived cohesion (H2), $\beta = 0.21$, $SE(\beta) = 0.015$, $t(478.07) = 13.71$, $p < .001$, and a positive relationship between perceived cohesion and workgroup commitment (H3), $\beta = 0.27$, $SE(\beta) = 0.013$, $t(376.60) = 20.86$, $p < .001$. And, as we expected (H4a), the duration of Stay-Home orders was associated with a decrease in members' workgroup commitment, $\beta = -0.09$, $SE(\beta) = 0.012$, $t(617.59) = -7.62$, $p < .001$.

Providing support for H4b, the 95% CI for the index of sequential mediation did not include zero, indicating a significant sequential mediation effect of Stay-Home orders on workgroup commitment through ritualistic workplace activities and perceived cohesion (Sequential Mediation Index = -0.025, $SE = 0.003$, 95% CI [-0.032, -0.019]). See Table 3.

To test H5a, we regressed standardized ratings of commitment and identification on duration of Stay-Home orders, an indicator for commitment ("1") or identification ("-1"), and their product term. There was an effect of Stay-Home orders, $\beta = -0.07$, $SE(\beta) = 0.010$, $t(602.63) = -7.21$, $p < .001$, but no effect of commitment/identification, $\beta = 0.01$, $SE(\beta) = 0.017$, $t(868.90) = 0.75$, $p = .455$, which indicates ratings of commitment and identification did not differ at wave 0. As predicted, an interaction emerged, indicating workgroup commitment and identification showed distinct patterns in relation to the duration of Stay-Home orders, $\beta = -0.02$, $SE(\beta) = 0.007$, $t(2856.60) = -2.38$, $p = .017$. Supporting H5a, the relationship between duration of Stay-Home orders and identification was weaker, $\beta = -0.06$, $SE(\beta) = 0.012$, $t(775.73) = -4.56$, $p < .001$, than was the relationship between duration of Stay-Home orders and commitment, $\beta = -0.09$, $SE(\beta) = 0.013$, $t(749.95) = -7.19$, $p < .001$. See Figure 3.

Providing support for our predicted moderated sequential mediation pathway in H5b, the 95% CI for the index of moderated sequential mediation did not include zero, indicating

commitment versus identification moderated the sequential mediation effect of ritualistic workplace activities and perceived cohesion (Moderated Sequential Mediation Index = -0.010, $SE = 0.004$, 95% CI [-0.016, -0.003]). As we predicted, the sequential mediation effect via ritualistic activities and perceived cohesion was stronger on commitment (Indirect Effect = -0.049, $SE = 0.006$, 95% CI [-0.062, -0.037]) than on identification (Indirect Effect = -0.040, $SE = 0.005$, 95% CI [-0.050, -0.030]). See Figure 4 and Table 4.

Robustness analyses. As shown in Table 5, the statistical significance and direction of H1-H5b remain unchanged when controlling for these variables, ruling out an array of alternative explanations for our findings. None of these alternative constructs, such as the amount of time workgroups spend communicating, account for our hypothesized relationships. See Appendix B.

Supplemental Data: Preregistered Replication

We conducted a preregistered survey to replicate our results, recruiting 203 U.S. adults who were working full-time from Prolific Academic (59% male; $M_{age} = 35.49$, $SD_{age} = 9.62$; <https://aspredicted.org/blind.php?x=ji3hf3>). We briefly summarize our findings here and provide complete details in Appendix C. We again surveyed participants about workgroup rituals following the onset of Stay-Home orders in the U.S. (March to June 2020). Replicating the results of our main study, engaging in ritualistic activities was associated with perceived workgroup cohesion (H2), $\beta = 0.32$, $p < .001$, and workgroup commitment, $\beta = 0.41$, $p < .001$. A significant interaction of cohesion $\times$ commitment/identification interaction emerged, $\beta = 0.17$, $p = .002$, such that perceived cohesion was more associated with commitment (H3), $\beta = 0.62$, $p < .001$, than with identification, $\beta = 0.45$, $p < .001$, again supporting (H5b).

Beyond providing a preregistered replication, the supplemental data offer two additional contributions. First, we used a different measure of workgroup identification (Mael & Ashforth,

1992), increasing construct validity. Second, we asked participants to code their workplace rituals to further validate our conception and operationalization of ritualistic workplace activities. Examining the mode value of participants' codes indicates that ritualistic workplace activities after the Stay-Home orders most commonly were: unconnected to consuming food or drink, performed online, social in nature, classified as work meetings (vs. social activities), attended by the entire (vs. part of the) workgroup, created by the manager of the group, and important to the workgroup's values. We summarize participants' responses in Table 6.

General Discussion

The COVID-19 pandemic has been unprecedented in terms of its suddenness and impact on modern employees' experience at work. We used this crisis to advance understanding about employees' commitment to and identification with their workgroup. Analyzing survey responses from 772 U.S. workers at the onset of the COVID-19 Stay-Home orders using a within-person, longitudinal data approach, we demonstrate that: (a) the duration of the Stay-Home orders was negatively related to workgroups engaging in ritualistic workplace activities, which was subsequently associated with lower perceived workgroup cohesion, and ultimately, less workgroup commitment; and (b) workgroup commitment declined more than workgroup identification over the period. Moreover, our analytic strategy, which eliminates between-person variation by focusing on within-person variation, combined with robustness tests that control for a host of employee and workgroup characteristics, provides evidence that our theorized relationships are not driven by likely alternative constructs (e.g., workgroup communication time).

Theoretical and Practical Implications

Our findings offer several theoretical and practical implications. First, our research serves as a timely attempt to capitalize on the COVID-19 pandemic to examine how a sudden, vast, and naturally occurring exogenous shock (Waldinger, 2012) affects workgroup commitment over time. As such, our study reveals insights about commitment that might not have emerged in more contrived laboratory settings or using static research designs, or even dynamic field designs over a stable period of time. Prior research has assumed that commitment is stable once it is developed (e.g., Meyer & Allen, 1997). By showing that workgroup commitment decreased as the duration of Stay-Home orders increased, and by examining the mechanisms underlying the decline in commitment (i.e., engagement in ritualistic activities and perceived cohesion), our work contributes to understanding the time trajectory of commitment following a change in work conditions. In so doing, we identify an essential quality of commitment: it declines as a function of members' reduced ability to engage in ritualistic activities which emphasize the group's cohesion. Our research thus adds to a growing but small literature on how commitment changes over time (e.g., Becker et al., 2012; Kam et al., 2016; Solinger et al., 2015). Practically, our results indicate that employees facing negative workplace events that interfere with their ability to interact with their workgroup, or even increasingly, working more frequently at a distance from their workgroups (e.g., Ashford, Caza, & Reid, 2018), may experience declining workgroup commitment—but that maintaining ritualistic workgroup activities could be a way to buffer against such events and trends.

Second, our results show that, despite their conceptual overlap, commitment appears to be less stable than identification during the COVID-19 Stay-Home orders. Commitment declined more as the duration of Stay-Home orders increased because commitment (vs. identification) is more influenced by perceptions that the workgroup is behaviorally cohesive, which is influenced

by engagement in ritualistic activities. Our results answer recent calls to differentiate the constructs of commitment and identification (e.g., van Rossenberg et al., 2018) and complement past work which has, for example, shown that that identification plays a role in the development of commitment, but not vice versa (e.g., Ellemers et al., 1997; Stinglhamber et al., 2015).

Lastly, we offer insights about the role of ritualistic workplace activities in perceived cohesion and member commitment, a phenomenon that has long been viewed as important in cultivating a strong organizational culture (Trice & Beyer, 1984; Islam & Zyphur, 2009) but rarely studied as influencing workgroup commitment. Our results show how engaging in rituals affects employees' perceptions of workgroup cohesion, which subsequently affects workgroup commitment. Moreover, we provide insight into how ritualistic activities take shape within organizations. While rituals have traditionally been studied from sociocultural perspectives (e.g., Durkheim, 1915; Rappaport, 1999), we show that workgroups can ritualize “mundane” activities (e.g., work meetings) when they make the activities more physically rigid (e.g., occurring at the same time/place) and imbue them with meaning by connecting them to workgroup values.

Limitations and Future Directions

Our research is subject to several limitations that present opportunities for future research. First, despite our longitudinal approach, we cannot rule out threats to causal identification from other forms of endogeneity (e.g., unobserved attributes of individuals that change over time), suggesting that field experiments that manipulate workgroup's ritualistic behaviors and measure subsequent group cohesion and commitment could be helpful. Second, we examined the immediate aftermath (three months post-onset) of the Stay-Home orders based on a specific crisis. Future research could explore the generalizability of our results to non-pandemic and more enduring challenges, examining how and when workgroups engage in

rituals. Given the value of remote work in increasing parity for diverse employees (Leslie et al., 2012), there may be ways to replicate the psychological advantages of rituals in more flexible settings. Third, future research might more directly examine the behaviors associated with workgroup rituals, rather than rely on self-reported evidence, to learn more about the differences in the types of practices that become highly ritualized. Fourth, our research focused on workgroups as a foci of commitment, but changes in commitment to other foci may exhibit a different pace in response to change (Becker, 2009). Future research could examine, for example, whether commitment to professional groups is less subject to decline as a result of engaging in ritualistic behaviors because there is a lower base rate of such behaviors to begin with. Finally, we focused on the mechanisms driving changes in commitment and how changes in identification with the workgroup was less sensitive to these mechanisms. Future research should continue to examine the mechanisms that are distinctly related to changes in identification (Bednar et al., 2020).

Conclusion

Workgroup commitment is an important bond that influences a variety of workplace behaviors. Our study suggests that the sudden and vast restrictions prompted by the COVID-19 pandemic has meaningfully influenced how employees engage with one another, and subsequently reduced employees' workgroup commitment. By offering insight into the reduction in workgroup commitment during a pandemic, we help to better illuminate how commitment operates and build a more robust theoretical understanding of commitment as well as how employees can withstand events that challenge their ability to connect with their workgroup.

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Tables

Table 1

Means, standard deviations, and zero-order correlations with confidence intervals

Variable	<i>M</i>	<i>SD</i>	1	2	3	4	5	6	7	8
1. Commitment	3.88	0.96								
2. Identification	3.82	1.52	.55** [.52, .58]							
3. Ritualistic activities	2.53	1.40	.26** [.22, .29]	.31** [.28, .35]						
4. Perceived workgroup cohesion	3.71	0.78	.73** [.71, .74]	.57** [.54, .60]	.35** [.32, .38]					
5. Female (d)	0.42	0.49	.05* [.01, .09]	-.11** [-.15, -.07]	-.01 [-.05, .03]	.02 [-.02, .06]				
6. Non-White (d)	0.24	0.43	-.03 [-.07, .01]	.05** [.01, .09]	.02 [-.02, .06]	-.01 [-.05, .03]	-.10** [-.13, -.06]			
7. Age	38.03	10.65	.16** [.12, .20]	-.03 [-.07, .01]	-.03 [-.06, .01]	.09** [.05, .13]	.12** [.09, .16]	-.16** [-.20, -.12]		
8. College (d)	0.19	0.39	.04* [.00, .08]	.04* [.00, .08]	.05* [.01, .09]	.02 [-.01, .06]	-.03 [-.07, .01]	.06** [.02, .10]	.12** [.08, .16]	
9. Income	5.08	1.51	.16** [.12, .20]	.08** [.04, .12]	.04* [.00, .08]	.12** [.08, .16]	-.07** [-.10, -.03]	-.01 [-.05, .03]	.10** [.06, .14]	.25** [.21, .28]

Notes: *M* and *SD* are used to represent mean and standard deviation, respectively. Values in square brackets indicate the 95% confidence interval for each correlation. * indicates $p < .05$. ** indicates $p < .01$. (d) = dummy coded variable.

Table 2

Illustrative Workgroup Activities That Participants Reported to be High or Low in Ritualism from the Longitudinal Data

Ritualism	High (5 out of 5 on Likert scale)	Low (1 out of 5 on Likert scale)
	Meeting: “We recited the organization's mission statement at the beginning of a zoom meeting.”	Meeting: “[We] meet to discuss [a] plan for a project.”
	Lunch: “[D]uring key events we planned group lunches to celebrate milestones.	Lunch: “[We have] informal lunches.”
	Conversation: “We have started doing a before work chant. We work 12 hours shifts so it's important for us to feel like a team.”	Conversation: “[We] chat after the shift.”

Table 3

Standardized Coefficients (β), Standard Errors (SE), Z values, and p values for Sequential Mediation Effect on Workgroup Commitment (H4b)

	β	SE	Z value	p value
M1: Ritualistic Workplace Activities				
Stay-Home Orders [X]	-0.30	0.019	16.11	< .001
M2: Perceived Workgroup Cohesion				
Stay-Home Orders [X]	-0.10	0.020	4.91	< .001
Ritualistic Activities [M1]	0.32	0.028	11.07	< .001
Y: Workgroup Commitment				
Stay-Home Orders [X]	-0.02	0.011	-1.83	.067
Ritualistic Activities [M1]	0.02	0.012	1.26	.206
Perceived Cohesion [M2]	0.27	0.016	16.87	<.001

Notes: Employing the within-between formulation (Bell & Jones, 2015), the coefficients represent the expected change in the dependent variable corresponding to a 1 SD change in the predictor relative to the participant's average (within-person effect), controlling for between-person differences in average levels of the predictor. For instance, the 0.32 coefficient linking ritualistic activities and perceived cohesion indicates a typical participant experienced a 0.32 SD unit increase in perceived cohesion when they experienced a 1 SD unit increase in their level of ritualistic activities (compared to the level of ritualistic activities they typically did in the survey time period).

Table 4

Standardized Coefficients (β), Standard Errors (SE), Z values, and p values for Sequential Mediation Effect of Workgroup Commitment versus Identification (H5b)

	β	SE	Z value	p value
M1: Ritualistic Workplace Activities				
Stay-Home Orders [X]	-0.30	0.019	16.13	< .001
M2: Perceived Group Cohesion				
Stay-Home Orders [X]	-0.10	0.020	-4.91	< .001
Ritualistic Activities [M1]	0.32	0.028	11.06	< .001
Y: Ratings of Workgroup Commitment or Identification				
Stay-Home Orders [X]	-0.02	0.015	-1.57	.116
Ritualistic Activities [M1]	0.05	0.019	2.69	.007
Perceived Cohesion [M2]	0.41	0.026	16.19	<.001
Commitment (vs. Identification) [W]	0.00	0.000	0.90	.370
Perceived Cohesion $\times$ Commitment (vs. Identification) [M2 $\times$ W]	0.10	0.034	2.94	.003

Notes: Commitment (vs. Identification) is dummy coded (“1” = commitment, “0” = identification). As in Table 3, the coefficients represent the expected change in the dependent variable corresponding to a 1 SD change in the predictor relative to the participant’s average (within-person effect), controlling for between-person differences in average levels of the predictor (Bell & Jones, 2015).

Table 5*Robustness Results Controlling for Employee and Workgroup Characteristics*

Hypothesis	Relationship	Standardized Coefficient and 95% CI
1	Stay-Home Orders [X] → Ritualistic Activities [M1]	-0.30 [-0.33, -0.27]
2	Ritualistic Activities [M1] → Perceived Cohesion [M2]	0.16 [0.14, 0.19]
3	Perceived Cohesion [M2] → Commitment [Y]	0.26 [0.24, 0.29]
4a	Stay-Home Orders [X] → Commitment [Y]	-0.11 [-0.14, -0.09]
4b	Serial Mediation Indirect Effect on Commitment [X → M1 → M2 → Y]	-0.03 [-0.03, -0.02]
5a	Interaction between Duration of Stay-Home Orders and Commitment/Identification [X × W]	-0.02 [-0.03, -0.006]
5b	Moderated Serial Mediation Indirect Effect on Commitment and Identification [X → M1 → M2 × W → Y]	-0.005 [-0.009, -0.001]

Notes: These analyses control for the following demographic variables (gender [male dummy], race/ethnicity [nonwhite dummy], age, education [four-year degree dummy], income [measured at wave 0]), individual differences (Big 5 personality traits, loneliness, perceived COVID-19 threat, individual COVID-19 infection [dummy]), and group-level characteristics (in-person communication time, total communication time, intimacy of interactions, group member COVID-19 infection [dummy]). All variables, except for the demographic variables and the Big 5 personality traits, are time-varying variables. All non-dummy variables are standardized.

Table 6*Participants' Responses About Their Own Ritualistic Activities (Supplemental Data)*

Survey Question	Survey Response Options	A	B	C
1. Which of the following best describes the activity? The activity:	A) Existed before the COVID-19 stay-at-home orders were enacted and was not changed at all. B) Existed before the COVID-19 stay-at-home orders were enacted but was modified (in some way) due to COVID-19. C) Was invented after the COVID-19 stay-at-home orders were enacted.	10%	47%	43%
2. Following the onset of the COVID-19 stay-at-home restrictions in the United States, the activity was performed:	A) In-person only B) Online only C) Both in-person and online	25%	58%	17%
3. Did the activity involve synchronized physical movements and/or utterances—such as, chanting, shouting, singing?	A) No B) Sometimes C) Yes	86%	7%	7%
4. Did the activity involve consuming food or drink?	A) No B) Sometimes C) Yes	69%	17%	14%
5. To what extent was the activity social?	A) Not at all social B) Somewhat social C) Very social	12%	47%	41%
6. Would you classify the activity as a:	A) Work meeting B) Social gathering C) Other	67%	23%	10%

7. How many people participated in the activity?	A) Part of my workgroup B) All of my workgroup	21% 79%	79% NA	NA
8. Who created the activity?	A) Manager/supervisor B) Self C) The workgroup	58% 15% 16%	15% 16%	16%
9. How often did you engage in the activity during the weeks following the onset of the COVID-19 stay-at-home restrictions in the United States (e.g., March to June 2020)?	A) Just once or a few times B) Once a week C) More than once a week	35% 35% 40%	35% 40%	40%
10. How important was the activity to your workgroup's identity?	A) Not at all important B) Somewhat important C) Very important	3% 33% 64%	33% 64%	64%

Notes: Because we asked participants to tell us about meaningful workplace activities, and not ritualistic activities per se, some participants recalled activities that would not meet our definition of ritual. Thus, to isolate the ritualistic activities in the dataset for this table, we filtered the data to focus on the top half of ritualistic activities (computed via median split). See Appendix C. For question number 8 (who created the activity?), additional survey response options included another peer in the workgroup (8%) and someone outside the workgroup (3%).

Figures

Figure 1

The Theoretical Model

Figure 2*Workgroup Identification Measure*

Notes: Wave 0 replaced “Work Group” with the letter “X.” In the instructions, we told participants that “X” stands for “your work group.” Bergami and Bagozzi (2000) showed this item achieved convergent validity with Mael and Ashforth’s (1992) measure of identification (see also, Bartel et al., 2012; Swann et al., 2009). Moreover, the use of a visual report methodologically separates our measures of commitment and identification and thus potentially reduces common method bias (e.g., Likert-type scale and Venn diagrams) (see Podsakoff et al. 2003).

Figure 3

The Relationship between the Duration of COVID-19 Stay-Home Orders and Standardized Ratings of Commitment and Identification

Notes: The shaded regions reflect 95% confidence interval bands. The y-axis reflects standardized ratings of commitment and identification (endpoints labeled in main text).

Figure 4*Moderated Sequential Mediation (H5b)*

Notes: Commitment (vs. Identification) is dummy coded ("1" = commitment, "0" = identification).

Appendices

Appendix A: Additional Methods

Additional sample recruitment information. We paid participants \$2.50 for completing the wave 0 survey and \$2.00 for each subsequent wave; participants received a \$3.00 bonus for completing all waves. Among the final sample ($N = 772$), $n = 454$ participated in all four waves, $n = 136$ participated in three waves, $n = 93$ participated in two waves, and $n = 89$ participated in one wave. Moreover, when recruiting our sample of $N = 1,000$ participants from Amazon Mechanical Turk, we further required that participants report that English was their native language, correctly answer two attention checks, and report being willing and able to participate in a longitudinal study.

Exclusions. We excluded 40 participants (total; 4.0%) from our study for one of three preregistered reasons (see updated preregistration): (1) writing gibberish for the open-ended text response questions; (2) writing gibberish when writing down the names/initials of group members; or, (3) providing a large number (20+) when asked how many group members have been diagnosed with COVID-19. We ask participants to recall small groups (e.g., six people in size), so large numbers indicate a failure to follow/read the instructions. We additionally excluded 188 subjects for experiencing unstable employment during our survey window; all analyses reported in the main text are based on the remaining 772 participants. We compare the excluded sample to the final sample in Appendix B.

Changes to surveys. We modified one question after wave 1 to remove scientific jargon. Specifically, we used a slightly different wording for the ritualistic activities question in waves 0 and 1: “To what extent do you believe that this activity is a work group ritual? Rituals are

predefined sequences of behaviors (or a single behavior) characterized by rigidity, formality, and repetition that are embedded in a larger system of symbolism and meaning.”

Control variables. We collected the following variables which we control for in the “Robustness analyses” section. Since prior work has examined how negative emotions and COVID-19 threat have influenced work attitudes (e.g., Chong, Huang, & Chang, 2020; Hu, He, & Zhou, 2020; Trougakos, Chawla, & McCarthy, 2020), we test whether our pathway through ritualistic activities and perceived cohesion is robust when controlling for individual differences in perceiving COVID-19 as a threat, individual and group member COVID-19 infection, and loneliness. Moreover, since workgroups that engage in more ritualistic activities might also communicate more and have more intimate interactions (e.g., Sezer et al., 2016), we control for in-person communication time (hours), total communication time (hours), and the intimacy of interactions (Likert rating) to test whether engagement in ritualistic activities uniquely predicts perceived cohesion, workgroup commitment, and workgroup identification.

Demographics. In wave 0, we collected the following demographics: age, gender, race/ethnicity, education (1 = *Some high school or less*, 2 = *High school diploma*, 3 = *Some college*, 4 = *Two-year college degree (A.A.)*, 5 = *Four-year college degree (B.A. or B.S.)*, 6 = *MA/PhD, MD, MBA, Law Degree*, 7 = *NA*), current income (1 = *\$9,999 or less*, 2 = *\$10,000 to \$19,999*, 3 = *\$20,000 to \$29,999*, 4 = *\$30,000 to \$49,999*, 5 = *\$50,000 to \$74,999*, 6 = *\$75,000 to \$99,999*, 7 = *\$100,000 to \$200,000*, 8 = *Greater than \$200,000*).

Personality. In wave 0, we collected individual differences in self-reported Big 5 factors personality (TIPI; Gosling, Rentfrow, & Swann, 2003).

Loneliness. In waves 0-3, we measured loneliness with the following three items: (1) “how often do you feel that you lack companionship?”; (2) “how often do you feel left out?”;

and, (3) “how often do you feel isolated from others?” (1 = *never*, 5 = *very often*; UCLA Loneliness Scale; $\alpha = .91$).

Perceived COVID-19 threat. In waves 1-3, we measured perceived COVID-19 threat with the following five items: (1) “Thinking about the coronavirus (COVID-19) makes me feel threatened”; (2) “I am afraid of the coronavirus (COVID-19)”; (3) “I am worried that I or people I love will get sick from the coronavirus (COVID-19)”; (4) “I am stressed around other people because I worry I’ll catch the coronavirus (COVID-19)”; and, (5) “I try hard to avoid other people because I do not want to get sick” (1 = *not at all*, 5 = *extremely*; Conway et al., 2020; $\alpha = .92$). To retain all data in Table 5, we inputted the average values from waves 1-3 in wave 0.

Individual COVID-19 infection. In waves 1-3, we measured personal COVID-19 infection with the following two items: (1) “Have you been diagnosed with Coronavirus (COVID-19)?”, and (2) “Have you had coronavirus-like symptoms?” (yes/no/not sure). Participants were coded “1” if they responded “yes” to being diagnosed or having coronavirus-like symptoms and 0 otherwise.

Communication time. In waves 1-3, we asked participants to specify the number of hours they spent communicating with their work group members using each of seven communication channels (Schroeder et al., 2017): (1) “In-person conversation”; (2) “Talking back-and-forth on video (e.g., Zoom/Skype/FaceTime)”; (3) “Talking back-and-forth with voice only (e.g., a phone call)”; (4) “Writing back-and-forth with text (e.g., texting on phone, instant messaging, or email)”; (5) “Exchanging one video message at a time (e.g., through Marco Polo or WhatsApp Video)”; (6) “Exchanging one audio message at a time (e.g., WhatsApp voice note)”; and, (7) “Other” with a textbox provided. The first question (in-person conversation) constituted our measure of in-person communication time. To compute total communication time, we summed

the responses across the seven methods to yield the total number of hours communicating. In wave 0, we used a different measure for communication time, asking participants to report the percent of time they communicated with seven communication mediums: “Prior to the COVID-19 pandemic, what percentage of total time spent communicating with work group members used the following platforms? Your answers should sum to 100%.” As we did not ask for the raw number of hours in wave 0, we were unable to incorporate this measure in our analyses. To retain all data in Table 5, we inputted the average values from waves 1-3 in wave 0.

Intimate interactions. In waves 2-3, we measured the perceived intimacy of group conversations using the following item, “In the last week, how connected and close were your interactions with your work group?” (1 = *not at all*, 5 = *extremely*). In waves 0 and 1, we used a slightly different wording for the intimacy question: “how intimate were your interactions with your work group?”

Group member COVID-19 infection. We measured group member COVID-19 infection with the following two items: (1) “How many, if any, members of this work group have been diagnosed with Coronavirus (COVID-19)?”, and (2) “How many, if any, members of your work group have had coronavirus-like symptoms?” Participants were coded “1” if they responded 1 person or more was diagnosed or had coronavirus-like symptoms and 0 otherwise.

Additional measures. The surveys collected additional measures, which were either outside the scope of this paper or only measured in one survey wave (thus not permitting within-person analyses). In addition to asking participants about the status of their workgroups during the onset of the pandemic, we also asked participants about the status of their families and friends. The full surveys are available on the Open Science Framework (https://osf.io/g6nua/?view_only=f79a6b5037ad49378ca986675a1bbd1b).

Appendix B: Additional Results

How do employees with stable employment compare with employees whose employment is unstable? In the main text, we excluded “unstable employees” ($N = 188$) who indicated they were not “Working for SAME company as prior to COVID-19 pandemic” in all the waves they participated. Thus, our sample reported in the main text ($N = 772$) consisted of “stable employees.” Because unstable employees were facing job termination in the midst of a global pandemic, it is unsurprising that the duration of Stay-Home orders in effect over time were associated with a larger decrease in unstable employees’ workgroup commitment, $\beta = -0.25$, $SE(\beta) = 0.03$, $t(765.60) = -9.85$, $p < .001$, than stable employees’ workgroup commitment, $\beta = -0.09$, $SE(\beta) = 0.01$, $t(780.84) = -6.80$, $p < .001$ (interaction: $\beta = 0.16$, $SE(\beta) = 0.03$, $t(768.89) = 5.66$, $p < .001$). Thus, our focus on stable employees during the onset of Stay-Home orders provides a conservative test of our hypotheses.

Are our results robust to a different specification of ritualistic activities? In the main text, we operationalized ritualistic activities by averaging participants’ responses to the 5-point ritual Likert questions. Here, we report an additional specification: the number of activities they listed (“number of rituals”; range: 0-3). Replicating H1 and H2, there was a negative relationship between duration of Stay-Home orders and ritualistic activities (H1; $\beta = -0.36$, $SE(\beta) = 0.02$, $t(812.15) = -21.83$, $p < .001$) and a positive relationship between ritualistic activities and perceived cohesion (H2; $\beta = 0.19$, $SE(\beta) = 0.02$, $t(452.27) = 12.05$, $p < .001$). Moreover, the 95% CI for the index of sequential mediation (H4b) and moderated serial mediation (H5b) did not include zero, indicating a significant serial mediation effect and moderated serial mediation effect, respectively (H4b, Index of Serial Mediation 95% CI [-0.040, -0.023]; H5b, Index of

Moderated Serial Mediation 95% CI [-0.016, -0.002]). Overall, these results indicate that our results are robust to an alternate specification of ritualistic activities.

What are the relationships between duration of Stay-Home order in effect over time and other potential mediator variables? In contrast to the decline in ritualistic activities, workgroup cohesion and workgroup commitment and identification, the duration of Stay-Home orders was associated with more in-person communication, $\beta = 0.02$, $SE(\beta) = 0.01$, $t(1712) = 1.82$, $p = .068$, more total communication time, $\beta = 0.07$, $SE(\beta) = 0.01$, $t(1436) = 5.45$, $p < .001$, and more intimate interactions, $\beta = 0.14$, $SE(\beta) = 0.01$, $t(661.16) = 9.68$, $p < .001$. Moreover, duration of Stay-Home orders was associated with less perceived COVID-19 threat, $\beta = -0.05$, $SE(\beta) = 0.01$, $t(1670) = -6.81$, $p < .001$, indicating that increasing threat (and associated emotions, such as anxiety) are not responsible for the decline in workgroup commitment. Altogether, these results indicate that employees were able to adjust to some of the workgroup challenges posed by the Stay-Home orders (e.g., evidenced by increased intimacy of interactions and decreased perception of COVID-19 threat) as the duration of Stay-Home orders increased. However, as our results in the main text show, employees were not able to accommodate ritualistic activities, resulting in decreased workgroup commitment.

Appendix C: Supplemental Data

Appendix C reports the full details for the supplemental data collection. The survey was preregistered on AsPredicted (<https://aspredicted.org/blind.php?x=ji3hf3>).

Participants. We predetermined our sample size to target 200 participants. To participate, we required that potential participants work full-time for the same organization, with the same workgroup, without disruption (i.e., not furloughed) from January 2020 to the present

day (January 2021). In total, 203 adults from the U.S. on Prolific Academic (121 male, 82 female; $M_{age} = 35.49$, $SD_{age} = 9.62$) completed the study in return for \$1.60.

Procedure. We asked participants to recall a meaningful activity shared with their workgroup: “Please think about a specific meaningful activity your workgroup did together during the weeks following the onset of the COVID-19 stay-at-home restrictions in the United States (e.g., March to June 2020). The activity can be in-person or remote/virtual.” We asked participants to elaborate on the activity with a writing prompt (e.g., What was the activity? Who participated in the activity?). Participants then completed our self-report questionnaire (details below).

Materials (Survey)

Engagement in ritualistic activities. As in the main survey, we asked participants “To what extent do you consider this activity to be ritualistic? Ritualistic workplace activities symbolize group values and tend to be done multiple times in the same specific way each time” (1 = *not at all ritualistic*, 7 = *very ritualistic*). To augment this measure, we also asked participants the following three-item ritual scale: “To what extent” (1) “is the activity done in the same specific way each time?”, (2) “is the activity meaningful to you?”, (3) “does the activity symbolize your workgroup values?” (1 = *not at all*, 7 = *very much*; $\alpha = .59$). As preregistered, we standardized the single-item and three-item measures and averaged them together to yield a standardized ritualism index. While we used a 5-point Likert scale in the longitudinal survey, we opted for a 7-point Likert scale in the cross-sectional replication to increase variation in responses.

Characteristics of ritualistic workplace activities. To conceptualize ritualistic activities, we asked participants the following questions: (1) “Which of the following best describes the

activity”: (a) “The activity existed before the COVID-19 stay-at-home orders were enacted and was not changed at all,” (b) “The activity existed before the COVID-19 stay-at-home orders were enacted but was modified (in some way) due to COVID-19,” (c) “The activity was invented after the COVID-19 stay-at-home orders were enacted”; (2) “Following the onset of the COVID-19 stay-at-home restrictions in the United States, the activity was performed”: (a) “In-person only,” (b) “Online only,” (c) “Both in-person and online”; (3) “Did the activity involve synchronized physical movements and/or utterances—such as, chanting, shouting, singing”: (a) “No,” (b) “Sometimes,” (c) “Yes”; (4) “Did the activity involve consuming food or drink”: (a) “No,” (b) “Sometimes,” (c) “Yes”; (5) “To what extent was the activity social”: (a) “Not at all social,” (b) “Somewhat social,” (c) “Very social”; (6) “Would you classify the activity as a”: (1) “Work meeting,” (b) “Social gathering (e.g., happy hour, lunch, party),” (c) Other (please describe); (7) “How many people participated in the activity”: (a) “Part of my workgroup,” (b) “All of my workgroup”; (8) “Who created the activity”: (a) “Manager/supervisor,” (b) “I did,” (c) “The workgroup,” (d) “Another peer in the workgroup,” (e) “Someone outside the workgroup,” (f) “Other”; (8) “How often did you engage in the activity during the weeks following the onset of the COVID-19 stay-at-home restrictions in the United States (e.g., March to June 2020)”: (a) Just once, (b) “A few times,” (c) “Once a week,” (d) “More than once a week”; (9) “How important was the activity to your workgroup's identity”: (a) “Not at all important,” (b) “Somewhat important,” (c) “Very important.”

Perceived cohesion. We asked participants to rate the perceived cohesion of the workgroup “after participating in the activity” using the same six-item scale reported in the main study (1 = *not at all*, 7 = *very much*; Denson et al., 2006; $\alpha = .84$).

Workgroup commitment. We asked participants to rate their commitment to the workgroup “after participating in the activity” using the same four-item scale reported in the main study (1 = *not at all*, 7 = *very much*; adapted from Klein et al., 2014, Roccas et al., 2008; $\alpha = .94$).

Workgroup identification. We asked participants to rate their identification to the workgroup “after participating in the activity” using a modified version of Mael and Ashforth’s (1992) Likert scale: (1) “If someone were to criticize this workgroup, it would feel like a personal insult to me”; (2) “When I talked about this workgroup, I would say ‘we’ rather than ‘they’”; (3) “This workgroup’s successes were my successes”; (4) “If someone praised this workgroup, it would feel like a personal compliment to me”; (5) “If a story in the media criticized this workgroup, I would feel embarrassed” (1 = *not at all*, 7 = *very much*; $\alpha = .84$).

Results and Discussion

As predicted, and consistent with the main study reported in the main paper, engagement in ritualistic activities (*ritualism* index) was positively related to perceived workgroup cohesion (H2), $\beta = 0.32$, $SE(\beta) = 0.07$, $t(201) = 4.76$, $p < .001$, and perceived workgroup cohesion was positively related to workgroup commitment (H3), $\beta = 0.70$, $SE(\beta) = 0.06$, $t(201) = 11.23$, $p < .001$. Providing support for H4b, workgroup commitment and identification showed differential associations with perceived workgroup cohesion, as evidenced by a significant interaction between perceived cohesion and standardized ratings of commitment and identification, $\beta = 0.17$, $SE(\beta) = 0.05$, $t(201) = 3.18$, $p < .001$. Decomposing the interaction revealed the association between perceived workgroup cohesion and commitment, $\beta = 0.62$, $SE(\beta) = 0.06$, $t(302) = 10.47$, $p < .001$, was stronger than the association between perceived workgroup cohesion and identification, $\beta = 0.45$, $SE(\beta) = 0.06$, $t(302) = 7.54$, $p < .001$.

Moreover, further supporting H5b, in a moderated mediation model (X = ritualistic activities, M = perceived cohesion, W = commitment or identification, Y = standardized ratings of commitment or identification), the 95% CI for the index of moderated mediation did not include zero, indicating commitment versus identification moderated the mediation effect of perceived cohesion (Index of Moderated Mediation = 0.055, SE = 0.021, 95% CI [0.013, 0.097]). Perceived cohesion more strongly mediated the relationship between engagement in ritualistic activities and workgroup commitment (Indirect Effect = 0.176, SE = 0.048, 95% CI [0.082, 0.270]) than the relationship between engagement in ritualistic activities and workgroup identification (Indirect Effect = 0.121, SE = 0.038, 95% CI [0.046, 0.196]).

This replication, using a different measure of identification, supports our theory and bolsters our findings that perceived workgroup cohesiveness influences workgroup commitment more than workgroup identification. Moreover, these results provide more insight about the nature of ritualistic workplace activities in workgroups (see Table 6).