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By

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ABSTRACT

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From the Walmart Cheer to the Cold Stone Creamery Tip Song, many groups engage in their own rituals (i.e., predefined sequences of symbolic actions). Despite the prevalence of rituals in group settings, the consequences of group rituals have rarely been studied by organizational researchers. This is surprising, given the potential for rituals to influence meaningful organizational outcomes. In this dissertation I develop a comprehensive theory of group rituals, including offering a more refined definition of rituals as compared to similar constructs such as norms, identifying how group rituals influence the key group construct of member commitment, and shedding light on a collateral influence that has not been considered before, the outsiders' perceptions of the group. I aim to show that group rituals physically represent the group's most important values and can consequently influence behavior in group settings. In particular, I examine the relationship between group rituals and group commitment among existing members and prospective members.

Among existing group members, I consider the bi-directional relationship between group rituals and group commitment. I suggest that engagement in rituals increases member commitment because rituals stand for the group's values, such that the enactment of rituals makes the group appear as a cohesive entity to members. Furthermore, I suggest that more (vs. less) committed members will be more likely to protect the group's rituals (e.g., punish those who attempt to alter the rituals), ensuring that rituals are faithfully executed over time within the group. In this way, rituals and commitment to the group reinforce each other in a recursive cycle, such that (a) rituals promote member commitment and (b) higher levels of member commitment promote adherence to rituals (yielding a "cycle of commitment").

Among prospective group members, I consider how observing the enactment of rituals influences prospective members' own willingness to commit to the group. I suggest that, because group rituals physically represent the group's values, they signal that the group has committed members to outsiders. I propose that perceptions that members are committed to a group can signal an elevated capacity to reach group goals, which will have a positive impact on own willingness to commit among those who share the values of the group (i.e., subjective value congruence) but a weaker or even negative impact among those who do not share the values of the group (i.e., subjective value incongruence). In this way, I theorize rituals promote commitment because rituals attract prospective members who are willing to be committed to the

group. This indicates that potential new members who perceive themselves as value-incongruent (such as demographically underrepresented individuals) will be less willing to commit to a group that engages in rituals.

The theory of group rituals and group commitment that I propose in this dissertation suggests that rituals promote commitment among existing members and prospective members with congruent values, which can often be beneficial, but, that a somewhat hidden cost is that they can also exclude prospective members who perceive themselves as value-incongruent with the group (such as demographically underrepresented individuals). I conducted eight studies that test the relationship between group rituals and member commitment using a mix of experimental and observational (e.g., longitudinal survey) research. Studies 1 and 2 tested the effect of workgroup rituals on employee commitment among U.S. workers in the context of the COVID-19 pandemic. Studies 3 and 4 examined national and cultural rituals, investigating whether more (vs. less) committed group members are most likely to protect the group's rituals from people who attempt to alter them. Finally, Studies 5, 6, 7, and 8 examined how the enactment of rituals in organizational settings interacts with values of the group to influence prospective group members' own willingness to commit to the group.

The results offer support for the bi-directional relationship between group rituals and group commitment among existing group members. On one hand, engagement in rituals increases commitment to the group (Studies 1-2). On the other hand, more (vs. less) committed group members are more likely to promote adherence to rituals by punishing those who attempt to alter them (Studies 3-4). In this way, rituals and commitment reinforce each other in a cycle among existing group members.

However, among prospective group members, the results offer mixed support. While I found some evidence that rituals signal the group has committed members (Studies 5, 7, 8), I do not find robust evidence that rituals influence an outsider's willingness to commit to the group (Studies 7-8). Accordingly, as value congruence did not moderate the effect of rituals on prospective member's own willingness to commit to the group, I also do not find evidence that people who are prone to perceive themselves as value-incongruent (e.g., demographically underrepresented individuals) are less willing to commit to a group that engages in rituals (Study 7). Thus, the hypothesized negative consequence of rituals—rituals exclude underrepresented minority individuals—was not supported.

In summary, this dissertation proposes and tests a theory of group rituals and group commitment, providing one of the first empirical investigations of rituals in organizational behavior. By conceptualizing rituals within organizational behavior and testing for their relationship with group commitment, future research can build on this dissertation to deepen our understanding of the functions of rituals in organizational setting.

ROADMAP FOR THIS DISSERTATION

In Chapter 1, I develop a comprehensive theory of the psychology of group rituals and group commitment, articulating 11 hypotheses. I begin by reviewing definitions of key constructs and the prior literature on rituals and commitment. Building on this past research, I theorize that rituals are important for promoting a group's shared values. As a result, I propose that strong (vs. weak) rituals (which reinforces the group's value system) increase existing members' commitment to the group and prospective members' willingness to commit to the group (as long as the prospective members believe the values of the group are congruent with their own values). I also consider how more (vs. less) committed group members are more likely to protect the group's rituals (i.e., by punishing those who attempt to alter the rituals) because more committed members care more about the group values that the rituals represent. In this way, I theorize rituals and commitment reinforce each other in a recursive cycle.

Chapters 2 through 4 include the methods and results for eight studies. In Chapter 2, two field studies of working professionals (Studies 1 and 2) find evidence that engagement in strong (vs. weak) workgroup rituals increase employees' commitment to their workgroup. In Chapter 3, two experiments (Studies 3 and 4) focused on sociocultural rituals find that more (vs. less) committed group members are more likely to protect their group's rituals by punishing those who attempt to alter them. In Chapter 4, four studies (Studies 5, 6, 7, and 8) situated in organizational contexts tested whether prospective group members are more willing to commit to a group that engages in strong (vs. weak) rituals.

Finally, Chapter 5 summarizes the findings and conveys how this dissertation contributes to existing research on rituals (e.g., Trice & Beyer, 1984), culture change (e.g., Chatman, 2021), and member commitment (e.g., Mowday et al., 1982). Chapter 5 also proposes a plan for a future research program on the functions of rituals in organizational settings. Specifically, Chapter 5 lays out theoretical insights and empirical roadmaps that link rituals with group diversity and with remote work.

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CHAPTER 1: A THEORY OF GROUP RITUALS AND GROUP COMMITMENT

"No society is devoid of what a reasonable observer would recognize as ritual..." - Anthropologist Roy Rappaport (1999, p. 31)

From social events like weekly "happy hours" among co-workers to certain ways of enacting meetings to make them more structured, to specific celebrations for workplace achievements like promotion ceremonies, rituals (i.e., predefined sequences of symbolic actions) pervade group settings. Despite the prevalence of rituals, very little is known about the organizational consequences of engaging in group rituals. That is not to say, however, that rituals have not been studied among social scientists. Rather, rituals have been a popular topic among cultural anthropologists and sociologists, who have identified the macro-level functions of ritual using cultural analyses and ethnographic research (e.g., Collins, 2014; Durkheim, 1915; van Gennep, 1909). More recently, research in psychology has started to investigate the functions of ritual, focusing on individual rituals (e.g., Norton & Gino, 2014; Vohs et al., 2013). In this dissertation I build on prior anthropological, sociological, and psychological accounts of ritual to advance our organizational understanding of group rituals, offering a theoretical model that bi-directionally links group rituals and group commitment.

Specifically, in my dissertation, I examine one potential way to promote member commitment: develop group rituals. I focus on the association between group rituals and group commitment because prior research has established that group commitment is a consequential outcome in the workplace. Committed employees—those with feelings of dedication and responsibility for the organization—are not only better employees (e.g., exhibiting stronger performance, higher organizational citizen behavior, lower absenteeism) but also happier ones (e.g., higher psychological well-being, stronger physical health; Meyer & Maltin, 2010; O'Reilly & Chatman, 1986). Indeed, at least one study (Sorenson & Garman, 2013) estimated that employees who lack commitment cost their organization $3,400 in lost productivity for every $10,000 of salary (or 34% of salary), suggesting that a low-commitment employee who makes $53,000 a year (approximately the average U.S. annual wage[1]) will cost their employer $18,020 a year in lost productivity. This money may come out of taxpayer dollars when in government offices, shareholder value as in the case of Fortune 500 companies, or in increased medical costs in the case of hospitals and medical offices. Thus, organizations may gain a competitive advantage by cultivating their employees' commitment. Moreover, recent labor market trends (e.g., increased job mobility in the "Great Resignation"; Klotz, 2021) in the United States have further highlighted the importance of commitment.

To understand how rituals manifest in the workplace, I offer two examples of rituals. Pathways to Education—a non-profit organization based in Toronto, Canada—hosts "Crockpot Mondays," a weekly communal meal shared among co-workers. Every Monday, another employee shares their culinary talents and—to be progressive—all meals are vegan and gluten-free. Or consider Talent Plus—a consulting firm based in Lincoln, Nebraska—which hosts a cocktail hour at the end of every new employee's first day. To signal supportiveness, the new

[1] https://www.ssa.gov/oact/cola/central.html

employee's family are also invited to the cocktail hour. In contrast to longstanding sociocultural rituals (e.g., religious ceremonies), these examples illustrate that groups and organizations are capable of crafting their own unique rituals

In the remainder of this section, I provide a preview of my theory before conceptualizing the key constructs and building the theoretical arguments. The current research considers the two-way association between group rituals and group commitment. In particular, I examine the relationship between rituals and commitment at two stages of group membership: among existing group members (the "internal pathway") and prospective group members (the "external pathway"). While existing members psychologically identify as being a member of the group, prospective members do not identify as being a member of the group and have yet to decide whether to establish a relationship with the group (Brown & Pehrson, 2019; Levine & Moreland, 1994). Because existing and prospective group members have different concerns, it is possible that they view rituals differently. However, the focus is still on rituals and commitment for both the internal and external pathways, which is why both pathways are included in a single theoretical model.

Among existing employees, I suggest that because group rituals can embody group values, the performance of rituals signal that the group is cohesive to members, which increases member commitment (internal pathway). I also consider how more committed members can promote the groups' rituals. Because group rituals can physically represent group values, I argue that more (vs. less) committed members (who more strongly internalize the group's values) will also care more about the group's rituals; as a result, more committed members will be more likely to protect the group's rituals, ensuring that rituals are faithfully executed over time. In this way, among existing members, I theorize rituals and commitment continuously reinforce each other in a recursive cycle.

I further consider how observing the enactment of rituals influences prospective members' (e.g., job applicants') own willingness to commit to the group (external pathway). Because group rituals can physically represent the group's values, I argue that rituals signal to outsiders that the group has committed members. I argue that a group that has committed members is also a group perceived to have an elevated capacity to reach group goals, which is desirable when one perceives the values of the group match one's own values (subjective value congruence) but undesirable (or even threatening) when one perceives the values of the group do not match one's own values (subjective value incongruence). As a result, among prospective members who perceive value congruence, I argue that observing strong (vs. weak) rituals will increase a prospective member's own willingness to commit to the group; however, among prospective members who perceive value incongruence, I propose that strong (vs. weak) rituals will have a weaker, or even negative, effect on own willingness to commit. In this way, I theorize rituals promote commitment by attracting prospective members who will be committed to the group. In doing so, I also test some negative consequences of rituals in prospective new member recruitment, suggesting that demographically dissimilar individuals may perceive a lack of shared values and thus be less willing to commit to a group that engages in strong (vs. weak) rituals.

Note that there are theoretical similarities between the internal and external pathways. Both pathways build on the theoretical insight that group rituals physically represent the group's values to formulate hypotheses about the effect of rituals on group commitment. The theoretical overlap is intentional as I build a theory of group rituals and group commitment that encompasses both existing and prospective members. Despite the theoretical overlap, I do theorize differences between the internal and external pathways which I empirically test. For instance, while the internal pathway focuses on rituals' *actual* impact on existing member commitment, the external pathway concerns how *beliefs* about rituals' impact on existing members may influence prospective members.

This dissertation contributes to existing research in at least three ways. First, I offer insights about the role of rituals, a phenomenon that has been viewed as an artifact of a strong culture (Schein, 2010; Trice & Beyer, 1984) but rarely studied as a causal driver of behavior in group settings. In doing so, I provide a clear definition of ritual that allows us to identify its unique effect on member commitment, independent of the effects of other constructs related to rituals. Moreover, complementing past work which has focused on the macro-level outcomes of rituals using qualitative research methods (e.g., Sosis, 2004), I contribute to our understanding of the organizational functions of ritual using experimental methods which allow tests for causality. Second, my work contributes to our understanding of how to effectively change group culture (e.g., Chatman, 2021; O'Reilly & Chatman, 1996), highlighting that culture change that starts with changing enacted behavior—that is, the rituals—will be counterproductive. Third, my work expands our understanding of group commitment by studying individuals' propensity to be committed before joining the group (e.g., Mowday et al., 1982; Pierce & Dunham, 1987). Specifically, my work shows that learning about rituals before joining the group can lead prospective members to experience pre-entry commitment.

Conceptualizing Rituals

This section unfolds as follows. First, I provide a definition of ritual, building on a rich history in anthropology, sociology, religious studies, and psychology. Second, I discuss how rituals relate to other constructs in the academic literature, showing that no existing construct in the organizational behavior literature fully encompasses the construct of ritual. Lastly, I review established micro-level antecedents and consequences of rituals, identifying holes in the existing literature.

Defining Rituals

Brief history of the construct. Although there is a record of humans participating in rituals since the beginning of recorded history, it was not until the nineteenth century that researchers began to consider ritual a topic of academic inquiry. Early theorists debated whether myths give rise to rituals or whether rituals give rise to myths (e.g., Müller, 1861). While ritual was viewed as an obligatory behavior (i.e., the thing done), myth was viewed as a recited story that attempted to explain and understand the world (i.e., the thing said; Robertson, 1889). This controversy aimed to resolve the larger question on the origin of religion, proposing that religion is either originally rooted in ritual or myth. Despite disagreeing whether rituals or myths are relatively

more stable, these researchers generally concluded that rituals and myths are mechanisms that people use to make sense of the human experience (Bell, 1997).

In the early twentieth century, researchers turned their attention to how rituals impose order in a group. Durkheim (1915) famously argued that rituals are "rules of conduct" (p. 52) that govern how people interact with sacred objects, which subsequently promote group unity by enabling members to identify with each other. Durkheim subsequently inspired many other researchers to consider the social functions of rituals. Radcliffe-Brown (1945), for instance, argued that rituals are "regulated symbolic expressions of certain sentiments" that reaffirm the social order by "transmit[ting] from one generation to another sentiments on which the constitution of the society depends" (p. 35). Rappaport (1968) extended the Durkheimian view of ritual by suggesting that rituals regulate not only the social world but also the physical world, defining rituals as "the performance of more or less invariant sequences of formal acts and utterances not entirely encoded by the performers" (p. 24).

Up until the middle of the twentieth century, rituals were defined primarily by their connection to Durkheim's notion of the sacred (Bell, 1997). While sacred primarily referred to religious acts, it is important to note that researchers considered non-religious acts as sacred as well. For instance, anthropologist van Gennep (1909) focused on rituals that accompany life transitions, such as births, marriages, and initiation ceremonies, and argued that such transitions mark passageways between the "profane" (secular) and the sacred.

However, in the middle of the twentieth century, researchers expanded the concept of ritual to include secular activities. Drawing on observations of mental patients in a research hospital, Goffman (1956) described a ritual by writing "this activity, however informal and secular, represents a way in which the individual must guard and design the symbolic implications of his acts" (p. 478). Similarly, anthropologist Max Gluckman (1962) defined rituals as "stylized ceremoni[es] in which persons… perform prescribed actions according to their secular roles" (p. 24). Henceforth, the study of ritual included secular and sacred acts, ranging from informal greeting rituals to formal religious ceremonies (e.g., Bell, 1997). In this way, sacred (religious) rituals can derive meaning from religious belief, while secular (non-religious) rituals can derive meaning from societal values such as justice, freedom, respect, community, and responsibility.

Consistent with the notion that rituals can constitute sacred or secular acts, recent research in developmental psychology has defined rituals as "conventional, causally opaque procedures" (Legare & Souza, 2012, p. 1; see also, Atkinson & Whitehouse, 2011). Actions are causally opaque when the specific action performed (e.g., dancing) and the desired outcome (e.g., making it rain) lack an observable, causal connection (e.g., Kapitány & Nielsen, 2015; Legare & Souza, 2014). This perspective—that rituals are defined by causal opacity—is based on early theorists distinguishing ritual from other actions through its nonrational qualities (e.g., Malinowski, 1948; Tylor, 1881). For instance, according to the causal opacity perspective, washing one's hands once is not a ritual because it is necessary to achieve a physical objective (e.g., cleanliness), but washing one's hands many times is not necessary to achieve a physical objective and is thus a ritual (e.g., Boyer & Liénard, 2006; Freud, 1963).

Despite the intensity of research on rituals across the social sciences as reviewed in this subsection, there has been minimal research on rituals in organizational behavior. This is surprising, given that rituals are pervasive in organizations and have the potential to influence meaning organizational outcomes. In notable exceptions, some researchers have conceptualized rituals as a manifestation of organizational culture (e.g., Schein, 2010; Trice & Beyer 1984) or as an element of newcomer socialization (e.g., Hallier & James, 1999; van Maanen & Schein, 1979; Vaught & Smith, 1980). More recent research has examined how individuals use rituals to manage the transition from work to home (Ashforth et al., 2008) and how rituals can support the maintenance of institutions in society (Dacin et al., 2010). This research has used qualitative research methods (Islam & Zyphur, 2009), which make it difficult to draw cause-and-effect relationships. The research has also only examined rituals in particular content domains (e.g., air traffic controllers; Hallier & James, 1999) or specific types of rituals (e.g., rituals as a role transition; Ashforth et al., 2008). By studying rituals as a separable phenomenon worthy of its own independent investigation, this dissertation aims to build a theory of group rituals and commitment that generalizes across groups and types of rituals.

Working definition of ritual. As the above review of the history of research on rituals illustrates, there is variation in how researchers have defined rituals. Table 1 summarizes the definitions of ritual across disciplines. As can be seen in Table 1, there are at least two themes that emerge in the definitions. First, rituals have a specific set of *physical* actions that tend to be structured in rigid, sequenced, and repetitive ways (e.g., Durkheim, 1915; Gluckman, 1962; Rappaport, 1968). Second, rituals carry significant *psychological* meaning to the performer, constituting symbolic expression (e.g., Goffman, 1956; Radcliffe-Brown, 1952; van Gennep, 1909).

Table 1

Select Ritual Definitions in the Academic Literature (in Chronological Order)

Reference	Discipline	Definition
Turner (1967, p. 19)	Anthropology	A ritual is "prescribed formal behavior for occasions not given over to technical routine, having reference to beliefs in mystical beings and powers."
Tambiah (1979, p. 119)	Anthropology	A ritual "is a culturally constructed system of symbolic communication. It is constituted of patterned and ordered sequences of words and acts, often expressed in multiple media, whose content and arrangement are

		characterized in varying degree by formality (conventionality), stereotypy (rigidity), condensation (fusion), and redundancy (repetition)."
La Fontaine (1985, p. 11)	Anthropology	Rituals are "the acts performed in an ordered sequence which has purpose and meaning for the people concerned."
Kertzer (1988, p. 1988)	Anthropology	A ritual is "symbolic behavior that is socially standardized and repetitive."
Douglas (2003, p. 9)	Anthropology	Ritualism is "a concern that efficacious symbols be correctly manipulated and the right words be pronounced in the right order."
Firth (1951, p. 222)	Ethnology	A ritual is a "kind of patterned activity oriented to control of human affairs, primarily symbolic in character with a non-empirical referent, and as a rule socially sanctioned."
Rook (1985, p. 252)	Marketing	"The term ritual refers to a type of expressive, symbolic activity constructed of multiple behaviors that occur in a fixed, episodic sequence, and that tend to be repeated over time."
Trice & Beyer (1984, p. 655)	Organizational Behavior	A ritual is a "standardized, detailed set of techniques and behaviors that manage anxieties, but seldom produce intended, technical consequences of practical importance."

Erikson (1966, p. 337)	Psychology	A ritual is "an agreed-upon interplay between at least two persons who repeat it at meaningful intervals and in recurring contexts."
Zuesse (1987, p. 405)	Religious Studies	Rituals are "conscious and voluntary, repetitious and stylized symbolic bodily actions that are centered on cosmic structures and/or sacred presences."
Bourdieu (1977, p. 92)	Sociology	A ritual is "a performative practice that strives to bring about what it acts or says."

Notes: This list is far from exhaustive; the list is intended to show that past definitions of ritual have included predefined physical behaviors and symbolic meaning. For the purpose of this list, rituals and rites are viewed as interchangeable.

Thus, I build on previous research to conceptualize *rituals* as (a) *predefined sequences of action characterized by rigidity, sequence, and repetition that are* (b) *embedded in a larger system of symbolism and meaning* (Stein et al., 2021). This definition is not only consistent with the way rituals have been defined in the past (Table 1) but also consistent with a recent definition proposed in the social psychological literature (Hobson et al., 2018). Note that this definition encompasses both religious and secular acts, such that any action embedded in a system of meaning with behavioral specificity can constitute a ritual. See Table 2 for additional examples of rituals in organizational settings that possess both the physical and psychological features of ritual.

Table 2

Examples of Rituals in Organizations

Organization	Activity	Physical Features of Ritual	Psychological Features of Ritual
Ruby Receptionists (Portland, Oregon, USA)	Fashion Friday	Each Friday, employees dress according to a chosen fashion theme.	The activity represents two of Ruby's core values: "Foster Happiness" and "Create Community."
Village Coffee Shop	Village Virginity	The staff welcomes	The activity serves as

(Boulder, Colorado, USA)		every new customer by telling guests, “We have a Village Virgin in the house!”, which is received with a round of applause.	an “initiation” into the Village Coffee community.
Stickyeyes (Leeds, United Kingdom)	Nerf Gun Gong Shoot	Team members mark the completion of a project with the shooting of a nerf gun at a gong.	The activity encourages team members to reflect, refresh, and reconnect before jumping to the next project.
Huge (Brooklyn, New York, USA)	Axe Club	Employees that work for Huge (an advertisement agency) for 5 years join the “Axe Club” and receive a custom axe.	The axe represents Huge's approach to work: If you don't love what you created, then take the “metaphorical” axe to it and start again.
R/GA (San Francisco, California, USA)	5×5 Storytelling Night	On the fifth day of each month, at 5 pm, five employees speak for 5 minutes (each) about a turning point in their lives while the rest of the organization listens.	This activity reinforces R/GA’s value of being “your authentic self” at work.

While causal opacity (defined earlier as an action that lacks an observable causal connection) is not a defining feature of ritual, causal opacity is one route by which a behavior can take on meaning to the performer. When actions are causally opaque, individuals tend to search for meaningful interpretations to explain the behavior, thus inviting individuals to infer symbolism (e.g., Legare & Watson-Jones, 2015; Whitehouse, 2004). In this way, actions become symbolic when non-instrumental explanations, rationalizations, and legitimations are used to undertake the behavior in a group (Pfeffer, 1981). For instance, imagine a workplace meeting always involves eating doughnuts. The doughnuts are not logically necessary for the successful completion of the meeting. The lack of an instrumental reason for the doughnuts can lead the doughnuts to take on additional meanings, representing different group values (e.g., be sweet to others). Thus, a set of causally opaque actions that become imbued with meaning—when performed in a rigid, repetitive way—is one example of a ritual.

Ritualism on a spectrum. Critical to the definition of ritual is that similar activities can be stronger or weaker rituals depending on their physical and psychological features. For example, even a simple work meeting can be transformed from a weaker ritual into a stronger ritual if it modified to include the physical features of a ritual—such as having a specified location or way of starting the meeting—and the psychological features of a ritual—such as affirming different values of the group. In this way, I operationalize ritual as a spectrum, not as binary, and thus distinguish between *strong rituals* (those group activities that have relatively *more* physical and psychological features of ritual) and *weak rituals* (those group activities that have relatively *less* physical and psychological features of ritual). Viewing rituals as a spectrum (ranging from strong to weak) is consistent with research in cognitive psychology, which suggests that concepts can have a relative degree of belonging to a conceptual category (such that strong rituals have a greater degree of belonging to the construct ritual compared to weak rituals) instead of defining concepts by necessary and sufficient conditions (e.g., Rosch, 1975).

As another example, consider the Walmart Cheer. The Walmart Cheer has the physical features of ritual (e.g., employees sing the structured chant three times a day, at the same time each day) and psychological features of ritual (e.g., the chant ends with affirming service to the customer, which is one of Walmart's core values; Bergdahl, 2004). If another organization's cheer has relatively fewer physical features of ritual (e.g., employees sing the chant whenever they are bored) and relatively fewer psychological features of ritual (e.g., the chant is loosely based on organizational values), then the Walmart Cheer is a stronger ritual in comparison. As this example illustrates, seemingly mundane activities (such as a cheer) can become a strong (vs. weak) ritual if it becomes rigid and linked to the group's value system (e.g., supporting the customer).

Distinguishing group vs. individual rituals. Rituals (as predefined sequences of symbolic actions) can definitionally operate at the group or individual level. The critical feature that distinguishes a group ritual from an individual ritual is whether the ritual originated from the group or an individual. Group rituals originate from the group context whereas individual rituals are developed by the individual performer. For instance, if an individual makes an oath before going to sleep, but the oath originated from a group, then the individual is engaging in a group ritual even if the ritual enactment is done in a private setting. As another example, consider an individual who always drinks a particular coffee before starting a work brainstorming session. If the individual created the ritual without input from the group, then it is an individual ritual even though it is performed in the physical presence of group members. However, if the individual learned about the ritual from other group members, then it would be a group ritual.

In contrast to group rituals, which are often linked to a group's value system, the meaningfulness of an individual ritual—for example, an athletes' pre-performance ritual—may be more idiosyncratic to the individual (e.g., Brooks et al., 2016). Since the basis of my theory is identifying the connection between rituals and group values, and because individual rituals are (usually) not connected to a group's value system, my theorizing and dissertation research focus exclusively on group rituals. I define a group as "two or more people possessing a common social identity" (Brown & Pehrson, 2019, p. 11). As a result, members of the same workgroup, organization, nation, or even religion are all members of the same group and thus included in my theorizing. In this way, I test my predictions using a host of different rituals and groups for

generalizability, aiming to build a theory of ritual and commitment that applies across rituals and groups.

Rituals and Related Constructs

In this section, I compare and contrast rituals from related constructs because doing so highlights the uniqueness of rituals compared to other activities and guides research to develop manipulations and measures which are consistent with the definition of ritual (and not related constructs), establishing construct and discriminant validity (Campbell & Fiske, 1959). Construct and discriminant validity are important because these types of validity enable me to theorize and test a unique relationship between rituals and commitment, above and beyond relationships with related constructs. In this section, I discuss the similarities and differences between rituals and habits, meaningful work, values, and culture (which includes social norms).

Habits. Habits are "learned dispositions to repeat past responses" (Wood & Neal, 2007, p. 843). Examples of habits include putting on a seat belt, eating popcorn in a movie theater, and going to the gym after work. Habits are automatically activated by cues (e.g., aspects of physical environments, other people), such that, once a contextual cue is perceived (e.g., getting into a car), people enact the habitual response without consciously deciding to do so (e.g., putting on a seat belt; Wood & Rünger, 2016). Thus, while automaticity is a necessary condition of habit, rituals can be activated automatically or require deliberate thought to perform (e.g., Grimes, 2014; Hobson et al., 2017). Moreover, habits contain similar physical features as rituals (repetitive, rigid behaviors), but habits do not necessarily have rituals' psychological features such as meaning (e.g., Gioia & Poole 1984).

Meaningful work. Meaningful work is work that holds purpose and significance for the individual (Rosso et al. 2010). For instance, when work is viewed as a personal calling (compared to economic or career advancement), work becomes more meaningful to the individual, which results in greater satisfaction (e.g., Wrzesniewski et al., 1997). Research in this area has also explored, for example, how employees find positive meaning in their work, even in work considered undesirable (Wrzesniewski & Dutton, 2001). While meaningful work contains similar psychological features as rituals (e.g., meaningfulness, symbolism), not all meaningful work is ritualized. For instance, a work experience (e.g., observing co-workers interacting with customers in a caring manner) could be appraised as meaningful but lacks the repetitive and rigid behaviors that constitute a ritual. In this way, rituals share similar psychological features as meaningful work, but rituals require a set of physical features which are not found in meaningful work.

Values. Values are "enduring belief that a specific mode of conduct or end-state of existence is personally or socially preferable to an opposite or converse mode of conduct or end state of existence" (Rokeach, 1973, p. 5). Examples of values include respect for individuals, adaptability, and risk-taking (e.g., Chatman, 1991). Values are organized into value systems, which rank values along a continuum of relative importance (Rokeach, 1973). In this way, values can be held by individuals and groups (e.g., Chatman, 1989). All groups around the world have a value system (e.g., Schwartz, 1992), although groups vary in the content of their values, the consensus that values are held across group members, and the intensity that their values matter

(Chatman & O'Reilly, 2016). Values are conceptually different from rituals because rituals are enacted behaviors, while values are a type of social cognition. Although I conceptualize values and rituals as distinct constructs, rituals can relate to values. I later theorize that group rituals can represent group values (see "Group Rituals Represent Group Values" section).

Culture (including norms). Schein's (2010) theoretical framework of culture consists of three interrelated layers: (1) underlying assumptions and beliefs (e.g., taken-for-granted beliefs, perceptions, thoughts, and feelings); (2) espoused values and norms (e.g., day-to-day operating principles that guide action); and (3) artifacts (e.g., symbols and language). I discuss each of these layers in turn and how rituals relate.

The first layer in Schein's (2010) 3-layer classification is underlying assumptions and beliefs. These underlying assumptions have become so taken for granted that there is wide within-group consensus on the importance of these beliefs. An example of an assumption that might become taken for granted is that the individual group members' rights supersede those of the group. While I later theorize that rituals may come to represent group beliefs and assumptions (see "Group Rituals Represent Group Values" section), rituals are conceptually different from the first layer of culture because the first layer concerns implicit beliefs and assumptions (a type of social cognition), while rituals are enacted behaviors.

Schein's second layer of culture is norms and values. Norms and values are closely related, such that norms specify the appropriate attitudes and behaviors, and values provide the rationale for the norms (Chatman & O'Reilly, 2016). As I already reviewed how values relate to rituals, I will focus on the relationship between norms and rituals. Although the social norms literature is characterized by a great amount of debate, most theories suggest that social norms: (a) are social (i.e., shared among members of a group) and (b) inform action-oriented decision making in some way (Legros & Cislaghi, 2020). When focusing on the norms that characterize a group, research distinguishes between the content (i.e., what is deemed important), consensus (i.e., the amount of agreement or consensus with which a particular norm is held), and intensity (i.e., the amount of disapproval evoked by violating the norm; Chatman et al., 2014). Related research has used the term "descriptive norm" to refer to consensus and "injunctive norm" to refer to intensity (e.g., Cialdini et al., 1990); I use consensus and intensity in this dissertation to be consistent with research on organizational culture (e.g., Chatman & O'Reilly, 2016). The degree an activity is ritualistic is not related to one specific norm content; for instance, the same ritualistic sequence (e.g., daily team check-in) can either serve as a support group or a way for a controlling manager to pressure team members. Although rituals are not systematically related to specific norm content, I view rituals as related to norm consensus and intensity. Strong (vs. weak) rituals tend to have high consensus (e.g., everyone agrees the action must be performed) and high intensity (e.g., the behavior has a great deal of importance and meaning). Given that prior research has extensively studied social norms in organizations (e.g., Dannals & Miller, 2017), I seek to control for "culture strength" (norm intensity and consensus) and culture content (e.g., teamwork, adaptability) to test whether the effect of rituals on organizational outcomes exists above and beyond the effect of these related cultural constructs. Although rituals tend to co-occur with high-intensity and high-consensus norms, I theorize that there are cases when rituals operate with low-intensity and low-consensus norms and can still influence behavior.

Schein (2010) defines the third layer of culture—artifacts—as "all the phenomena that one sees, hears, and feels when one encounters a new group with an unfamiliar culture" (p. 24). While artifacts are the most visible element of culture for ingroup and outgroup members to observe, artifacts are also the most difficult to accurately decipher, sometimes (but not always) reflecting the first and second layers of culture (espoused values/norms and underlying assumptions). Schein (2010) considers "observable rituals and ceremonies" to be one type of artifact (p. 26) because rituals are enacted behaviors that one can see, hear, and feel within a group setting. Thus, although I view rituals as related to all three layers of culture, prior research suggests that rituals are most closely related to this layer of culture. Other artifacts of culture include language, physical environment, technology, clothing, myths, and emotional displays. In this way, rituals are one of many cultural artifacts that constitute the third layer of culture.

As Schein's (2010) theory illustrates, culture is complex, encompassing many different concepts and phenomena. My focus is exclusively on rituals, which are most closely related to the third layer of culture (artifact). Just as different components of culture deserve their own independent research investigation—such as research on norms (e.g., Chatman & Flynn, 2001) through language in organizations (e.g., Srivastava et al., 2018)—rituals also merit their own independent investigation (see Hobson et al., 2018, Trice & Beyer, 1984, for calls for research).

Established Antecedents and Consequences of Rituals

Although rituals have been a popular topic in the social sciences, researchers focused their investigation on broader macro-level accounts instead of the micro-level perspective (Hobson et al., 2018). Thus, our understanding of the micro-level antecedents and consequences of ritual is limited. In this section, I draw on relevant sources to establish what is presently known about the micro-level antecedents and consequences of rituals. By micro-level antecedents, I mean the individual or situational conditions that increase the likelihood that an individual engages in a ritual. By micro-level consequences, I mean the psychological consequences of enacting the ritual for the performer.

Empirical research suggests that there are two main micro-level antecedents to ritual engagement: emotions and social connection. First, research suggests that emotional deficits can lead to ritualistic behavior. Individuals are more likely to engage in ritualistic activities during times of anxiety and negative emotion, such as times of war (e.g., Sosis & Handwerker, 2011). This is also evident in clinical psychology studies on obsessive-compulsive disorder (OCD), such that ritualistic activities are used as a strategy to ward off anxious thoughts (e.g., Reuven-Magril et al., 2008). Second, social deficits can promote ritualistic behavior. For instance, Watson-Jones and colleagues (2016) found that socially ostracizing (vs. including) young children produced more imitation of ritual-like actions from ingroup members. Another study found that priming ostracism increases young children's imitation of group rituals (Watson-Jones et al., 2014). Moving beyond this research, I examine how an individual difference—group commitment—serves to protect group rituals, helping rituals persist across time.

In identifying micro-level consequences, recent research in social psychology has examined the consequences of rituals using experimental methods (e.g., Norton & Gino, 2014; Vohs et al., 2013). This research has shown that engaging in rituals can promote individual

performance (Brooks et al., 2016), self-control (Tian et al., 2018), satisfaction with relationships (Garcia-Rada et al., 2019), cooperative behavior (Schroeder et al., 2018), enjoyment (Sezer et al., 2016), and prosocial behaviors (Kim et al., 2021). As this research has focused on ritual engagement by individuals or dyads (e.g., romantic partners), the consequences of rituals at the group-level are not well explicated. By focusing on group rituals and the connection between group rituals and group values, my dissertation expands our understanding of how rituals operate in groups and potentially affect meaningful group-level consequences such as member commitment.

Conceptualizing Group Member Commitment

Given I am focused on the bi-directional association between group rituals and group commitment (given commitment is a consequential outcome in organizational behavior; Meyer, 2016), I also conceptualize group member commitment. This section unfolds as follows. First, I provide a definition of group commitment, reviewing prior academic debates about the nature of commitment. Second, I discuss how commitment relates to other constructs in the academic literature, establishing construct and discriminant validity (Campbell & Fiske, 1959). Lastly, I review established antecedents and consequences of commitment, identifying holes in the existing literature.

Defining Commitment

Similar to the history of ritual in the academic literature, there has been a great deal of debate about how to define commitment to a group. First, the 1960s saw the emergence of the behavioral view on commitment (e.g., Becker, 1960; Staw, 1977; Salancik, 1977). From the behavioral view, commitment is viewed as the propensity to engage in consistent lines of activity in which individuals can “become locked into a costly course of action" (Staw, 1981, p. 577). In this way, the behavioral view of commitment focuses on the process by which an individual commits to a course of action (i.e., a consistent behavior).

In contrast to the behavioral view, the attitudinal view of commitment rose to prominence in the 1970 and 1980s and considered commitment to be an attitude toward an entity (i.e., a group). Most of the research during this time debated the number and nature of the psychological dimensions (bases) of “attitudinal commitment.” For example, Porter and colleagues (1974) defined commitment as the “the strength of an individual's identification with and involvement in a particular organization” (p. 3) which has three dimensions: (1) belief in and acceptance of goals and values; (2) willingness to exert effort; and (3) strong desire to maintain membership (Mowday et al., 1979). Although Porter and colleagues focused on attitudinal commitment, some have justifiably criticized their work for conflating their definition of attitudinal commitment (e.g., willingness to exert effort) with potential consequences of commitment (e.g., O’Reilly & Chatman, 1986). Building off this multidimensional view of commitment, O’Reilly and Chatman (1986) used developmental psychology models (e.g., Bowlby & Ainsworth, 1991) to theorize that there are three forms of commitment based on psychological attachment (compliance, identification, and internalization), developed a self-report scale to measure these three forms, and found that each has differing implications for organizational behavior.

In the 1990s and early 2000s, Meyer and Allen's (1997) three-component model of commitment rose to prominence. Aligned with the attitudinal view of commitment, this model suggests that there are three distinct dimensions of commitment, each characterized by a different underlying mindset: a "desire" to remain in the group (affective commitment), a "need" to remain in the group (continuance commitment), and an "obligation" to remain in the group (normative commitment; Meyer & Allen, 1991; Meyer et al., 2002). Despite widespread usage, measures reflecting the three-component model have been criticized for including extraneous concepts—such as confounding commitment with identification—and imprecise theorizing on the content and number of dimensions (e.g., Gautam et al., 2004; Jaros, 2007; Klein et al., 2014). Due to these issues as well as ambiguity regarding the boundaries and structure of the three-component model and concerns that the three-component model includes antecedents and unrelated constructs as part of the measurement, researchers have started to use measures reflecting a unidimensional view of commitment (e.g., Klein & Park, 2016).

Given the theoretical and empirical issues with the three-component model of commitment, I align with a recent unidimensional reconceptualization of commitment, defining commitment as "a volitional psychological bond reflecting dedication to and responsibility for a particular target" (Klein et al., 2012, p. 137). This definition of commitment is "target-free," such that one can be committed to a host of groups (e.g., workgroup, organization, nation, or religion). Given this dissertation aims to be applicable across a host of groups (e.g., workgroups, organizations, nations, religions), the target-neutral conceptualization of commitment allows me to directly measure and compare commitment across targets without the need to offer a new definition or measure of commitment for each target (Klein et al., 2012; Klein & Park, 2016).

Commitment and Related Constructs

Because commitment is conceptualized as a psychological bond, commitment is similar to and different from other constructs. In particular, I discuss the similarities and differences between commitment and group affect (satisfaction), engagement, identification, and social status. This discussion of the uniqueness of commitment is important because the establishment of construct validity and discriminant validity enables me to theorize and test a unique relationship between rituals and commitment, above and beyond relationships with related constructs.

Group affect (satisfaction). Affect towards the group—which is conceptualized as "satisfaction" in organizational behavior—is defined as "an evaluative state that expresses contentment with and positive feelings about" one's group (Judge & Kammeyer-Mueller, 2012, p. 343). In this way, satisfaction is an attitude, reflecting a summary evaluation of an entity (i.e., one's group) along a valanced dimension (e.g., good/bad; Eagly & Chaiken, 1998). The degree one is dedicated to a group (commitment) is often positively related to one's affective evaluation of the group (satisfaction); however, the two constructs do not need to co-occur theoretically. For instance, an individual could find it pleasant to be a member of the group (high satisfaction) but show minimal dedication to the group's overall well-being (low commitment), constituting a free rider. Empirically, prior work has shown that group satisfaction (but not group commitment) is associated with legitimizing the group's misdeeds, presumably because people high in

satisfaction (but not high in commitment) resist and downplay negative portrayals of the group to maintain their positive feelings toward the group (Leach et al., 2008).

Engagement. Engagement is defined as a "positive, fulfilling work-related state of mind that is characterized by vigor, dedication, and absorption" (Schaufeli et al., 2006, p. 72). While both engagement and commitment contain a sense of dedication, the target of the dedication differs between engagement and commitment. Engagement concerns dedication to the work itself (i.e., whether one feels engaged doing the work), while group commitment concerns dedication to the group (i.e., whether one feels psychological attached to the group's well-being). Engagement also differs from commitment because engagement is a high-arousal affective state (e.g., expressed as energy and enthusiasm) while commitment is a relatively low-arousal psychological state (e.g., experienced as contentment and comfort; Albrecht & Dineen, 2016; Maslach et al., 2001).

Identification. Identification is defined as a "perception of oneness with a group of persons" (Ashforth & Mael, 1989). Theoretically, the role of the self has been emphasized as a key distinction between commitment and identification: identification (but not commitment) is the degree the group is part of one's self-concept (Dutton et al. 1994; Hogg & Terry, 2000; van Knippenberg & Sleebos, 2006). For instance, 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, whereas identification addresses the degree to which a person integrates their group into their self-concept, answering the question, "*How much do I view the group as a part of who I am?*" (e.g., Cooper & Thatcher, 2010; Haslam, 2004; Hogg & Terry, 2000; Riketta & van Dick, 2009). Thus, while commitment and identification both refer to a person's attachment to a group and are closely related at an empirical level (e.g., van Dick et al., 2006), identification (but not commitment) requires merging one's sense of self with the group.

Social Status. Social status is defined as "the extent to which an individual or group is respected or admired by others" (Magee & Galinsky, 2008, p. 359). While status refers to an assessment that one is respected and admired by others, commitment concerns the degree an individual feels attached to the group. Thus, while status is conferred by others, commitment depends on an individual's feelings about the group. In this way, commitment is viewed as a strong predictor of status: individuals who signal that they are committed to the group are viewed as providing more value to the group, enhancing their social status (e.g., Anderson & Kilduff, 2009). Moreover, higher status can yield stronger commitment: higher status individuals tend to view their group more positively, which increases their group commitment (Willer, 2009).

Established Antecedents and Consequences of Commitment

Prior research has documented a host of antecedents and consequences of group commitment. In identifying antecedents, prior research has shown that individual differences (e.g., personality; Choi et al., 2015), group characteristics (e.g., salience of group; Mueller & Lawler, 1999), interpersonal factors (e.g., social exchange; Rhoades & Eisenberger, 2002), and societal factors (e.g., national culture; Wasti, 2016) influence the development of group commitment (see Klein et al., 2012). For instance, O'Reilly and Chatman (1986) propose that commitment is typically established through regular, vivid (e.g., physical presence), and

meaningful social bonding with other organizational members, the importance of which has been confirmed by developmental psychology models showing the dramatic negative consequences of involuntary separation from those to whom a person is attached (e.g., Bowlby & Ainsworth, 1991).

Relevant to the current focus on rituals, researchers have proposed that culture can shape member commitment (e.g., O'Reilly, 1989). However, as reviewed earlier, culture is a complex and multilayered construct, encompassing many narrow, nuanced constructs. It is thus of importance to distinguish which specific aspects of culture can drive member commitment. For practitioners focused on enhancing member commitment, culture is often too broad and diverse to effectively change; however, if the field understood which aspect(s) of culture relate to member commitment, then practitioners can alter specific aspects of culture to bring changes in member commitment. Prior research has established that cultural consensus (i.e., the existence of a common set of organizational values) is associated with stronger member commitment (Caldwell et al., 1990), but prior research has not directly investigated whether artifacts of culture can alone influence commitment. If artifacts of culture are sufficient to shape member commitment, then this would suggest that practitioners aiming to leverage culture to influence member commitment do not need to change the essence of culture (e.g., cultural consensus, intensity, content) and instead can focus on the behavioral artifacts of culture (such as the rituals).

In identifying consequences, prior research has shown that member commitment is associated with a wide range of job-relevant outcomes (e.g., higher motivation, better performance, lower turnover) and non-job relevant outcomes (e.g., greater well-being, less work–family conflict; Meyer et al., 2002). For example, O'Reilly and Chatman (1986) found that MBA students more (vs. less) committed (predicated on value congruence) to the graduate business school were more likely to make a financial donation to the school's annual fundraising campaign (average pledge was $175), showing that commitment is linked to more prosocial behavior even when financially costly to do so. Commitment not only leads to a wide range of positive member outcomes but also reduces the likelihood of negative member outcomes, such as turnover and counterproductive behaviors (e.g., Marcus, 2016). For instance, casino dealers more committed to their casino have been found to engage in fewer undesirable behaviors such as seducing customers to gamble and taking their frustration out on customers, showing that the lack of commitment is not only harmful for the organization but also stakeholders (e.g., customers; Tian et al., 2014).

Relevant to the current focus on the protection of rituals as a consequence of member commitment, prior research has not examined how group commitment affects how members approach elements of a group's culture. Thus, it remains an open question whether high versus low committed members view and interpret cultural phenomena differently. If member commitment shapes the willingness of members to protect cultural artifacts (such as group rituals), then this would indicate that more (vs. less) committed members are obstacles to culture change, ensuring that cultural artifacts persist across time. Prior research has shown that group members who are higher in ingroup identification experience more positive emotions when they comply (vs. violate) a descriptive or injunctive norm (Christensen et al., 2004), but prior research

has not examined whether group commitment facilitates the protection of rituals, thus contributing to the persistence of cultural artifacts over time.

Group Rituals Represent Group Values

I argue that values lie at the heart of group rituals; as a result, the connection between group values and group rituals can account for how rituals affect performers' commitment to the group (internal pathway) and how the enactment of rituals signal commitment to others, increasing prospective members' commitment (external pathway). My theorizing is consistent with Schein's (2010) 3-layer theory of culture, which proposed that rituals (as an artifact of culture) are "the ways in which a group celebrates key events that reflect important values or important 'passages' by members" (p. 13). Moving beyond Schein's theory, I theorize a psychological process which exemplifies *how* values reflect group rituals. Specifically, in this section, I theorize that group rituals *publicize*, *preserve*, and *perpetuate* a group's value system, such that group rituals can serve as behavioral manifestations of a group's value system.

First, rituals *publicize* group values. Group rituals provide a physical manifestation of the group's values, visibly publicizing values that the group often considers most important (e.g., Bell, 1997; Turner, 1969). To understand how the simple but structured physical gestures in a ritual become symbolically linked to a group value, researchers have proposed the theory of "causal opacity" (e.g., Kapitány & Nielsen, 2015; Legare & Souza, 2012). Rituals often appear causally opaque in that they often lack a direct observable causal connection between the specific action performed and the desired physical outcome (i.e., lack overt instrumental purpose). As an example, Cold Stone Creamery employees sing a song whenever they receive a tip. Since singing a song is not required for proper serving of ice cream, the act of singing has become an end unto itself and has come to represent Cold Stone Creamery values ("Profit by Making People Happy"). In essence, the causal opacity of ritual leads the physical procedure of the ritual to become less instrumental and more symbolic, leading the physical actions of ritual to publicize group values (Rossano, 2012).

Second, rituals *preserve* group values. Enacting rituals reinforces group beliefs and narratives, reminding the performers and observers of the meaning behind the ritual: the groups' core values (McCauley & Lawson, 2002; Whitehouse, 2004). Thus, rituals serve as memory cues (Rossano, 2012), helping to recall group values in the present moment. Therefore, rituals' specific physical characteristics (e.g., repetition, rigidity) seek to inculcate group values through repetition, implying continuity from the past to the present moment (Hobsbawm & Ranger, 1983). Consider, for instance, the ritualistic Walmart Cheer (see "Conceptualizing Rituals" section). The rigid and repetitive nature of the Walmart Cheer ensures that employees are consistently reminded that one of Walmart's core values is "Service to the Customer." In other words, since values can be upheld as sacred within a group, the values that underly rituals can also be upheld as sacred within a group (Atran & Ginges, 2012; Morris et al., 2015). As a result, rituals—as physical manifestations of group values—contribute to the stability and consistency of group values among members (Sosis et al., 2007; Watson-Jones & Legare, 2016).

Finally, rituals *perpetuate* group values into the future. Rituals' specific physical characteristics (e.g., repetition, rigidity) allow rituals to be learned, imitated, and shared with

others, especially with new members (Hobson et al., 2018; Kapitány & Nielsen, 2015). Specifically, Legare and Nielsen (2015) argue that rituals are ideal for high-fidelity cultural transmission over time because rituals' causal opacity shifts the focus of the ritual from the desired outcome to the physical procedure, which (through making the physical procedure salient) encourages imitation in others. Indeed, past research on the vertical transmission of rituals suggests that people have a developmental sensitivity for observing and imitating ritual-like behaviors, which makes it easier to transmit rituals—and hence, group values—to new members (e.g., Clegg & Legare, 2016; Liberman et al., 2018).

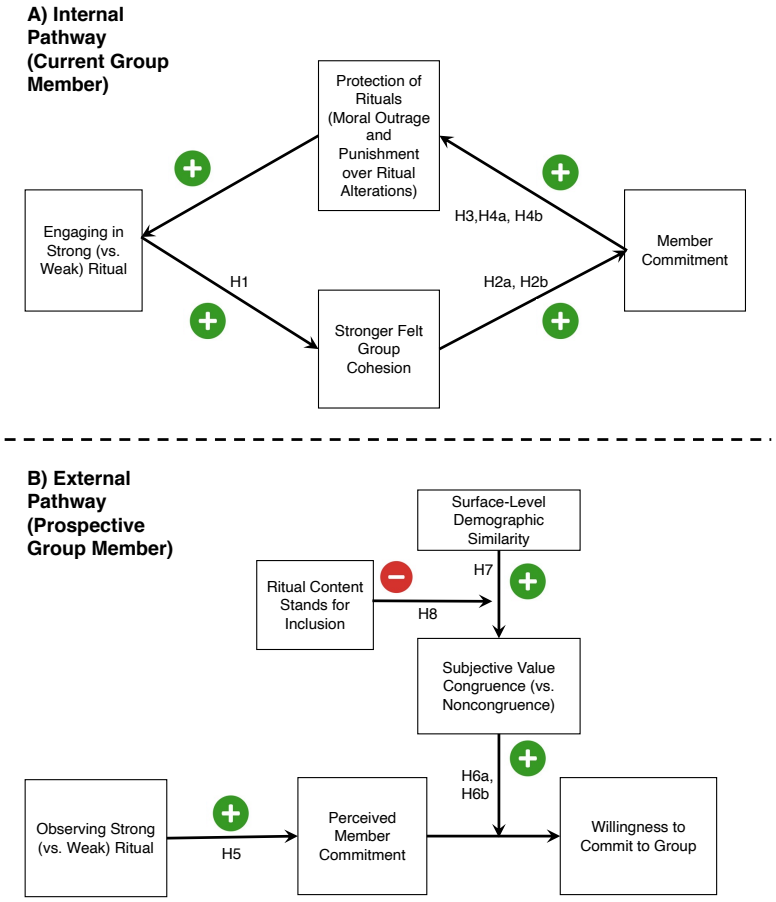
Internal Pathway of Rituals: Existing Member Perspective

In this section, I focus on how performing rituals—as a physical manifestation of group values—may lead members to be more committed to the group. I also consider the reverse relationship: are more (vs. less) committed group members more likely to protect group rituals? In considering the protection of group rituals, I study how people react when their rituals are altered. I define a ritual alteration as any modification to either the physical or psychological features of the ritual. Altering the day or time a ritual is performed, how the elements are performed (including omitting elements or reordering them), the context in which it is performed, who performs it, or even not performing the ritual entirely all fall under the definition of ritual alteration. Group members can protect rituals by punishing those who alter them. If more (vs. less) committed members are more likely to protect rituals (e.g., by punishing those who alter them), then more (vs. less) committed members may enable groups to engage in stronger rituals over time because they ensure (through the threat of punishment) the features of the ritual do not vary between performances. See Figure 1, Panel A ("Internal Pathway") for the theoretical model.

As a preview to the theory, if strong (vs. weak) rituals promote commitment to the group, and more (vs. less) committed group members are more likely to protect group rituals, then the relationship between rituals and commitment is bi-directional. In such a way, strong (vs. weak) rituals elicit higher levels of commitment to the group, which, in turn, lead more members to protect rituals, resulting in stronger (vs. weaker) rituals in the group over time, and so on. I theorize that this recursive cycle is functional for groups, ensuring a high level of commitment while also clearly demarcating group boundaries.

Figure 1

The Theoretical Model



Rituals Promote Commitment

Given that I theorize that group rituals *publicize*, *preserve*, and *perpetuate* a group's value system, engaging in rituals may be linked to people's felt cohesion, or perception that the group is unified (Hamilton et al., 2002). Prior research indicates that the more groups engage intimately with one another, share common goals and rules, and feel emotionally close to one another, the more that members experience felt cohesion (e.g., Campbell, 1958; Yzerbyt et al., 2004). A strong group value system might be associated with felt group cohesion because shared values foster feelings of similarity and intimacy, enabling group members to engage in similar behaviors and more effectively coordinate together (Chatman & O'Reilly, 2016; Sørensen, 2002; Wageman & Gordon, 2005).

Integrating prior research that a group's shared value system results in feelings of similarity and intimacy (e.g., Wageman & Gordon, 2005), with research that feelings of similarity and intimacy are cues linked to felt group cohesion (e.g., Yzerbyt et al., 2004), leads me to propose that engaging in strong (vs. weak) rituals—which more strongly represent the group's value system—will enhance members' felt group cohesion. In this way, I theorize that rituals serve as behavioral cues that re-orient the subjective perception that the group constitutes a single unified entity. Consistent with this reasoning, engaging in synchronous behavior (which is one element of ritual)—compared to asynchronous behavior—leads to more ingroup

cooperation (e.g., Wiltermuth & Heath, 2009), although prior research has not examined whether engaging in rituals influences members' felt group cohesiveness, as I suggest here.

> **Hypothesis 1**: Engaging in strong (vs. weak) rituals will lead to greater felt group cohesion among group members.

I propose that felt cohesion can positively influence group member commitment because more cohesive groups are perceived to be more able to effectively provide resources and support to improve individual outcomes in return for higher levels of commitment. Prior research on leader-member exchange (e.g., Basu & Green, 1997), perceived organizational support (e.g., Eisenberger et al., 1986), and psychological contracts (e.g., Robinson & Rousseau, 1994) has established that social exchange promotes higher levels of commitment: employees who perceive a positive supervisor relationship, a high level of organizational support, and a fulfilled employment relationship are more likely to feel an obligation to "repay" the group or organization with higher commitment. As groups perceived to be more cohesive are also perceived to be better equipped (through the behavioral linkage) to support one another and exchange "resources" in return for commitment, I propose that higher felt cohesion within a group will promote higher commitment (e.g., Zaccaro & Dobbins, 1989). This line of reasoning is also consistent with work on escalation of commitment, such that engaging in ritualistic behavior might be one route in which one escalates their commitment to an entity (Becker, 1960).

> **Hypothesis 2a**: Engaging in strong (vs. weak) rituals will lead members to be more committed to their group.

> **Hypothesis 2b**: The positive relationship between engagement in strong (vs. weak) rituals and commitment will be mediated by increases in felt group cohesion.

Commitment and Protection of Rituals

At times, group members may try to change their group rituals. Consider, for example, Google CEO Sundar Pichai canceling Google's weekly all-hands meetings in 2019 or American athletes kneeling on one knee during the U.S. national anthem in 2016 (as famously exemplified by American football player Colin Kaepernick). In response to the group ritual being altered, group members may, for instance, accept the alteration or protest the alteration by punishing those individuals who initiated the alteration. In this context, protecting the group ritual—to ensure it remains unchanged—involves expressing outrage over the alteration and ultimately punishing those who attempt to alter it.

In considering the protection of group rituals, prior research offers promising hints that more (vs. less) committed group members are more likely to care about the values underlying the ritual. In general, the internalization of group values is a fundamental element of commitment: more committed members are more likely to adopt and internalize the values of the group (Klein et al., 2012; Meyer & Allen, 1991). Some researchers even consider value congruence a definitional component of commitment; for example, Mayer and Shoorman (1992) defined "value commitment" as the "belief in and acceptance of organizational goals and values" (p.

673) and O'Reilly and Chatman (1986) conceptualized commitment (internalization dimension) as felt attachment for a group predicated on value congruence.

If group rituals can represent the group's values, as I contend, and more (vs. less) committed members care more about the group's values, then more (vs. less) committed group members will view alterations to group rituals as a larger affront to the group's value system. In particular, altering a group ritual—which I theorize undermines the values of the group and thus harms the group—may be construed as a moral violation because people view their group values as moral. Moral foundations theory (Graham et al., 2011, 2013; Haidt, 2012) proposes that moral intuitions are shaped in part by their development within groups and cultures (Greenwood, 2011). In particular, this theory suggests that there are innate moral "foundations" (e.g., Care/Harm, Loyalty/Betrayal) on which morality is constructed within specific groups (Graham & Haidt, 2012). As moral foundations are broad and abstract (e.g., "Harm"), they are translated into idiosyncratic behavioral guidelines that are defined by the group (Ellemers, 2017; Leach et al., 2015). Because group membership defines what is morally "right" behavior, people view their groups as moral anchors and thus as a moral entity (Ellemers & van der Toorn 2015). This suggests that alterations to rituals—by challenging the values of the group—should provoke more moral outrage (anger at the violation of a moral standard; Haidt, 2003) among group members high (vs. low) in commitment because the more committed group members are more likely to view the alteration as an affront to the group's value system.

Hypothesis 3: More (vs. less) committed group members will experience greater moral outrage in response to a ritual alteration.

I further propose that, beyond eliciting moral outrage, individuals who alter group rituals will be punished. Across groups and cultures, individuals who engage in moral transgressions are punished, with social exclusion being a common consequence (Rai & Fiske, 2011; Tooby & Cosmides, 2010). People punish those who violate moral standards, even when facing personal costs for doing so, and anger following moral violations is the proximate mechanism underlying punishment (e.g., de Kwaadsteniet et al., 2013; Fehr & Fishbacher, 2004; Lotz et al., 2011).

Hypothesis 4a: More (vs. less) committed group members will inflict greater punishment at group members who alter rituals.

Hypothesis 4b: The relationship between member commitment and punishment will be mediated by increases in moral outrage.

This theory contends that more (vs. less) committed group members find that altering group rituals constitute a stronger moral violation because more committed group members care more about the values represented in the ritual. As a result, because group members can perceive values to be non-negotiable, sacred principles (e.g., Atran, 2010; Baron & Leshner, 2000; Tetlock, 2003), even well-intentioned alterations that bring benefits to the group should elicit outrage and punishment among those committed to the group. Consistent with this reasoning, people express moral outrage when others broach even the possibility of considering the legitimacy of the values they deem sacred (e.g., Baron & Spranca, 1997). However, if the ritual is unaltered, there should be no relationship between member commitment and moral

outrage/punishment—because the values of the group are not threatened. In such a way, the effect of ritual alteration on moral outrage/punishment should also be stronger among ingroup than outgroup members because outgroup members do not share the group's values.

Overall, support for Hypotheses 3, 4a, and 4b would indicate that more (vs. less) committed group members are more likely to protect group rituals by punishing those who alter them. Beyond the protection of group rituals, I further theorize that groups that more strongly protect rituals promote stronger (vs. weaker) ritual engagement in the future (see Figure 1, Panel A). Although not a formal hypothesis because it is not tested in my dissertation, I theorize there is an association between the protection of group rituals and engagement in rituals over time. The logic is as follows. If, for instance, ritual alterations were not punished, resulting in the ritual procedure changing frequently, then it would be difficult for the group to develop an ordinary group activity into a strong, established ritual that publicizes, preserves, and perpetuates the group's value system. In ensuring that slight deviations from the ritual procedure are met with swift moral outrage and punishment (irrespective of the rationale for the alteration), committed group members—through their protection of group rituals—may further enable groups to practice strong (vs. weak) rituals over time. In this way, I theorize that committed group members are the stalwarts of the groups' rituals, protecting the rituals from those who attempt to alter them over time.

External Pathway of Rituals: Prospective Member Perspective

In contrast to the internal pathway (which focuses on existing members), the external pathway concerns how prospective members (e.g., job applicants) view strong (vs. weak) rituals in new member recruitment. As a preview to this section, I focus on the attributions that prospective members draw about the effect of rituals on existing members' commitment. In contrast to the *actual* effect of rituals on existing members' commitment (internal pathway), I theorize that prospective members' *beliefs* about the effect of rituals can be consequential in that they affect prospective members' own willingness to commit to the group (external pathway).

In the context of new member recruitment, previous research on signaling theory (Spence, 1973) suggests that prospective members—facing incomplete information about potential groups—rely on various environmental variables as signals of stable group characteristics (e.g., Ehrhart & Ziegert, 2005; Karasek & Bryant, 2012). For instance, prospective members interpret recruitment delays and the gender composition of existing members to be signals of the group's inefficiency and commitment to diversity, respectively (e.g., Rynes et al., 1991). Moreover, the signals that prospective members receive are consequential, in turn predicting the individual's willingness to commit to the group (Turban, 2001). Following this research tradition, I propose that strong (vs. weak) rituals—when observed by prospective members—act as a signal of stable group characteristics. Although I focus on strong versus weak rituals as a conservative comparison, my theorizing also applies to the enactment of rituals (as opposed to not enacting rituals). See Figure 1, Panel B ("External Pathway") for the theoretical model.

I note that the theorizing in Figure 1, Panel A ("Internal Pathway") and Figure 1, Panel B ("External Pathway") is consistent and reinforcing. Building on my theorizing that rituals

publicize, preserve, and perpetuate a group's value system, I theorize that rituals make the group's value system appear salient. This has consequences for current members (increasing their felt cohesion, and ultimately, their own commitment) but also potentially for prospective members. This section explores the consequences of rituals—as a signal of a strong value system—for prospective members.

Prior research in anthropology has examined the signaling value of ritual. The costly signaling theory of ritual proposes that rituals—when costly to perform (e.g., in time, energy expenditure, pain, or sacrifice)—act as hard-to-fake signals of commitment to the group. In this view, rituals serve as a reliable signal of commitment to the group and thus allow ingroup members to trust one another, which increases members' commitment to the group (Henrich, 2009; Sosis & Alcorta, 2003; Watson-Jones & Legare, 2016). I depart from this research in numerous ways. First, I do not consider the costliness of the ritual as a critical input into the theoretical model, such that I suggest even rituals lacking in cost may act as a signal of the group's characteristics and ultimately influence prospective members (though greater cost may provide a stronger signal). Second, the costly signaling theory of ritual is focused on how the enactment of ritual influences existing members, while I investigate how observing the enactment of rituals influences prospective members.

Rituals Signal Member Commitment

The enactment of rituals may signal to prospective members that members are committed to the group because rituals (as an artifact of culture; Schein, 2010) are a component of the group's actual culture strength, such that groups that engage in more ritualistic behavior have stronger cultures. As reviewed earlier (see "Defining Rituals" section), culture strength is a multidimensional construct: (1) the consensus or how widely shared values are held across people, and (2) the intensity of feelings about the importance of the values. Stronger rituals are associated with a stronger culture because stronger rituals—by publicizing, preserving, and perpetuating a group's value system (see "Group Rituals Represent Group Values" section)—make salient shared group values whenever the ritual is performed, increasing consensus around the values and the importance people attach to them (Deal & Kennedy, 1982; Trice & Beyer, 1984).

Strong cultures represent an example of a "strong situation" as opposed to a "weak situation" (Mischel, 1977), such that the situation conveys uniform expectations leading to similar behavior across people (irrespective of individual traits and dispositions). Applying the concept of a strong situation to culture, strong cultures convey to others that the group has a host of expected patterns of behaviors (injunctive norms), which yield social disapproval for non-compliance (e.g., Chatman, 1989; Gelfand et al., 2011). Consistent with this past work, prior research has shown that strong cultures are able to influence a particular type of member attribute: commitment to the group (e.g., Caldwell et al., 1990). Strong cultures cultivate member commitment because culture—when held intensely—is a form of social control, leading members to feel and act in ways that are congruent with the group's objectives (O'Reilly, 1989; Schein, 2010).

If group rituals are a component of a group's actual culture strength, and groups with a strong culture tend to have committed group members, I propose that prospective members may view strong (vs. weak) rituals as a signal that the group has committed members. Extending prior work, I focus on prospective group members' *perceptions* of how rituals influence member commitment instead of the actual effect of rituals on member commitment. In this way, if individuals recognize the empirical associations between rituals and strong cultures, and strong cultures and commitment, then prospective members will perceive strong (vs. weak) rituals as a signal that members are committed to the group.

This prediction is also consistent with and reinforced by the theorized internal pathway of ritual (Figure 1, Panel A). I theorize that engaging in rituals—which publicize, preserve, and perpetuate a group's value system—make the group feel like a cohesive entity (i.e., felt cohesion), and ultimately, increase existing members' commitment to the group. By observing current group members engage in rituals, prospective members may infer such rituals have the effect of increasing member commitment because the ritual signals that the group has a strong value system.

Hypothesis 5: Strong (vs. weak) rituals will lead prospective members to view the group as having committed members ("perceived commitment").

Groups that have committed members may also be perceived more capable of achieving group objectives. Individuals who are more committed to their group are more willing to comply with group leaders, enabling the group to more seamlessly direct effort and action toward a desired group outcome (Blader et al., 2017). Moreover, according to social identity theory, more committed group members are more likely to adopt the values, norms, and goals of the group as their own, leading the group to become more capable of unified and collective action (e.g., Ashforth & Mael, 1989; Brown & Pehrson, 2020). In this way, the more individuals are committed to the group, the more the individuals have a vested interest in the group's successes and failures (Roccas et al., 2008).

Therefore, prospective members may find it desirable that a group has committed members because the group will be viewed as more capable of achieving desired outcomes, as long as the prospective member perceives that the values of the group match their own values (subjective value congruence; Chatman, 1989; Cable et al., 2000; Edwards & Cable, 2009). Aligned with this thinking, the "attraction" component in Schneider's (1987) attraction-selection-attrition model proposes that individuals are attracted to groups that share similar values because these value-congruent groups enable them to attain their individual goals. Thus, I theorize that strong (vs. weak) rituals—by signaling members are committed—will increase a prospective member's own willingness to commit to the group but only if the prospective member perceives value congruence and thus believes the group is a vehicle to attain individual goals. However, if a prospective member perceives a group does not match their own values (subjective value incongruence), then a group that is capable of unified action could be an undesirable—or even threatening—attribute to a prospective member.

For example, if a prospective member values risk-taking, and perceives a group also values risk-taking (indicting subjective value congruence), then a group that has committed

members may produce greater own willingness to commit to the group than a group that does not have committed members—because the former group is perceived to be more capable of achieving one's desire for risk than the latter group. On the other hand, a group that has committed members and is perceived to be value incongruent (e.g., prospective member values risk-taking and perceives that a group values risk-aversion) may be viewed undesirable by the prospective member, thus producing less own willingness to commit to the group.

Taken together, if strong (vs. weak) rituals signal a group has high member commitment (H5), and if people are willing to commit to groups that have more committed members conditional on subjective value congruence (vs. incongruence), I predict that strong (vs. weak) rituals will produce a greater own willingness to commit to the group when there is subjective value congruence but not when there is subjective value incongruence.

Hypothesis 6a: Strong (vs. weak) rituals will produce greater own willingness to commit to the group among prospective members' who perceive that their values are congruent with the groups', but not among prospective members who perceive that their values are incongruent with the group.

Hypothesis 6b: Stronger perceived member commitment will mediate the relationship between strong (vs. weak) rituals and willingness to commit among prospective members' who perceive that their values are congruent with the groups', but not among prospective members who perceive that their values are incongruent with the group.

Hypotheses 6a and 6b concern *subjective* value congruence (conceptualized as the judgment that a person's values are congruent with the group's values) instead of *objective* value congruence (conceptualized as a verifiable assessment, without asking for judgments of value congruence, that a person's values are congruent with the group's values; Cable & Judge, 1996; Kristof, 1996). While subjective value congruence is often measured through asking an individual whether value congruence exists, objective value congruence is often measured through comparing the individual's value preferences with the organization's value system (for example, see the Organizational Culture Profile; Chatman, 1989, 1991). I focus on subjective value congruence because I examine how value congruence influences prospective members' feelings about the group, as represented by a willingness to commit to the group. Feelings about the group are also subjective and therefore should relate more strongly to subjective value congruence perceived by the prospective member rather than objective value congruence which may or may not be perceived by the individual (Kristof-Brown et al., 2005).

How is Subjective Value Congruence Inferred?

Prior research has suggested that perceived similarity is one route in which individuals evaluate their subjective value congruence within a group (e.g., Jackson et al., 1991; Schneider, 1987). In general, group values are difficult for prospective members to ascertain directly; as a result, the more similar the prospective member is to the rest of the group, the more the prospective member assumes similarity on underlying (hard-to-detect) characteristics, including values (Harrison et al., 2002; Zellmer-Bruhn et al., 2008). Moreover, prospective members are particularly sensitive to similarity in observable, surface-level characteristics (e.g., gender, race)

because these characteristics are salient and quickly discovered compared to similarity in underlying, deep-level characteristics (e.g., attitudes, skills) which are only discovered through interaction (Jackson & Joshi, 2011; Jehn et al., 1999). For example, past work has demonstrated that Black professionals are attuned to minority representation in prospective employers and feel threatened when there is low minority representation (e.g., Avery et al., 2008; Purdie-Vaughns et al., 2008). Because deep-level diversity (e.g., personality, skills) is difficult for prospective members to discover and learn about (Harrison et al., 2002), I focus my initial theorizing and investigation on diversity as an observable, surface-level characteristic that can be used to infer perceived similarity.

In this way, I expect demographically similar prospective members to infer value congruence but demographically dissimilar prospective members to infer value incongruence. If, as I contend (H6a), the positive effect of strong (vs. weak) rituals on own willingness to commit is weakened or even reversed when the prospective member perceives subjective value incongruence, then demographically similar prospective members—but not demographically dissimilar prospective members—should have a greater own willing to commit to a group that engages in strong (vs. weak) rituals. For example, consider a group that is predominately White. I expect strong (vs. weak) rituals to increase White prospective members' willingness to commit to the group because they perceive value congruence, but for non-White prospective members (e.g., Black, Asian, Latin), I expect strong (vs. weak) rituals to have a weakened or negative effect on own willingness to commit because these individuals perceive value incongruence. In this way, I consider demographic similarity as an indirect operationalization of subjective value congruence, such that the moderating influence of demographic similarity will show the same pattern of results as subjective value congruence. Past research has demonstrated that groups with a strong culture tend to recruit individuals with a similar demographic and cultural background (e.g., Rivera, 2012, 2016), but past work has not investigated the effect of strong cultural practices (such as rituals) on recruitment from the perspective of prospective members, which is the focus of the current hypothesis.

> **Hypothesis 7**: Strong (vs. weak) rituals will produce greater own willingness to commit to the group among prospective members' whose surface-level demographics are represented in the group, but not among prospective members whose surface-level demographics are underrepresented in the group.

If H7 is supported, then this would yield a double-edged sword for groups. While groups that develop strong rituals cultivate high levels of commitment (H1, H2a, H2b), prospective members from underrepresented demographic groups will systematically have a weaker own willingness to commit to the group as a function of the strong (vs. weak) rituals. In this way, rituals promote commitment at a cost: the exclusion of underrepresented individuals and greater homogeneity. These predicted results are consistent with research which suggests that despite organizational commitments to diversity, members of underrepresented demographic groups face discrimination in hiring, networking opportunities, and leadership opportunities (e.g., Avery, 2003; Cheryan & Markus, 2020; Dreher & Cox, 2000; Kang et al., 2016).

There is one condition which my theory predicts will harness the power of rituals (in promoting member commitment) but also make underrepresented individuals feel included in the

group: make it clear that the values underlying the rituals are consistent with Diversity, Equity, and Inclusion (DEI). Thus, in addition to the demographic composition of the group, prospective members may also make inferences about their subjective value congruence based on the content of the ritual. For instance, imagine a group that structures meetings such that the first part of the meeting is always devoted to sharing current problems and frustrations. The value content of the ritual could reflect non-DEI related values such as benevolence and honesty or DEI-related values such as respect, dignity, and equality. In such a way, rituals that stand for DEI values provide behavioral evidence of a group's commitment to DEI, which I theorize will promote subjective value congruence among prospective group members who are demographically underrepresented.

Prior research offers support for the prediction that behavioral evidence of commitment to DEI (such as imbuing DEI values within rituals) can alleviate underrepresented prospective member's concerns about subjective value congruence (Lehman et al., 2019; Windscheid et al., 2016). For instance, one study found that behavioral-based cues to an organization's diversity climate (which "show" prospective members whether the organization has a positive or negative diversity climate), but not verbally expressed cues (which "tell" observers about the organization's diversity climate), influenced a demographically underrepresented prospective member's feelings about the group (Wilton et al., 2020).

Hypothesis 8: If rituals signal the group's interest in diversity and inclusion, strong (vs. weak) rituals will produce a greater own willingness to commit to the group irrespective of whether prospective members' surface-level demographics are underrepresented in the group.

Overview of the Present Research

I test these hypotheses in eight studies across three chapters. Chapter 2 contains two studies and examines how rituals influence existing members' commitment to the group (H1, H2a, H2b). Specifically, I leverage a naturally occurring shock that made it difficult for workgroups to engage in rituals: the COVID-19 pandemic. Due to the rapid spread and virulence of COVID-19, federal, state, and local governments in the U.S. introduced physical distancing regulations, which limited in-person gatherings across all domains of life in late March 2020. The resulting physical distancing behaviors—which were designed to slow the spread of the virus—had the additional effect of reducing the frequency and duration of visits to workplaces (e.g., Kniffin et al. 2021; van Bavel et al., 2020). In this way, COVID-19 workplace physical distancing may have suddenly and unexpectedly reduced engagement in workgroup rituals because rituals rely on rigid adherence to performing sequenced actions (Boyer & Liénard, 2020). By exploiting rapid fluctuations in ritual engagement during the onset of COVID-19 workplace physical distancing, I can test for relationships between rituals, felt group cohesion, and commitment (H1, H2a, H2b) that may not have otherwise emerged in static research designs. Thus, Study 1 leverages changes brought on by COVID-19 workplace physical distancing to examine the relationship between workplace rituals and employees' commitment when engagement is rituals is suddenly and unexpectedly disrupted by an external force (the COVID-19 pandemic) using a longitudinal survey of adult U.S. workers and a geographic-based unobtrusive measure of workplace physical distancing. Study 2 replicates Study 1 in a cross-

sectional survey of U.S. workers. Altogether, Studies 1 and 2 suggest that strong (vs. weak) rituals enhance commitment to the group by making the group seem like a cohesive entity.

Chapter 3 contains two studies and examines whether more (vs. less) committed group members are more likely to protect their groups' rituals by punishing those who alter them (H3, H4a, H4b). Study 3 tests how Americans react to an alteration to the U.S. Pledge of Allegiance ritual as a function of their commitment to the U.S. Study 4 presents Jews and non-Jews with a series of alterations to the Jewish Passover Seder ritual, testing whether ritual alterations uniquely trigger moral outrage and punishment among more committed ingroup (vs. outgroup) members. Across both religious and secular rituals, Studies 3 and 4 demonstrate that the more group members are committed to the group, the more they protect their rituals—by punishing those who alter them.[2]

Chapter 4 contains four studies and examines how observing strong (vs. weak) rituals influences prospective members' own willingness to commit to the group (H5, H6a, H6b, H7, H8[3]). Study 5 presents participants with hypothetical strong (vs. weak) organizational rituals and measures resulting perceptions that employees are committed to the organization. Study 6 tests whether learning about an organization's strong (vs. weak) ritual within company employee reviews influences perceived member commitment and ultimately, own willingness to commit. Study 7 collects reports of real-world organizational rituals that working adults learned about in the context of job interviews, testing whether strong (vs. weak) rituals increase own willingness to commit to the organization among job seekers who perceive subjective value congruence and are demographically represented (but not among those who perceive subjective value incongruence and are demographically underrepresented). Finally, Study 8 manipulates organizational rituals using the vignette methodology and measures individual differences in subjective value congruence, testing whether the theoretical model linking rituals, perceived member commitment, and own willingness to commit is moderated by subjective value congruence when the enactment of rituals is manipulated.

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[2] Another way to organize Chapter 2 and 3 would be to focus on the consequences of altering a ritual. Chapter 2 uncovers the relationship between group rituals and group commitment by examining COVID-induced alterations to rituals. Chapter 3 investigates the emotional and behavioral consequences of altering a ritual. Under this framing, Chapters 2 and 3 suggest that alterations to rituals reduce member commitment (Chapter 2) and provoke punishment directed at the ritual alterer (Chapter 3).

[3] Note that testing for H8 is conditional on finding support for H6a, H6b, and H7. If subjective value congruence or demographic similarity does not emerge as a moderator, then it is not feasible to design and test an intervention (H8) to reduce the negative effects of rituals on excluding underrepresented group members because such negative effects would not be empirically identified. Because I found that H6a, H6b, and H7 were not supported, I did not ultimately test H8.

CHAPTER 2: EFFECT OF ALTERING RITUALS ON GROUP COMMITMENT

In Chapter 2, I seek to understand how engaging in strong (vs. weak) rituals influences existing members' commitment to the group. I hypothesized that: (1) group rituals are related to felt group cohesion (H1); felt workgroup cohesion fosters group commitment (H2a); and group rituals promote group commitment by increasing felt workgroup cohesion (H2b). To test these hypotheses, I decided to take advantage of the COVID-19 global pandemic because it suddenly and unexpectedly reduced engagement in workplace rituals, thus providing a natural, quasi-exogenous manipulation of ritual engagement (Reis & Judd, 2000). Specifically, the COVID-19 physical distancing behaviors—which were designed to slow the spread of the virus—had the additional effect of preventing workgroups from engaging in workplace rituals together. Consider, for instance, rituals 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. Each of the aforementioned rituals were significantly disrupted in the context of workplace physical distancing. Given that real-world rituals are often not amenable to experimental control, the COVID-19 pandemic represents a unique opportunity to study quasi-exogenous changes to real-world group rituals and the resulting impact on group commitment.

Study 1 tested my hypotheses using a panel survey of working Americans (which allowed me to measure workgroup rituals, felt cohesion, and workgroup commitment) and an unobtrusive behavioral measure of workplace physical distancing. Study 2 conceptually replicated the findings from Study 1 using a cross-sectional survey. The study contexts allow me to test for the relationship between workgroup rituals and workgroup commitment in an externally valid setting when real-world workgroup rituals were suddenly and unexpectedly changed.

Study 1: Longitudinal Survey of Working Americans during COVID-19

Study 1 tested my hypotheses (H1, H2a, H2b) using a longitudinal design, collecting time-varying assessments of data from the same individuals to examine changes in workgroup rituals, felt workgroup cohesion, and workgroup commitment during the onset of COVID-19. I combined these panel data with an unobtrusive measure of COVID-19 workplace physical distancing. While workplace physical distancing is not part of my theoretical model, I use workplace physical distancing as a quasi-exogenous shock to understand how workgroup rituals (which were suddenly and unexpected altered by workplace physical distancing) influenced felt workgroup cohesion and workgroup commitment. The use of quasi-exogenous shocks in research permits greater internal validity than typical observational research designs, but there are limitations which I return to in the study discussion (e.g., Grant & Wall, 2008).

Specifically, I operationalized workplace physical distancing using county-level data on the frequency of visits to workplaces and duration of visits to workplaces, as compared to a pre-COVID baseline period. Using natural variation in geographic differences as a quasi-exogenous shock, I examined how workplace physical distancing-induced changes to workgroup rituals influenced a workgroup's felt cohesion and ultimately, an individual's commitment to the workgroup.

In doing so, I took two steps to address issues with a causal interpretation. First, I considered an array of alternative explanations for these findings (such as amount of time workgroups spent communicating) and sought to identify the relationship between workgroup rituals and workgroup commitment after accounting for these potentially confounding variables. Second, I examined within-person changes over time, allowing me to hold characteristics of the individual constant and isolate within-person changes in commitment as a function of physical distancing-induced alterations to workgroup rituals over time.

Method

A preliminary preregistration was posted after collecting wave 0 (https://tinyurl.com/covidprereg1) and an updated preregistration was posted after collecting wave 2 (https://tinyurl.com/covidpreregv2).[4] Although I preregistered the survey measures and sample size, I did not preregister specific hypotheses or analyses.

Participants. I administered four surveys to adult U.S. workers at the onset of COVID-19 workplace physical distancing in the U.S. At wave 0, which occurred shortly after the physical distancing measures were announced, I recruited $N = 1{,}000$ U.S. participants from Amazon Mechanical Turk (TurkPrime; Litman et al., 2017) who had been employed full-time prior to the COVID-19 pandemic, reported that English was their native language, correctly answered two attention check questions, and reported being willing and able to participate in a longitudinal study. I selected this sample size based on funding resources in recruiting a panel survey. Excluding those who failed to follow instructions[5] ($N = 40$), I invited all participants recruited in wave 0 to participate in waves 1-3; no one was excluded for nonresponse, and I retained partial responses in analyses.

To distinguish results from the independent effects of job termination on commitment (e.g., Porter et al., 1974), in waves 1-3, I measured current employment status with the following item, "Which best describes your employment situation at the present time?" I excluded participants who reported that they were not "working for the same company as prior to the COVID-19 pandemic" for the duration of the study ($N = 188$), resulting in a sample of 772 participants. This exclusion, while not preregistered, provides a more conservative test of the hypotheses. Moreover, the results are robust when including this subset of participants. I also excluded 64 participants because their postal zip code was invalid, so I was not able to link their

[4] Notably, the survey was intended to capture a much broader set of research questions, which are reported in the preregistration. The additional measures were either outside the scope of this dissertation or only measured in one survey wave (thus not permitting within-person analyses). In addition to asking participants about their workgroups during the onset of COVID-19, I also asked participants about their family and friend groups. All survey items are available on the OSF Repository for this project (https://osf.io/g6nua/?view_only=f79a6b5037ad49378ca986675a1bbd1b).

[5] I excluded 40 participants (total; 4.0%) from the study for one of three preregistered reasons (see updated preregistration): (1) writing gibberish for the open-ended text response questions; (2) writing gibberish for 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. I asked participants to recall small workgroups (e.g., six people in size), so large numbers indicate inattentive responding (Fleischer et al., 2015).

responses with the county-level measure of workplace physical distancing (see "Quasi-exogenous shock: COVID-19 workplace physical distancing" section). This resulted in a final sample of 708 participants.

The response rate for the final sample (N = 708) was 80.79% in wave 1 (572/708), 73.02% in wave 2 (517/708), and 68.79% in wave 3 (487/708). Among the final sample (N = 708), n = 415 participated in all four waves, n = 123 participated in three waves, n = 85 participated in two waves, and n = 85 participated in one wave. The results are robust when only analyzing participants who completed all four waves (see "Subsidiary Analyses" section). I 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 four waves.

Procedure. In wave 0 (collected on April 10, 2020, but measuring participants' recollection of January 2020), I 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*." To make the workgroup concrete and salient, I asked participants to "write each person's initials and their relation to you (e.g., my peer co-worker, my boss)." I then asked participants to retrospectively recall their work experiences before COVID-19 physical distancing: "*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 the measures. In waves 1-3, participants reported their experiences with the same workgroup "*at the present time*": wave 1 (April 18–22, 2020), wave 2 (May 19–24, 2020), and wave 3 (June 22–28, 2020). To ensure participants wrote about the same workgroup, in waves 2 and 3, I 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).

Quasi-exogenous shock: COVID-19 workplace physical distancing. I extracted publicly available, county-level data on human mobility from Google. To help public health officials combat COVID-19, Google released daily mobility data, aggregated from smartphone location information. Google reports the daily percentage change in time people spent at work within a U.S. County, compared to a baseline median value of the same day of the week between January 3 and February 6, 2020 (Aktay et al., 2020). While 0% reflects no change from baseline, 100% represents the complete change from baseline. For instance, Miami-Dade County (within the U.S. state of Florida) experienced a 49% decline in frequency and duration of visits to workplaces in April 2020, while Dixie County (also within the U.S. state of Florida) only experienced a 22% decline in frequency and duration of visits to workplaces in April 2020. Moving to May 2020, Miami-Dade County experienced a 30% decline in frequency and duration of visits to workplaces while Dixie County only experienced a 16% decline, compared to a pre-COVID baseline period. This example illustrates that different geographic regions within the U.S. experienced different levels of workplace physical distancing which changed overtime during the early stages of the pandemic.

I used this measure—based on smartphone activity from nearly half of Americans—to represent the broad patterns and trends for a particular county. I then applied these county-wide patterns to each individual in the sample. Although there is variance within counties on workplace physical distancing that this measure is not able to identify, I believe that this

approach is valid because the determinants of workplace physical distancing are partially based on regional factors. For instance, factors such as population density, COVID cases, school closures, mask policies, and local pandemic declaration contributed to workplace physical distancing but also are the same within regions.

I used these data because the Google Android operating system has approximately 42% of the market share in the U.S., capturing almost half of all Americans. While other providers have published mobility data, they did not provide information on the type of mobility (work vs. residential). Since the focus is on physical distancing at work, I selected Google's mobility data because Google is the only major provider to report mobility patterns specific to the workplace.

Numerous published academic articles have used these mobility data as an indicator of physical distancing and provided validity evidence (e.g., Chan et al., 2021; Nouvellet et al., 2021). For instance, Clinton et al. (2021) showed that Google's county-level mobility data correlated $r = .91$ with a participant's individual self-reported physical distancing behavior (e.g., whether a participant reported going to work in the last 24 hours).

I took the following steps to clean Google's publicly available data and merge them with the survey responses. Physical distancing behavior for wave 0 (collected April 10 but recalling pre-COVID) was inputted to be 0%, indicating the baseline period (no change). For waves 1-3, since Google reports mobility trends per day and per county in the U.S., I aggregated mobility patterns across days according to the survey wave schedule. Physical distancing for: (a) wave 1 (collected April 18–22, 2020) averaged daily mobility data from April 11 through April 17, 2020; (b) wave 2 (collected May 19–24, 2020) averaged daily mobility data from April 23 through May 18, 2020; and (c) wave 3 (collected June 22-28, 2020) averaged daily mobility data from May 25 through June 21, 2020. To merge these data with the survey measures, I used participants' self-reported U.S. postal zip codes (collected at wave 0). Because Google reports mobility data for each county instead of each zip code, I first linked zip codes to counties, which subsequently allowed me to link Google mobility data to survey responses. However, 168 participants listed zip codes that corresponded to two or more counties. For these participants, I averaged the mobility data across counties.

Survey Measures.

Workgroup rituals. To measure the degree to which individuals engaged in workgroup rituals, I asked participants to "please think about some specific instances of meaningful activities your work group did together in the past few weeks" and "list at least one and up to three activities below." I also told participants that the "activities can be remote/virtual." For each reported activity (up to three, in order to keep the survey brief), I 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).[6] I used a

[6] Note that I used a slightly different wording for this 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)

single item to keep the survey brief, but I provide supplemental data which shows this single item correlates with a multi-item ritualism scale.[7] I used this two-step procedure (nominate meaningful activities, rate degree of ritualism) because I wanted to collect a spectrum of low to high ritualistic activities and asking people to only recall rituals would have resulted in a restricted range of ritualism (e.g., strong rituals only). Table 3 presents illustrative workgroup activities that participants reported to be high or low in ritualism. To compute the ritual index, for each survey wave I averaged participants' ratings of the workgroup activities' ritualism (α = .71). Because participants could list zero to three activities (M = 2.25 activities, SD = 0.95), I substituted missing ritual ratings with 0 if a participant listed fewer than three activities. The results show an identical pattern when operationalizing ritual as simply the number of activities listed (i.e., 0, 1, 2, or 3 activities; see "Subsidiary Analyses" section).

Table 3

Illustrative Workgroup Activities That Participants Reported to be High or Low in Ritualism from Study 1

Ritualism	High (5 out of 5 on Likert scale)	Low (1 out of 5 on Likert scale)
	Meeting: "We start all of our daily team meetings with an icebreaker."	**Meeting**: "We had a roundtable on a new program we are looking to initiate."
	Food: "After work, we go every other Friday to a local Asian infusion restaurant to eat and have drinks."	**Food**: "[We] ordered lunch for our group from a local pizza place."
	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 had a group chat to check in our progress and goals."

characterized by rigidity, formality, and repetition that are embedded in a larger system of symbolism and meaning." The question was modified after wave 1 to remove jargon.

[7] In Study 2, I found this single-item ritual scale correlates r = .40, p <.001 with a three-item ritual scale: (1) "Is the activity done in the same specific way each time?"; (2) "Is the activity meaningful to you?"; and (3) "Does the activity symbolize your workgroup values?" (1 = *not at all*, 7 = *very much*).

Felt workgroup cohesion. To measure felt workgroup cohesion, I 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; $\alpha = .87$).

Workgroup commitment. I 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; $\alpha = .92$).

Control variables. I collected the following variables which I control for in the "Subsidiary Analyses" section.

Demographics (time-invariant). Given that demographics influence an individual's commitment level (Meyer et al., 2002), and given it is possible that demographics are linked to engagement in rituals (e.g., perhaps older workers are more likely to engage in rituals than younger workers), I tested whether the effects hold when accounting for individual demographics. In wave 0, I collected participants' age, gender, race/ethnicity, education, and current income.

Personality (time-invariant). Given that personality is related to group commitment (e.g., Choi et al., 2015), and given it is possible that personality is linked to engagement in rituals (e.g., perhaps more conscientious individuals are more likely to engage in rituals), I tested whether the effects hold when accounting for personality. In wave 0, I collected individual differences in self-reported Big 5 factors personality (TIPI; Gosling et al., 2003).

Communication (time-varying). Given that communication quantity and quality can influence group commitment (e.g., Klein et al., 2012), and given the COVID-19 pandemic changed how workgroups communicate—leading to a shift from in-person work to virtual work (e.g., Yang et al., 2022), I tested whether the effects hold when accounting for communication quantity and quality. In waves 0-3, I asked participants to specify the number of hours they spent communicating with their workgroup members using each of the following seven communication channels (modified from Johnson et al., 2009): (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 a measure of in-person communication time. To compute total communication time, I summed the responses across the seven methods to yield the total number of hours communicating.[8]

[8] In wave 0, instead of asking participants to report the number of hours spent communicating with each channel, I asked participants to report the "percentage of total time spent communicating with workgroup members" using

Intimate interactions (time-varying). Given that intimate conversations can foster group commitment (e.g., Postmes et al., 2001), I tested whether the effects hold when accounting for intimate conversations. In waves 0 and 1, I measured the perceived intimacy of group conversations using the following item: "In the last week, how intimate were your interactions with your work group?" (1 = *not at all*, 5 = *extremely*). In waves 2 and 3, I used a slightly different wording for this question to remove the adjective "intimate", which some participants may not understand: "In the last week, how connected and close were your interactions with your work group?"

Loneliness (time-varying). Given that loneliness can foster a drive to affiliate with social groups (e.g., Baumeister & Leary, 1995), and given the COVID-19 pandemic increased people's loneliness (e.g., Luchetti et al., 2020), I tested whether the effects hold when accounting for loneliness. In waves 0-3, I 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;* Hughes et al. 2004; $\alpha = .91$).

Individual COVID-19 infection (time-varying). Given that the COVID-19 pandemic led some people to become sick with COVID-19, and given that it is possible that becoming sick with COVID-19 reduced one's commitment level, I tested whether the effects hold when accounting for individual COVID-19 infection. In waves 1-3, I 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. In wave 0, I imputed the value "0" to reflect that COVID-19 had not yet spread.

Group member COVID-19 infection (time-varying). Given that the COVID-19 pandemic led some people's group members to become sick with COVID-19, and given that it is possible that sick group members reduced one's commitment level, I tested whether the effects hold when accounting for group member COVID-19 infection. In waves 1-3, I 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. In wave 0, I imputed the value "0" to reflect that COVID-19 had not yet spread.

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each channel (and required responses to sum to 100%). To retain all data, I computed in-person communication for wave 0 by multiplying the proportion of time communicating in-person by each participants' average number of hours spent communicating across waves 1-3. If the participant only participated in wave 0, and thus did not have an average number of communication hours in waves 1-3, I instead used the sample mean to calculate in-person communication time for wave 0. I also used each participant's average number of hours spent communicating across waves 1-3 as total communication time in wave 0; for those who did not have an average number of hours spent communicating because they only participated in wave 0, I instead used the sample mean for wave 0.

Perceived threat (time-varying). Given that the COVID-19 pandemic led some individuals to feel threatened and associated negative emotions (e.g., Trougakos et al., 2020), and given that negative emotions can reduce commitment (Klein et al., 2012), I tested whether the effects hold when accounting for perceived threat. In waves 1-3, I 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; α = .92). In wave 0, I asked participants "prior to the COVID-19 pandemic": (1) "how often did you feel that you were unable to control the important things in your life?"; (2) "how often did you feel confident about your ability to handle your personal problems?" (R); (3) "how often did you feel that things were going your way?" (R); and (4) "how often did you feel difficulties were piling up so high that you could not overcome them?" (1 = *not at all*, 5 = *extremely*; Cohen et al., 1983; α = .70).

Results

As is typical when multiple variables are measured, I provide descriptive statistics and zero-order relationships for the study variables (across waves) in Table 4.

Table 4

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

Variable	*M*	*SD*	1	2	3	4
1. Workplace physical distancing (%)	28.66	21.21				
2. Ritualistic activities	2.54	1.40	-.33** [-.36, -.29]			
3. Felt cohesion	3.73	0.78	-.10** [-.15, -.06]	.34** [.30, .37]		
4. Workgroup commitment	3.89	0.95	-.05* [-.09, -.00]	.25** [.21, .28]	.72** [.70, .74]	
5. Female (d)	0.43	0.49	-.01 [-.05, .03]	-.02 [-.06, .03]	-.00 [-.05, .04]	.02 [-.02, .06]
6. Non-White (d)	0.24	0.43	.05* [.00, .09]	.01 [-.03, .05]	.01 [-.03, .05]	-.01 [-.06, .03]
7. Age	38.18	10.81	-.02	-.03	.09**	.17**

			[-.06, .02]	[-.08, .01]	[.05, .13]	[.13, .21]
8. College (d)	0.71	0.46	.07** [.03, .11]	.04 [-.00, .08]	.02 [-.02, .06]	-.00 [-.04, .04]
9. Income	5.06	1.52	.07** [.03, .11]	.04 [-.00, .08]	.10** [.06, .14]	.15** [.11, .19]
10. Conscientiousness	4.10	0.87	-.01 [-.05, .03]	.00 [-.04, .04]	.23** [.19, .26]	.29** [.25, .33]
11. Emotional Stability	3.78	1.07	-.04* [-.08, -.00]	.00 [-.04, .04]	.20** [.16, .24]	.26** [.22, .30]
12. Extraversion	2.80	1.13	-.03 [-.07, .01]	.10** [.06, .14]	.16** [.12, .20]	.17** [.13, .21]
13. Openness	3.78	0.91	.02 [-.02, .06]	.00 [-.04, .05]	.17** [.13, .21]	.18** [.14, .22]
14. Agreeableness	3.97	0.89	-.02 [-.06, .02]	.02 [-.02, .06]	.26** [.22, .29]	.31** [.27, .34]
15. Total communication time (hours)	24.93	27.50	.02 [-.02, .06]	.17** [.13, .21]	.16** [.12, .20]	.10** [.06, .14]
16. In-person communication time (hours)	8.17	13.41	-.30** [-.34, -.27]	.22** [.18, .26]	.15** [.11, .19]	.10** [.06, .14]
17. Intimate interactions	2.81	1.16	-.06** [-.10, -.02]	.28** [.24, .31]	.47** [.44, .50]	.38** [.34, .41]
18. Loneliness	2.48	1.09	.11** [.07, .15]	.02 [-.02, .06]	-.16** [-.20, -.12]	-.21** [-.25, -.17]
19. Individual COVID-19 (d)	0.05	0.21	.12** [.07, .16]	-.01 [-.05, .03]	-.07** [-.11, -.02]	-.08** [-.12, -.04]
20. Group COVID-19 (d)	0.08	0.27	.18** [.14, .22]	.03 [-.01, .07]	-.02 [-.06, .02]	-.04 [-.08, .00]

21. Perceived threat	3.00	1.13	.38** [.35, .42]	-.08** [-.12, -.04]	-.06** [-.10, -.02]	-.07** [-.11, -.03]

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) indicates dummy variable.

Analytic Strategy

The longitudinal design allows me to test within-person changes over time. To account for the nested data structure and to model the within-person effects, I analyzed the data using multilevel modeling with observations nested within participants and a random slope to allow intra-individual predictors to vary between participants (i.e., a random intercept and slope model; Aguinis et al., 2013). I 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). Importantly, this within-between approach removes average variation between participants and focuses on within-person variation, which corrects for unobserved heterogeneity bias. All analyses are conducted using the R statistical program.

Main Results

I first tested whether COVID-19 workplace physical distancing influenced engagement in rituals. I assumed that these workplace physical distancing behaviors would make it more difficult to engage in rituals, which provides an empirical setting to understand how workplace physical distancing-induced changes in workgroup rituals influenced workgroup felt cohesion and workgroup commitment. Consistent with this assumption, there was a negative relationship between workplace physical distancing and workgroup rituals such that, as physical distancing increased, workgroup rituals declined, $\beta = -0.34$, $SE(\beta) = 0.017$, $t(914.19) = -19.78$, $p < .001$.

Moving onto my theoretical model, I also found, as predicted, a positive relationship between workgroup rituals and felt workgroup cohesion (H1), $\beta = 0.21$, $SE(\beta) = 0.016$, $t(448.19) = 13.21$, $p < .001$, and a positive relationship between workgroup rituals and workgroup commitment (H2a), $\beta = 0.13$, $SE(\beta) = 0.014$, $t(369.53) = 9.25$, $p < .001$. Testing for mediation[9] (H2b), the 95% CI for the index of mediation did not include zero, indicating a significant mediation effect of workgroup rituals on workgroup commitment through felt workgroup cohesion (Mediation Index = 0.100, $SE = 0.010$, 95% CI [0.078, 0.119]).

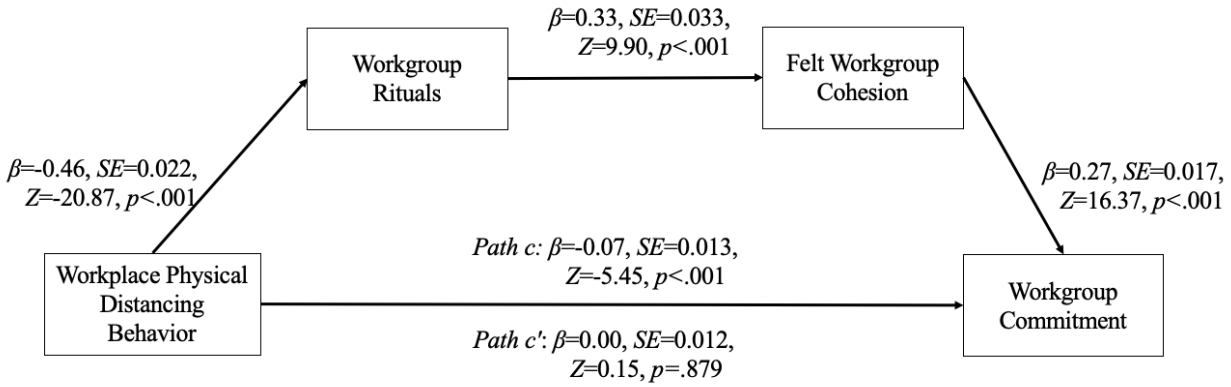
As an expansion to my theoretical model, I additionally tested workplace physical distancing as a distal antecedent to workgroup rituals. This allows me to examine how workplace physical distancing-induced changes to rituals influenced felt workgroup cohesion and

[9] To account for the nested data structure and resulting data non-independence, I employed a within-between formulation and used cluster-robust standard errors using a path model.

workgroup commitment. I found that workplace physical distancing was associated with a decrease in members' workgroup commitment, β = -0.07, $SE(\beta)$ = 0.013, $t(1,447.49)$ = -5.25, p < .001. Furthermore, I tested whether the relationship between workplace physical distancing and workgroup commitment was mediated by workgroup rituals and felt workgroup cohesion. The 95% CI for the index of sequential mediation did not include zero, indicating a significant sequential mediation effect of workplace physical distancing on workgroup commitment through workgroup rituals and felt workgroup cohesion (Sequential Mediation Index = -0.041, SE = 0.005, 95% CI [-0.052, -0.030]). See Figure 2. These results suggest that workplace physical distancing changed workgroup rituals, which subsequently shaped felt workgroup cohesion, and ultimately workgroup commitment.

Figure 2

Sequential Mediation Model in Study 1



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.

Subsidiary Analyses

Alternative operationalization of workgroup rituals. Although I operationalized workgroup rituals by averaging participants' responses to the 5-point ritual Likert item, the results are robust when I alternatively operationalize workgroup rituals as the number of activities listed ("number of meaningful activities" ranging from 0 to 3). There was a positive relationship between workgroup rituals and felt workgroup cohesion (H1), β = 0.19, SE(β) = 0.017, $t(415.93)$ = 11.29, p < .001, and a positive relationship between workgroup rituals and workgroup commitment (H2a), β = 0.12, SE(β) = 0.014, $t(363.24)$ = 8.65, p < .001. Moreover, the 95% CI for the index of mediation did not include zero (H2b), indicating a significant mediation effect (Index of Mediation 95% CI [0.069, 0.110]). Overall, these results indicate that the results are robust to an alternate operationalization of workgroup rituals.

Control variables. As shown in Table 5, the statistical significance and direction of the predicted relationships remain unchanged when controlling for the variables listed in the

"Control variables" section, indicating that none of the control variables measured accounted the hypothesized relationships.

Table 5

Robustness Results Controlling for Employee and Workgroup Characteristics

Hypothesis	Standardized Coefficient and 95% CI
H1: Workgroup Rituals → Felt Cohesion	0.17 [0.13, 0.20]
H2a: Workgroup Rituals → Commitment	0.10 [0.07, 0.13]
H2b: Mediation Indirect Effect on Commitment	0.09 [0.07, 0.11]

Notes: These analyses control for the following variables: gender [female dummy], race/ethnicity [nonwhite dummy], age, education [four-year degree dummy], income [measured at wave 0], Big 5 personality traits, in-person communication time, total communication time, intimacy of interactions, loneliness, perceived COVID-19 threat, individual COVID-19 infection [dummy], and 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.

Two results in particular are helpful for ruling out alternative accounts for the effect of physical distancing measures on declining commitment. First, the hypothesized relationships remain robust when controlling for in-person and total communication time. This suggests that communication changes brought about by the transition from in-person to virtual work are unlikely to be fully responsible for the observed results, and that the theorized effects persist above and beyond communication changes (e.g., Davenport & Daellenbach, 2011). Second, the results are robust when accounting for perceived threat, COVID-19 diagnosis, and loneliness. This suggests that even though negative emotions (which can be caused by perceived threat, COVID-19 diagnosis, and loneliness) can reduce group commitment (Klein et al., 2012), the theorized effects persist above and beyond such alternative explanations.

Excluding partial responses. As reported in the "Participants" section, I retained all partial responses in the analyses. Thus, the results reported thus far ($N = 708$) have consisted of participants who completed one, two, three, and four waves. I note that the results remain robust when only analyzing responses from participants who completed all four waves ($N = 415$), excluding participants who completed three waves or less (partial responses). There was a positive relationship between workgroup rituals and felt workgroup cohesion (H1), $\beta = 0.20$, SE(β) = 0.019, $t(318.57) = 11.29$, $p < .001$, and a positive relationship between workgroup rituals and workgroup commitment (H2a), $\beta = 0.11$, SE(β) = 0.016, $t(232.56) = 7.06$, $p < .001$. Moreover, the 95% CI for the index of mediation did not include zero (H2b), indicating a significant mediation effect (Index of Mediation 95% CI [0.067, 0.112]). Overall, these results indicate that the results are robust when analyzing participants who completed all four survey waves.

Discussion

Combining county-level workplace physical distancing (unobtrusively recorded with smartphones) with survey responses from 705 U.S. workers at the onset of the COVID-19 using a within-person, longitudinal data approach, Study 1 showed that engagement in ritualistic workplace activities promotes felt workgroup cohesion, which ultimately fosters workgroup commitment (H1, H2a, H2b). To test for these relationships, Study 1 leveraged geographic differences in workplace physical distancing as a quasi-exogenous shock to workgroup rituals. In this way, the use of archival "Big Data" from smartphones for measuring workplace physical distancing and a longitudinal employee survey for measuring workgroup rituals, felt cohesion, and workgroup commitment combines two distinct data sources, which reduces common method variance and thus increases internal validity (George et al., 2016).

Although quasi-exogenous shocks (such as COVID-19) can combine internal validity with external validity, I recognize this event was not a pure natural experiment. Many social processes were in flux at the onset of workplace physical distancing in the U.S., which provide alternative explanations for the observed results. To rule out some of the likely alternative explanations, I conducted robustness tests controlling for time-varying assessments of communication time (quantity and medium), intimacy of workgroup interactions, loneliness, perceived COVID-19 threat, individual-level COVID-19 diagnosis, and workgroup-level COVID-19 diagnosis. That said, my ability to make causal claims is limited because of the possibility that there are other attributes of individuals and workgroups that change over time which I did not measure and influence the effects observed. This suggests that field experiments that manipulate workgroup rituals using random assignment, and measure subsequent felt cohesion and commitment could be helpful.

Study 2: Cross-Sectional Survey Replication

The goals of Study 2 were: (1) to replicate the results of Study 1 using a preregistered design to increase credibility in the results; (2) use an additional measure of ritual engagement to increase construct validity; and (3) further validate the conceptualization and operationalization of rituals in organizations.

Methods

The survey was preregistered on AsPredicted (https://aspredicted.org/blind.php?x=ji3hf3).

Participants. I predetermined the sample size to recruit 200 participants. To participate, potential participants must have indicated they worked 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. Based on a sensitivity power analysis[10], this sample size has 80% power to detect effects of r = 0.20.

Procedure. I 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." I 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 a self-report questionnaire (details below).

Materials (Survey).

Engagement in workgroup rituals. As in Study 1, I 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, I 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) and "Does the activity symbolize your workgroup values?" (1 = *not at all*, 7 = *very much;* α = .59). As preregistered, I standardized the single-item and three-item measures and averaged them together to yield a standardized ritualism index (α = .64). As evidence of scale validation for the measure used in Study 1, the single-item ritual measure correlated r = .40, p <.001 with the three-item ritual scale.

Felt cohesion. I asked participants to rate felt workgroup cohesion "after participating in the activity" using the same six-item scale reported in Study 1 (1 = *not at all*, 7 = *very much;* Denson et al., 2006; α = .84).

Workgroup commitment. I asked participants to rate their commitment to the workgroup "after participating in the activity" using the same four-item scale reported in Study 1 (1 = *not at all*, 7 = *very much;* adapted from Klein et al., 2014, Roccas et al., 2008; α = .94).

Characteristics of workgroup rituals. For exploratory descriptive purposes, I had participants code their workgroup rituals on several dimensions to provide clarity about the shape and structure of rituals in organizational settings. I 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-

[10] A sensitivity power analysis uses a given α-level, available sample size, and a desired level of power to inform the minimum effect size the sample is sensitive to detect. Unlike a traditional power analysis, sensitivity analyses do not require collecting pilot data to calculate an expected effect size (Fritz et al., 2012). For all sensitivity analyses reported in this dissertation, I set the α-level to 0.05 and desired level of power to 80%.

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."

Results

As predicted, and consistent with Study 1, ritual engagement (ritualism index) was positively related to felt workgroup cohesion (H1), $\beta = 0.32$, SE(β) = 0.07, $t(201) = 4.76$, $p < .001$, and workgroup commitment (H2a), $\beta = 0.41$, SE(β) = 0.063, $t(201) = 6.36$, $p < .001$. Testing for mediation (H2b), the 95% CI for the index of mediation did not include zero, indicating a significant mediation effect of workgroup rituals on workgroup commitment through felt cohesion (Mediation Index = 0.174, $SE = 0.047$, 95% CI [0.081, 0.266]).

Moreover, examining the mode value of participants' codes indicates that workgroup rituals after the enactment of COVID-19 workplace physical distancing were most commonly: 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. See Table 6. These results serve to guide the conceptualization and operationalization of rituals in organizations.

Table 6

Participants' Responses About Their Own Rituals in Study 2

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%	NA
8. Who created the activity?	A) Manager/supervisor B) Self C) The workgroup	58%	15%	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%
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%

Notes: Because participants were asked to report meaningful workplace activities, and not ritualistic activities per se, some participants recalled activities that would not meet the definition of ritual. Thus, to isolate the rituals in the dataset for this table, I filtered the data to focus on the top half of rituals (computed via median split). Specifically, the median ritualism was *Med* = 4.50 (measured on a 7-point Likert scale), so all activities above this point are included in Table 6, which focuses on the properties of rituals (vs. non-rituals). For question number 8 (who created the activity?), additional survey response options included another peer in the workgroup (8%) and someone outside the workgroup

Discussion

Study 2, using an augmented measure of workgroup rituals, bolstered my findings that ritual engagement is related to workgroup commitment because engaging in workgroup rituals increases felt workgroup cohesion. Moreover, these results provided more insight about the nature of rituals in workgroups, indicating that rituals at work tend to be categorized as meetings, involve the entire workgroup, and were invented by the manager/supervisor (top-down process). Through identifying the consequences of altering workgroup rituals, my work shows that rituals are linked to employee commitment, which is a consequential psychological state responsible for a host of beneficial individual and organizational outcomes (e.g., Meyer, 2016).

Chapter 2 Summary

Altogether, Studies 1 and 2 suggest that existing members who engage in strong (vs. weak) group rituals experience greater felt cohesion and ultimately greater commitment to the group (H1, H2a, H2b). This provides initial support for the internal pathway of rituals, linking rituals and commitment among existing members. The use of a naturalistic field survey (Study 1) coupled with a scenario survey (Study 2) presents a multi-method approach to research. There are numerous advantages to this approach, such as establishing both experimental control (internal validity) and natural proof of the phenomenon (external validity). Although both methods have their own weaknesses, in combination, the methods complement each other and increase the strength of evidence through triangulation (Chatman & Flynn, 2005).

CHAPTER 3: GROUP COMMITMENT AND THE PROTECTION OF RITUALS WHEN RITUALS ARE ALTERED

In Chapter 3, I seek to understand whether individuals who are more (vs. less) committed to their group are more likely to protect their groups' rituals from alteration (i.e., the modification of either the physical or psychological features of the ritual). In this context, I view group members' moral outrage towards, and intent to punish, those who attempt to alter rituals as a method of protecting their rituals because offenders who receive moral outrage and are punished should be less likely to try to alter the ritual again (i.e., specific deterrence). Furthermore, if other group members who contemplate altering a ritual witness a target individual receive moral outrage and punishment (for altering a ritual), then other group members are less likely to try to alter a ritual (i.e., general deterrence). Both specific and general deterrence result in the protection of group rituals. Thus, I hypothesized that: (1) more (vs. less) committed group members will experience greater moral outrage in response to a ritual alteration (H3); (2) more (vs. less) committed group members will inflict greater punishment at group members who alter rituals (H4a); and (3) the relationship between member commitment and punishment will be mediated by increases in moral outrage (H4b).

To test these hypotheses, I manipulated (using the vignette methodology) whether a ritual is altered or unaltered, measured the group member's commitment, and measured resulting consequences. Specifically, participants in Studies 3 and 4 learned that an individual did or did not alter a group ritual, reported their level of commitment to the group, and then reported their level of moral outrage and willingness to punish the target individual. I predicted that when individuals are high (vs. low) in commitment, ritual alteration (vs. no alteration) produces more protection of the ritual, as evident by greater moral outrage (combination of anger and perceived immorality) and willingness to punish the target individual (H3, H4a, H4b).

By using an experimental manipulation (alteration vs. unaltered) to moderate the relationship between group commitment and the protection of rituals, Studies 3 and 4 can strengthen internal validity by ruling out alternative explanations for the results. For instance, perhaps more (vs. less) committed individuals are more likely to express negative emotions (such as moral outrage) in general, even in the absence of a ritual alteration. In this way, the experimental manipulation enables me to test whether the relationships between group commitment and moral outrage/punishment are unique to when the ritual is altered.

Moreover, Studies 3 and 4 examined two naturally occurring sociocultural rituals: a U.S. ritual and a Jewish ritual. I selected sociocultural rituals for theoretical and methodological reasons. There is theoretical value in generalizing the model to sociocultural rituals, which tend to be more deeply institutionalized and may take on a different structure and shape than rituals which are invented in workplace settings. Methodologically, cultural groups are large (with millions of members) and thus have many members which are familiar with and care for the ritual, allowing me to recruit a large sample of participants and experimentally manipulate whether the same ritual is altered or unaltered—holding all characteristics of the group and ritual constant between conditions.

Study 3: Alterations to the U.S. Pledge of Allegiance Ritual

I recruited a sample of United States (U.S.) citizens to report their reactions to an ingroup member altering the U.S. Pledge of Allegiance by not standing. The Pledge is an important U.S. ritual with predefined physical behaviors (i.e., standing up, facing the U.S. flag, putting one's right hand over one's heart, and publicly stating the pledge) and meaning (i.e., symbolizing U.S. values such as freedom and liberty). This paradigm was inspired by Colin Kaepernick, who kneeled (rather than stood) during the national anthem to protest racial injustice in 2016. Because I contend that group rituals can reflect group values, I theorized that group members will perceive the ritual alteration as an infringement on U.S. values. Therefore, people higher in commitment to the U.S. will experience relatively greater moral outrage and intent to punish when the Pledge is altered as compared to individuals who are less committed to the U.S. because more committed individuals care more about the U.S. values which are being altered (H3, H4a, H4b). However, when the Pledge is unaltered, I predicted that group commitment will not facilitate greater moral outrage or punishment.

For generalizability, I also examined different intentions for the ritual alteration. In the real world, ritual alterations are rarely undertaken without a stated justification or rationale. For instance, ritual alterations can be undertaken with a positive intention (e.g., make the group more inclusive) or accidental intention. Because group rituals stand for non-negotiable, sacred group values, I predicted that more (vs. less) committed group members will experience greater moral outrage and willingness to punish over any alteration—regardless of the intention behind it. As a result, even a benevolent-intent alteration or an accidental alteration should provoke relatively more outrage and willingness to punish than no alteration. I thus provided four different reasons for why the target individual altered the Pledge: trying to help the U.S. (benevolent-intent condition), trying to harm the U.S. (ill-intent condition), not being able to stand due to a medical condition (lacking-ability condition), and forgetting to stand (accidental condition). I also included two baseline comparison conditions: one in which the participant learned about the alteration but not the reason why it was altered (unknown-intent condition) and another in which there was no alteration.

Method

The analysis plan was preregistered on AsPredicted (https://aspredicted.org/7im7h.pdf).[11]

Participants. I predetermined the sample size to recruit 100 participants in each of the six experimental conditions; in total, 604 adults from Amazon's Mechanical Turk who identified as U.S. citizens (280 females, 324 males; M_{age} = 36.89, SD_{age} = 11.64) participated in exchange

[11] Note that the preregistration focused on testing whether an alteration (vs. no-alteration) to the ritual provokes moral outrage. The moderation by commitment (referred to as "U.S. identification") was mentioned in Section 8. The preregistered analyses are reported in Stein et al. (2021). I deviated from the preregistration to test research questions relating to the theoretical model proposed in this dissertation. Specifically, Stein et al. (2021) examined the main effect of ritual alterations on moral outrage and punishment, operationalizing the independent variable as ritual alteration (altered vs. unaltered). In this work, I focus on group commitment as the independent variable and operationalize ritual alteration as a moderator, such that there is an effect of group commitment on moral outrage and punishment when the ritual is alerted but not when the ritual is unaltered.

for $0.50. Based on a sensitivity power analysis, this sample size has 80% power to detect between-subject effects of $d = 0.40$.

Experimental design. The experimental design was six between-subjects conditions: no-alteration, unknown-intent alteration, benevolent-intent alteration, ill-intent alteration, accidental alteration, and lacking-ability alteration.[12]

Procedure. After completing an eligibility pre-screen (to filter out non-U.S. citizens, per the preregistration), participants read a vignette about the United States President's State of Union address:

> Imagine that you are watching the U.S. President's State of the Union address on TV. The camera hovers over another U.S. citizen just like yourself. They are on a live video stream, as is everyone else in the room. As is typical, the ceremony starts with everyone in the room standing up to recite the Pledge of Allegiance.

In the no-alteration experimental condition, participants then read, "As no surprise, the U.S. citizen stands up while reciting the pledge, choosing to stand with everyone else in the room." In all the alteration conditions, participants instead read, "Much to your surprise, the U.S. citizen stays seated while reciting the pledge, choosing not to stand with everyone else in the room." In the unknown-intent alteration, participants read no further information. In the benevolent-intent, ill-intent, accidental, and lack-ability alteration conditions, they were given a reason to explain why the U.S. citizen altered the ritual. Participants in the benevolent-intent condition read the following:

> You later hear from a friend that the U.S. citizen stayed seated during the pledge because they are part of a new movement of citizens who are trying to make the pledge more inclusive to Americans with disabilities who may not be able to stand. The intent of the movement is to help America become a stronger nation.

Participants in the "ill-intent" condition read the following:

> You later hear from a friend that the U.S. citizen stayed seated during the pledge because they are part of a new movement of citizens who are protesting the pledge because they think that the U.S. has a problematic value system. The intent of the movement is to make America a weaker nation.

Participants in the "accidental" condition read the following:

> You later hear from a friend that the U.S. citizen stayed seated during the pledge because they forgot to stand.

---

[12] The preregistration contained slightly different labels for the experimental conditions: unknown-intent alteration was called "no intent," benevolent-intent alteration was called "good intent alteration," ill-intent alteration was called "bad intent alteration," accidental alteration was called "mistake condition," and lacking-ability alteration was called "justified condition."

Finally, participants in the "lack-ability" condition read the following:

> You later hear from a friend that the U.S. citizen stayed seated during the pledge because they recently injured a leg, and the doctor ordered them to stay seated.

Materials (Survey)

Manipulation check. To measure perceived intention for the ritual alteration, participants in the five alteration conditions answered the following question: "To what extent did the U.S. citizen have a good intention when sitting down during the Pledge of Allegiance?" (1 = *very bad intention*, 7 = *very good intention*).

Moral outrage. I measured moral outrage with self-reported intensity of anger and perceived immorality. To measure anger, I asked participants how (1) angry, (2) mad, (3) irritated, (4) annoyed, and (5) frustrated they would feel toward the U.S. citizen (1 = *not at all*, 7 = *extremely;* $\alpha = .98$). To measure perceived immorality, I asked participants how (1) wrong–right, (2) inappropriate–appropriate, (3) immoral–moral, and (4) inoffensive–offensive the target's decision during the Pledge of Allegiance was (7-point bipolar scales, e.g., 1 = *very wrong*, 7 = *very right*; $\alpha = .92$). Anger and perceived immorality were positively related ($r = .61$, $p < .001$), indicating that the anger was moral outrage.

Punishment. To measure intent to ostracize, I asked participants to indicate the extent to which they would do the following if they saw the U.S. citizen in their community: (1) avoid–meet them, (2) ignore–acknowledge them, and (3) keep them at a distance–keep them close (7-point bipolar scales, e.g., 1 = *definitely avoid them*, 7 = *definitely meet them*; $\alpha = .92$; Ferris et al., 2008).

Group commitment. To measure participants' self-reported commitment to the U.S., I asked participants to rate their agreement with following four items: (1) "I feel strong ties with Americans"; (2) "I see myself as an American"; (3) "I am glad to be an American"; and (4) "I identify as an American" (1 = *not at all*, 7 = *extremely*; $\alpha = .93$; Ellemers et al., 1997).

Results

The means and standard deviations for each experimental condition and dependent measure are shown in Table 7.

Table 7

Participants' Ratings for each Dependent Measure by Experimental Condition in Study 3

Experimental Conditions

	No-Alteration	Unknown-Intent Alteration	Benevolent-Intent Alteration	Ill-Intent Alteration	Accidental Alteration	Lacking-Ability Alteration
Perceived (Benevolent) Intentions	N/A	3.97 (1.50)[a]	5.92 (1.42)[b]	3.89 (1.97)[a]	3.87 (1.30)[a]	5.62 (1.52)[b]
Anger	1.41 (0.99)[a]	2.55 (1.79)[b]	2.18 (1.91)[b]	3.11 (2.07)[c]	2.45 (1.72)[b,c]	1.64 (1.49)[a]
Perceived Immorality	2.56 (1.25)[a]	4.08 (1.34)[c]	3.34 (1.56)[b]	4.40 (1.51)[c]	4.43 (1.30)[c]	2.49 (1.35)[a]
Punishment	3.41 (1.04)[a,b]	3.73 (1.28)[a,c]	3.41 (1.34)[a,b]	4.25 (1.63)[d,e]	3.97 (1.24)[c,e]	3.21 (1.22)[b]

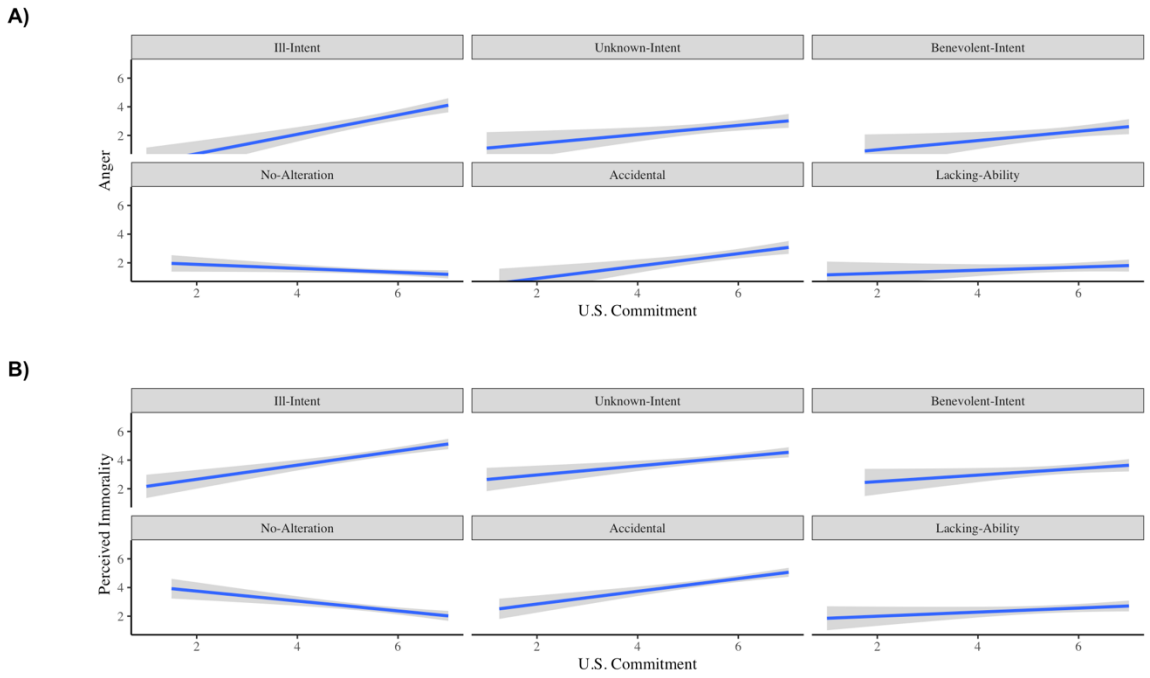
Notes: The mean (standard deviation) is presented for each experimental condition. The superscripts reflect whether the value is statistically significantly different (at the $p = .05$ level) from the other values in the same row.

I first examined whether the experimental manipulation had its intended effect on the manipulation check. Supporting the manipulation, the benevolent-intent and lack-ability conditions did not differ, $t(497) = 1.35$, $p = .176$, $d = 0.20$, and were perceived as more positive than the ill-intent, unknown-intent, and accidental-alteration conditions, $ts(497) > 7.52$, $ps < .001$, $ds > 1.09$. The ill-intent, unknown-intent, and accidental-alteration conditions did not differ, $ts(497) < 0.45$, $ps > .650$, $ds < 0.07$.

I predicted that when individuals are high (vs. low) in commitment, ritual alteration (vs. no alteration) produces more protection of the ritual, as evident by greater moral outrage (combination of anger and perceived immorality) and willingness to punish the target individual (H3, H4a). Supporting these predictions, the effect of commitment to the United States on anger, perceived immorality, and punishment intent was moderated by alteration condition, $Fs(5, 592) = 6.53$, 11.10, and 6.53, $\eta^2_{ps} = 0.05$, 0.09, and 0.05, such that more (vs. less) commitment was associated with more anger, perceived immorality, and punishment in the ill-intent alteration ($\beta s = 0.55$, 0.45, and 0.47, $ts(592) > 5.83$, $ps < .001$), accidental alteration ($\beta s = 0.35$, 0.40, and 0.34, $ts(592) > 3.65$, $ps < .001$), unknown-intent alteration ($\beta s = 0.26$, 0.29, and 0.26, $ts(592) > 2.88$, $ps < .004$), and benevolent-intent alteration conditions ($\beta s = 0.26$, 0.21, and 0.20, $ts(592) > 2.00$, $ps = .007$, .019, and .046). There was no association between commitment and the dependent variables in the lack-ability alteration condition ($\beta s = 0.08$, 0.13, and 0.13, $ts(592) = 1.01$, 1.66, and 1.41, $ps = .312$, .098, and .158) while the association was negative in the unaltered condition ($\beta s = -0.11$, -0.31, and -0.22, $ts(592) = -1.21$, -3.71, and -2.33, $ps = .223$, <.001, and .019; see Figure 3).

Figure 3

Association between Commitment to the U.S. and Anger (Panel A) and Perceived Immorality (Panel B) by Experimental Condition in Study 3

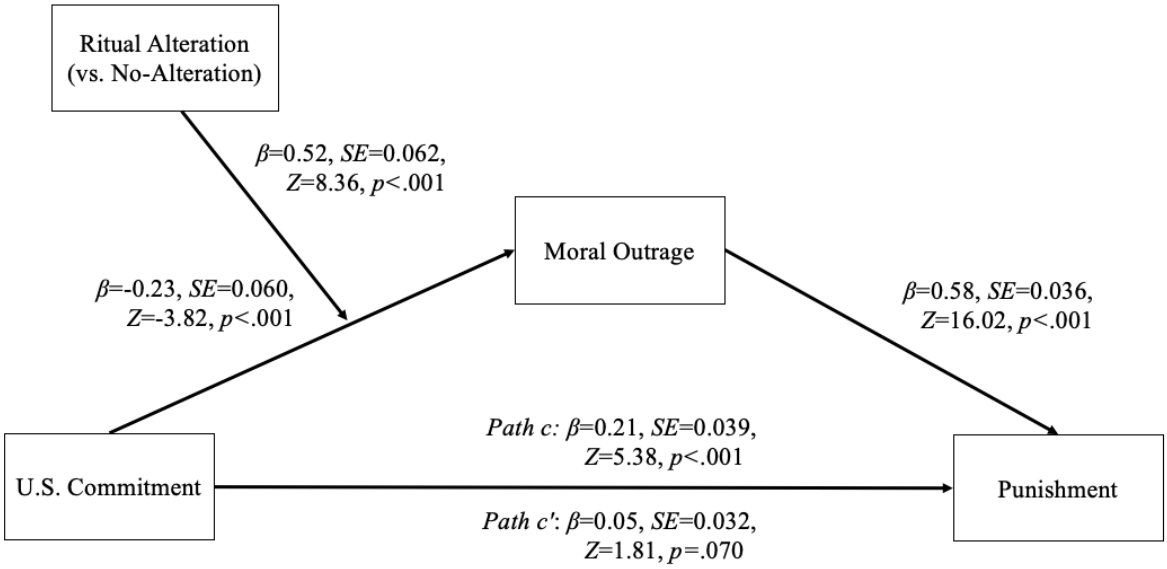


Notes: Visualizations for the relation between the commitment to the U.S. and anger (Panel A) and immorality (Panel B) in Study 3. The results for ostracism are not presented due to space constraints. The grey region reflects 95% confidence interval bands.

Further, I predicted that moral outrage would account for why more (vs. less) committed group members are more likely to punish following an altered ritual (H4b). Testing this mediation prediction, I computed a bootstrapped moderated mediation model (B=5000) using group commitment as the predictor, moral outrage as the mediator (mean composite index), the experimental alteration condition as the moderator, and ostracism as the dependent measure. Specifically, the five alteration conditions were collapsed into a single alteration condition ("1") compared to no-alteration ("0"). Providing support for the predicted pathway (H4b), the 95% confidence level for the estimate of the index of moderated mediation did not include zero, indicating a significant moderated mediation effect (Mediation Index = 0.308, *SE* = 0.046, 95% CI [0.224, 0.408]). As expected, the conditional indirect effect of member commitment on intent to punish through moral outrage was positive when the ritual was altered (Mediation Index = 0.182, *Boot SE* = 0.026, 95% CI [0.136, 0.235]) but negative when the ritual was unaltered (Mediation Index = -0.125, *Boot SE* = 0.034, 95% CI [-0.201, -0.064]). In other words, when a ritual was altered, higher member commitment was associated with *greater* moral outrage, which resulted in punishment of the target individual; in contrast, when the ritual was unaltered, higher member commitment was associated with *less* moral outrage, which resulted in less punishment of the target individual. See Figure 4.

Figure 4

Moderated Mediation Results in Study 3



Notes: Ritual alteration is dummy coded ("1" = alteration, "0" = no-alteration). *Path c* is the total effect of U.S. commitment on punishment (intent to ostracize) and *Path c'* is direct effect of U.S. commitment on punishment, controlling for the mediator and moderator.

Subsidiary analyses. I also examined the main effects of the six experimental conditions on each dependent variable (one-way ANOVAs of experimental condition on anger, perceived immorality, and punishment: $Fs(5, 598) = 13.60, 40.77, \& 9.12$, $ps < .001$, $\eta_p^2 = 0.10, 0.25, \& 0.07$, respectively). For anger, the ill-intent alteration condition produced the most anger, more so than any other experimental condition, $ts(598) > 2.35$, $ps < .019$, $ds > 0.29$. The unknown-intent alteration, benevolent-intent alteration, and accidental alteration conditions produced the next most anger (none significantly different from each other, $ts(598) > 1.52$, $ps > .128$, $ds > 0.20$), compared to the remaining conditions (no-alteration and lacking-ability alteration conditions, $ts(598) > 2.23$, $ps < .026$, $ds > 0.32$). The no-alteration and lacking-ability alteration conditions produced the least anger (no different from each other, $t(598) = -0.97$, $p = .331$, $d = -0.18$).

For perceived immorality, the ill-intent alteration, accidental-alteration, and unknown-intent alteration conditions were seen as most immoral as compared to all other conditions, $ts(598) > 3.77$, $ps < .001$, $ds > 0.51$, and no different from each other, $ts(598) < 1.80$, $ps > .071$, $ds < 0.27$. The benevolent-intent alteration condition was considered significantly less immoral than the three aforementioned conditions, but still more immoral than the no-alteration and lacking-ability alteration conditions, $ts(598) > 3.95$, $ps < .001$, $ds > 0.55$, which were considered least immoral and no different from each other, $t(598) = 0.35$, $p = .726$, $ds = 0.05$.

Finally, for ostracism likelihood, participants were most likely to ostracize targets in the ill-intent alteration condition (as compared to all other conditions, $ts(598) > 2.81$, $ps < .005$, $ds > 0.35$), yet similarly highly likely to do so in the accidental-alteration condition (not different

from the ill-intent alteration condition, $t(598) = 1.52$, $p = .128$, $d = 0.19$). The likelihood of ostracizing in the accidental-alteration condition did not significantly differ from the unknown-intent alteration condition, $t(598) = 1.28$, $p = .200$, $d = 0.19$. As compared to the accidental-alteration condition, participants were relatively less likely to ostracize those in the benevolent-intent alteration, lacking-ability alteration, and no-alteration conditions, $ts(598) > 3.00$, $ps < .003$, $ds > 0.43$ (no different from each other, $ts(598) < 1.12$, $ps > .261$, $ds < 0.16$).

Discussion

Study 3 demonstrated that altering a ritual for any reason other than lacking the ability to perform it properly led participants who were more (vs. less) committed to their group (here, the United States) to feel relatively more outraged by the alteration and more willing to punish the target individual. These results indicated that lacking the ability to perform a ritual is a scope condition of the effect because more (vs. less) committed group members do not exhibit greater moral outrage and intent to punish when the ritual alterer lacks the ability to perform the ritual; simultaneously, these results highlight the ubiquity of moral outrage and punishment for any other reason. Thus, Study 3 supported the theoretical model by demonstrating that more (vs. less) committed individuals are more likely to protect their group rituals by punishing those who attempt to alter them (H3, H4a, H4b). From a methodological standpoint, Study 3 interplayed group commitment (a measured individual difference variable) with an experimental manipulation (ritual alteration), showing that member commitment only affected moral outrage and punishment when the ritual is altered but not unaltered. The experimental manipulation thus ruled out various alternative explanations for the results (e.g., more committed group members are not more likely to be outraged in general), which enhances internal validity.

Study 4: Alterations to the Passover Seder Ritual

In Study 4, I varied the extent to which a host of a Passover dinner, or Seder, alters the Seder plate ritual. Seder is a ritualistic meal conducted annually by Jewish people to retell the story of Passover. According to Jewish belief, each of six items on the Seder plate symbolizes one component of the story of Passover. I predicted that when individuals are high (vs. low) in commitment to Judaism, altering (vs. not altering) the Seder plate produces more protection of the Seder plate, as evident by greater moral outrage (combination of anger and perceived immorality) and willingness to punish the target individual (H3, H4a, H4b).

In addition to examining existing members' (i.e., Jewish participants') reactions to the ritual alteration, I also examined the reactions of outgroup members (i.e., non-Jews). While I predicted that more (vs. less) committed group members will be more outraged when the Seder plate is altered (but not when unaltered), I additionally predicted that there will be no relationship between commitment to Judaism and moral outrage for outgroup members who do not care about the ritual or group values it represents.

For generalizability, I also manipulated the magnitude of the ritual alteration. In the real-world, ritual alterations can constitute a relatively major change (e.g., the entire Seder plate is different) or a relatively minor change (e.g., just one item on the plate is different). Because even minor alterations to a ritual can undermine group values by questioning the legitimacy of the

values it represents, I predicted that high (vs. low) committed group members will experience greater moral outrage and willingness to punish irrespective of whether the alteration is major or minor. To experimentally examine the magnitude of alteration to the ritual, in this vignette experiment, the host either alters zero, one, two, three, four, five, or all six of the Seder items.

Method

The analysis plan and hypotheses are preregistered on AsPredicted (https://aspredicted.org/yq9vq.pdf).[13]

Participants. I predetermined the sample size to recruit 200 participants in each of 14 experimental conditions. However, after running the experiment for two weeks, I encountered difficulties recruiting sufficient Jewish subjects from the Amazon Mechanical Turk subject pool. Without analyzing any data except for the question on participants' religious identification, I posted an amendment to the preregistration (https://osf.io/6kc3z/?vew_only=3cdf0edaeaa245b081893b2331e7f596) and changed the stopping rule to recruit as many Jewish subjects as feasible during the following seven weeks. In total, 2,444 adults (N_{Jews} = 731, $N_{Non\text{-}Jews}$ = 1,713) from Amazon's Mechanical Turk (1,107 females, 1,324 males, 13 other gender; M_{age} = 35.41, SD = 10.81) participated in exchange for $0.50.

Experimental design. The experiment used a 2 (religious identification: Jewish or non-Jewish) × 7 (alteration magnitude: zero, one, two, three, four, five, or six) between-subjects design.

Procedure. First, participants read information about the Seder plate ritual and answered basic comprehension questions. Specifically, they read:

> The Jewish Seder retells the ancient story of Israel's redemption from bondage in Egypt. The word "Seder" means "order" in Hebrew, referring to the specific sequence of events in the Seder ritual, which centers around the Passover Seder meal. The Seder ritual serves to teach the lesson of Exodus, God's saving of the Jewish people from slavery. Tell us (in your own words) what you know about the six items on the Passover Seder plate: Maror, Z'roa, Charoset, Chazeret, Karpas, Beitzah.

I provided participants with a free response box and required them to write at least 25 characters about the Passover Seder to make salient their own understanding of the Jewish ritual. I told participants, "It is completely acceptable if you are unfamiliar with these items" and "There is no right or wrong answer" to this question. Next, participants imagined the following scenario:

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[13] As in Study 3, note that the preregistration focused on testing how the magnitude of alteration affects moral outrage and punishment. The moderation by commitment (referred to as "Jewish identification" in the preregistration) was mentioned in Section 8. The preregistered analyses are reported in Stein et al. (2021). I deviated from the preregistration to test research questions relating to the theoretical model advanced in this dissertation.

> Imagine that you recently moved to an area and your neighbors invite you to attend a Passover Seder. You do not need to be Jewish to attend a Passover Seder – individuals of all faiths are welcome to attend and enjoy the festive meal. When you arrive at the house for Seder, you are greeted by the host, who will be leading the Seder. The host walks with you to the Seder table.

Depending on the assigned condition, participants then learned about the items on the host's Seder plate. Participants learned that the host either alters zero, one, two, three, four, five, or all six of the Seder items, thus testing whether the relationship between commitment and protection of rituals is generalizable to different magnitudes of alterations. In the zero-alteration condition, all the items on the Seder plate were consistent with the Passover Seder ritual. Participants in the zero-alteration condition read the following (which shows the correct Seder plate items):

> The host's Seder plate contains the following items:
> 1. A roasted bone (Zeroah)
> 2. A hard-boiled egg (Beitzah)
> 3. Horseradish root (Marror)
> 4. Mixture of chopped apple, walnuts, and red wine (Charoset)
> 5. Sprigs of parsley (Karpas)
> 6. Romaine lettuce (Chazeret)

In the other alteration conditions, the host's Seder plate included one, two, three, four, five, or six altered items. I randomly selected common party food items to replace the traditional Seder plate items. Participants in the six-alteration condition read the following (which shows all of the alterations that we selected):

> The host's Seder plate contains the following items:
> 1. Peanut butter (Zeroah)
> 2. Spaghetti (Beitzah)
> 3. Yogurt (Marror)
> 4. Chocolate chip cookie (Charoset)
> 5. Ice cream (Karpas)
> 6. Cupcake (Chazeret)

To ensure that the effects would not be unique to the specific item altered, I created conditions for all possible combinations of alterations (i.e., zero-alteration = one condition, one-alteration = six conditions for each of the six possible items, two-alterations = 15 conditions, three-alterations = 20 conditions, four-alterations = 15 conditions, five-alterations = six conditions, six-alterations = one condition). For instance, in the two-alteration condition, the 15 conditions included all possible combinations of two alterations (e.g., one combination was peanut butter for Zeroah and spaghetti for Beitzah, another combination was yogurt for Marror and chocolate chip cookie for Charoset, and so on). As I expected the same pattern of results for each alteration condition, my analyses collapsed the alteration conditions (items altered: 1, 2, 3, 4, 5, 6) into a single alteration condition.

Materials (Survey).

Manipulation check. I assessed the perceived magnitude of alteration with the following question: "How different was the host's Seder plate from the traditional Seder plate?" (0 = *completely the same (i.e., 0 items altered)*, 6 = *completely different (i.e., 6 items altered)*).

Moral outrage and punishment. I measured anger and perceived immorality using the scales described in Study 3 (αs = .98 and .96, respectively). Anger and judgments of immorality were positively related, $r = .87$, $p < .001$, indicating that the anger was moral outrage. To measure intent to ostracize, I used the same scale from Study 3, but changed the Likert scale options to increase the variation in participant responses. Specifically, participants indicated the extent to which they would do the following if they saw the target in their community: (1) avoid them, (2) ignore them, (3) keep them at a distance, and (4) have nothing to do with them (1 = *not at all*, 7 = *extremely*; $\alpha = .98$).

Group commitment. I measured group commitment ($\alpha = .91$) with the scale described in Study 3, except I substituted "American(s)" or "United States" with "Jew(s)" or "Jewish."

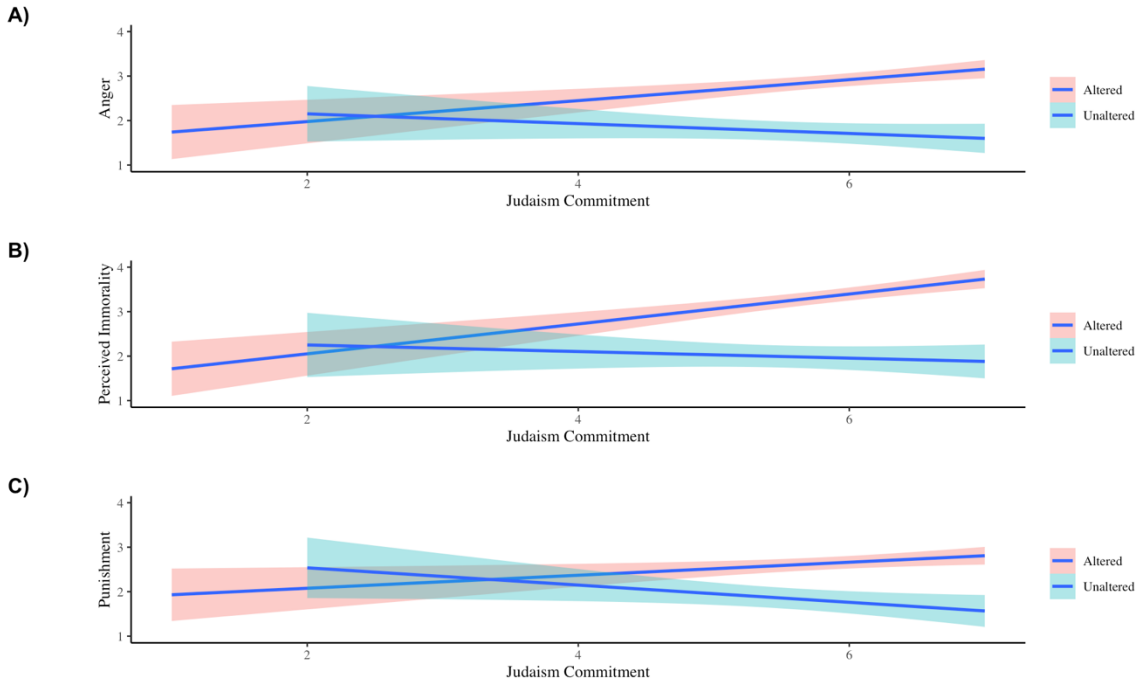
Results

I first examined whether the alteration manipulation had its intended effect on the manipulation check. As expected, the altered Seder plate was viewed as more different from the traditional Seder plate ($M = 4.71$, $SD = 1.56$) than the unaltered Seder plate ($M = 3.78$, $SD = 1.55$), $t(2442) = 10.37$, $p < .001$, $d = 3.23$. Moreover, Jewish participants viewed the alteration as larger ($M_{altered} = 5.16$ vs. $M_{unaltered} = 3.59$, $SD_{altered} = 1.52$ vs. $SD_{unaltered} = 1.45$), $t(2440) = 9.92$, $p < .001$, $d = 3.50$, than non-Jewish participants ($M_{altered} = 4.51$ vs. $M_{unaltered} = 3.86$, $SD_{altered} = 1.53$ vs. $SD_{unaltered} = 1.59$), $t(2440) = 6.13$, $p < .001$, $d = 3.16$ (interaction: $F(1, 2440) = 23.19$, $p < .001$, $\eta^2_p = 0.01$). I further compare reactions of ingroup vs. outgroup members at the end of this section.

I predicted that when ingroup members are high (vs. low) in commitment to Judaism, ritual alteration (vs. no alteration) produces relatively greater moral outrage and punishment intent (H3, H4a). To test this, I regressed anger, perceived immorality, and punishment on group commitment, the experimental alteration manipulation, and the interaction term. As expected, interactions emerged for anger, perceived immorality, and punishment, $Fs(1, 727) = 5.95$, 8.15, and 6.03, $ps = .015$, .004, and .014, $\eta^2_p = 0.01$, .01, and .01, suggesting the ritual alteration condition moderated the relation between group commitment and anger, perceived immorality, and punishment. When the ritual was altered, group commitment positively predicted anger, perceived immorality, and punishment, $\beta s = 0.13$, 0.22, and 0.10, $ts(727) = 3.95$, 5.57, and 2.51, $ps < .012$. In contrast, when the ritual was unaltered, group commitment was unrelated to anger, perceived immorality, and punishment, $\beta s = -0.06$, -0.05, and -0.13, $ts(727) = -0.85$, -0.57, and -1.54, $ps = .391$, .570, and .123. See Figure 5.

Figure 5

Association between Commitment to the Judaism and Anger (Panel A), Perceived Immorality (Panel B), and Punishment (Panel C) by Experimental Condition in Study 4

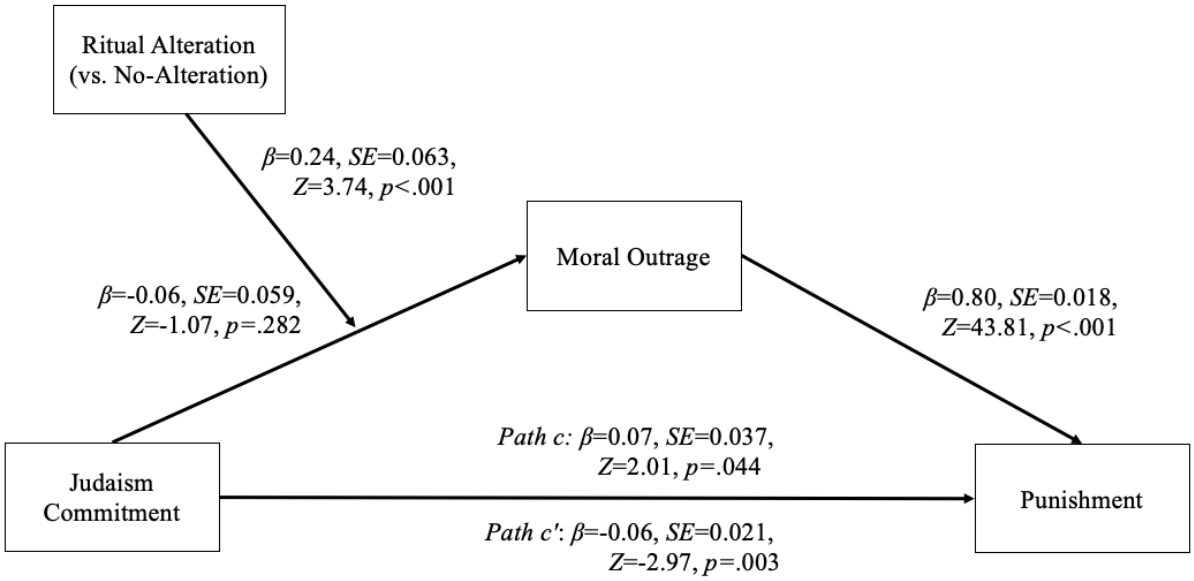


Notes: Visualizations for the relation between the commitment to the Judaism and anger (Panel A), immorality (Panel B), and punishment (ostracism intent; Panel C) among ingroup members in Study 4. The shaded region reflects 95% confidence interval bands.

I further predicted that moral outrage accounts for why more (vs. less) committed ingroup members are more likely to punish those who alter their rituals (H4b). Testing this mediation prediction, I computed a bootstrapped moderated mediation model (B=5000) using group commitment as the predictor, moral outrage as the mediator (composite index), the experimental alteration condition as the moderator, and punishment as the dependent measure. Providing support for the predicted pathway (H4b), the 95% confidence level for the estimate of the index of moderated mediation did not include zero, indicating a significant moderated mediation effect (Mediation Index = 0.357, *SE* = 0.097, 95% CI [0.168, 0.550]). Consistent with my prediction, the conditional indirect effect of member commitment on punishment through moral outrage was positive when the Seder ritual was altered (Mediation Index = 0.270, *Boot SE* = 0.053, 95% CI [0.166, 0.375]) but non-significant when the Seder ritual was unaltered (Mediation Index = -0.087, *Boot SE* = 0.081, 95% CI [-0.246, 0.072]). See Figure 6.

Figure 6

Moderated Mediation Results in Study 4



Notes: Ritual alteration is dummy coded ("1" = alteration, "0" = no-alteration). *Path c* is the total effect of Judaism commitment on punishment (intent to ostracize) and *Path c'* is direct effect of Judaism commitment on punishment, controlling for the mediator and moderator.

Subsidiary analyses. Lastly, I tested whether commitment to the group predicted outrage and punishment when a ritual was altered (vs. unaltered) among ingroup members but not outgroup members. To do so, I regressed anger, perceived immorality, and ostracism on group commitment, the experimental alteration manipulation, group membership (ingroup, outgroup), and their interaction terms. Three-way interactions emerged on anger, perceived immorality, and ostracism, *Fs*(1, 2436) = 4.46, 7.51, and 6.09, *ps* = .035, .006, and .014, η^2_p = 0.002, .003, and .002, indicating group membership (ingroup vs. outgroup) moderated the two-way interactions between ritual alteration and commitment to Judaism. In particular, the two-way interactions between ritual alteration and commitment emerged for ingroup members, *ts*(2436) = 2.95, 3.48, and 2.88, *ps* = .003, <.001, and .004, η^2_p = 0.003, .002, and .000, but was eliminated for outgroup members, *ts*(2436) = 1.41, 1.05, and 0.39, *ps* = .160, .293, and .694, η^2_p = 0.003, .002, and .000. These results suggest that ingroup members, but not outgroup members, protect the ingroups' rituals as a function of commitment to the group.

Discussion

Study 4 demonstrated that Jews with higher (vs. lower) levels of commitment to Judaism experienced more outrage and intent to punish those who attempt to alter the group's Passover Seder ritual (H3, H4a, H4b). When presented with a target individual adhering to the Passover ritual (unaltered condition), moral outrage and punishment did not vary as a function of group commitment, indicating that commitment only affected outrage and punishment when the ritual was altered. Moreover, outgroup members who were more committed to Judaism did not react to the ritual alteration with outrage or punishment, further indicating that the link between group commitment and protection of rituals is unique to committed ingroup members who recognize that the ritual stands for group values. Furthermore, existing members high (vs. low) in group

commitment exhibited a willingness to protect rituals from both major and minor alterations, indicating that a minor alteration is not a scope condition of the effect.

Chapter 3 Summary

Altogether, Studies 3 and 4 suggested that when existing members are high (vs. low) in group commitment, altering a ritual (versus not altering a ritual) produces a willingness to protect group rituals by expressing moral outrage and punishing those who attempt to alter them (H3, H4a, H4b). This provides additional support for the internal pathway of rituals, linking commitment and the protection of rituals among existing members. The use of a variety of different rituals as stimuli and sociocultural groups enhances external validity, while the vignette experimental methodology strengthens internal validity by offering a causal interpretation of the results.

CHAPTER 4: RITUALS AS SIGNALS OF COMMITMENT

In Chapter 4, I seek to understand whether strong (vs. weak) rituals influence prospective group member's willingness to commit to the group. I also examine whether such processes depend on the perception that one's values are congruent with the group's values (subjective value congruence) and the degree one is demographically represented in the group. I hypothesized that: (1) strong (vs. weak) rituals lead prospective members to view the group as having committed members (H5); (2) strong (vs. weak) rituals will produce greater own willingness to commit to the group among prospective members' who perceive that their values are congruent with the groups', but not among prospective members who perceive that their values are incongruent with the group (H6a); (3) stronger perceived member commitment will mediate the relationship between strong (vs. weak) rituals and willingness to commit among prospective members' who perceive that their values are congruent with the groups', but not among prospective members who perceive that their values are incongruent with the group (H6b); and (4) strong (vs. weak) rituals will produce greater own willingness to commit to the group among prospective members' whose surface-level demographics are represented in the group, but not among prospective members whose surface-level demographics are underrepresented in the group (H7).

To test these hypotheses, I conducted Studies 5-8, which had participants imagine being job applicants or recall past job interviews and then evaluate potential work organizations as a function of whether the organization engages in strong or weak rituals. In Study 5, participants learned about a hypothetical organization's workplace activity that is strongly or weakly ritualized and subsequently rated the degree to which they perceive members of the organization as committed to the organization (testing H5). In doing so, Study 5 also established construct validity for the manipulation of ritual by controlling for a wide range of alternative confounding variables. Study 6 tested whether learning about a hypothetical organization's strong or weak ritual influenced perceived member commitment and ultimately, an individual's own willingness to commit to the organization in an experimental design using fictional company reviews (testing H5 and H6a). Study 7 tested the predictions in an externally valid context, asking participants to recall prior job interviews and rate the degree that they perceive that the organization they interviewed with engages in ritualistic behavior (testing H5 and H6a). Study 7 also collected participant's judgments about the degree to which their own values overlapped with the organization's values as well as the perceived diversity of the organization, enabling me to test whether subjective value congruence and perceived organizational diversity moderated the relationship between perceived member commitment and own willingness to commit (testing H6b and H7). Finally, Study 8 tested the possible moderating influence of subjective value congruence using an experimental design in which participants imagined that an organization engages in a strong or weak ritual (testing H5, H6a, and H6b).

Study 5: Ritualizing Cultural Artifacts

The goal of Study 5 was to test whether the enactment of organizational rituals influenced perceived member commitment (H5). To do this, Study 5 presented participants with cultural artifacts that are either high or low in ritualism. Artifacts, which are defined as "all the phenomena that one sees, hears, and feels when one encounters a new group with an unfamiliar

culture" (Schein, 2010, p. 25), include the visible products of the group, such as language, artistic creations, emotional displays, and myths told about the organization. I theorized that cultural artifacts can be ritualized when they include the physical features of ritual (e.g., sequence, rigidity) and psychological features of ritual (e.g., meaning). For instance, the telling of organizational myths can be ritualized when the myth is told at the same time, place, and day, with the same people, as opposed to being told in more variable circumstances. I selected three different cultural artifacts to be used as stimuli in Study 5 based on Schein's (2010) list of cultural artifacts: myths, artistic creations, and emotional displays. I orthogonally manipulated the degree to which each artifact is ritualized, and I expected the same pattern of results to emerge for each artifact.

At its core, ritualism involves a certain physical structure and psychological meaning. To manipulate ritualism as cleanly as possible, I varied whether workplace activities are strongly or weakly physically sequenced (e.g., occur in a rigid order). My objective was to manipulate ritualism without manipulating related constructs (such as norm content and intensity, attributes about the organization). Thus, I selected the sequencing manipulation because it holds constant the type and content of the activity performed (e.g., both the strong and weak ritual is the same activity, except for the sequence it is performed). As an empirical check that the ritualism manipulation did not conflate ritual with other constructs, I measured a host of control variables about the ritual and organization to test whether the ritual manipulation influenced perceived member commitment above and beyond these related constructs: culture strength (measured through descriptive and injunctive norms), culture content (collectivism, adaptability, integrity values), activity valence, co-worker liking, perceived hierarchy, organization size, and perceived average salary. I predicted that job applicants who learn about a workplace activity that is strongly (vs. weakly) ritualized will perceive greater commitment among employees, even when controlling for these related constructs.

Method

The analysis plan was preregistered on AsPredicted (https://aspredicted.org/X6N_947).

Participants. I predetermined the sample size to recruit 225 participants in each of the six experimental conditions; in total, 1,355 adults from Prolific Academic who reported current full-time employment (541 females, 799 males, 15 other gender; M_{age} = 34.95, SD_{age} = 10.10) participated in exchange for $0.75. Based on a sensitivity power analysis, this sample size has 80% power to detect between-subject effects of d = 0.27. I increased the sample size per condition in Study 5 (compared to Studies 3 and 4) to 225 because pilot testing revealed that the hypothesized effect size in Study 5 was substantially weaker than the observed effect size in Studies 3 and 4.

Experimental design. The experiment used a 2 (ritual: strong or weak) × 3 (vignette: myth, art, or emotion) between-subjects design.

Procedure. To begin, I asked participants to "imagine that you are considering joining a new work organization" and that "we are interested in your thoughts and perceptions about the organization." I presented participants with one of three vignettes. In each vignette, participants

learned that the work organization engages in a strong or weak ritual in one of the following three domains: organization onboarding activity (myth condition), office decorations (art condition), and birthday celebration (emotion condition). I selected these three domains because Schein (2010) referenced them as examples of cultural artifacts. I manipulated the artifacts' ritualism by describing the activity as relatively sequenced (i.e., activities always occur in same order) or not sequenced (e.g., activities occur in different order). In doing so, the content of activity itself was held constant between conditions.

Participants in the **strong** or weak ritual condition, and in the myth, art, or emotion vignette, read the following information presented in Table 8. After reading the vignette, participants completed a questionnaire which assessed their beliefs about the work organization.

Table 8

Ritual Vignettes in Study 5

Myth	Art	Emotion
The first cohort of employees at this organization were onboarded the following way: employees first completed a scavenger hunt, then received a company t-shirt, and, lastly, learned a myth about the organization. Since the first cohort was onboarded in the early days of the company, the employee onboarding process has occurred in **[exactly the same way]**/[different ways] each year. **[Just like]**/[Unlike] the original cohort of employees, each new cohort of employees complete the following activities in **[the same order]**/[a random order that varies each year]: [sometimes] completing a scavenger hunt first, receiving a company t-shirt second, and, learning a myth about the organization third. **[Specifically]**/[For instance], in the year 2021, employees **[completed a scavenger hunt]**/[received a company t-shirt] on Monday at 9 am. On Tuesday at noon, they **[received a company t-shirt]**/[learned a myth about the organization]. On Wednesday at noon, they **[learned a myth about the organization]**/[completed a scavenger hunt]. The next cohort of new employees will complete the activities in **[the same]**/[a different] order and at **[the same]**/**[a different]** time of week. The order of the onboarding process is [not] important to the employees. Employees generally agree that the onboarding process should work **[the same way]**/[in different ways] each time.	The founder of the company decorated her office the following way: she first selected the colors, then made a sketch of the new office design, and, finally, picked out the new office furniture. Since the founder decorated her office in the early days of the company, employees have decorated their office in **[exactly the same way]**/[different ways]. **[Just like]**/[Unlike] how the founder decorated her office, employees complete the following activities in **[the same order]**/[a random order that varies each time]: [sometimes] making a sketch first, picking out the new office furniture next, and selecting the colors last. **[Specifically]**/[For instance], in the year 2021, employees **[selected the colors during the first week of office redesign]**/[made a sketch during the first week of office redesign]. In the second week, they **[they made a sketch]**/[picked out the new office furniture]. In the third week, they **[picked out the new office furniture]**/[selected the colors]. Future employees will complete the activities in **[the same order]**/[a different order] each week. The order of the decorating process is [not] important to the employees. Employees generally agree that teams should be decorating their offices in **[the same]**/[different ways] each time.	The two company founders celebrated birthdays in the following way: they first told a joke, then gave a present, and, finally, sang the "Happy Birthday" song. Since the founders celebrated each other's birthday in the early days of the company, teams have celebrated each member's birthday in **[exactly the same way each time]**/[in different ways each time]. **[Just like]**/[Unlike] the founders, employees complete the following activities in **[the same order]**/[random order that varies each time]: [sometimes] telling a joke first, giving presents second, and, singing the "Happy Birthday" song third. **[Specifically]**/[For instance], for the most recent birthday, employees **[told jokes]**/[gave presents] at 4:30 pm. At 4:40 pm, they **[gave presents]**/[sang the "Happy Birthday" song]. At 4:55 pm, they **[sang the "Happy Birthday" song]**/[told jokes]. Employees will complete the activities in **[the same order]**/[a different order] and **at [the same]**/[a different] time of day for the next birthday. The order of the birthday celebrations is [not] important to the employees. Employees generally agree that teams should celebrate birthdays in **[the same way]**/[different ways] each time.

Materials (Survey).

Manipulation check. To measure the degree the organization engages in rituals, I asked participants the following single-item measure modified from Study 1 ("To what extent do you consider these activities to be ritualistic? Ritualistic workplace activities symbolize

organizational values and tend to be done multiple times in the same specific way each time"; 1 = *not at all*, 7 = *very much*). The activities rated are described in Table 8.

Dependent variable: Perceived member commitment. To measure the degree participants believed the work organization has committed employees, I asked participants the following three questions: "To what extent do you think employees at this organization are expected to": (a) "be committed to the organization?", (b) "care about the organization?", and (c) "be dedicated to the organization?" (1 = *not at all*, 7 = *very much*; Klein et al., 2014; α = .92).

Control variable: Culture strength. To empirically disentangle the effects of ritual (as an artifact of culture) from the effects of culture strength (i.e., the intensity and consensus of norms), I measured the organization's culture strength with survey items for descriptive norms as a proxy for norm consensus and injunctive norms as a proxy for norm intensity. I asked participants the following single-item measure to measure descriptive norms ("To what extent do you consider these three activities to be descriptive norms for this organization? Descriptive norms are behaviors that most people actually engage in, exhibit, or express in a given situation"; 1 = *not at all*, 7 = *very much*). I asked participants the following single-item measure to measure injunctive norms ("To what extent do you consider these three activities to be injunctive norms for this organization? Injunctive norms are behaviors that people believe others should engage in, exhibit, or express in a given situation"; 1 = *not at all*, 7 = *very much*). Lastly, I included an additional injunctive norm scale to increase construct validity, asking participants the following: "To what extent is it:" (a) "acceptable", (b) "appropriate", and (c) "comfortable" to complete the three activities?" (1 = *not at all*, 7 = *very much*; Dannals & Miller, 2017; α = .85).

Control variable: Culture content. To disentangle the effects of rituals (as an artifact of culture) from the effects of the content of culture, participants rated this organization's collectivism norm with four items (individualistic [reverse-scored], competitive [reverse-scored], collectivistic, team-oriented; α = .25), adaptability norm with three items (rule-oriented [reverse-scored], adaptable, fast moving; α = .38), and integrity norm with three items (honest, respect, integrity; α = .85). All items were rated on a 7-point scale (1 = *not at all*, 7 = *very much;* Chatman & Barsade, 1995*).* Unexpectedly, the reliabilities for the collectivism and adaptability scales were below acceptable levels; thus, the results using these measures should be interpreted with caution.

Control variable: Activity valence. As more ritualistic activities might be viewed as more positive, and positivity could influence judgments of perceived commitment, I conducted robustness tests by controlling for activity valence. To measure the degree the activities are positive or negative, I asked participants the following single-item measure ("From very negative to very positive, how do you think employees feel about these activities?"; 1 = *very negative*, 7 = *very positive*).

Control variable: Co-worker liking. As more ritualistic activities might be viewed as a signal that co-workers like each other, and the degree that co-workers like each other could positively influence perceived commitment, I conducted robustness tests by controlling for perceived co-worker liking. To measure the degree that co-workers like each other, I asked

participants the following single-item measure ("Do you think employees at this organization like each other?"; 1 = *not at all*, 7 = *very much*).

Control variable: Perceived hierarchy. As more ritualistic activities might be viewed as a signal that the organization has more hierarchy, and the organization's perceived hierarchy could positively influence perceived commitment (because more hierarchy translates into more control, boosting compliance), I conducted robustness tests by controlling for perceived hierarchy. To measure the perceived hierarchy of the organization, I asked participants the following single-item measure ("To what extent do you think this organization is hierarchical?"; 1 = *not at all*, 7 = *very much*).

Control variable: Organizational size. As more ritualistic activities might be viewed as a signal that the organization is smaller in size, and the organization's size could negatively influence perceived commitment (because smaller organizations are less desirable to work for), I conducted robustness tests by controlling for the organization's perceived size. To measure the perceived size of the organization, I asked participants the following single-item measure ("How many employees do you think work at this organization?"; 1 = *1-99*, 2 = *100-249*, 3 = *250-499*, 4 = *500-999*, 5 = *1000 or more*).

Control variable: Average salary. As more ritualistic activities might be viewed as a signal that the organization is more cohesive and thus has better pay, and the organization's average salary could positively influence perceived commitment, I conducted robustness tests by controlling for the organization's perceived average salary. To measure the perceived average salary at this organization, I asked participants the following single-item measure ("What do you think is the average salary at this organization?"; 1 = $20,000 to $29,999, 2 = $30,000 to $49,999, 3 = $50,000 to $74,999, 4 = $75,000 to $99,999, 5 = $100,000 to $200,000, 6 = Greater than $200,000).

Results

As is typical when multiple self-report variables are measured, Table 9 presents zero-order correlations among measured study variables.

Table 9

Means, standard deviations, and correlations with confidence intervals in Study 5

Variable	1	2	3	4	5	6	7	8
1. Ritual (Manipulation Check)								
2. Perceived Commitment	.32** [.27, .37]							
3. Descriptive Norm	.23** [.17, .28]	.26** [.21, .31]						
4. Injunctive Norm (Primary)	.21** [.16, .26]	.27** [.22, .32]	.37** [.33, .42]					
5. Injunctive Norm (Alternative)	.07* [.01, .12]	.30** [.25, .35]	.36** [.31, .41]	.33** [.28, .38]				
6. Integrity Norm	.05 [-.00, .10]	.34** [.30, .39]	.32** [.27, .37]	.22** [.17, .27]	.48** [.43, .52]			
7. Adaptability Norm	-.24** [-.29, -.19]	.04 [-.01, .09]	.08** [.03, .13]	-.01 [-.06, .05]	.24** [.19, .29]	.46** [.42, .50]		
8. Collectivism Norm	.29** [.24, .34]	.17** [.12, .22]	.09** [.03, .14]	.12** [.07, .18]	.08** [.03, .13]	-.05* [-.11, -.00]	-.31** [-.35, -.26]	
9. Activity Valence	.04 [-.01, .10]	.28** [.23, .33]	.30** [.25, .35]	.24** [.19, .29]	.52** [.48, .56]	.61** [.57, .64]	.44** [.40, .48]	-.08** [-.13, -.02]
10. Co-worker liking	.10** [.05, .15]	.28** [.23, .33]	.27** [.22, .32]	.24** [.19, .29]	.43** [.39, .48]	.59** [.55, .62]	.37** [.33, .42]	.03 [-.02, .08]
11. Hierarchy	.23** [.18, .28]	.25** [.20, .30]	.11** [.06, .16]	.12** [.07, .18]	.02 [-.03, .08]	.02 [-.04, .07]	-.19** [-.24, -.14]	.08** [.02, .13]
12. Size	.01 [-.05, .06]	-.01 [-.06, .05]	-.04 [-.09, .02]	.03 [-.02, .08]	.02 [-.03, .08]	-.01 [-.06, .05]	.08** [.03, .13]	-.08** [-.13, -.02]
13. Avg. Salary	-.01 [-.06, .05]	.08** [.03, .14]	.06* [.00, .11]	.03 [-.02, .09]	.11** [.06, .16]	.07** [.02, .13]	.03 [-.03, .08]	-.02 [-.07, .04]

Note: M and SD are used to represent mean and standard deviation, respectively. Values in square brackets indicate the 95% confidence interval for each correlation. The confidence interval is a plausible range of population correlations that could have caused the sample correlation. * indicates $p < .05$. ** indicates $p < .01$.

I first tested whether the ritual manipulation had the intended effect on the manipulation check. As expected, strong rituals (M = 5.99, SD = 1.23) were perceived as more ritualistic than weak rituals (M = 4.63, SD = 1.67), $t(1353) = 17.05$, $p < .001$, $d = 0.93$. Testing whether the effect of the ritual condition varied depending on the vignette (artifact type), there was an interaction between ritual condition and vignette on the manipulation check, $F(2, 1349) = 13.87$, $p < .001$, $\eta^2_p = 0.02$, which suggested the effect of ritual condition on the manipulation check did vary depending on vignette. The effect of ritual condition on the manipulation check was significant for each vignette individually, $ts(1349) > 6.48$, $ps < .001$, $ds > 0.63$, although the effect was stronger in the emotion and art vignettes than the myth vignette, $\beta s > 0.27$, $ts(1349) > 2.31$, $ps < .021$.

I next tested whether the ritual condition influenced perceived member commitment. As hypothesized (H5), strong rituals (M = 5.94, SD = 0.95) made participants perceive relatively greater commitment than weak rituals (M = 5.66, SD = 0.99), $t(1353) = 5.28$, $p < .001$, $d = 0.29$. Testing whether the effect of the ritual condition varied depending on the vignette (artifact type), there was an interaction between ritual condition and vignette on perceived commitment, $F(2, 1349) = 3.53$, $p = .029$, $\eta^2_p = .00$. While the effect of ritual on perceived commitment was non-significant in the myth vignette, $t(1349) = 1.01$, $p = .314$, $d = 0.10$, the effect of ritual on perceived commitment was significant in the emotion and art vignettes, $ts(1349) = 3.44$, $ps < .001$, $ds > 0.33$.

Lastly, I tested whether the effect of ritual on perceived commitment (H5) held when accounting for cultural variables (culture strength, culture content), organizational characteristics (co-worker liking, size, average employee salary), and the valence of the activity. As shown in Table 10, the effect of ritual on perceived commitment remains significant when controlling for these variables, $ts(1346) > 3.68$, $ps < .001$.

Table 10

Control Variable Results in Study 5

	Dependent variable:	
	Perceived Member Commitment	
	(Model 1)	(Model 2)
Ritual Condition (strong=1, weak=0)	0.186***	0.253***
	(0.051)	(0.049)
Emotion Vignette Dummy	-0.047	0.049
	(0.059)	(0.060)
Myth Vignette Dummy	0.013	0.060
	(0.059)	(0.061)
Descriptive Norm	0.069***	
	(0.020)	

Injunctive Norm	0.096***	
	(0.018)	
Integrity Norm	0.293***	
	(0.027)	
Adaptability Norm	-0.049	
	(0.031)	
Collectivism Norm	0.167***	
	(0.034)	
Activity Valence		0.152***
		(0.024)
Perceived Co-Worker Liking		0.144***
		(0.029)
Perceived Organizational Hierarchy		0.179***
		(0.018)
Perceived Organizational Size		-0.020
		(0.022)
Perceived Average Salary		0.050**
		(0.023)
Constant	2.786***	3.118***
	(0.229)	(0.170)
Observations	1,355	1,355
R^2	0.205	0.185
Adjusted R^2	0.201	0.181
Residual Std. Error (df = 1346)	0.876	0.887
F Statistic (df = 8; 1346)	43.516***	38.314***

Note: *p<0.1; **p<0.05; ***p<0.01. The control table uses the primary injunctive norm measure instead of the alternative injunctive norm measure; results are robust to either measure.

Discussion

Study 5 demonstrated that prospective group members interpret the enactment of strong (vs. weak) rituals to be a signal that the organization has higher member commitment (H5). To control for the possibility that the ritual manipulation confounds other related constructs—perhaps organizations that engage in stronger rituals are expected to pay higher salaries, which accounts for effect on perceived member commitment—I measured and conducted robustness tests with an array of organizational characteristics: cultural variables (culture strength, culture content), organizational characteristics (co-worker liking, size, average employee salary), and the valence of the activity. The results were robust with the inclusion of these control variables,

suggesting that rituals uniquely influenced the extent that prospective group members believed that the organization has committed group members. In this way, Study 5 provided preliminary support for the external pathway of rituals, suggesting that prospective members view the display of ritual as a signal about the underlying characteristics of group members' perceived commitment (H5).

Study 6: Rituals and Company Reviews

In Study 6, participants read reviews of a fictional company that manipulated whether the organization engages in strong or weak rituals. Job applicants often read employer reviews on sites such as Glassdoor when evaluating whether to join a prospective organization (e.g., Corritore et al., 2020), suggesting that the manipulation of ritual within company reviews is externally valid. As in Study 5, I predicted that job applicants will evaluate the organization that engages in strong (vs. weak) rituals as having stronger member commitment (H5). Moving beyond Study 5, Study 6 tested whether strong (vs. weak) rituals influenced a prospective group member's own willingness to join the group (H6a). To control for the possibility that the ritual manipulation confounds other related constructs, I again measured and controlled for an array of organizational characteristics.

Method

The analysis plan was preregistered on AsPredicted (https://aspredicted.org/VZG_TJQ).

Participants. I predetermined the sample size to recruit 400 participants per between-subjects condition; in total, 803 adults from Prolific Academic (400 females, 397 males, 6 other gender; M_{age} = 36.42, SD_{age} = 12.67) participated in exchange for $1.05. Based on a sensitivity power analysis, this sample size has 80% power to detect between-subject effects of d = 0.20.

Design. The experiment was a 2(ritual: strong vs. weak) between-subjects factor × 7 (vignette) within-subjects factor.

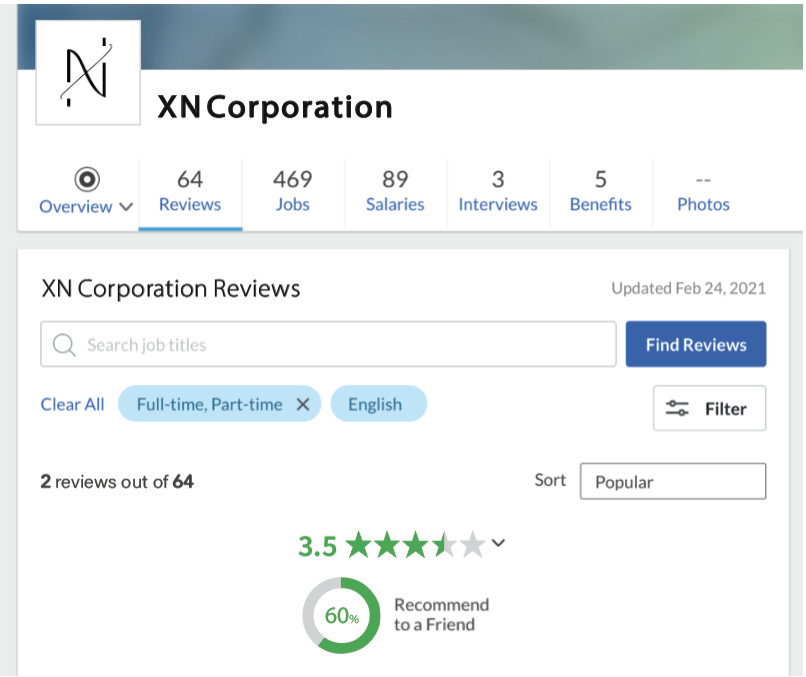
Procedure. To begin, participants read a fictional profile about a work organization named XN Corporation:

> "XN Corporation is an international company focusing on consumer-packaged goods. With over 10,000 employees, XN Corporation has typical business functions including research and development, accounting and finance, human resource management, sales, legal, and marketing. XN Corporation is hiring employees in your area and has a job opening that matches your qualifications."

To increase the realism of the experiment, and provide more context about the XN Corporation, I presented participants with a standardized profile about XN Corporation, which was identical between ritual conditions (see Figure 7).

Figure 7

Company Profile in Study 6



To manipulate whether the XN Corporation engages in strong or weak rituals, I presented participants with "excerpts of employee reviews about XN Corporation" and told them that "the reviews are from employees who work within the job unit you applied to join." The seven excerpts of employee reviews which manipulated ritualism are presented in Table 11. These seven excerpts are based on Schein's (2010) discussion of organizational rituals and informal anecdotes I collected from colleagues across different industries (e.g., high technology, marketing consulting). The strong rituals were written to possess more physical features of ritual (e.g., rigid, repetitive, sequenced actions) and psychological features of ritual (e.g., expression of organizational values) compared to the weak rituals. For instance, farewells to departing employees can either be strongly ritualized (e.g., ceremony where the employee's "number" is retired) or weakly ritualized (e.g., group meeting to discuss how to work in the employee's absence). In this case, the activity remains the same (a farewell to a departing employee), but the activity is either relatively more or less ritualistic. To confirm this manipulation affected ritualism, participants completed a manipulation check. Participants either read about seven strong rituals or seven weak rituals (between subjects).

After reading each review, participants judged the review writer's commitment to the organization. Then, after reading all seven reviews, participants judged the review writer's overall commitment and completed questions about the organization as a whole.

Table 11

Employee Review Excerpts in Study 6

Type of Activity	Strong Ritual	Weak Ritual
Farewell	Whenever a top performer leaves the organization, we have a company ceremony where we symbolically retire their "number" in the company hall of fame.	Whenever a top performer leaves the organization, we have a company conversation about how to fill the hole the person left.
Lunch	There is a formal company-wide lunch every other Tuesday, which is delicious.	The company eats lunch together a few times a month, which is delicious.
Chant	If you join this organization, then you'll need to learn the organization's chant, which we recite every morning.	If you join this organization, then you'll need to learn the U.S. Pledge of Allegiance, which we recite every morning.
Learning	Our "lunch and learn" events always happen on Friday at 3. Learning is a big part of the organization's philosophy.	Our "lunch and learn" events happen once a week and the time varies each week. Learning is a big part of the events.
Communication	If you are sending an important email, then you should end it with an affirmation of the organization's motto.	If you are sending an important email, then you should mark it as priority.
Socializing	Employees always get drinks together Friday after work.	Employees will get drinks together, usually about once a week.
Dinner	The Friday before Thanksgiving, we have an organization-wide potluck dinner, which always involves bringing family members into work for the day.	Once a year (the date varies), we have an organization-wide potluck dinner, which sometimes involves bringing family members into work for the day.

Materials (Survey).

Manipulation check. To measure the degree participants believed the organization engages in rituals, I asked participants the same single-item measure from Study 5 ("To what

extent do you consider these activities to be ritualistic? Ritualistic workplace activities symbolize organizational values and tend to be done multiple times in the same specific way each time"; 1 = *not at all*, 5 = *extremely*).

Dependent variable: Perceived member commitment. To measure participant's global beliefs that the organization has committed members, I asked participants the same three questions as in Study 5: "Given what you read about the organization from the review excerpts, overall, to what extent do you think that current employees voluntarily:" (a) "are committed to the organization?", (b) "care about the organization?", and (c) "are dedicated to the organization?" 1 = *not at all*, 7 = *extremely*; Klein et al., 2014; α = .89). I refer to this measure as "global" perceived commitment.

As discussed during the Procedure, I also asked participants to judge the review writer's commitment after reading each review ("To what extent do you think the writer of this review [the employee] feels voluntarily committed to the organization?"; 1 = *not at all*, 5 = *extremely*). Since participants completed this measure seven times (once for each review), I aggregated this variable to the participant-level. I refer to this measure as the "aggregated" perceived commitment measure. I expected both measures (global and aggregated perceived commitment) to show a similar pattern of results.

Dependent variable: Willingness to commit. To measure the participant's own willingness (propensity) to commit to the organization, I asked participants the following three questions: "Imagine that you received an offer from this organization within the job unit to which you applied. You decide to accept the offer and join the organization. Given what you read about the organization from the review excerpts, to what extent would you:" (a) "be committed to the organization?"; (b) "care about the organization?"; and (c) "be dedicated to the organization?" (1 = *not at all*, 7 = *extremely*; Klein et al., 2014; α = .95). Because participants are imagining that they are prospective members (not actual members), this is a measure of willingness to commit instead of actual commitment.

Control variable: Culture strength and content. To control for the organization's perceived cultural strength and content, I asked participants the following four items: "To what extent do you XN Corporation has": (a) "a strong culture?", (b) "a team-oriented culture?", (c) "an adaptable culture?", and (d) "a respectful culture?" (1 = *not at all*, 7 = *extremely*; modified from Chatman & Barsade, 1995). The items were not collapsed together; the first item measured culture strength, the second item measured a collectivism culture, the third item measured an adaptable culture, and the final item measured an ethical culture.

Control variables: Co-worker liking, hierarchy, and average salary. I used the same measures for perceived co-worker liking, hierarchy, and average salary as in Study 5 (1 = *not at all*, 7 = *extremely*). Unlike Study 5, I did not measure organization size because participants were told the organization has 10,000+ employees (see vignette).

Results

First, I examined whether the manipulation had the intended effect on the manipulation check. As expected, in the strong ritual condition, the organization was rated as engaging in stronger rituals ($M = 4.57$, $SD = 0.65$) than in the weak ritual condition ($M = 4.17$, $SD = 0.80$), $t(801) = 7.74$, $p < .001$, $d = 0.55$.

Second, I examined whether the ritual condition influenced perceived member commitment and own willingness to commit (H5, H6a). There was a marginally significant difference between the strong ritual condition and weak ritual condition on global perceived commitment ($M_{strong} = 3.68$ vs. $M_{weak} = 3.58$, $SD_{strong} = 0.61$ vs. $SD_{weak} = 0.52$), $t(801) = 1.86$, $p = .063$, $d = 0.13$, providing a weak conceptual replication of Study 5 and weak evidence for H5. However, when controlling for the predetermined control variables (culture intensity, culture content, hierarchy, organization size, average salary, co-worker liking), the effect is not robust, $t(793) = 0.70$, $p = .487$. Moreover, there was no effect of condition on aggregated perceived commitment ($M_{strong} = 3.45$ vs. $M_{weak} = 3.46$, $SD_{strong} = 0.69$ vs. $SD_{weak} = 0.69$), or own willingness to commit ($M_{strong} = 3.37$ vs. $M_{weak} = 3.32$, $SD_{strong} = 0.98$ vs. $SD_{weak} = 0.91$), $ts(801) = -0.22$ and 0.77, $ps = .827$ and $.439$, $ds = -0.02$ and 0.05, respectively, indicating no evidence for H6 or H6a.

Lastly, for exploratory purposes, I examined whether the experimental condition influenced the control variables. I reasoned that if the experimental condition influenced one or more control variables, then this would indicate the manipulation has some effect on perceived cultural and organizational characteristics—but not the hypothesized effect. The organization in the strong (vs. weak) ritual condition was viewed as having a stronger culture ($M_{strong} = 4.11$ vs. $M_{weak} = 3.81$, $SD_{strong} = 0.82$ vs. $SD_{weak} = 0.80$), more team-oriented culture ($M_{strong} = 4.22$ vs. $M_{weak} = 4.07$, $SD_{strong} = 0.78$ vs. $SD_{weak} = 0.73$), less adaptable culture ($M_{strong} = 3.02$ vs. $M_{weak} = 3.21$, $SD_{strong} = 1.09$ vs. $SD_{weak} = 0.95$), more hierarchy ($M_{strong} = 3.82$ vs. $M_{weak} = 3.59$, $SD_{strong} = 0.86$ vs. $SD_{weak} = 0.85$), and paying employees more ($M_{strong} = 3.21$ vs. $M_{weak} = 3.06$, $SD_{strong} = 0.98$ vs. $SD_{weak} = 0.83$), $ts(801) > 2.53$, $ps < .011$, $ds > 0.18$. There were no differences between the strong and weak ritual condition on an ethical culture ($M_{strong} = 3.60$ vs. $M_{weak} = 3.62$, $SD_{strong} = 0.90$ vs. $SD_{weak} = 0.79$) and perceived co-worker liking ($M_{strong} = 3.02$ vs. $M_{weak} = 3.21$, $SD_{strong} = 0.74$ vs. $SD_{weak} = 0.70$), $ts(801) < -0.38$, $ps > .702$, $ds < -0.03$. These results indicate the experimental condition influenced a host of attributes about the organization (ranging from cultural variables to hierarchy), indicating the manipulation did have some effect (but not the hypothesized effect) on how participants perceived the organization.

Discussion

Using fictional employee reviews, Study 6 failed to support my hypotheses: strong (vs. weak) organizational rituals did not influence perceived member commitment or the prospective member's own willingness to commit. Although the experimental condition had a marginally significant effect on one of the operationalizations of perceived commitment, the effect was not robust when including the control variables, indicating weak support for the hypothesis. Despite the null results on the hypothesized dependent variables, the experimental manipulation influenced a host of control variables (such as the culture and different organizational characteristics), indicating that the manipulation did influence perceptions about the organization but not in the hypothesized manner.

Hypothesis 6a proposes that strong (vs. weak) rituals will produce greater own willingness to commit to the group among prospective members who perceive value congruence, but not among prospective members who perceive value incongruence. Study 6 did not manipulate or measure perceived value congruence, so it is possible the null results for H6a are due to not incorporating a critical moderator variable. That said, the failure to account for subjective value congruence does not explain the null results for H5.

A methodological reason may account for the null results on H5 and H6a. Although the ritual manipulation check was statistically significant, the manipulation was relatively weak. Specifically, participants viewed the weak ritual as at least somewhat ritualistic (M = 4.17, SD = 0.80), and the strong ritual was only rated 0.40 higher on a 7-point scale (M = 4.57, SD = 0.65). This indicates the ritual manipulation may have failed to capture the full breadth of strong to weak rituals because participants viewed the weak ritual as somewhat ritualistic (above midpoint on 7-point scale) and the strong ritual as only a bit more ritualistic. However, in the real-world, activities do vary from very high in ritualism to very low in ritualism. As a result, the experimental manipulation—which attempted to cleanly manipulate ritualism in a controlled manner—may have artificially reduced the possible range of ritualism observed in the real-world, leading to the non-significant findings on the dependent variables. Thus, to capture a wider variation in ritualism and provide a more rigorous test of the hypotheses, Study 7 turned to the field and collected real-world observations of rituals which can capture the full breadth of strong to weak rituals that exist in the real-world.

Study 7: Recalling Real Job Interviews

Study 7 recruited a sample of full-time employees and asked them to reflect on past job interviews. For each job interview, participants reported the degree to which the organization they interviewed with engages in ritualistic behavior, the perceived commitment of employees, as well as their own willingness to commit based on their observations during the interview. I predicted that observing strong (vs. weak) organizational rituals would lead job applicants to perceive employees are committed (H5). Moreover, participants also reported the degree to which the organization's values are compatible with their own values as well as the perceived racial/gender diversity of the organization, allowing me to test whether subjective value congruence and perceived racial/gender diversity of the organization moderate the hypothesized relationship between perceived member commitment and willingness to commit (H6a, H6b, H7). As in Studies 5 and 6, to control for the possibility that the ritual manipulation confounds other related constructs, I again measured and controlled for an array of organizational characteristics.

Method

The analysis plan was preregistered on AsPredicted (https://aspredicted.org/M9P_XQF).

Participants. I predetermined the sample size to recruit 300 participants; in total, 304 adults from Prolific Academic who reported current full-time employment (220 females, 77 males, 7 other gender; M_{age} = 24.29, SD_{age} = 6.83) participated in exchange for $0.80. Based on a

sensitivity power analysis, this sample size has 80% power to detect between-subject effects of $r = 0.16$.

Procedure. To begin, I asked participants (all of whom had full-time work experience) to write down names of organizations that they interacted with as a job applicant:

> "Please recall the organizations that you interviewed with in the past ten years but ultimately did not join. Please recall both organizations you did and did not receive a job offer from (as long as you didn't join the organization). The interviews could have been in-person or remote (e.g., phone or teleconference). While internships, part-time jobs, and full-time jobs count, internal interviews (e.g., interviewing for a job at an organization you're already working for) do not count."

Participants recalled up to 10 work organizations. I chose the selection criteria that the interview must have occurred in the past ten years because participants were asked to recall details of the organization, so recalling interviews 10+ years ago is subject to difficulty in memory recall. Furthermore, I asked participants not to recall cases when they joined the organization or interviewed for an internal position because the theoretical predictions focus on prospective group members, not existing group members, and joining the organization after the interview or applying for an internal position indicates that the participant had more familiarity with the organization than a prospective member would. Thus, data from these cases were not collected and thus these conditions cannot be analyzed.

After writing the names of up to 10 work organizations, participants answered the survey questionnaire about up to five organizations. If participants wrote down more than five organizations, the survey randomizer selected five of the organizations to be rated. If participants wrote down less than five organizations, they rated all the organizations they listed. The survey questionnaire contained the independent variable (ritualistic workplace activities) and dependent variables (perceived member commitment, willingness to commit), among other exploratory variables and control variables.

Given participants rated multiple organizations, my focus is on within-person effects. That is, the more a focal participant believes the organization engages in rituals, the more I predict the participant will perceive members to be committed. The decision to focus on within-person variance is useful because it eliminates confounds that arise from between-person differences, providing a more robust test of the hypotheses. Since at least two observations per participant are required to study relationships at a within-person level, $n = 30$ participants who listed 1 or 0 organizations were excluded from analysis.[14]

Materials (Survey).

[14] This exclusion may pose a threat to internal validity if the excluded participants are more likely than the included participants to exhibit no actual relationship between the independent and dependent variables. To rule out this possibility, I find that the results are robust when including these participants.

Independent variable: Organizational rituals. To measure the organization's engagement in rituals, I asked participants the following four items: "To what extent did workplace activities (on average):" (a) "need to be done in the same specific way each time?"; (b) "occur at the same location and time of day/week, with the same people?"; (c) "bring meaning to employees?"; and (d) "symbolize the organization's values?" (1 = *not at all*, 7 = *extremely*; α = .73). These items are modified from the survey scale in Study 2.

Dependent variable: Perceived member commitment. To measure the degree participants believed the organization has committed members, I asked participants the same three questions modified from Studies 5-6: "To what extent did employees at [organization] voluntarily:" (a) "be committed to the organization?", (b) "care about the organization?", and (c) "be dedicated to the organization?" 1 = *not at all*, 7 = *extremely*; Klein et al., 2014; α = .96).

Dependent variable: Willingness to commit. To measure the participant's own willingness to commit to the organization, I asked participants the same three questions as in Studies 5-6: "Now imagine that you received an offer and joined [organization] within the job unit you applied to join. To what extent would you have:" (a) "been committed to the organization?"; (b) "cared about the organization?"; and (c) "been dedicated to the organization?" (1 = *not at all*, 7 = *extremely*; Klein et al., 2014; α = .96).

Moderator: Subjective culture fit. To measure the participant's subjective culture fit, I asked participants the following two questions: (a) "I felt I fit with the culture of this organization"; and (b) "The organizational culture was similar to my desired organizational culture" (1 = *not at all*, 7 = *extremely*; modified from Resick et al., 2007; *r* = .82).

Moderator: Perceived gender and race demographic similarity. To measure the participant's perceptions of gender and race demographic similarity, I asked participants the following two questions: "Now imagine that you received an offer and joined [organization] within the job unit you applied to join:" (a) "What percent of your co-workers would be the gender race as you?" and (b) "What percent of your co-workers would be the same race as you?" (0 to 100%). I operationalized these measures as perceived gender similarity and perceived race similarity, respectively.

Control variables: Culture strength, culture content, co-worker liking, average salary, and organizational size. I used the same measures for culture strength, culture content, co-worker liking, organizational size, and average salary as in Study 6. I used the same measure for organizational size as in Study 5.

Control variable: Interview characteristics. As characteristics of the interview may influence both judgments of rituals and own willingness to commit, representing a within-person confound, I also measured the interview format (*Phone*, *Teleconference [e.g., Zoom]*, or *In-person*), the number of people the participant met during the interview, the outcome of the interview (*Did not receive an offer* or *Received an offer but did not accept it*), and organization type (publicly listed, private enterprise, government, other).

Results

Table 12 presents zero-order correlations among measured study variables.

Table 12

Means, standard deviations, and correlations with confidence intervals in Study 7

Variable	1	2	3	4	5	6	7
1. Ritualistic Workplace Activities							
2. Perceived Member Commitment	.56** [.52, .60]						
3. Willingness to Commit	.49** [.44, .54]	.73** [.70, .76]					
4. Subjective Norm Congruence	.45** [.40, .50]	.60** [.56, .64]	.72** [.69, .75]				
5. Perceived Racial Similarity	-.01 [-.07, .05]	-.00 [-.06, .06]	.01 [-.05, .07]	-.00 [-.07, .06]			
6. Perceived Gender Similarity	.03 [-.04, .09]	.02 [-.05, .08]	.02 [-.04, .08]	.02 [-.04, .08]	.17** [.10, .23]		
7. Strong Culture	.54** [.50, .58]	.71** [.67, .74]	.61** [.57, .65]	.61** [.57, .65]	-.03 [-.09, .03]	.02 [-.04, .08]	
8. Collectivism Norm	.50** [.45, .54]	.59** [.55, .63]	.52** [.47, .56]	.51** [.46, .55]	-.02 [-.08, .04]	.02 [-.04, .08]	.67** [.63, .70]
9. Adaptability Norm	.46** [.41, .51]	.57** [.52, .61]	.55** [.51, .60]	.59** [.55, .63]	-.05 [-.11, .02]	.03 [-.03, .09]	.63** [.59, .67]
10. Integrity Norm	.51** [.46, .55]	.64** [.61, .68]	.61** [.57, .65]	.63** [.59, .67]	-.03 [-.09, .03]	.03 [-.03, .10]	.70** [.67, .73]
11. Hierarchy	.13** [.07, .19]	.05 [-.01, .11]	-.02 [-.08, .04]	-.05 [-.11, .02]	-.06 [-.12, .00]	-.05 [-.11, .01]	.06 [-.00, .12]
12. Size	-.07* [-.13, -.00]	-.13** [-.19, -.07]	-.09** [-.15, -.03]	-.09** [-.15, -.03]	-.09** [-.15, -.03]	-.12** [-.18, -.05]	-.03 [-.09, .04]
13. Avg. Salary	.10** [.04, .16]	.16** [.10, .22]	.15** [.09, .21]	.09** [.03, .16]	-.06* [-.13, -.00]	-.05 [-.11, .01]	.18** [.12, .24]
14. Co-worker liking	.42** [.37, .47]	.58** [.54, .62]	.54** [.50, .59]	.53** [.49, .58]	-.00 [-.07, .06]	.00 [-.06, .06]	.55** [.50, .59]

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 $p < .01$.

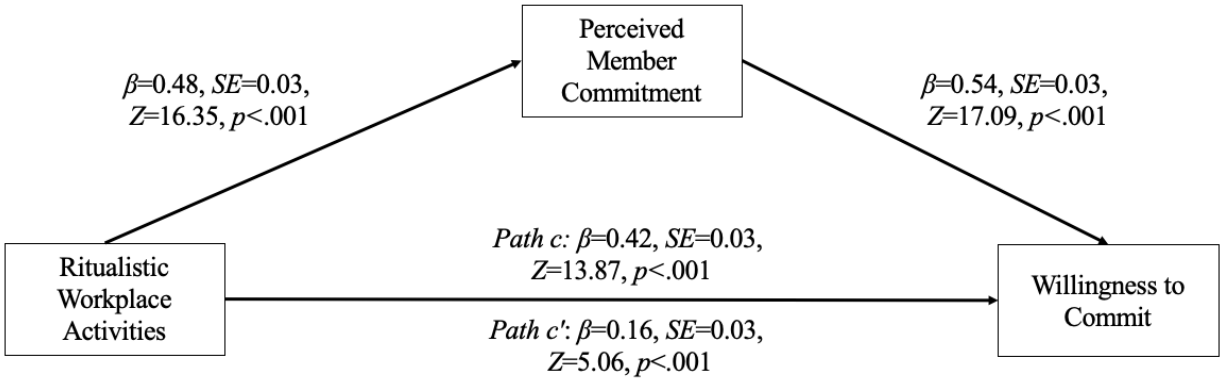
To account for the nested data structure (participants provide two or more observations), I conducted multilevel modeling with observations nested within participants and a random slope to allow intra-individual predictors to vary between participants (i.e., a random intercept and slope model; Aguinis et al., 2013). I 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). Importantly, this within-between approach indicates that the coefficients represent within-person changes, controlling for between-person differences. Thus, the statistical model removes concerns that between-person differences drive the correlational results (see Study 1 for a similar method).

As expected, there was a positive relationship between organizational rituals and perceived member commitment (H5), $\beta = 0.41$, SE(β) = 0.03, $t(231.46) = 12.81$, $p < .001$. I next tested whether subjective value congruence and perceived demographic similarity moderated the relationships between organizational rituals, perceived member commitment, and own willingness to commit (H6a, H6b, H7). Not supporting my hypotheses, subjective value congruence, racial demographic similarity, and gender demographic similarity did not moderate the relationship between organizational rituals and own willingness to commit (H6a, H6b, H7), $\beta < 0.05$, $p > .376$. Moreover, none of the variables moderated the relationship between organizational rituals and perceived member commitment, $\beta < 0.05$, $p > .334$, nor the relationship between perceived member commitment and own willingness to commit, $\beta < 0.03$, $p > .540$.

Subsidiary analyses. Given H5 was supported but my predictions regarding moderation by subjective value congruence and demographic similarity were not supported (H6a, H6b, H7), I tested for the relationships between organizational rituals, perceived member commitment, and own willingness to commit independent of the hypothesized moderators (subjective value congruence and demographic similarity). I found a positive relationship between perceived member commitment and own willingness to commit, $\beta = 0.50$, SE(β) = 0.03, $t(245.72) = 16.80$, $p < .001$. Furthermore, perceived member commitment mediated the relationship between ritualistic workplace activities and own willingness to commit (Indirect Effect = 0.25, $SE = 0.02$, 95% CI [0.214, 0.301]). See Figure 8. As shown in Table 13, these results remain robust when accounting for the culture control variables (culture strength, culture content), organizational-level control variables (hierarchy, organization size, average salary, co-worker liking), and interview control variables (interview format, number of interviewers, outcome, organization type). Specifically, Model 1 shows the relationship organizational rituals and perceived member commitment remained robust, Model 2 shows the relationship between perceived member commitment and willingness to commit (without controlling for organizational rituals) remained robust, and Model 3 shows the relationship between perceived member commitment and willingness to commit (while controlling for organizational rituals) remained robust. Although not supporting my moderation hypotheses (H6a, H6b, H7), these results show the pathways between organizational rituals, perceived member commitment, and own willingness to commit is remarkable robust after accounting for alternative explanations.

Figure 8

Mediation Results in Study 7



Notes: Coefficients are standardized. Path c is the total effect of ritualistic workplace activities on willingness to commit and Path c′ is direct effect of ritualistic workplace activities on willingness to commit, controlling for the mediator (perceived member commitment).

Table 13

Control Variable Results in Study 7

	Dependent variable:		
	Perceived Commitment	Willingness to Commit	
	(1)	(2)	(3)
Organizational rituals (within)	0.131***		0.075***
	(0.026)		(0.026)
Organizational rituals (between)	0.136***		0.003
	(0.033)		(0.043)
Perceived commitment (within)		0.290***	0.270***
		(0.030)	(0.031)
Perceived commitment (between)		0.315***	0.324***
		(0.032)	(0.036)
Strong Culture	0.197***	0.064***	0.057***
	(0.020)	(0.021)	(0.021)
Collectivism Norm	0.050***	0.003	-0.001
	(0.019)	(0.020)	(0.020)
Adaptability Norm	0.029	0.057***	0.060***
	(0.020)	(0.021)	(0.021)
Integrity Norm	0.081***	0.101***	0.095***
	(0.021)	(0.022)	(0.022)
In-person interview (d)	-0.096**	0.050	0.045
	(0.043)	(0.046)	(0.046)

Number of interviewers	0.014	-0.008	-0.011
	(0.015)	(0.016)	(0.016)
Hierarchy	0.013	-0.020	-0.023
	(0.014)	(0.015)	(0.015)
Size	-0.051***	-0.003	-0.001
	(0.013)	(0.014)	(0.014)
Average Salary	0.017	0.033**	0.036**
	(0.015)	(0.016)	(0.016)
Co-worker liking	0.141***	0.086***	0.084***
	(0.022)	(0.023)	(0.023)
Receive offer (d)	-0.129***	-0.045	-0.040
	(0.043)	(0.046)	(0.046)
Constant	-2.899***	-2.874***	-2.830***
	(0.172)	(0.168)	(0.199)
Observations	981	962	951
Log Likelihood	-954.635	-979.660	-973.264
Akaike Inf. Crit.	1,951.270	2,001.321	1,992.528
Bayesian Inf. Crit.	2,053.930	2,103.570	2,104.251

Notes: Regression coefficients for organizational rituals, perceived member commitment, and willingness to commit are standardized. Dummies for organization type are included. Both within-person and between-person relationships are presented, although the hypotheses concern within-person relationships. p<0.1; **p<0.05. (d) = dummy variable.

Discussion

Study 7 provided support for the hypotheses that learning about an organization's rituals during applicant recruitment promotes the perception that members are committed (H5). Notably, this result was observed solely relying on within-person variance, eliminating concerns that between-person differences drive this observed relationship. Furthermore, I conducted robustness tests which control for a host of observation-level covariates that vary within-person, such as culture strength, culture content, and the number of interviewers the participant (i.e., the applicant) met, further providing evidence that the observed relationship was not driven by alternative confounds.

However, subjective value congruence and perceived gender/racial similarity did not emerge as significant moderators, failing to support H6a, H6b, and H7. I hypothesized that perceived gender/racial similarity are moderators because diversity is a cue to subjective value congruence; thus, the non-significant findings for the perceived gender/racial similarity moderator (H7) might be due to the non-significant findings for subjective value congruence (H6a, H6b). That said, Study 7 unexpectedly revealed that organizational rituals, perceived member commitment, and own willingness to commit are positively related without accounting for value congruence or perceived gender/racial similarity, opening several possibilities such as my hypotheses (H6a, H6b, and H7) need revision.

Thus, I conducted Study 8 to test whether the non-significant results for H6a, H6b, and H7 are due to: (1) the hypotheses needing revision (such that value congruence and perceived gender/racial similarity are not critical moderators in the relationship between organizational rituals and own willingness to commit); or (2) or an idiosyncratic paradigm issue produced null results (perhaps my measurement of subjective value congruence and perceived gender/racial similarity failed to capture the intended constructs).

Study 8: Workplace Activity Vignette

In Study 8, participants imagined that they were job applicants for a hypothetical work organization and either learned that the organization engages in a strong or weak ritual. To manipulate ritualism, participants learned about a rigid, repetitive workplace activity (consistent with the physical features of ritual) that was either connected to organizational values (strong ritual) or unconnected to organizational values (weak ritual). In this way, Experiment 8 held constant the physical features of ritual and directly manipulated the psychological features of ritual (symbolism, meaning). Thus, Study 8 built on Study 7 by testing for the relationships between organizational rituals, perceived member commitment, and own willingness to commit in an experimental design (H5). Study 8 also tested for moderation by subjective value using a different operationalization of subjective value congruence as in Study 7 (H6a, H6b). Instead of measuring subjective value congruence with statements about general culture alignment, which encompasses many beliefs outside the scope that the ritual signals, Study 8 measured value congruence by asking participants the extent they like the actual values represented in the ritual. In this way, Study 8 used a more specific and targeted measure of value congruence as opposed to Study 7.

Method

The analysis plan was preregistered on AsPredicted (https://aspredicted.org/Z8R_8R8).

Participants. I predetermined the sample size to recruit 250 participants per between-subjects condition; in total, 503 adults with full-time work experience from Prolific Academic (247 females, 249 males, 6 other gender; M_{age} = 35.74, SD_{age} = 16.78) participated in exchange for $0.75. Based on a sensitivity power analysis, this sample size has 80% power to detect between-subject effects of d = 0.25.

Design. The experimental design is a 2(experimental ritual condition: strong ritual vs. weak ritual) between-subjects design.

Procedure. To begin, participants read the same profile about XN Corporation as in Study 6:

> "XN Corporation is an international company focusing on consumer-packaged goods. With over 10,000 employees, XN Corporation contains several different departments including research and development, accounting and finance, human resource management, sales, legal, and marketing. XN Corporation is hiring employees in your area and has a job opening that matches your qualifications."

To increase the realism of the experiment, participants also viewed the XN Corporation's logo. Next, participants read about a specific event that XN Corporation holds for employees: "A unique aspect of XN Corporation is that it hosts an employee story-telling event five times per year. At each event, five employees each share a personal story for five minutes each. The event takes place on the fifth weekday of each month." The event has the physical features of ritual: rigid, sequenced, and repetitive elements. For instance, the event always happens five times a year, on the same day of the month, and involves sharing a story for five minutes. This event is based on an actual employee story-telling event hosted by R/GA San Francisco (a marketing organization).

To manipulate ritualism, I varied whether the event was connected or unconnected to organizational values. Participants learned that "one of the most important values of the XN Corporation is treating its employees like family" and the story-telling event is either connected (strong ritual) or unconnected (weak ritual) to organizational values. Participants in the strong ritual condition read the following:

> "Therefore, during the story-telling event, each employee shares a story about a time when they treated another employee like family. Last year, some employees discussed how they celebrated holidays with their peers. In this way, the story-telling event exemplifies XN Corporation values."

Participants in the weak ritual condition read the following:

> "During the story-telling event, employees do not need to share stories related to the corporate values; they can discuss anything they want. Last year, some employees discussed personal memories from their summer outdoor trips. In this way, the event does not always exemplify XN Corporation values."

After reading the vignette, participants in both experimental conditions completed an identical survey.

Materials (Survey).

Ritual manipulation check. To measure the degree participants believed the organization engages in rituals, I asked participants the same single-item measure from Study 5 (1 = *not at all*, 5 = *a great deal*). As a secondary manipulation check, I measured the degree the activity is connected to organizational values with the following single-item measure ("To what extent does the story-telling event stand for XN Corporation values?"; 1 = *not at all*, 5 = *a great deal*). The two items were positively related, $r = .38$, $p < .001$.

Perceived member commitment. To measure members' perceived commitment, I asked participants the following two questions: "Based on the story-telling event that you read about, how much would you estimate that the average XN Corporation employee voluntarily:" (a) "feels a sense of 'ownership' for the company rather than feeling like just an employee?" and (b) "tells their colleagues that the company is a great one to work for?" (1 = *not at all*, 5 = *a great*

deal; O'Reilly & Chatman, 1986; $r = .67$). I used O'Reilly & Chatman's (1986) commitment scale instead of Klein et al.'s (2014) from Studies 5-7 to increase the commitment's construct validity.

Willingness to commit. To measure the participant's own willingness to commit to the organization, I asked participants the following two questions: "Now please imagine that you applied to work organizations and received an offer and ultimately decided to join the XN Corporation. Based on the story-telling event that you read about, to what extent would you:" (a) "feel a sense of 'ownership' for the company rather than feeling like just an employee?" and (b) "tell your colleagues that the company is a great one to work for?" (1 = *not at all*, 5 = *a great deal*; O'Reilly & Chatman, 1986; $r = .78$). Consistent with the perceived member commitment scale, I used O'Reilly & Chatman's (1986) commitment scale instead of Klein et al. (2014) to increase construct validity.

Subjective value congruence. To measure the participant's personal congruence with XN Corporation's focus on treating employees like family, I asked participants the following question ("To what extent do you personally like the idea of treating employees as family in the workplace?"; 1 = *not at all*, 5 = *a great deal*).

Control variables: Culture strength, culture content, co-worker liking, average salary, and perceived hierarchy. I used the same measures for culture strength, culture content, co-worker liking, average salary, and perceived hierarchy as in Study 6, except I used "an ethical culture" instead of "a respectful culture" to measure culture content related to an ethical culture.

Results

First, I examined whether the manipulation had the intended effect on the manipulation check. As expected, in the strong ritual condition, the organization was rated as engaging in more ritualistic behavior ($M = 3.97$, $SD = 0.90$) than in the weak ritual condition ($M = 3.48$, $SD = 0.93$), $t(500) = 5.95$, $p < .001$, $d = 0.53$. Furthermore, in the strong ritual condition, the story-telling event was perceived as more connected to organizational values ($M = 3.84$, $SD = 0.95$) than in the weak ritual condition ($M = 2.93$, $SD = 1.05$), $t(500) = 10.21$, $p < .001$, $d = 0.91$.

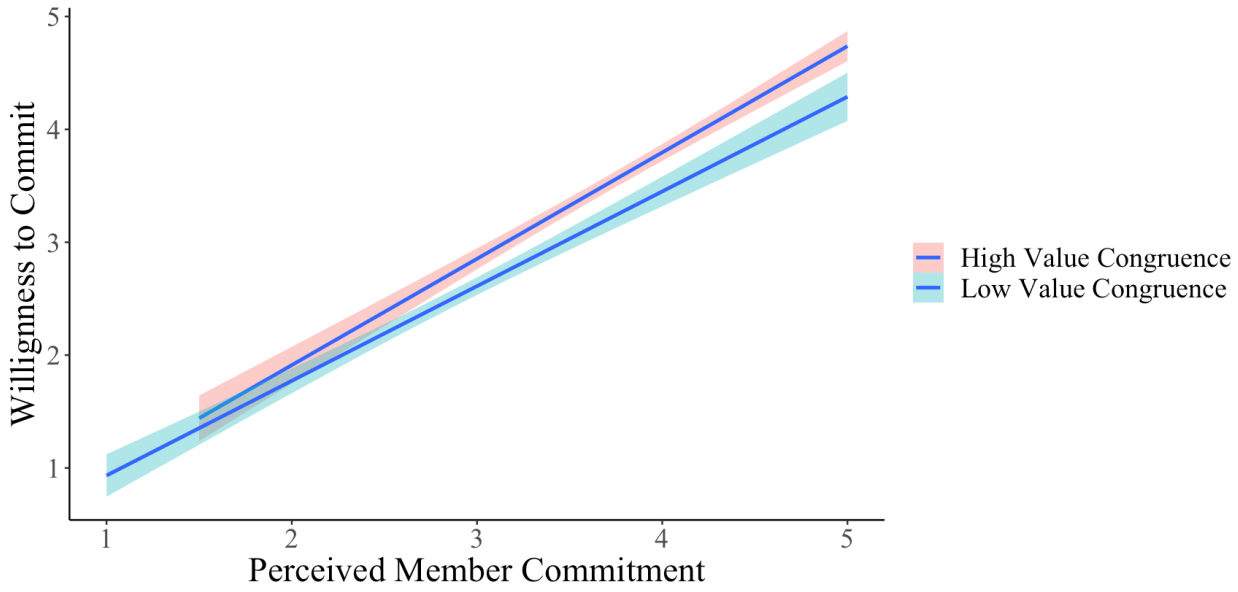
Second, I examined whether the experimental condition influenced perceived commitment and own willingness to commit. Supporting my hypothesis (H5) and replicating Study 6, in the strong ritual condition, participant's perceived stronger member commitment ($M = 3.39$, $SD = 0.96$) and reported greater own willingness to commit ($M = 3.11$, $SD = 1.10$) than in the weak ritual condition ($Ms = 3.08$ and 2.81, $SDs = 0.86$ and 1.02), $ts(500) = 3.80$ and 3.18, $ps < .001$, $ds = 0.34$ and 0.28. These results remained significant when controlling for culture (strength, team-oriented content, adaptability content, ethical content), perceived co-worker liking, perceived average salary, and perceived hierarchy, $ts(492) = 2.94$ and 2.58, $ps = .005$ and .010.

Third, I theorized that subjective value congruence moderates the relationship between perceived member commitment and own willingness to commit (H6a), such that the relationship is stronger when participants perceive value congruence but weakens (or even reverses) when

participants perceive value incongruence. In a multiple regression, the relationship between perceived member commitment and own willingness to commit remained robust while controlling for subjective value congruence, $\beta = .71$, $t(498) = 24.9$, $p < .001$, indicating perceived member commitment exerts an independent influence above and beyond value congruence. Replicating prior research on the functions of value congruence (e.g., Chatman, 1989), there was also a relationship between subjective value congruence and own willingness to commit while controlling for perceived member commitment, $\beta = 0.21$, $t(498) = 7.27$, $p < .001$. Beyond these "main effects" and consistent with my hypothesis (H6a), there was an interaction between perceived member commitment and subjective value congruence on own willingness to commit, $\beta = .06$, $t(498) = 2.69$, $p = .007$. The nature of the interaction indicated the relationship between perceived member commitment and own willingness to commit was stronger when participants were higher (+1 SD) in value congruence, $\beta = .78$, $t(498) = 21.21$, $p < .001$, than when lower (-1 SD) in value congruence, $\beta = .65$, $t(498) = 17.10$, $p < .001$. See Figure 9.

Figure 9

Moderation Results in Study 8



Notes: For visualization purposes, subjective value congruence was converted to a binary variable via median split. The colored regions reflect 95% confidence interval bands.

Lastly, I examined whether perceived member commitment mediated the relationship between experimental condition and own willingness to commit. Specifically, I examined whether the mediating influence of perceived member commitment between the experimental ritual condition and own willingness to commit is moderated by subjective value congruence (H6b). Inconsistent with my hypothesis, the 95% CI for the moderated mediation indirect effect included 0, indicating non-significant moderated mediation (Moderated Mediation Effect = 0.02, SE = 0.01, 95% CI [-0.004, 0.045]).

Discussion

Study 8 demonstrated that applicants who learned about a strong (vs. weak) organizational ritual believed that employees are committed to the organization (H5). Notably, Study 8 observed this pattern of results in an experimental design, enabling me to draw causal inferences about the effects of organizational rituals on prospective group members.

Study 8 also offers statistical support for the hypothesis that the positive effect of organizational rituals on own willingness to commit is stronger when applicants perceive subjective value congruence (H6a). That said, the magnitude of the effect was small in magnitude: the relationship between perceived member commitment and own willingness to commit was $\beta = .78$ among applicants high in subjective value congruence and $\beta = .65$ among applicants low in subjective value congruence. Thus, irrespective of whether the applicant was high or low in subjective value congruence, the relationship between perceived member commitment and own willingness to commit was positive and large in magnitude.

I interpret these results to signify the moderating influence of subjective value congruence exists but is likely to be too small in magnitude to influence behavior on a practical level. Supporting this viewpoint, subjective value congruence did not moderate the mediating influence of perceived member commitment between organizational rituals and on own willingness to commit, failing to support H6b. The non-significant moderated mediation results suggest that the pathways organizational rituals → perceived member commitment and perceived member commitment → own willingness to commit did not meaningfully or practically depend on levels of subjective value congruence, failing to support the hypothesis (H6b) implicating subjective value congruence. As organizational rituals exerted a direct effect on own willingness to commitment independent of subjective value congruence, there are likely other mechanisms at play which explain this relationship. I return to this discussion point in Chapter 5.

Chapter 4 Summary

Altogether, Studies 5, 7, and 8 supported my hypothesis (H5) that observing strong (vs. weak) rituals leads prospective members to view the group as having committed members; however, Study 6 failed to provide support for H5, suggesting that H5 may have untheorized but meaningful boundary conditions. Furthermore, subjective value congruence did not emerge as a robust and consistent moderator, such that Study 8 offered weak support for H6a and H6b and Study 7 offered no support. Moreover, perceived demographic similarity did not emerge as a moderator (H7) in Study 7. Overall, Chapter 4 offers partial support for the external pathway of rituals, suggesting that observing rituals can influence perceptions that the group has committed members but does not reliably influence an individual's own willingness to commit to the group. In Chapter 5, I discuss numerous next steps to advance the external pathway of ritual, both theoretically and methodologically.

CHAPTER 5: CONCLUSION AND FUTURE DIRECTIONS

Despite the prevalence of rituals in group settings, researchers have relatively ignored studying the psychological and organizational functions of group rituals. This is surprising, given researchers in sociology, anthropology, and religious studies have theorized about the macro-level functions of rituals for over 100 years. To fill this hole in the literature, this dissertation tested the functions of rituals using quantitative research methods from psychology and organizational behavior. My work shows that there is a bi-directional relationship between group rituals and group commitment.

Specifically, I document a recursive cycle of rituals and commitment: stronger rituals foster higher levels of commitment—and higher levels of commitment ensure that rituals are faithfully executed within groups over time. Among existing group members (internal pathway), my research demonstrates that (a) engaging in strong (vs. weak) rituals promote higher member commitment and (b) more (vs. less) committed are more likely to protect their groups' rituals by punishing those who attempt to alter them. Among prospective members (external pathway), I have shown that observing the enactment of rituals increases prospective members' perception that members are committed to the group. Although I hypothesized that demographically underrepresented prospective members would not be more willing to commit to a group that engages in strong (vs. weak) rituals, this moderation prediction was not supported. As a result, my results thus far indicate that rituals promote commitment for individuals irrespective of their surface-level demographic characteristics.

In Studies 1 and 2 (Chapter 2), I tested for the relationship between ritual engagement and workgroup commitment during the onset of the COVID-19 pandemic. Studies 1 and 2 demonstrated that ritual engagement promotes commitment because rituals make the group appear as a cohesive entity. In Studies 3 and 4 (Chapter 3), I tested for the relationship between group commitment and the protection of group rituals (operationalized as willingness to punish those who attempt to alter them) using sociocultural rituals (U.S. Pledge of Allegiance and the Jewish Passover Seder). Studies 3 and 4 demonstrated that more (vs. less) committed group members were more likely to punish those who attempt to alter the group's ritual. Lastly, among organizational rituals in a job recruitment context, Studies 5, 6, 7, and 8 (Chapter 4) showed that learning about the enactment of strong (vs. weak) rituals increased job applicants' perceptions that members are committed to the organization. Inconsistent with my predictions, strong (vs. weak) rituals did not increase job applicants' own willingness to commit to the organization, and this null relationship was not moderated by subjective value congruence or demographic representation. I return to the connection between group rituals and diversity in my future research directions.

Implications

This dissertation contributes to existing research in at least three ways. First, I offer insights about the role of rituals, conceptualizing rituals in organizational settings. While rituals have traditionally been studied from sociocultural perspectives (e.g., Durkheim, 1915; Rappaport, 1999), my results show that groups 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 group values. As illustrated through studying cultural, religious, organizational, and workgroup rituals within the same dissertation, my work offers initial support that the underlying psychology of rituals is fundamentally the same across different types of groups.

Moreover, although rituals are a phenomenon that has long been viewed as important in cultivating a strong organizational culture (e.g., Deal & Kennedy, 1982; Trice & Beyer, 1984), rituals have rarely been studied as causally influencing commitment. Prior approaches to studying rituals in organizational settings have relied mostly on qualitative research methods, such as participant observation (e.g., Vaught & Smith, 1980), mixed researcher and participant observation (e.g., van Maanen & Schein, 1979), external observation and interviews in naturalistic settings (e.g., Ashforth et al., 2008; Gephart, 1978), and articles and books in the popular press (e.g., Dacin et al., 2010). The preponderance of qualitative research articles on rituals in organizational settings led one review paper to write that "in fact, of all empirical work reviewed, only one study used only quantitative methods, and this study did not directly address rituals" (Islam & Zyphur, 2009, p. 116). The lack of quantitative research on rituals in group settings is problematic because prior case studies are unique to a particular group or organization, so they do not easily permit aggregation to generalizable conclusions about group rituals. Across different types of groups and settings, my work addresses prior shortcomings by testing whether rituals causally affect group commitment—a consequential psychological state which leads to a host of beneficial outcomes for individuals and groups (e.g., Meyer et al., 2002).

Second, my work contributes to our understanding of how to effectively change group culture. Although some argue that culture is hard to change because culture is viewed as a reflection of personality and personality is stable and enduring (e.g., Neville & Schneider, 2021), empirical research has established that culture change is possible (e.g., Chatman, 2021). Prior research has identified three levers of culture change: (a) formal institutions and structure (e.g., Tsui et al. 2006), (b) leaders (e.g., Schein, 2010), and (c) daily practices (e.g., Howard-Grenville et al., 2011). Focusing on the third lever (daily practices), the ethnographic perspective views culture as dynamic and continually evolving (e.g., Fayard & van Maanen 2015). Ordinary group members (lacking leadership authority) can initiate incremental culture change in daily practices by recombining new cultural resources with the existing cultural repertoire (Howard-Grenville et al., 2011). In this way, ordinary daily actions contribute to a process of culture change which include selection, variation, and retention of values and norms (Weeks & Galunic, 2003). Building on this perspective, my research shows that culture change that starts with changing daily enacted behavior—that is, the rituals—could be counterproductive. Specifically, my research shows that changes to rituals trigger reduced commitment and behaviors which lead to the protection of rituals (e.g., punishment), making it functionally difficult for changing rituals to serve as a starting point for culture change. For example, if a group leader-initiated culture change by changing the procedure of rituals and their intended meanings, then my results predict that fellow group members would attempt to protect the ritual by punishing the leader and reducing their commitment to the group. The negative reactions of group members (punishment, reduced commitment) make it difficult for the leader to exert change and influence group members, potentially reducing leadership effectiveness. Thus, my research adds to our existing knowledge on culture change by showing which daily practices are not amendable to initiating culture change.

Third, my work contributes to our understanding of how commitment develops before an individual joins an organization. Prior research on employee commitment has shown that a host of individual and organizational characteristics can influence the development of commitment (e.g., Mathieu & Zajac, 1990; Meyer et al., 2002). However, previous research has largely investigated the development of commitment within the boundaries of group membership. Only a few studies have examined the temporal structure of commitment by examining the development of commitment before individuals become members (e.g., Mowday et al., 1982; Pierce & Dunham, 1987). Research which has explored this notion —using the terms pre-entry commitment or commitment propensity—have theorized that personal characteristics (e.g., values, beliefs, and personality), expectations about future rewards and benefits, and circumstances associated with the new member's decision to join the organization (e.g., job choice) are each individual-level drivers that can influence the development of commitment before an individual joins a group (Cohen, 2007). Moving beyond individual-level conditions that influence willingness to commit to a group, my work shows that strong (vs. weak) rituals signal that current members are committed to the group. Given that the internet has created many opportunities for prospective members to learn about potential groups before formally joining them, these findings point to much needed work on how commitment develops when the individual is not yet a member of the group (Breitsohl & Ruhle, 2013).

Limitations and Future Directions

First, future research would benefit from refining the theoretical model. I did not find support for my hypotheses concerning subjective value congruence and perceived demographic similarity as moderators of the relationship between organizational rituals and own willingness to commit (H6a, H6b, H7), which did not allow me to test an intervention which can reduce the hypothesized negative consequences of rituals (H8). Future work may consider testing feelings of fit instead of subjective value congruence as a moderator of this relationship. While subjective value congruence is one element of perceived fit, there are many other dimensions of perceived fit, such as personality, abilities, emotions, psychological needs, and perceptual congruence (e.g., Cable & Edwards, 2004; Kristof-Brown et al., 2005; Lu et al., 2020). While values are an important element of fit, it is possible that prospective members put less importance on subjective value congruence and instead weigh more heavily other elements of fit. A broader, more holistic conceptualization and operationalization of fit may find support for the hypothesized relationships, such that stronger (vs. weaker) rituals promote greater own willingness to commit when there are greater feelings of fit. Furthermore, I did not propose hypotheses surrounding value congruence and rituals among existing members (internal pathway). If value congruence (or fit more broadly) moderates the relationship between rituals and commitment for prospective members (external pathway), I would expect a similar pattern of results for existing members (internal pathway). Future research should consider theorizing and testing for moderation by fit among both prospective and existing members.

Counter to my hypotheses, two studies (Studies 7 and 8) find a direct link between organizational rituals and own willingness to commit (independent of subjective value congruence and perceived demographic similarity). However, it is possible that this relationship is a measurement artifact. Given that perceived member commitment and own willingness to

commit used nearly identical scale items, it is possible that direct link between organizational rituals and own willingness to commit (in Studies 7 and 8) was observed due to common method variance. Future research should use separate the operationalization of perceived member commitment and own willingness to commit to isolate unique relationships between these respective constructs.

Future research may also more strongly distinguish between engaging versus observing rituals in the theoretical model. I theorize that existing members who engage in strong (vs. weak) rituals will experience higher levels of commitment, while prospective members who observe strong (vs. weak) rituals will experience stronger own willingness to commit. While prospective members do not have the opportunity to engage in rituals, existing members can both engage in and observe rituals. Thus, there might be different consequences for existing members who engage in (vs. observe) rituals. For instance, perhaps the effect of engaging in rituals is stronger than the effect of observing rituals on member commitment. In this way, future theorizing could draw a distinction between engaging in and observing rituals in the theoretical model, uncovering nuanced differences.

Moreover, to refine the theoretical model, future researchers may also benefit from empirically testing some of my underlying assumptions. For instance, I theorize that felt cohesion accounts for the relationship between group rituals and group commitment (H2a) because groups perceived to be more cohesive are more effectively able to provide resources and support to improve individual outcomes. However, I did not empirically test for the perception that more cohesive groups can provide individual members with more resources. Similarly, I theorize that prospective members are more willing to commit to organizations that are perceived to have committed members because more committed members signal the organization can achieve objectives, which is desirable when one perceives subjective value congruence (H6a). However, I did not empirically test whether stronger perceived member commitment bolsters perceptions of organizational effectiveness.

Second, future work can enhance the validity of the findings. From a methodological orientation, I designed and carried out the studies to follow a full-cycle approach to organizational behavior research, traveling back and forth between observational research and manipulation-based research (Chatman & Flynn, 2005). Specifically, this dissertation combines field surveys (both longitudinal and cross-sectional) and scenario-based experiments to derive insights. That said, I believe future work could use an even more diverse methodological toolkit to further enhance validity. For instance, field experiments could manipulate the degree that job advertisements discuss organizational rituals and measure resulting changes in perceived member commitment and own willingness to commit. A field experiment would offer the advantages of causal inference (internal validity), natural proof of the phenomenon (ecological validity), and generalization to another context and sample (external validity).

Future research can also consider different operationalizations of ritualism, enhancing construct validity. Although the conceptualization of ritual is based on many different sources with a high degree of consensus (see “Conceptualizing Rituals” in introduction), the operationalization of rituals in psychology and organizational behavior is a new topic. As a result, there were minimal sources that I could leverage to design my manipulations and

measurements of ritual using a psychological frame. In Studies 5 and 6, I introduced the manipulation of a strong versus weak ritual; however, I found that this manipulation did not fully capture the breadth of ritualism, such that the weak ritual was viewed as moderately ritualistic, and the strong ritual was only viewed as slightly more ritualistic. In Study 8, I introduced a new ritual manipulation which varied whether the group activity was connected or unconnected to group values, which I found had a much stronger impact on the ritual manipulation check and key dependent variables. I encourage future researchers to continue to experiment with new manipulations and measures of ritual (building on the manipulation from Study 8), but I also call on future researchers to formally establish test validity (Campbell & Fiske, 1959) once a given manipulation begins to take on widespread usage.

Related to a valid and reliable manipulation of work rituals, future research would benefit from developing a valid and reliable self-report measure of work rituals. Studies 1, 2, and 7 use a face valid measure of work rituals that I have developed in my research, but key issues surrounding validity (e.g., content, predictive, convergent, convergent, discriminant) and reliability (test-retest) have yet to be established (Campbell & Fiske, 1959; Reis & Judd, 2000). As an example of why it is problematic to rely on a self-report scale without established psychometric properties, Study 1 found that mean ritualism (mean on the self-report scale) produced the same pattern of results as merely counting the number of meaningful activities completed. This raises the question as whether the results in Study 1 are due to an activity's ritualism (as theorized) or the number of meaningful activities completed. These issues can be partially addressed with the establishment of a reliable and valid measure of work rituals.

Moreover, future research should also investigate the construct validity of willingness to commit to an organization. Although prior researchers have conceptualized willingness to commit as a distinct construct in organizational behavior (e.g., Mowday et al., 1982; Pierce & Dunham, 1987), future research should map the nomological network of this construct (e.g., Reis et al., 2000). For instance, it is unclear whether willingness to commit is conceptually and operationally redundant with willingness to join an organization or applicant organizational attraction (e.g., Cable & Judge, 1996).

Third, future research would benefit from examining other antecedents and consequences of rituals beside group commitment. As an initial step into research on rituals, I focused my investigation on the interplay between rituals and group commitment. That said, I believe that rituals interplay with a host of other variables of considerable organizational importance. For instance, future research may investigate how work rituals shape feelings of subjective well-being (e.g., Diener, 2009). Engaging in work rituals may promote subjective well-being because rituals can provide a sense of control over work tasks (e.g., Hobson et al., 2018), and prior research suggests that a sense of control and feeling that one has enough resources to complete work tasks can lead to higher well-being (e.g., Whillans et al., 2017). Subjective well-being is just one potential outcome of rituals; for example, future work may also consider how rituals affect employee development (e.g., Holton, 1996) and motivation (e.g., Ryan & Deci, 2000). Beyond consequences of rituals, future research should also consider micro-level antecedents of ritual emergence (e.g., the creation of new rituals). Research can uncover the conditions that lead groups to take a mundane activity and transform it into a ritual.

Future Research Agenda

In this section, I discuss future research plans to study how the following domains intersect with group rituals: diversity and remote work.

Group Rituals and Diversity

More research needs to be conducted to understand the connection between group rituals and group diversity. As an initial entry into this area, I tested whether the effect of strong (vs. weak) rituals on own willingness to commit was weakened (or reversed) when the prospective group member was demographically underrepresented (vs. represented) in the group. I failed to find support for this prediction, which I theorize is due to the null results of subjective value congruence as a moderator. In future work, I will consider the impact of strong (vs. weak) rituals on existing group members from underrepresented demographic groups instead of focusing on prospective group members.

I theorize that strong (vs. weak) rituals will reduce the commitment of new members who come from underrepresented (vs. represented) demographic groups, but that there are important boundary conditions to this effect. New members often feel uncertain and anxious when joining a new group (e.g., Bauer & Erdogan 1996), but these feelings are particularly pronounced among new group members who have been historically excluded from the group (e.g., Mendoza-Denton et al., 2002). To counteract new members' anxiety, groups strategically invest in structured tactics to enculturate employees—that is, transmit the group's culture—which enables new members to understand the group's culture and thus feel greater belonging and certainty in how to act in the group (e.g., van Maanen & Schein, 1979). Strong (vs. weak) rituals may reduce the commitment of new members who come from underrepresented (vs. represented) demographic groups because strong rituals often contain opaque features which (on the surface) are not logically necessary for effective group performance, which prompts uncertainty. This uncertainty and ambiguity may have the effect of harming underrepresented (but not represented) new group members' commitment because underrepresented new members are especially prone to interpret uncertainty and ambiguity as signals of rejection (e.g., Steele & Aronson, 1995). However, if the group ritual's opaque features are demystified and the importance of the ritual is clearly conveyed, then strong (vs. weak) rituals may increase commitment irrespective of demographic representation. Learning about rituals, which tend to be a mystery to outsiders, may signal to new members that the group has trust in them and expects them to become a full-fledged member, alleviating underrepresented new members' concerns about whether the group is accepting.

To test these predictions, I will conduct a field survey and ask respondents who recently joined a new organization (in the last month) about whether the organization engages in strong or weak rituals and their relative level of demographic representation on observable characteristics such as gender, race, and age. I will follow-up with this sample two months later and measure the degree such rituals are clear to new members and participant's level of organizational commitment. I predict that demographically represented new members will experience stronger commitment when the organization engages in strong (vs. weak) rituals irrespective of whether the ritual is opaque; however, among demographically underrepresented new members, I predict

weaker commitment when the organization engages in opaque strong (vs. weak) rituals but stronger commitment when strong (vs. weak) rituals are not opaque.

Remote Work

The sudden shift to remote work brought on by the COVID-19 pandemic raises many new questions about rituals in organizations. How can organizations create new remote rituals and sustain old in-person rituals which must be enacted remote? Does the effect of rituals on commitment vary whether the ritual is enacted in-person or remote? What is the shape and structure of in-person rituals as opposed to remote rituals? As an initial entry into this area, I will test whether individuals view face-to-face interaction as a critical ingredient to ritual (e.g., Collins, 2020). Specifically, I predict that the same workplace activity (in terms of rigidness, sequenced, and meaning) will be viewed as more ritualistic when enacted in-person as opposed to remote. For instance, imagine a team begins every morning with a "scrum" call to make goals for the day. The activity has physical features of ritual (e.g., beginning at exactly 10:00 AM, with the same people) and psychological features of ritual (e.g., reflecting performance-oriented values), but the call is either performed in-person or over videoconference. Even if the activity is identical in structure, content, and format, I predict that group members will view the in-person version of the activity as more ritualistic than the remote version because the in-person (vs. remote) version will produce greater feelings of uniform emotion and joint attention, heightening the meaning attached to the activity. In this way, I predict identical in-person (vs. remote) activities are viewed as more ritualistic because the face-to-face interaction—by yielding uniform emotion and joint attention—permits the creation of more meaning (psychological features of ritual).

To test this idea, I will describe to participants a workplace activity that is identical in structure and shape, except for being in-person versus remote, and measure the degree participants believe the activity has the physical and psychological features of ritual. I predict the activities will be comparable on physical features of ritual but the in-person (vs. remote) activity will be viewed as possessing more psychological features. If these results are supported, I will further test under what conditions the remote activity is viewed as ritualistic as the in-person activity. If the remote activity was orchestrated in a manner that permitted greater uniform emotion and joint attention, I predict no differences in the degree that in-person versus remote activities are viewed as ritualistic. These results would offer practical implications for organizations aiming to build strong rituals in a remote or hybrid setting, pointing to two features (uniform emotion and joint attention) which are necessary for remote activities to have the same psychological meaning as in-person activities.

Conclusion

Group commitment is an important bond that influences a variety of individual and group behaviors. This dissertation examined how rituals influence existing and prospective group members' commitment to the group. By uncovering a robust relationship between group rituals and group commitment using a variety of groups (from religious to work organizations) and methodologies (from experiments to surveys), my work provides insight onto the broader question of whether rituals are functional for groups (e.g., Trice & Beyer, 1984). I hope my work

inspires others to study how rituals operate in groups, identifying both the positive and negative functions of ritual.

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