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Solinger, Omar N.; Joireman, Jeff; Vantilborgh, Tim; Balliet, Daniel P.

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Change in unit-level job attitudes following strategic interventions: A meta-analysis of longitudinal studies

Omar N. Solinger | Jeff Joireman | Tim Vantilborgh | Daniel P. Balliet

Summary

The present meta-analysis tests how cost- and people-oriented strategic interventions impact temporal-dynamic changes in unit-level job attitudes within organizations. Analyses are based on 573 effect sizes across 137 longitudinal studies containing unit-level change in job attitudes across three time periods (pre-change, during change, and post-change). Results reveal that unit-level job attitudes (a) decline during cost-oriented changes (e.g., restructuring) and remain at lower levels following the changes (supporting a sustained change model); (b) increase during people-oriented changes (i.e., HRM investments) and remain at higher levels following the interventions (consistent with a sustained change model); and (c) remain unchanged over time when cost- and people-oriented interventions are combined. Tests of a process model further reveal that cost-oriented (people-oriented) interventions impact unit-level job attitudes by reducing (increasing) perceived support (relative to a no intervention control). The pattern of findings suggests that long-term, unit-level change in job attitudes can be anticipated to follow from strategic interventions, although some of the negative impact of cost cutting can be mitigated by maintaining perceptions of support.

KEYWORDS

commitment, HRM, job attitudes, job satisfaction, longitudinal, meta-analysis, organizational change, set point, social exchange, temporal, unit level

1 INTRODUCTION

Organizational survival often requires strategic change, broadly defined as an attempt to alter current modes of cognition and/or action in a group to take advantage of opportunities or cope with threats (Gioia & Chittipeddi, 1991). The process of strategic change, including cutting costs and investing in employees (Beer & Nohria, 2000), raises many important questions for organizational behavior scholars. For example, do strategic changes affect unit-level job attitudes? If so, are responses to strategic change temporary or sustained? Finally, what explains the effects of strategic change on unit-level attitudes?

A review of the literature reveals that answers to these questions are both plentiful and lacking. The majority of meta-analyses have identified predictors of individual job attitudes including stable personality traits (Connolly & Viswesvaran, 2000), work experiences (Meyer et al., 2002), and job characteristics (Fried & Ferris, 1987). Additional meta-analyses have shown that attitudes are associated with operational performance and retention (Jiang et al., 2012; Meyer et al., 2002) and with mental and physical health outcomes (Faragher...
et al., 2005). Other meta-analyses have tested for effects at the unit level of analysis (Harter et al., 2002; Heavey et al., 2013), whereas a handful of meta-analyses have addressed longitudinal effects such as test–retest stabilities (Dormann & Zapf, 2001) and time-lagged effects (Harrison et al., 2006). Finally, a few meta-analyses have considered how specific cost-(people-)focused interventions (Neuman et al., 1989; van Dierendonck & Jacobs, 2012) impact job attitudes.

Notwithstanding its accumulated value, we contend that close inspection of prior work identifies several methodological limitations clouding an understanding of the true effects of strategic change on unit-level shifts in job attitudes. First, prior meta-analyses on job attitudes have never directly demonstrated that cost- and/or people-oriented interventions cause unit-level job attitudes (or related constructs) to change over time. The correlational nature of prior meta-analytical findings precludes such causal or dynamic inferences. Furthermore, prior job attitude meta-analyses have never fully integrated mediation analyses, strategic interventions, and temporal-dynamic effects at a group level of analysis. Lacking a unifying framework, the present evidence base leaves skeptics unconvinced that attitudes can change over time on a long-term basis in response to interventions.

Second, prior meta-analyses on the impact of strategic interventions have not focused on the temporal-dynamic aspects, where shifts in job attitudes following strategic interventions are tracked over time. Even longitudinal studies on cost- or people-oriented interventions cannot be used to infer the average trajectory of group level responses because these studies (1) typically lack a control group, undermining confidence that observed changes over time are due to the intervention and (2) often fail to include three relevant time points (i.e., pre-intervention, intervention, and post-intervention), obscuring the temporal pattern of outcomes over time.

Third, whereas some meta-analyses in the people-oriented domain are based on studies at the unit-level of analysis (Heavey et al., 2013; Jiang et al., 2012), meta-analyses in the cost-oriented domain have been restricted to the individual level. Whereas individual job attitudes are indeed an important focus in the study of organizational behavior, unit-level attitudes arguably represent a more telling bellwether of the success (or failure) of a strategic intervention (Blau, 1964; Whitman et al., 2010). In fact, it is very likely that strategic interventions will have a dynamic and collective impact on job attitudes. Strategic interventions confront units of employees with the same event, which creates a bounded setting that makes for a great deal of sharedness in interpretations within organizational units. Dynamic-unit level effects can be expected to look different than what has been observed at the individual level. For instance, although at the individual level, some might find the flexibility and resiliency to bounce back from a negatively experienced event (e.g., Solinger et al., 2016), this will prove more difficult at the unit level where conformity and social policing ensure adherence to group norms.

To address these issues, we report a large-scale meta-analysis of longitudinal studies assessing how cost- and people-oriented interventions affect exchange processes (e.g., perceived support), unit-level change in job attitudes (i.e., satisfaction and commitment) over three time periods (pre-change, during change, and post-change). To enhance the validity of our causal conclusions, we compare the results of these strategic interventions over time to those derived from studies that adopted pretest posttest control group designs and studies that did not undertake strategic interventions. Utilizing these data, we develop and test two competing models concerning the immediate and delayed effects of such interventions (i.e., return to baseline and sustained change) and perform mediation tests where strategic interventions impact unit-level change in job attitudes via changes in perceived support.

In so doing, our study makes (1) empirical, (2) substantive, and (3) theoretical contributions. Empirically speaking, by comparing the effects of strategic interventions to control groups over time, we offer the strongest test to date of whether strategic changes cause a dynamic shift in unit-level job attitudes over short-time and longer time intervals. Substantively, we contribute to the organizational change and HRM literatures by piecing together, via a dynamic meta-analytical approach, an average trajectory of the unit-level job attitude dynamics over pre-intervention, during intervention, and post-intervention measurements following cost- and people focused interventions. Without such descriptive temporal knowledge, firms remain in the dark regarding whether the negative effects of cost-cutting can be weathered over the long haul or whether people-oriented interventions are worth the investment in the long run. Finally, our study makes a theoretical contribution to the job attitudes literature. Although the job attitude literature has largely adopted person-centric assumptions in line with a return to baseline model (i.e., strictly individual level; Riemer et al., 2014; Solinger, 2019), our data support a sustained change model in which collective attitudes fluctuate and can recalibrate toward different levels following strategic interventions. This implies that strategic change is not experienced in a social vacuum and should not be explained merely by individual-level phenomena (e.g., stable dispositions).

In what follows, we first review work on job attitudes and strategic change and discuss social exchange theory, a linchpin in our theoretical model. Next, we explicate competing temporal models concerning how collective job attitudes are likely to change in response to cost- and people-oriented interventions over time and a process model linking strategic change to job attitudes via changes in perceived support.

2 | THEORETICAL DEVELOPMENT AND HYPOTHESES

2.1 | Job attitudes

Job attitudes are defined as “evaluations that express one’s feelings toward, beliefs about, and attachment to one’s job” (Judge & Kammeyer-Mueller, 2012, p. 344). There are many types of job attitudes (e.g., job satisfaction, job involvement, and organizational commitment; Cohen, 2003). Among these, job satisfaction and organizational commitment are the most widely studied. Job satisfaction (JS) is defined as “an evaluative state that expresses contentment
with and positive feelings about one's job” (Judge & Kammeyer-Mueller, 2012, p. 343), whereas organizational commitment (OC) is defined as an individual's psychological bond with the organization, “reflected in a combination of affect (emotional attachment, identification), cognition (identification and internalization of its goals, norms, and values), and action readiness (a generalized behavioral pledge to serve and enhance the organization's interests)” (Solinger, van Ollfen, & Roe, 2008, p. 80). Although job satisfaction and organizational commitment have different targets, they share considerable overlap which is interpreted as "job attitude" (Harrison et al., 2006; Judge & Kammeyer-Mueller, 2012; Le et al., 2010). As noted earlier, one key question is how job attitudes are affected by cost- and people-oriented changes.

2.2 | Responses to strategic change: Extant (meta-analytic) insights

2.2.1 | Cost-oriented change

According to Beer and Nohria (2000), cost-oriented interventions are a “hard” approach to strategic change characterized by an emphasis on cost-cutting and improvements of operational efficiency. This type of strategic change is considered to have a “hard” character because the human factor is typically not considered as a criterion of success (Beer & Nohria, 2000). Examples of cost-oriented strategic changes include downsizing, offshoring, restructuring, and some types of efficiency fallouts following technological change and mergers and acquisitions. Although often necessary in cases of excessive slack and/or declining markets (Tan & Peng, 2003), a number of scholars have cautioned managers of the “hidden costs” that accompany cost-oriented strategic change. Some examples include collective deterioration of employee learning and innovation, job attitudes, and performance, with potentially devastating implications for the organization's long-term viability (e.g., Cascio et al., 1997; Fisher & White, 2000).

Several reviews on reactions to cost-cutting operations appear to validate these concerns. Meta-analytic summaries of studies of downsizing survivors, for example, reveal that job attitudes are lower when downsizing is perceived as unfair (O'Hare & Vilardi, 1993; van Dierendonck & Jacobs, 2012) and supervisor and coworker support are perceived to be low (O'Hare & Vilardi, 1993), which predicts higher turnover intentions (O'Hare & Vilardi, 1993). A third meta-analysis shows that perceived job insecurity (in general) is negatively related to job attitudes and employee physical and mental health and positively related to turnover intentions (Sverke et al., 2002; see also O'Hare & Vilardi, 1993).

2.2.2 | People-oriented change

In contrast to cost-cutting, people-oriented change is defined as a form of strategic renewal, characterized by targeted investments in people (i.e., an HR investment); “targeted”, in this case, refers to renewal actions that shape a finite set of new capabilities and ensure employee devotion (Ouchi, 1980). Beer and Nohria’s (2000) original dichotomy emphasizes that people-oriented interventions are particularly targeted at revitalizing a firm's human capital as a strategic capability, such that the renewal creates “the motivational basis to secure the employees likely to play a decisive role in the long-run direction of industry change” (Boxall, 1998, p. 271).

Accumulating evidence shows that strategic HR investments are best operationalized within the framework of “high-commitment” or “high-performance” HR programs (e.g., Appelbaum et al., 2000; Combs et al., 2006; Jiang et al., 2012). Appelbaum et al. (2000) argue that people-oriented programs generate new capabilities and competitive advantage by improving employees’ abilities (fostering individual and organizational learning/development), enhancing motivation (by strengthening organizational cultures and aligning individual and organizational goals), or increasing opportunities to contribute (by enhancing autonomy and involvement; see also Combs et al., 2006; Jiang et al., 2012).

Meta-analytic summaries in the strategic HRM literature offer some evidence in support of people-oriented interventions (Crook et al., 2011; Heavey et al., 2013; Hong et al., 2001; Jiang et al., 2012; Neuman et al., 1989). For instance, extending Appelbaum et al.’s (2000) focus on ability, motivation, and opportunity, Jiang et al. (2012) found support for a model in which “High-Commitment” HR practices have an indirect effect on firm financial outcomes via human capital, employee motivation, turnover, and operational outcomes.

The findings just reviewed suggest cost-oriented (people-oriented) changes will negatively (positively) impact job attitudes. Yet several methodological and empirical limitations discussed earlier suggest these conclusions are not fully justified by the available evidence. Further, the question remains which theoretical mechanisms might explain these effects.

2.3 | Social exchange theory

Many theoretical mechanisms have been proposed to account for the effect of strategic change on job attitudes (e.g., appraisal theory, need fulfilment, and coping). The vast majority of studies, however, have drawn on social exchange theory (Blau, 1964). Social exchange theory accounts for (changes in) job attitudes by looking at a range of social phenomena (e.g., reciprocity, fairness, indebtedness, and support) that come with the give and take between interaction partners. In line with social exchange theory, an impressive volume of correlational work has examined how social exchange constructs (e.g., justice, support, and the psychological contract) relate to between-individual differences in job attitudes (i.e., the static-individual level; Kurtessis et al., 2017) and has linked HRM investments with unit-level attitudes using a social exchange perspective (i.e., the static-unit level; D. G. Allen et al., 2003). Recently, there has also been a surge of studies examining how shocks to social exchange relationships influence individual-level job attitudes over time (i.e., the dynamic-individual level).
level; Solinger et al., 2016). Although informative, what is missing is a systematic account of how major shocks, such as strategic interventions, bring about dynamic responses at the unit-level of analysis (our dynamic-unit level approach).

Although Blau's (1964) original social exchange theory is often cited in support of static-individual level exchange between a person and their organization, his work in fact discusses at length how mechanisms of social exchange and reciprocity alter unit-level processes and structures over time. He emphasized, for instance, how a superior's authority legitimizes through employees' shared approval of a superior's behavior and that these perceptions may solidify through social norms. Conversely, if a superior's demands become excessive and exploitative toward employees, employees "are likely to communicate their feelings of anger, frustration, and aggression to each other ... It is out of such shared discontent that opposition ideologies and movements develop." (Blau, 1964, p. 25). It is thus fair to say that social exchange theory was originally developed as a theory of dynamic-unit level processes. Over time, however, as researchers began to apply social exchange theory to psychological processes, many studies retained the language of social exchange theory while departing from this original intent, using designs that examined static (as opposed to dynamic) and/or individual (as opposed to collective) processes. Importantly, this subtle "ontological drift" (Thompson, 2011) from Blau's theory to the way it has often been tested has left a surprising and critical gap in our understanding of responses to strategic change.

Blau's (1964) explicit support for a combined dynamic-unit level approach to social exchange in response to managerial interventions offers compelling theoretical motivation to investigate the impact of strategic change on dynamic-unit level attitudes. In fact, when contrasting the currently predominant person-centric (i.e., dynamic-individual level) assumptions with theory and processes of social exchange that pertain to the unit-level of analysis, we argue that they suggest two distinct models that offer competing accounts of how job attitudes change over time in response to cost- and people-oriented interventions. As shown in Figure 1, we refer to these models as the return to baseline and sustained change models.

2.4 | Competing temporal models of the change process

2.4.1 | Return to baseline model

A return to baseline model suggests attitude change in organizations will be short-lived. This model is grounded in two assumptions, both rooted in a dispositional, Western, and person-centric (individual-level) view of attitudes (Riemer et al., 2014). First, job satisfaction, especially organizational commitment, is believed to be stable individual predispositions (Bowling et al., 2005; Staw et al., 1986). Second, attitudes operate according to the principle of homeostasis (Bowling et al., 2005), suggesting that although they may fluctuate, attitudes have a "preferred state" or a set point to which they tend to return following various disruptions.

The available literature provides some support for the position that job attitudes are relatively stable and tend to return to baseline following significant organizational events. For example, one argument often advanced in support of the stability assumption is that job attitudes show a reasonable degree of test–retest reliability (Dormann & Zapf, 2001; Newton & Keenan, 1991) and are related to (presumably stable) dimensions of personality (Arvey et al., 1989; Ilies & Judge, 2002; Staw et al., 1986). Consistent with a return to baseline model, several studies also suggest that "time heals all wounds" following adverse organizational events (T. D. Allen et al., 2001; Grunberg et al., 2008; Ritter et al., 2016; Solinger et al., 2016), whereas a boost in job satisfaction following people-oriented interventions may be short-lived (Campion & McClelland, 1993; Griffin, 1988, 1991; Lam et al., 2002). Based on

![FIGURE 1 Competing temporal models for impact of cost-oriented and people-oriented interventions on unit-level job attitudes over time. Note: Gray band represents a non-significant change in job attitudes from baseline (dashed line)](image-url)
their review of the transient nature of these people-oriented interventions, Bowling et al. (2005) proposed that “the strength of the effects of work-related events upon job satisfaction dissipates across time” (p. 1,048). If the return to baseline model indeed represents a dominant tendency (e.g., > 50% of the unit), then it can affect mean-level aggregates within organizational units, although such aggregate effects could be dampened by individual differences within the unit. Based on this logic of the return to baseline model, we derived and tested the following hypotheses.

**Hypothesis 1a.** (Immediate effect): Cost-oriented interventions initially reduce unit-level job attitudes (from pre-intervention to during the intervention).

**Hypothesis 1b.** (Return to baseline effect): Following the cost-oriented intervention, unit-level job attitudes will subsequently return to their pre-intervention baseline levels.

**Hypothesis 1c.** (Immediate effect): People-oriented interventions initially increase unit-level job attitudes (from pre-intervention to during the intervention).

**Hypothesis 1d.** (Return to baseline effect): Following the people-oriented intervention, unit-level job attitudes will subsequently return to their pre-intervention baseline levels.

### 2.4.2 Limitations of the return to baseline model

Although the evidence just reviewed provides some support for a return to baseline model, this model is open to a number of criticisms. To begin, using test–retest reliability coefficients to infer that job attitudes are stable is problematic. Although a high test–retest correlation does suggest that the relative ranking of employees on job attitudes remains reasonably consistent over time, it is still possible that mean-level job attitudes have changed (Roberts et al., 2006). Accordingly, test–retest correlations cannot address the question of whether job attitudes are stable over time. Similarly, evidence that job attitudes are related to presumably stable aspects of an employee’s personality does not rule out the possibility that job attitudes can, at the same time, change in response to environmental events (Grunberg et al., 2008; Judge & Kammeyer-Mueller, 2012; Solinger et al., 2016), a position in line with the general attitudes literature, which has begun to acknowledge that attitudes are dynamic and sensitive to context (Eagly & Chaiken, 2007).

Finally, although set-point theory—the foundation of a return to baseline model—purportedly holds for any system (at the individual or group level), there are reasons to expect that the dynamics of attitude change within individuals and groups will be different. The distinction between individual-level and unit-level attitudes, in turn, has important implications for whether job attitudes will return to baseline (a position more consistent with an individual-level analysis) or will endure (a position more consistent with a collective-level analysis). Specifically, individual employees may adapt to a variety of different work-related events, making the effects of positive or negative events somewhat transitory. In contrast, when groups of employees are exposed to the same (positive or negative) exogenous “shock,” these collectively experienced events may activate a variety of group-level processes which both magnify and solidify changes in group members’ attitudes over time. The sustained change model, to which we now turn, reflects this reasoning.

### 2.4.3 Sustained change model

Like the return to baseline model, the sustained change model predicts immediate shifts in job attitudes. Unlike the return to baseline model, however, the sustained change model proposes that such shifts will persist. The assumption of sustained change emanates from a synthesis of unit-level attitude theory and group-level processes. The group level introduces a number of social exchange processes that are not considered at the individual level, such as social norms, contagion, and consensus building (Foulk et al., 2016; Schaft et al., 2020).

Accordingly, we argue that these group-level processes of social exchange will contribute to shared, within-unit change in job attitudes following strategic interventions. This is consistent with the normative-contextual perspective on attitudes which holds that attitudes (and attitude change) are inherently embedded within a group context (Riemer et al., 2014; Solinger, 2019). Indeed, strategic change in organizations is often experienced through a relational (social exchange) lens (e.g., Brockner et al., 1993), suggesting that the resulting job attitudes are situated within a group frame of reference and are susceptible to being shaped by group-level processes.

Theoretically, group-level processes cause (sustained) changes in unit-level attitudes. Whitman et al. (2010, p. 48), for instance, stated, “In the case of collective satisfaction, a predisposition to collaborate, share, and accept organizational goals emerges that results in a narrowing of the range of workplace behaviors that are deemed appropriate by the unit.” Conceived as such, unit-level attitudes can be defined as a locally shared evaluative system that authorizes certain subjective experiences and public expressions of satisfaction and commitment, and fixes the desired degree of investment, effort, or quality of work at a given level. For example, in an effort to maintain relationships within a group, fulfill social roles, manage one’s reputation, and conform to group norms, organizational members often adjust their attitudes to be consistent with those of their fellow group members (Chiaburu & Harrison, 2008; Riemer et al., 2014). Similarly, in the context of cost-cutting interventions, groups of employees may reinforce negative attitudes toward management, who they see as exploiting their group, and consequently elect to engage in lower levels of cooperation with management (e.g., via reduced performance). In fact, research on cooperation reveals that groups quickly establish norms concerning appropriate levels of cooperation (Fehr & Fischbacher, 2004), and these norms are used to evaluate and reward (or sanction) other group members’ behavior contingent on
conformity with group norms (Balliet et al., 2011). Taken together, unit-level attitude theory and group-level processes suggest the competing sustained change hypothesis (for the immediate effects, see Hypotheses 1a–1c):

**Hypothesis 2a.** (Sustained change effect): Following the immediate effect of cost-oriented interventions, unit-level job attitudes will subsequently remain at these lower levels.

**Hypothesis 2b.** (Sustained change effect): Following the immediate effect of people-oriented interventions, unit-level job attitudes will subsequently remain at these higher levels.

### 2.5 Process model

In addition to testing the rival temporal-dynamic models just outlined, we developed and tested a process model explaining how cost- and people-oriented changes affect social exchange processes and job attitudes (see Figure 2). We propose that strategic change impacts unit-level job attitudes as a result of a punctuation in the symbolic exchange relationship between management and employees. Whereas strategic change will surely impact transactional elements of exchange (e.g., affecting the perceived pay off structure), social exchange theory also emphasizes that much social exchange carries symbolic meaning (Blau, 1964), defined as the degree to which an episode of exchange signals the social value of an interactant (e.g., employees’ belief they are valued and supported in an organization). We therefore argue that cost- and people-oriented changes convey symbolic meaning, operationalized here as changes in perceived support.

For example, cost-oriented changes almost inevitably entail a shock to the social exchange relationship, which leads to the impression that an organization has treated its employees unfairly (Brockner et al., 1993; Degoey, 2000; Robbins et al., 2012; van Dierendonck & Jacobs, 2012), no longer cares for employees’ well-being (Grant et al., 2008; Rhoades & Eisenberger, 2002), and has failed to live up to its promises to its employees, implying a breach of the psychological contract (Conway et al., 2011; Morisson & Robinson, 1997). In contrast, people-oriented interventions are likely to engender perceptions of support and positive exchange between worker and employer and communicate that the organization is committed to employees, values their contributions, and cares for their well-being (Brockner et al., 1993; Grant et al., 2008; Rhoades & Eisenberger, 2002). Perceptions of support, in turn, enhance trust, reciprocity, and commitment to an interaction partner. Moreover, given the link between perceived organizational support and job attitudes (e.g., Kurtessis et al., 2017), it stands to reason that perceived support will mediate between strategic change and job attitudes (D. G. Allen et al., 2003). This reasoning led us to test the following mediation hypotheses:

**Hypothesis 3a.** Cost-oriented interventions have a negative indirect effect on job attitudes through perceived support.

**Hypothesis 3b.** People-oriented interventions have a positive indirect effect on job attitudes through perceived support.

### 3 METHODS

#### 3.1 Search strategy and selection criteria

To test the two competing temporal models and process model, we conducted a systematic meta-analysis of longitudinal studies on job satisfaction and organizational commitment. We identified relevant...
papers by performing an extensive search for studies in Google Scholar, EBSCOHost, Emerald, JSTOR, Wiley Online Library, Springer-Link, and Science direct. Critical search terms were job satisfaction and/or organizational commitment in combination with terms like “time,” “longitudinal,” “time 2,” “wave,” “temporal,” and “dynamic.” This resulted in an initial set of 342 longitudinal studies on job attitudes as key construct. From this initial screening, we found that many articles were either focused on interventions (of cost- or people focused type), had no interventions, or were about socialization contexts. We discarded the studies on socialization (116 articles) but retained “no intervention” studies as control group. We then further refined our search for longitudinal job attitude studies in intervention contexts, using more specific search terms such as “organizational change,” “downsizing,” “merger,” “acquisition,” “restructuring” for cost focus and “intervention,” “HRM,” “redesign,” “training,” “learning,” “autonomy,” and “self-managing teams” for HRM investments. We also consulted reference lists of key articles (e.g., prior narrative reviews and meta-analyses) and manually checked recent online databases of major journals in management (Academy of Management Journal and Journal of Management) and applied psychology (Journal of Applied Psychology). Finally, we posted a call for unpublished work on the Organizational Behavior listerv and emailed 12 experts in the field with the request to forward the e-mail.

Next, additional criteria were used to identify studies to be included in the meta-analysis. First, studies had to include conceptually comparable measures of job attitudes, meaning job satisfaction (JS), and/or affective organizational commitment (OC) over time. To ensure comparability, studies measuring only one sub-facet of JS (e.g., pay satisfaction or supervision satisfaction) were discarded. Furthermore, studies focusing on other types of job attitudes (e.g., career commitment, job involvement, work ethic, and work centrality) were also discarded. In the end, and consistent with Harrison et al.’s (2006) meta-analysis, we included studies using the Affective Commitment Scale (ACS), the Organizational Commitment Questionnaire (Mowday et al., 1979), Cook and Wall’s (1980) measure of commitment, and a small number (6%) of miscellaneous commitment attitude measures. Eight studies had to be discarded based on this criterion.

Second, studies had to contain group level data (e.g., mean-level attitudes in a work unit, department, or organization). This resulted in the exclusion of 32 papers with longitudinal designs on JS and/or OC which only make sense within an individual-level lens (e.g., random household samples).

Third, studies needed to employ a longitudinal design involving a minimum of two measurements over time from the same group of respondents which allowed for at least one comparison of means between three phases: before, during, or after the intervention. This resulted in exclusion of six papers which only reported job attitudes in a single phase. Fourth, studies had to have a minimum 1-month time between different waves (pre-intervention, during intervention, and post-intervention). This led us to exclude 10 experience sampling studies because the time frames used in these studies (e.g., several days or weeks) are very different from the average time frames used in organizational change studies (which average 12 months). Fifth, we made sure studies in the categories of cost- and people-focused interventions were sufficiently comparable. We did this, for instance, by making sure that studies included had a verifiable cost cutting emphasis (as described in more detail below).

Finally, studies needed to report both means and standard deviations in their respective time moments, which were required to calculate change scores as effect sizes. Because of these criteria, we had to exclude 42 longitudinal studies. Note also that we encountered 16 studies which reported various combinations of cost- and people-oriented intervention elements. We analyzed these studies separately to avoid contamination effects. After exploring results for this category, we retained them due to the fact that these “hybrid” studies represented a theoretically interesting category.

Our search resulted in 203 independent samples from 137 studies, and 573 different effect sizes. A total of 54 samples included both job satisfaction and commitment. In these cases, we averaged the effect sizes to create sample-specific effect sizes to secure the assumption of independent samples, which is a recommended practice (Roberts et al., 2006).

### 3.2 Effect size calculation

The primary dependent variable in the meta-analysis was the effect size of the mean level change in job attitudes from an earlier wave to a later wave. To calculate these effect sizes, we computed a d value based on an established formula for computing longitudinal change (single-group pretest–posttest raw scores; Morris & DeShon, 2002; Roberts et al., 2006). Each effect size is based on two measurements with the same sample and is calculated by subtracting the pretest from the post-test means (e.g., $M_3 - M_4$), and dividing by the standard deviation of the pretest score ($s_{d1}$). A pooled standard deviation (e.g., $s_{d1}$ and $s_{d2}$) was not used because the standard deviations for longitudinal change scores are not independent (Morris & DeShon, 2002). If a single study had three measurements across time, we calculated three effect sizes (i.e., $T_2 - T_1$, $T_3 - T_2$, and $T_3 - T_1$). If a study had more than three waves, we averaged all of the change scores which fell within the same wave. For example, in a study with four waves, including two measurement waves after the treatment (i.e., $T_2$ and $T_3$), we computed two effect sizes for the “during change to post change” time comparison, namely, $T_4 - T_2$ and $T_3 - T_2$. When this occurred, we computed the average effect size of $T_4 - T_2$ and $T_3 - T_2$ and used this average in our analyses. If “no intervention” studies (i.e., without a reported context of strategic change of any sort) had more than two time comparisons, we computed an average effect size to create a single independent effect size per sample. If the means or standard deviations were not provided, the t test or F statistic of longitudinal pre-test post-test difference scores was used to calculate the effect sizes (Morris & DeShon, 2002). If the information necessary for calculating the effect size was missing in the paper, authors were asked via email to provide this information.

We computed the sampling variance for each effect using Morris and DeShon’s (2002) formula for single-group pretest-posttest raw
scores, which corrects raw \( d \) scores for sample size (at the latest time point) and the pretest/posttest correlation, as raw \( d \) scores from studies with a high sample size and high reliabilities have relatively low sampling variance.

If the necessary statistics on measurement error and/or test-retest correlations were not reported in the manuscript or provided by the authors via email, we imputed the values necessary to calculate the effect size based on the averages in our sample of studies. The average Cronbach’s alphas reported for job satisfaction and commitment were .82 and .83, respectively. For job satisfaction, the imputed test–retest correlations were .54 for \( T_2–T_1 \), .50 for \( T_3–T_1 \), and .49 between \( T_3–T_2 \). For organizational commitment, the imputed test–retest correlations were .62 for \( T_2–T_1 \), .59 for \( T_3–T_1 \), and .61 between \( T_3–T_2 \).

### 3.3 Coding study characteristics

#### 3.3.1 Type of organizational change

In total, 573 effect sizes (\( k = 137 \)) were classified into four categories based on the type of change: no intervention (265 effect sizes from 71 primary studies), cost-oriented interventions (148 effect sizes from 38 primary studies), people-oriented interventions (114 effect sizes from 31 primary studies), and interventions that involved a combination of cost-oriented and people-oriented interventions (46 effect sizes from 16 primary studies). Note that the number of primary studies (71, 38, 31, and 16) does not add up to 137, because some studies contained multiple types of change. No intervention studies included longitudinal studies during relatively uneventful circumstances (222 effect sizes) and control group samples in longitudinal field experiments (43 effect sizes).

**Cost-oriented intervention** studies included: (1) miscellaneous reforms and restructuring operations (\( k = 18 \), of which \( k = 5 \) occurred in combination with other cost-oriented interventions); (2) downsizing (\( k = 13 \), of which \( k = 5 \) occurred in combination with other cost-oriented interventions); (3) mergers and acquisitions (\( k = 9 \), of which \( k = 1 \) occurred in combination with other cost-oriented interventions); (4) implementing new technologies (\( k = 2 \), of which \( k = 1 \) occurred in combination with other cost-oriented interventions); and (5) office relocations (\( k = 1 \)).

**People-oriented interventions** included operations designed to improve the organization’s human capital, such as enhancing learning (e.g., training, mentoring, and coaching; \( k = 6 \)), investing in strong organizational cultures (\( k = 6 \)), enhancing autonomy (\( k = 9 \)), aligning personal and organizational goals (\( k = 9 \)), and organizational learning (\( k = 1 \)).

The 16 cost- + people-oriented interventions involved a variety of combinations. Downsizing interventions (\( k = 4 \)) were accompanied by interventions to align individual and organizational goals (\( k = 3 \)) and enhance individual learning (\( k = 1 \)). Restructuring interventions (\( k = 9 \)) were accompanied by interventions to invest in strong organizational cultures (\( k = 5 \)), enhance individual learning (\( k = 2 \)), and enhance autonomy (\( k = 2 \)). Technological change interventions (\( k = 2 \)) were accompanied by interventions to enhance autonomy (\( k = 1 \)) or individual learning (\( k = 1 \)). Finally, one study combined a merger and acquisition with an intervention to enhance organizational culture.

#### 3.3.2 Job attitudes

Our analysis focused on the two most common forms of job attitudes (organizational commitment and job satisfaction). We obtained 351 job satisfaction effect sizes from 101 studies (of which 54 also contained organizational commitment effect sizes) and 222 organizational commitment effect sizes from 75 studies (of which 54 also contained job satisfaction effect sizes). The most frequently used measures of OC were the Affective Commitment Scale (\( k = 27 \); N. J. Allen & Meyer, 1990; Meyer et al., 1993) and Organizational Commitment Questionnaire (\( k = 30 \); Mowday et al., 1979; Porter et al., 1976; Porter et al., 1974). Other measures included the Cook and Wall (1980) scale (\( k = 11 \)) and six miscellaneous measures of commitment. There was considerably more variety in measures of job satisfaction: 82 samples used various facet measures of job satisfaction (where overall satisfaction was computed as an average across different aspects of the job, such as quality of supervision, coworkers, work, and payment). Beyond these facet measures of JS, we coded 26 effect sizes based on “evaluative” measures of job satisfaction which directly assessed respondents’ overall satisfaction/contentment with the job.

In addition to job attitudes, we computed effect sizes for changes in perceived support, time interval, attrition rates, job tenure, and the gender balance in the sample (Supporting Information).

#### 3.3.3 Perceived support

We obtained 140 effect sizes from 33 primary studies for (aggregate) changes in perceived support coming from organizational agents, including two-way communication and participation in decisions (\( k = 6 \)), justice perceptions (\( k = 2 \)), high quality exchange relationship with supervisor (\( k = 4 \)), organizational support (\( k = 8 \)), psychological contract fulfillment (\( k = 2 \)), supervisor support (\( k = 6 \)), a supportive culture (\( k = 4 \)), and a supportive management style (\( k = 1 \)). The average Cronbach’s alpha for these measures was \( \alpha = .86 \), and the average test–retest reliability was \( r = .55 \).

#### 3.3.4 Time interval

The average time interval we coded for was \( M = 20.68 \) months (SD = 20.84), and the median time interval was 12 months. Of particular note, the average time interval for initial effect (Pre–During) was \( M = 9 \) months (SD = 8.49) for cost-oriented change, \( M = 8.78 \) months (SD = 6.40) for people-oriented change, and \( M = 12.28 \) months (SD = 7.86) for combinations of cost- and people-oriented change. The average time interval for the sustained effect (Pre–Post) was \( M = \)}.
20.26 months (SD = 17.12) for cost-oriented change, 
M = 13.67 months (SD = 10.98) for people-oriented change, and 
M = 30.35 months (SD = 18.33) for combinations of cost- and 
people-oriented change. The average time interval for the post-
intervention effect (During–Post) was M = 13.82 months (SD = 11.97) 
for cost-oriented change, M = 15.30 months (SD = 9.93) for people-
oriented change, and M = 20.27 months (SD = 14.61) for combina-
tions of cost- and people-oriented change. The average time interval 
was M = 26.62 months (SD = 25.62) for the no intervention studies. 
Because time interval appeared to differ between types of changes, 
we controlled for time interval in our meta-analytic regression models 
to rule it out as an alternative explanation for our findings.

3.3.5 | Attition

We coded attrition level as it may serve as an alternative explanation 
for increasing job attitudes after change. To examine the possible 
effect of attrition, following Roberts et al. (2006), we calculated the 
percentage of attrition between the relevant time comparisons. For 
example, if there were 100 respondents at Time 1 and only 40 at Time 
2, the attrition rate for the T2–T1 comparison would be 60%. The 
average attrition was 4.49% (SD = 22.7%). In the end, we did not find 
any effect of attrition on sustained changes in job attitudes from T1 to 
T3 or from T2 to T3 (b = .002, p = .15; b = .002, p = .26, respectively).

3.3.6 | Job tenure and gender

The average job tenure was 9.41 years (SD = 5.36). To explore 
gender differences, we also coded the proportion of males in the sam-
ple (M = .50, SD = .50). We found a negative effect of tenure, but not 
of gender, on changes in job attitudes from T1 to T3 (b_tenure = −.02, 
p = .01; b_gender = .21, p = .0501). Tenure and gender had no effects 
on changes in job attitudes from T2 to T3 (b_tenure = −.01, p = .08; 
b_gender = .12, p = .12).

4 | RESULTS

4.1 | Overview of analyses

We divide our analyses into four sections. We first test whether job 
attitudes change over time without an intervention. Although not a 
major focus, a null result would be consistent with set-point theory 
assumptions that attitudes remain at a baseline level when no shocks 
to the system are present. Next, we analyze changes in job attitudes 
with an intervention. Using job attitudes before (T1), during (T2), and 
after (T3) the intervention, we compute three comparisons: pre-
intervention vs. during the intervention (T2–T1) to test for “initial 
effects”; during versus after the intervention (T3–T2) to test for “post-
intervention effects”; and pre-intervention versus after the interven-
tion (T3–T1) to test for “sustained effects.” After testing the overall 
effect size for these comparisons, we assess whether heterogeneity in 
effect sizes is explained by differentiating between the various inter-
vention conditions. Finally, we test our model’s hypothesized indirect 
effects (Figure 2), focusing on the pre-post comparisons (from T1 to 
T3). This involved testing whether strategic change has an indirect 
effect on a change in job attitudes via changes in perceived support.

We performed meta-analytical tests using the metafor package in 
R (Viechtbauer, 2010). We used a random effects restricted maximum 
likelihood estimator, as we assume that primary studies are drawn 
from a population. Effect sizes were corrected for reliability (Hunter & 
Schmidt, 2014). We did not correct for range restriction as effect size 
variances did not decrease over time (b = .07, p = .18). We also esti-
imated the variance in the effect size distribution and tested for publica-
tion bias using Duval and Tweedie’s (2000) trim and fill approach 
and Egger’s regression intercept (Egger et al., 1997).

4.2 | Attitude change over time in the absence 
of an intervention

We first tested for changes in unit-level job attitudes over time in the 
absence of an intervention. We initially coded 265 effect sizes for 
changes in job attitudes over time when no intervention was present. 
collapsing non-independent effect sizes reduced the sample to 
90 effect sizes. As anticipated, there was no significant change in job 
attitudes over time (d = .06; 95% CI [−.05, .18]). Duval and Tweedie’s 
trim and fill approach pointed to a publication bias to the right of the 
mean (k = 29), suggesting there may be missing studies that support a 
positive increase in job attitudes over time. Correcting for this bias 
resulted in a positive (and significant) effect size (d = .25; 95% CI [.14, 
.35]). In addition, Egger’s regression test for funnel plot asymmetry 
(z = 3.05, p = .002) confirmed the presence of a publication bias.

4.3 | Impact of interventions on change in unit-
level job attitudes over time

As noted, our first aim was to evaluate how cost- and people-oriented 
interventions impact collective job attitudes over time. For each of 
the three time comparisons (i.e., T1 → T2, T2 → T3, T1 → T3), we first 
examined the overall effect sizes (collapsing over type of intervention) 
along with heterogeneity and publication bias statistics (Table 1). If 
heterogeneity was present, we ran a moderator analysis in which we 
used dummy-coded variables representing cost- and people-oriented 
change to explain heterogeneity in effect sizes (Table 2). We con-
trolled for length of time interval in all meta-regressions.

4.3.1 | Initial changes (T1 to T2)

We coded 355 (T2–T1) effect sizes for mean level changes in job atti-
tudes, of which 84 pertained to changes from before to during an 
intervention and 271 to changes when there was no intervention
(control group). Collapsing these 355 non-independent effect sizes reduced the total number of effect sizes to 134. Results revealed no significant changes in unit-level job attitudes from $T_1$ to $T_2$ ($d = .02; 95\% \text{ CI } [-.06; .11]$). A trim and fill analysis showed that there may be a publication bias to the right of the mean ($k = 40$). Correcting for this bias yielded a significant increase in job attitudes over time ($d = .20; 95\% \text{ CI } [.11, .28]$). Likewise, Egger’s intercept was significant ($z = 3.02, p = .003$), suggesting the presence of publication bias. However, this could be due to the combination of intervention and no intervention studies. Indeed, Egger’s intercept became non-significant when introducing moderators in the subsequent step of the analysis ($z = 1.78, p = .07$).

As shown in Table 1, there was also substantial heterogeneity in the effect sizes attributable to systematic differences between the studies ($I^2 = .23$; $I^2 = 98.09)$

### Table 1: Overall effect sizes, heterogeneity, and publication bias in change in unit-level job attitudes over time

<table>
<thead>
<tr>
<th>Time</th>
<th>Overall effect size</th>
<th>Heterogeneity</th>
<th>Publication bias</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$k$</td>
<td>$d$</td>
<td>95% CI</td>
</tr>
<tr>
<td>Initial changes ($T_1$ to $T_2$)</td>
<td>134</td>
<td>.02</td>
<td>[-.06; .11]</td>
</tr>
<tr>
<td>Post-intervention changes ($T_2$ to $T_3$)</td>
<td>143</td>
<td>.02</td>
<td>[-.05; .10]</td>
</tr>
<tr>
<td>Sustained changes ($T_1$ to $T_3$)</td>
<td>190</td>
<td>.02</td>
<td>[-.05; .09]</td>
</tr>
</tbody>
</table>

Note: $k$ = the number of effect sizes; $d$ = standardized mean difference (subjective time period minus prior time period); $d$ (left/right) = re-estimation of effect sizes using the trim-and-fill approach with random effects with either studies added to the left or the right; ERp = the $p$ value for Egger’s Regression Intercept.

### Table 2: Moderation tests for intervention effects on unit-level job attitudes over time

<table>
<thead>
<tr>
<th>Time and type of intervention</th>
<th>$Q$</th>
<th>$K$</th>
<th>$d$</th>
<th>95% CI</th>
<th>$I^2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Initial changes ($T_1$ to $T_2$)</td>
<td>20.29***</td>
<td>132</td>
<td>.18</td>
<td>.98</td>
<td></td>
</tr>
<tr>
<td>Cost-oriented</td>
<td>22</td>
<td>.19</td>
<td>[.35; -.02]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>People-oriented</td>
<td>14</td>
<td>.22</td>
<td>[.02; .43]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cost + people-oriented</td>
<td>8</td>
<td>.01</td>
<td>[-.21; .24]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No intervention</td>
<td>88</td>
<td>.02</td>
<td>[-.07; .11]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-intervention changes ($T_2$ to $T_3$)</td>
<td>10.98</td>
<td>141</td>
<td>.16</td>
<td>.97</td>
<td></td>
</tr>
<tr>
<td>Cost-oriented</td>
<td>26</td>
<td>.04</td>
<td>[-.10; .18]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>People-oriented</td>
<td>17</td>
<td>.06</td>
<td>[-.23; .11]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cost + people-oriented</td>
<td>10</td>
<td>-.04</td>
<td>[-.22; .15]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No intervention</td>
<td>88</td>
<td>.01</td>
<td>[-.07; .10]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sustained changes ($T_1$ to $T_3$)</td>
<td>26.35***</td>
<td>186</td>
<td>.15</td>
<td>.97</td>
<td></td>
</tr>
<tr>
<td>Cost-oriented</td>
<td>38</td>
<td>-.22</td>
<td>[-.33; -.11]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>People-oriented</td>
<td>41</td>
<td>.18</td>
<td>[.06; .29]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cost + people-oriented</td>
<td>21</td>
<td>-.08</td>
<td>[-.22; .06]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No intervention</td>
<td>86</td>
<td>.04</td>
<td>[-.05; .12]</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Q = test of heterogeneity, indicative of statistical power of the moderation test; $k$ = number of independent effect sizes; $d$ = standardized mean difference (effect size); $I^2$ is an estimate of the variance of the true effect sizes, whereas $I^2$ is a measure for the proportion of observed variance that reflects real differences in effect size (Borenstein et al., 2009). $I^2$ indicates whether a moderator analysis is necessary, while $I^2$ is used mainly to construct prediction intervals. Effect sizes were aggregated to ensure independence of observations. Estimates for the no intervention condition differ slightly between initial, post-intervention, and sustained change rows, as they are adjusted for the remaining effects in their respective analyses.

*p < .05.

***p < .001.
sum, these results suggest a net negative initial effect of cost-oriented interventions, and a net positive initial effect of people-oriented interventions.

4.3.2 | Post-intervention changes (T2 to T3)

We coded 372 (T3–T2) effect sizes, of which 101 represented mean-level attitude change from during to following the intervention, and 271 effect sizes during which there was no intervention (control group). Collapsing these 372 non-independent effect sizes reduced the total number of effect sizes to 143. Results revealed no significant overall change in unit-level job attitudes from T2 to T3 (d = .02; 95% CI [−.05, .10]). A trim and fill analysis showed that there may be a publication bias to the right of the mean (k = 46). Correcting for this bias resulted in an increase of job attitudes over time (d = .19; 95% CI [−.11, .26]). Likewise, Egger’s test was significant (z = 2.82, p = .005), suggesting the presence of publication bias. Again, this could be due to the combination of intervention and no intervention studies. However, Egger’s intercept remained significant when introducing moderators in the subsequent step of the analysis (z = 2.46, p = .01).

Though there was significant heterogeneity in effect sizes (I2 = .19, I2 = 97.70), it could not be explained by the intervention types as their effects were non-significant while controlling for the length of the time interval (btime interval = .01, 95% CI [.002; .01]). In particular, there was no significant change over time for cost-oriented (d = .04, 95% CI [−.10, .18]), people-oriented (d = −.06, 95% CI [−.23, .11]), the combination of cost- and people-oriented (d = −.04, 95% CI [−.22, .15]) interventions, or no interventions (d = .01 95% CI [−.07, .10]).

4.3.3 | Pre- to post-intervention changes (T1 to T3)

We coded 425 (T3–T1) effect sizes, of which 154 represented unit-level attitude change from before to after the intervention, and 271 effect sizes during which there was no intervention (control group). Collapsing these 425 non-independent effect sizes reduced the total number of effect sizes to 190. Results revealed no significant overall change in collective job attitudes from T1 to T3 (d = .02, 95% CI [−.05, .09]). A trim and fill analysis indicated a bias to report negative changes. Correcting for this bias (with k = 55) resulted in a significant positive change over time (d = .19, 95% CI [.12, .26]). Likewise, Egger’s intercept test was significant (z = 2.52, p = .01), suggesting that a publication bias may be present. Again, this could be due to the combination of intervention and no intervention studies. Indeed, Egger’s intercept became non-significant when introducing moderators in the subsequent step of the analysis (z = 1.86, p = .06).

Further analysis revealed significant heterogeneity in the effect sizes (I2 = .15, I2 = 96.63), attributed to differences between the interventions (see Tables 1 and 2) while controlling for the length of the time interval (btime interval = .005, 95% CI [.002; .01]). Subsequent analysis, summarized in Table 2, revealed that cost-oriented interventions led to a significant decline in job attitudes from T1 to T3 (d = −.22, 95% CI [−.33, −.11]), whereas people-oriented interventions led to a significant increase in job attitudes from T1 to T3 (d = .18, 95% CI [.06, .29]). Note that T3–T1 estimates are not simply the addition of T2–T1 and T3–T2 effect sizes. Any discrepancies are due to the fact that T2–T1, T3–T2, and T3–T1 estimates are not based on the exact same number of studies (see also Morris & DeShon, 2002). Interventions combining cost- and people-oriented changes resulted in no change in job attitudes from T1 to T3 (d = .08, 95% CI [−.22, .06]). Likewise, no significant changes over time could be observed in the absence of interventions (d = .04, 95% CI [−.05, .12]).

4.3.4 | Pattern of job attitude change as a function of intervention type

To visualize the overall pattern of unit-level job attitude changes over time, and its correspondence with the competing temporal models, Figure 3 displays the initial change (T2–T1) and sustained change (T3–T1) effect sizes as a function of intervention type. As can be seen, the overall pattern for cost-oriented and people-oriented interventions corresponds clearly to the sustained change model (with significant initial and sustained changes).7 Interventions combining cost- and people-oriented approaches showed a slight initial increase followed by a minor sustained drop, but statistically speaking, no significant departures from baseline. Likewise, the no intervention control group displayed a non-significant small initial and sustained drop. As an additional robustness check, we also ran these analyses on the 106 primary studies that included all three time comparisons. Results show no substantial differences in the findings. The results of this robustness check are reported in Table B1. Therefore, Hypotheses 1b–1d (derived from the return to baseline model) must be rejected, whereas Hypotheses 2a and 2b (derived from the sustained change model) are supported.

4.4 | Patterns of unit-level job attitude change for intervention subtypes

Given the large heterogeneity in the observed changes across studies (Table 2), we performed a supplementary analysis where we explored whether and how the different subtypes within the categories of cost- and people-focused change differed in their impact on unit-level change in job attitudes. As can be seen from Table 3, mergers and acquisitions (M&A’s) and restructuring have a sustained negative impact on job attitudes. Technological changes, albeit a cost-oriented intervention, have a sustained positive impact on job attitudes. Finally, enhancing learning and growth of individual employees (e.g., via training) have a positive effect, which becomes only apparent after the intervention is completed. Note, however, that these subtype effect sizes are based on a smaller number of effects.
We next tested the indirect effects implied in our process model (Figure 2). We hypothesized the interventions would have an indirect effect on job attitudes via perceived support (H3a and H3b). To test these hypotheses, we estimated path models in Mplus Version 8 using sustained (T3 - T1) effect sizes as raw data (see Table A1). We used Bayesian estimation techniques to obtain indirect effects and controlled for time interval.

**TABLE 3** Moderation tests for the effects of intervention subtypes on unit-level job attitudes over time

<table>
<thead>
<tr>
<th>Intervention Type</th>
<th>Initial changes (T1 to T2)</th>
<th>Post-intervention changes (T2 to T3)</th>
<th>Sustained changes (T1 to T3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>k</td>
<td>d</td>
<td>95% CI</td>
</tr>
<tr>
<td>No intervention (control)</td>
<td>74</td>
<td>.05</td>
<td>[−.05; .16]</td>
</tr>
<tr>
<td>Cost-oriented</td>
<td>7</td>
<td>.36</td>
<td>[−.49; .21]</td>
</tr>
<tr>
<td>Downsizing</td>
<td>10</td>
<td>−.03</td>
<td>[−.36; .28]</td>
</tr>
<tr>
<td>Restructuring</td>
<td>21</td>
<td>−.20</td>
<td>[−.45; .05]</td>
</tr>
<tr>
<td>Technological change</td>
<td>4</td>
<td>.52</td>
<td>[0.04; 1.01]</td>
</tr>
<tr>
<td>Mergers and acquisitions</td>
<td>10</td>
<td>−.36</td>
<td>[−.69; −.04]</td>
</tr>
<tr>
<td>Relocating</td>
<td>0</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>People-oriented</td>
<td>7</td>
<td>.31</td>
<td>[−.08; .70]</td>
</tr>
<tr>
<td>Strengthening org. culture</td>
<td>9</td>
<td>.20</td>
<td>[.55; .15]</td>
</tr>
<tr>
<td>Enhancing autonomy</td>
<td>13</td>
<td>−.01</td>
<td>[.31; .29]</td>
</tr>
<tr>
<td>Aligning goals</td>
<td>10</td>
<td>−.07</td>
<td>[.38; .24]</td>
</tr>
<tr>
<td>Enhancing individual learning</td>
<td>7</td>
<td>.31</td>
<td>[.08; .70]</td>
</tr>
<tr>
<td>Enhancing org. learning</td>
<td>2</td>
<td>.42</td>
<td>[−.24; 1.08]</td>
</tr>
</tbody>
</table>

Note: Total number of studies k not equal to sum of number of studies for each intervention type, as some studies contained a mixture of multiple intervention types. Initial changes (Q = 21.66**, k = 133, T = .45, T² = .21, I² = .98); post intervention (Q = 9.56, k = 142, T = .43, T² = .19, I² = .98); sustained changes (Q = 37.41***, k = 191, T = .42, T² = .18, I² = .97).

* p < .05.
** p < .01.
*** p < .001.

**FIGURE 3** Initial and sustained changes in unit-level job attitudes over time as a function of the type of intervention.

Note: Circled changes represent a statistically significant change (from pre-intervention baseline).
4.5.1 | Indirect effects of interventions to change in job attitudes

Figure 4 shows the path estimates of the indirect effects model linking strategic interventions to change in job attitudes via a proxy for symbolic social exchange (perceived support). The Posterior Predictive $P$ value indicated that an indirect effects model, which also included direct effects of interventions to change in job attitudes, fit the data well ($\text{PPP}^2 = .68$; DIC = 284.98), and fit the data better than a model without direct effects (DIC = 289.50). Figure 4 shows that cost-oriented interventions were significantly related to a reduction in perceived support ($b = -.18$, 95% CI = $[-.36, -.03]$), whereas people-oriented interventions were significantly related to increases in perceived support ($b = .25$, 95% CI = $[.11, .39]$). Further, support related positively to job attitudes ($b = .85$, 95% CI = $[.73, .97]$), and there was a direct negative effect of cost-oriented interventions on changes in job attitudes ($b = -.13$, 95% CI = $[-.25, -.02]$). Turning to the indirect effects, people-oriented interventions were significantly and positively related to changes in job attitudes via changes in perceived support (estimate = $.21$, 95% CI = $[.09, .34]$), whereas cost-oriented interventions were significantly and negatively related to changes in job attitudes via changes in perceived support (estimate = $-.15$, 95% CI = $[-.30, -.03]$). Results thus support Hypotheses 3a and 3b. Additional analyses indicated that time interval did not moderate any of the estimated relationships.

5 | DISCUSSION

The purpose of the present study was threefold, namely, to (1) test whether job attitudes are dynamically impacted by cost- and people-focused interventions at the unit level of analysis, (2) determine if those changes are temporary (Return to Baseline) or enduring (Sustained Change), and (3) identify mediators which explain shifts in unit-level job attitudes over time. To address these issues, we conducted a meta-analysis of longitudinal studies assessing change in unit-level job attitudes over three time periods (pre-intervention, during intervention, and post-intervention) in response to cost- and people-oriented interventions (vs. no intervention control conditions). Results support two conclusions. First, cost-oriented (people-oriented) changes lead to sustained reductions (increases) in job attitudes, clearly supporting a sustained change model. Second, both cost- and people-oriented interventions produced unit-level change in job attitudes via negative (positive) changes in perceived support, respectively. More fine-grained analyses showed nuances in the impact of specific intervention types on unit-level change in job attitudes. Below, we articulate the theoretical implications of our findings, and acknowledge several limitations. With these in mind, we then consider several practical implications.

5.1 | Theoretical implications

5.1.1 | Support for a sustained change model

Our results confirmed the notion that when studied at the unit level, changes do not revert back to initial levels, supporting the sustained change hypothesis (Figure 3). At first glance, this seems to directly contradict ideas that attitudes revert back to baselines (Bowling et al., 2005), that commitment can bounce back after breaches of the psychological contract (Solinger et al., 2016), that the impact of individual HR perceptions on outcomes fades over time (Piening et al., 2013), and the passage of time can heal wounds (T. D. Allen et al., 2001; Grunberg et al., 2008).

Crucial to understanding these contradictory findings, however, is the level of analysis applied in a given study. A return to baseline logic, applied in the examples above, is more apt for explaining individual employee attitudes in the absence of group-level interventions. At the group level, change can refreeze into a new and different state (for a review of the literature on Kurt Lewin’s famous three-step model of change, see Cummings et al., 2016), implying a more enduring quality of change. Reframed, our results indicate that set points around which
collective attitudes fluctuate can recalibrate toward different levels following strategic interventions. This result cannot be accounted for by individual-level theorizing (e.g., dispositional setpoint theories) but can be expected within a normative-contextual paradigm (Riemer et al., 2014; Solinger, 2019; van der Schaft et al., 2020). In particular, in the change management literature (e.g., Stouten et al., 2018), it is commonplace that for any change effort to be enduring, it should “institutionalize” by being locally affirmed and sanctioned as “the new normal” in social interactions. As a result of such institutionalization effects, attitudinal set points can recalibrate (indeed “refreeze”) at new socially sanctioned levels. Our pattern of findings thus helps bridge between “macro” views typically held by change management experts and “micro” views typically held by psychologists studying the impact of disruptive events.

5.1.2 | Social exchange at individual versus unit level of analysis

The apparent support for the sustained change model signals that future research would do well to emphasize more than simply individual dyadic exchange with the organization during organizational change. When applied to the immediate impact of strategic change (i.e., $T_2$ to $T_3$), both individual dyadic and collective exchange would yield the same conclusions: strategic change disrupts the balance of social exchange and either degrades (cost-oriented change) or improves job attitudes (people-oriented). However, immediate change is only part of the story. Equally important to recognize is that strategic interventions play out over time and typically affect large collectives of interacting agents, rather than isolated employees. Results support the idea that employees in a work unit subject to cost-cutting develop shared perceptions that the organization does not care for their group. Viewed through an ingroup-outgroup lens, such negative perceptions are likely to become polarized and reinforced within the group. For example, in the wake of cost-cutting, group members may be rewarded for expressing negative attitudes toward the firm, who they view as a “cheater” in social exchange relationships with the group (a form of indirect reciprocity). In contrast, after a company invests in groups of employees, they are likely to show more favorable attitudes, which again are reinforced through group processes such as conformity to group norms and indirect reciprocity (e.g., punishing employees seen as free-riding on the goodwill of the company). Either way, collective attitudes are likely to recalibrate and stabilize at new levels.

5.2 | Limitations

Although the present work offers valuable insights, three limitations should be kept in mind. First, mean-level attitude change is only a proxy for attitude change at the (sub-)unit level (e.g., work group, department, and division). Aggregate change, therefore, is likely to underestimate change in these more fine-grained communities. This suggests a caveat when interpreting our findings, namely, that the overall trends documented in the present work still permit a wide variety of “exotic” pathways at the sub-group level.

Second, there was variation in the time intervals used in initial changes ($T_1$ to $T_2$) and sustained changes ($T_1$ to $T_3$). Of particular note, cost-oriented change had slightly longer time intervals than people-oriented change (14.97 vs. 13.11 months), whereas combinations of cost- and people-focused change had the longest time intervals (23.82 months on average). Time interval did not significantly impact change in job attitudes, and we controlled for time interval in each analysis. For instance, our analyses suggest that time does not heal wounds after cost-oriented interventions.

Third, it is important to acknowledge there was significant variability in the effect sizes for different types of attitude, interventions, and in different measures of perceived support (a full accounting can be obtained from the first and third author). Although such heterogeneity puts a caveat on our use of broad, composite measures, this variability is also interesting and warrants future research.

Lastly, there was also variation in the quality of primary studies, judging from the outlet in which they appeared. Although controlled for measurement unreliability and artificial homogeneity coming from small samples in our Bayesian analytical approach, variation in the quality of the primary studies should be kept in mind.

5.3 | Practical implications

Despite the aforementioned limitations, the present results offer several possible implications for managerial practice. The logic of evidence-based management states that practitioners need studies with strong research designs which offer rigorous summaries of the effects of managerial actions; to date, evidence has not been as encompassing or rigorous as it could be (Stouten et al., 2018; ten Have et al., 2017). Our results indicate that managers embarking on cost-oriented interventions (e.g., following financial or COVID-19 crises) should anticipate enduring losses in esprit de corps unless steps are taken to uphold a balanced set of transactions (e.g., keep work load reasonable) and increase perceived support, perhaps through a yoked HRM program. In other words, if cost saving is imperative, organizations should try to invest in those who remain. This advice is informed by the fact that none of the effects of strategic change were found for 16 “hybrid” interventions that combined cost- with people-oriented approaches. In fact, many contemporary change ideas contain mixtures of cost- and people-focused elements (e.g., Lean, Six Sigma, and Agile). The observed non-effect for this combined intervention form could be interpreted as a “best practice” when cost cutting is necessary, where the best option for remaining efficient and competitive is to keep investing in employees and retaining their goodwill (Iverson & Zatzick, 2007; Love & Nohria, 2005; Trevor & Nyberg, 2008). This advice is corroborated by our finding that both increases and decreases in unit-level attitudes were explained by corresponding changes in perceptions of support.
In line with these recommendations, more fine-grained analyses of separate effects among intervention subtypes (Table 3) confirmed that cost-cutting per se does not necessarily hurt job attitudes. In fact, our results showed sustained increases in unit-level job attitudes following technological changes. This can be explained in light of the symbolic social exchange mechanisms that are attached to perceived support. If cost cutting comes with rational improvements in the workflow, it is unlikely to be construed as a negative act on the part of the organization. In fact, technological improvements were probably interpreted as a form of support. After all, the mediation analyses clearly show that people-oriented interventions bring about change in unit-level attitudes via changes in unit-level perceptions of support.

Further, previous research has found that downsizing has a negative effect on job attitudes (e.g., van Dierendonck & Jacobs, 2012). Therefore, we found it surprising that downsizing did not significantly impact changes in job attitudes, while both restructuring and M&A's did lower job attitudes over time. We observed high variety in attitudinal responses to downsizing. Although cutting slack is often perceived as negative for increasing job insecurity, a yoked HRM program can compensate a great deal (e.g., Trevor & Nyberg, 2008). Greenberg (1990), for instance, showed how after providing thorough and sensitive explanations to employees as to why cost cutting was imperative, feelings of negative exchange and counter-productive work behavior were reduced. And in light of other available evidence on cutting slack (Iverson & Zatzick, 2007; Love & Nohria, 2005; Tan & Peng, 2003), employees might be more forgiving if an organization cuts “fat” (excess slack) rather than “meat” (e.g., critical functions, core identities, and cherished social relationships). It stands to reason M&A's and Restructurings were of the latter kind. An important caveat, however, is that subtype analyses were obtained from a smaller number of effect sizes in comparison with our main analyses. What can be said about the pattern of findings is that if the negative repercussions following cost cutting are not managed actively (e.g., through concomitant investment in people), there will be a long-lasting negative effect on collective attitudes. Time does not seem to heal these wounds.

Finally, our findings support arguments voiced by proponents of strategic HRM, namely that HR investments bring sustained competitive advantage (Barney, 1991). Our results also support the “mutual gains” perspective on the HRM-performance relationship (e.g., Appelbaum et al., 2000) which holds that employers and employees will both benefit from HRM investments. Results also debunk a persistent popular narrative in the change management community that 70% of strategic changes would fail (Beer & Nohria, 2000; Hughes, 2011). Our study shows that people-oriented interventions have a decent chance of producing lasting positive effects as a way to renew and re-tool the organization and sustain the organization's esprit de corps. In this context, it was informative to see investments in individual employee learning (e.g., training) pay off as the most effective. Most probably, this happened because it symbolizes organizational support. An important note here is that the effects did not show until some months after the intervention.

6 | CONCLUSION

In summary, the present meta-analysis adopted a temporal-dynamic approach to study the impact of cost- and people-oriented interventions on mean-level changes in job attitudes. We found that such interventions incur sustained unit-level change in attitudes. Cost-oriented (people-oriented) interventions decreased (increased) unit-level job attitudes (vs. no intervention controls), and these effects were explained by changes in the balance of social exchange (unit-level changes in perceived support). The present results imply that people-oriented investments pay off while cost cutting does long-term damage to an organization’s esprit de corps. Whenever possible, cost cutting is best combined with continued signaling that people are the firm’s most prized asset.

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ORCID

Omar N. Solinger  https://orcid.org/0000-0003-4240-2625

ENDNOTES

1 We did not code for normative or continuance commitment (see Meyer et al., 2002) as these constructs have been critiqued for referring to different types of bonds (e.g., Klein et al., 2012) and for being conceptually distinct from the more attitudinal notion of affective commitment (our focus; see Harrison et al., 2006; Solinger et al., 2008). Neither did we code for motivational states, such as work engagement (Crawford et al., 2010). Although empirically related, a general energetic state at work is conceptually distinct from an attitude, which is inherently more evaluative and necessarily focused on an object (Eagly & Chaiken, 2007).

2 Not all technological changes necessarily focus on cost saving, but many of them do emphasize improving business process where the ultimate goal is cost-saving and operational efficiency (e.g., Grover & Malhotra, 1997). We discarded technological change studies if they did not have a cost saving emphasis. Similarly, not all mergers and acquisitions (M&A’s) emphasize cost saving, but many do. Cost saving is, in fact, found to be the most dominant behavioral emphasis of organizations that merge or acquire (Hitt et al., 1996). Even if some decisions to merge or acquire have not been legitimated a priori with referents to cost saving, the latter is often the behavioral focus in the second phase of M&A implementation (Barkema & Schijven, 2008). If cost cutting was not a behavioral focus at any point of the M&A, we discarded the study.

3 $T^2$ is the estimate of variance in the effect size distribution, whereas $P$ is the percentage of total variance attributable to systematic variation in the effect sizes (Borenstein et al., 2009).

4 Given the possibility that return to baseline effects only become apparent when using longer time lags, such as 3 to 4 years, we ran an
additional sensitivity analysis in which we examined whether the length of the time-interval interacted with the intervention type (i.e., cost-oriented and people-oriented interventions) to predict changes in job attitudes. A significant interaction effect may indicate that effects of interventions change as the time-lag increases. However, we found that the length of time interval did not interact with the intervention type to predict $T_3 - T_1$ changes in job attitudes (likelihood-ratio test ($df = 2$) = 1.88, $p = .39$).

5 PPP values below .05 indicate poor fit; PPP values approaching .50 suggest good fit (Zyphur & Oswald, 2015); the deviance information criterion (DIC) can be used to compare non-nested models, with smaller values indicating better fit of the model to the data.

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**AUTHOR BIOGRAPHIES**

**Omar N. Solinger** is an Associate Professor at Vrije Universiteit Amsterdam, in the Department of Management and Organization. He obtained his PhD in Organizational Behavior from Maastricht University (2010), the Netherlands. His current research centers around the topics of the dynamics of commitment, leadership, and organizational change.

**Jeff Joireman** is a professor in the Department of Marketing and International Business at Washington State University. He obtained his PhD in psychology at the University of Delaware (1996). His research focuses on decision-making in temporal dilemmas, environmental decision-making, service failures, corporate social responsibility, and public policy issues.

**Tim Vantilborgh** is an Associate Professor at Vrije Universiteit Brussel, in the Department of Experimental and Applied Psychology. He obtained his PhD in Psychology at Vrije Universiteit Brussel, Belgium (2012). His research centers around the dynamics of wellbeing, stress, and the psychological contract.

**Daniel Balliet** is a professor in the Department of Department of Experimental and Applied Psychology at Vrije Universiteit Amsterdam. He obtained his PhD in Experimental Psychology (2007) at Washington State University. His research centers around the topics of cooperation, interdependence, social exchange theory, and social dilemmas.

**SUPPORTING INFORMATION**

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

APPENDIX A.

<table>
<thead>
<tr>
<th></th>
<th>1.</th>
<th>2.</th>
<th>3.</th>
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<td>Cost-oriented</td>
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<td>Change in perceived support</td>
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<td>.85 **</td>
<td>-.61 ***</td>
<td>.24 ***</td>
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Note: Correlation matrix was obtained from Mplus Version 8, using the Maximum Likelihood algorithm. N = 252.
*p < .05.
**p < .01.
 ***p < .001.

APPENDIX B.

<table>
<thead>
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<th>Time and type of intervention</th>
<th>Q</th>
<th>k</th>
<th>d</th>
<th>95% CI</th>
<th>r²</th>
<th>η²</th>
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<td>[-0.36; -0.02]</td>
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<td>13</td>
<td>0.22</td>
<td>[0.01; 0.43]</td>
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<td>8</td>
<td>0.01</td>
<td>[-0.22; 0.24]</td>
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<tr>
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<td>88</td>
<td>0.02</td>
<td>[-0.07; 0.12]</td>
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<tr>
<td>Post-intervention changes (T₂ to T₃)</td>
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<td>134</td>
<td>0.06</td>
<td>[-0.10; 0.22]</td>
<td>0.17</td>
<td>0.97</td>
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<tr>
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<tr>
<td>People-oriented</td>
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<td>8</td>
<td>-0.02</td>
<td>[-0.23; 0.19]</td>
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<tr>
<td>No intervention</td>
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<td>88</td>
<td>0.02</td>
<td>[-0.07; 0.10]</td>
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<tr>
<td>Sustained changes (T₁ to T₃)</td>
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<td>0.22</td>
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<td>0.16</td>
<td>0.96</td>
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<tr>
<td>Cost-oriented</td>
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<td>24</td>
<td>-0.22</td>
<td>[-0.36; -0.08]</td>
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<tr>
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<td>28</td>
<td>0.18</td>
<td>[0.04; 0.33]</td>
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<td>[-0.23; 0.13]</td>
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<tr>
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<td>86</td>
<td>0.02</td>
<td>[-0.07; 0.10]</td>
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</table>

Note: Q = test of heterogeneity, indicative of statistical power of the moderation test; k = number of independent effect sizes; d = standardized mean difference (effect size); T = Tau coefficient of heterogeneity (indicating the degree heterogeneity across studies that might be explained by additional predictors). Effect sizes were aggregated to ensure independence of observations. Estimates for the no intervention condition differ slightly between initial, post-intervention, and sustained change rows, as they are adjusted for the remaining effects in their respective analyses.
*p < .05.
**p < .01.
 ***p < .001.