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The Promise of Just-in-Time Adaptive Interventions for Organizational Scholarship and Practice: Conceptual Development and Research Agenda

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Abstract

Organizational researchers are now making widespread use of ecological momentary *assessments* but have not yet taken the logical next step to ecological momentary *interventions*, also called Just-in-Time Adaptive Interventions (JITAIs). JITAIs have the potential to test within-person causal theories and maximize practical benefits to participants through two developmental phases: The microrandomized trial and the randomized controlled trial, respectively. In the microrandomized trial design, within-person randomization and experimental manipulation maximize internal validity at the within-person level. In the randomized controlled trial design, interventions are delivered in a timely and ecological manner while avoiding unnecessary and ill-timed interventions that potentially increase participant fatigue and noncompliance. Despite these potential advantages, the develop- ment and implementation of JITAIs require consideration of many conceptual and methodological factors. Given the benefits of JITAIs, but also the various considerations involved in using them, this review introduces organizational behavior and human resources researchers to JITAIs, provides guidelines for JITAI design, development, and evaluation, and describes the extensive potential of JITAIs in organizational behavior and human resources research.

Keywords

just-in-time adaptive intervention, ecological momentary intervention, within-person, randomized controlled trial, microrandomized trial

A central insight over the last two decades is that organizational phenomena once thought to be largely stable are instead quite variable over time and across work situations, fluctuating within and across days for a given employee (Dalal et al., 2020a; Shipp & Cole, 2015). This recognition has largely arisen from the use of ecological momentary assessments (EMA; also called experience sampling methods) to assess within-person variance in emotion states, momentary cognitions, and momentary job performance (Beal & Weiss, 2003; Dalal et al., 2009; Gabriel et al., 2019; McCormick et al., 2020).

That levels of these phenomena can change so frequently, though, also suggests another opportunity: Researchers can go beyond merely *documenting* within-person variability and relationships among constructs to actively *manipulating* levels of those dynamic organizational phenomena. Thus, in the current manuscript, we introduce the use of dynamic, ecological momentary *interventions* for dynamic organizational phenomena. These interventions go beyond what EMA studies currently do: In addition to capturing momentary state scores in relevant outcomes, they deliver customized support (i.e., interventions) to each employee based on dynamic, momentary factors (e.g., changes in the time of day and/or an employee's location, activity, or psychological state) to modify momentary, and ultimately long-term, employee outcomes. These interventions are also known as Just-in-Time Adaptive Interventions (JITAIs). Formally defined, the JITAI is "an intervention design aiming to provide just-in-time support, by adapting to the dynamics of an individual's internal state and context" (Nahum-Shani et al., 2018, p. 448).

Specifically, what distinguishes JITAIs from other types of interventions is the incorporation of momentary states of vulnerability, opportunity, and receptivity as necessary conditions in designing the intervention. Vulnerability refers to periods of increased susceptibility to negative outcomes. A vulnerable state can emerge rapidly and organically (e.g., urge to enact counterproductive work behavior [CWB] when an individual is experiencing negative emotion; Dalal et al., 2009). Because of their just-in-time nature, JITAIs can target the state of vulnerability when intervention is most needed, such as when a person is experiencing significant stress or low motivation.

Conversely, opportunity refers to periods of increased susceptibility to positive changes (Nahum-Shani et al., 2015). The underlying assumption is that identifying these momentary learning opportunities is crucial for facilitating behavioral, emotional, or cognitive improvements. For instance, in an emotion regulation intervention, when individuals experience negative work events, there is an opportunity to enhance their awareness of affective states, understand how their perception and interpretation of work events influence their feelings, and learn adaptive strategies for regulating their work-related emotions (e.g., Erber & Erber, 2001; McHale et al., 2015). Therefore, because JITAIs can provide timely support, they capitalize on the teachable moments when individuals are most susceptible to the intervention's potential effects.

JITAIs are also intended to provide support when the individual is receptive to intervention. Receptivity refers to moments when individuals can receive, process, and utilize the provided support (Nahum-Shani et al., 2015). It reflects factors such as the demands of the support itself and the recipients' ability or motivation to utilize a specific intervention. For instance, during an important presentation, individuals may be anxious and thus vulnerable to negative outcomes that could be alleviated by an intervention (i.e., high vulnerability) but nonetheless unable to receive, process, and utilize intervention support delivered via video (i.e., low receptivity). JITAIs can use momentary factors to determine the receptivity of an intervention to avoid unnecessary interventions that potentially lead to participant noncompliance with the intervention and, over time, participant fatigue, boredom, or hostility toward the intervention (Nahum-Shani et al., 2021). Continuing the previous example, the intervention could be deployed shortly before the presentation, perhaps triggered by the alert regarding the upcoming presentation on the person's electronic calendar.

The design of a JITAI typically involves two experimental phases (or studies): (1) A microrandomized trial (MRT) to optimize the design of the JITAI, and (2) a randomized controlled trial (RCT) to implement and evaluate the optimized JITAI. See Figure 1 for an illustration of MRT and RCT designs. The MRT design involves within-person random assignments of intervention and control conditions in a repeated measures design and experimental manipulation for each participant (Klasnja et al., 2015). Such a design maximizes internal validity at the within-person level. Thus, improving upon observational EMA studies, the MRT permits causal tests of within-person theories (e.g., whether negative emotions causally influence CWB). Moreover, given that the MRT design captures time-varying factors in a shorter timeframe than traditional, static organizational interventions, the JITAI can shed light on the temporal specificity of the relationships (e.g., how the relation- ships manifest and evolve across different temporal intervals, such as hours, days, or weeks). Thus, the MRT phase of JITAIs is intended to be highly pertinent to advancing the scientific understanding of fundamental knowledge (Stokes, 1997).

The RCT phase of the JITAI involves between-person randomization to JITAI versus control conditions, maximizing internal validity at the between-person level. In the RCT, unlike the MRT, in the intervention group, within-person assignment to one of the intervention conditions is not random; rather, individuals in the intervention group receive one intervention versus another at the right time and place, based on the findings from the MRT (note that the MRT as the optimization phase of JITAIs is discussed in more detail in a subsequent section). Thus, ideally, JITAIs could provide interventions when the participant needs them most, and could avoid unnecessary interventions that may lead to participant fatigue and attrition. Moreover, optimized JITAIs can provide customized intervention support over time by adapting to transient information (i.e., individual customization by selecting intervention options based on each participant's momentary personal or situational factors; Nahum-Shani et al., 2018). Both the just-in-time and the adaptive aspects of the JITAI are intended to increase its efficacy as an intervention. Thus, the JITAI in an RCT design is highly pertinent in practical application and benefit to the participant (Stokes, 1997).

In combining the strengths of the MRT design and the RCT design to effect change in dynamic phenomena, we believe that the JITAI has the potential to meaningfully advance theory and practice in organizational behavior and human resources (OBHR). The JITAI is, therefore, among one of the relatively rare sets of research designs that blend the rigor of basic science with the relevance of applied science (see Stokes's, 1997, description of "Pasteur's Quadrant"). Accordingly, the JITAI can contribute meaningfully to the scientist-practitioner model that is often held up as the ideal for OBHR (Rupp & Beal, 2007). Various JITAIs have been developed in, and have shown much promise in, the health behavior domain (Heron & Smyth, 2010;

Nahum-Shani et al., 2015). However, JITAIs have thus far received almost no attention in OBHR.[1] Therefore, the goal of this paper is to translate JITAI research to OBHR and provide methodological guidance to researchers and practitioners (in OBHR but more broadly as well) hoping to implement a JITAI, while also noting areas in need of further exploration.

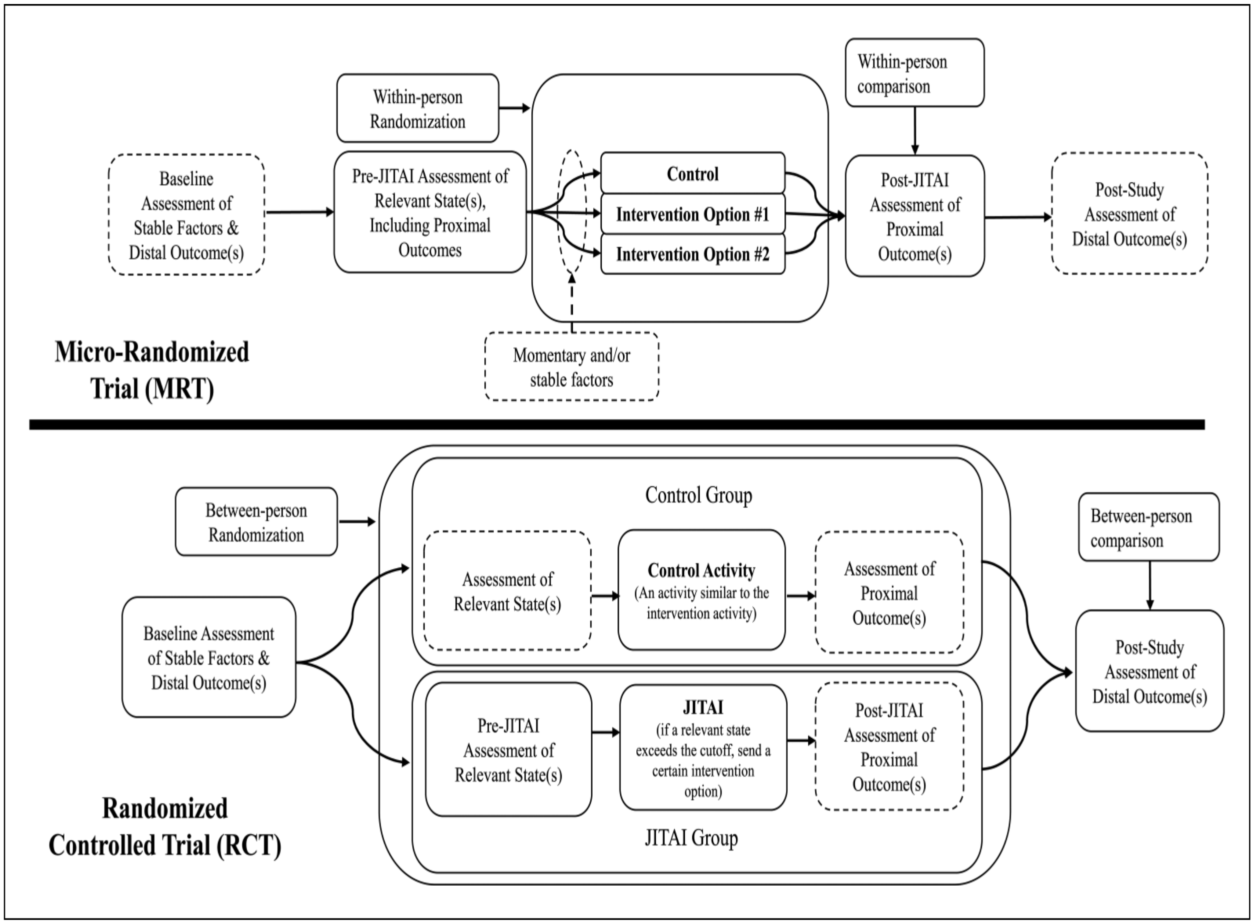

Figure 1. An illustration of a MRT and a RCT of just-in-time adaptive interventions.
Note. Dashed (rather than solid) lines indicate that this step is optional depending on the design of the MRT or RCT. MRT =microrandomized trial; RCT = randomized controlled trial.

In this review, we show the potential of JITAIs in OBHR research by discussing how JITAIs are consistent with influential dynamic theories in OBHR (e.g., Mischel & Shoda, 1995; Weiss & Cropanzano, 1996) and by reviewing the levels of dynamism and customization in existing OBHR experimental research. Next, we describe the elements and process of designing and con- ducting JITAIs. After that, we review the

JITAI literature in other (nonorganizational) research domains. Last, we discuss data analytic approaches relevant to JITAIs as well as the limitations of JITAIs.

The Potential of JITAIs in OBHR Research

Testing Dynamic OBHR Theories with JITAIs

JITAIs are consistent with and can serve to causally test important dynamic theories in OBHR. In what follows, we briefly illustrate how JITAIs align with two such theories of affect and personality that have been used to study OBHR phenomena (Dalal et al., 2020a): Affective events theory (AET: Weiss & Cropanzano, 1996) and Cognitive Affective Processing System (CAPS: Mischel & Shoda, 1995). These theories provide useful conceptual frameworks with which to illustrate the applicability of a JITAI in modifying levels of affect and personality states, respectively.

According to AET (Weiss & Cropanzano, 1996), discrete events elicit affective (i.e., emotional) reactions. These resultant affective states have immediate influences on proximal workplace behaviors and cumulative influences over time on longer-term outcomes. Extending AET, Beal et al. (2005) proposed various cognitive mechanisms (e.g., attentional and self-regulatory processes during task execution) through which affect impairs momentary job performance. Therefore, AET emphasizes within-person event-emotion-performance linkages. Consistent with AET, JITAIs are inherently within-person designs that can actively influence these event-emotion-performance link- ages when and where they occur. For example, a JITAI on emotion regulation can causally identify and then attenuate connections between discrete events and negative affect, thereby minimizing the cumulation, as well as the ultimate behavioral consequences (e.g., poor performance), of negative affect at work.

Turning to CAPS, the focal prediction is that discrete situations give rise to specific patterns of cognitions, affects, and ultimately behaviors for each individual (Mischel & Shoda, 1995). Each individual has their own if-then contingent (situation-behavior) personality profile as a result of their cognitive-affective processes (e.g., Minbashian et al., 2010). Some situation-behavior contingencies are likely to be maladaptive, and JITAIs can be designed to interrupt and, in the longer term, recon- figure these maladaptive contingencies. For example, JITAI researchers could first causally identify the unique situational cues associated with low job performance for each individual (i.e., personalization). Subsequently, when these situational cues are detected for a given individual, JITAI researchers can trigger an intervention to facilitate personality state expressions consistent with high rather than low job performance.

In summary, JITAIs align with and have the potential to contribute to, organizational theories that emphasize within-person dynamics at work. In what follows, we review existing (non-JITAI) OBHR experimental research based on the degree to which this existing research captures and capitalizes on dynamic organizational processes. Based on this review, we then demonstrate the potential use for JITAIs.

Dynamism and Customization of OBHR Experimental Research

Existing OBHR experimental research, including intervention-focused research, varies along a number of dimensions (see Stouten et al., 2018). What most distinguishes JITAIs from other experimental designs is their high level of dynamism and customization, respectively. Dynamism refers to the degree to which a design captures momentary changes in phenomena over time. Customization is defined as the degree to which an intervention or manipulation uses an individual's momentary and/ or stable personal and/or contextual characteristics to select when and how to intervene.

Each of these dimensions is a continuum. At the low end of the dynamism continuum are static (e.g., one measurement) experimental designs. At the high end of the dynamism continuum are studies that include multiple manipulations or interventions as well as multiple measurement occasions over time. At the low end of the customization continuum are studies in which everyone (in a given condition) receives the same manipulation or intervention. At the high end of the customization continuum are studies that tailor manipulations or interventions based on time-varying (i.e., within-person) and/or time invariant (i.e., between-person) factors.

To gain a clearer picture of how JITAIs potentially could benefit OBHR, we conducted a systematic literature review on methodological aspects of existing (non-JITAI) experimental OBHR research, with a focus on the levels of dynamism and customization in such research. Appendix A in the supplemental materials describes our method for the systematic literature review as well as the complete results. Table 1 summarizes the frequency and percentage of each level of dynamism and customization from the literature review. We organize this section around dynamism, additionally incorporating customization where applicable.

Low Level of Dynamism. Most current OBHR experimental research is static with a low level of dynamism and without customization (75 of 116 studies, or 64.66%).[2] Examples include any traditional interventions or experimental designs where all participants (in the same condition) receive the same intervention support or manipulation without any customization (e.g., Brosi & Gerpott, 2022). The limited scope of only one manipulation and one or two measurement points in a low-level dynamism OBHR experimental study fails to fully capture the complex nature of the change process. Moreover, the delivery (i.e., presence vs. absence) of the manipulation is not based on participants' states of vulnerability (i.e., a transient tendency to experience adverse outcomes), opportunity (i.e., periods of opportunity for positive change), or receptivity (i.e., to receive the manipulation). Therefore, these studies have low levels of dynamism and customization.

Table 1. Dynamism and Customization of Experimental Research Design in the Organizational Behavior and Human Resources Literature.

	Customization			
Dynamism	No customization	Between-person customization	Within-person customization	Total
Low	75 (64.66%)	1 (0.86%)	3 (2.59%)	79 (68.10%)
Medium	19 (16.38%)	2 (1.72%)	1 (0.86%)	22 (18.97%)
High	15 (12.93%)	0 (0.00%)	0 (0.00%)	15 (12.93%)
Total	109 (93.97%)	3 (2.59%)	4 (3.45%)	116 (100.00%)

Note. $N = 116$. Numbers in parentheses are percentages. Percentages may not sum precisely to 100.00% due to rounding.

Among these static OBHR experimental studies, very few studies customize the intervention or manipulation options at the between-person level based on static factors

(1 of 116 studies, or 0.86%) or at the within-person level based on momentary factors (3 of 116 studies, or 2.59%). Notable exceptions include an intervention study by Fontes and Dello Russo (2021), in which employees completed a preintervention questionnaire, reflected on their work experiences in four individual sessions with a coach during the intervention, and completed a postintervention questionnaire afterward. Although this study has a low level of dynamism (because momentary changes are not captured), it incorporates within-person customization through the one-on-one coaching sessions where employees reflected on their idiosyncratic work experiences. A design like this one seems appropriate for the study in question because the design captures between-person differences (which were of interest in the study) but does not manipulate or capture momentary (changes in) states, such as mood.

Medium Level of Dynamism. A study with a medium level of dynamism captures within-person changes but not momentary changes specifically. We operationalize a medium level of dynamism as having a single manipulation and three or more measurement points for outcome variables. For example, traditional interventions with multiple measurement occasions (e.g., before the intervention, immediately after the intervention, and one month after the intervention) would be candidates for medium dynamism. Some OBHR experimental research has utilized a design with a medium level of dynamism (22 of 116 studies, or 18.97%). Few of these studies specifically tailored the timing, type, or amount of intervention support or manipulation on the basis of stable or momentary factors associated with each participant. Specifically, among the 22 studies, two studies applied between-person customization (1.72%), and only one study applied within-person customization (0.86%). For example, Ilies et al. (2013) conducted a study involving an initial survey, a customized feedback manipulation, and two follow-up surveys. In the initial survey, participants reported their CWBs. Then, participants were randomly assigned to either a feedback or no-feedback condition. In the feedback condition, participants received their CWB score and feedback on whether their CWB score was higher or lower than the average score for the sample (between-person customization). This study demonstrates a medium level of dynamism due to having a single manipulation and three measurement occasions that capture within-person changes. Additionally, between-person customization was applied through personalized feedback based on CWB scores from the initial survey. Studies of medium-level dynamism are appropriate for constructs that exhibit within-person variability; however, they fall short of JITAIs, which further allow for capturing states of vulnerability, opportunity, and/or receptivity.

High Level of Dynamism. Based on the literature review of prior OBHR experimental research, we find that OBHR research has not embraced high levels of both dynamism and customization (0 of 116 studies, or 0.00%). A small number of OBHR experimental studies (15 of 116 studies, or 12.93%) captured a high level of dynamism (i.e., within-person experimental manipulation is delivered more than two times, and outcome variables are measured at least three times). However, these studies did not additionally capitalize on momentary states (i.e., no customization). Specifically, these studies used a within-person approach in which they delivered a manipulation with a daily survey at a fixed time on multiple occasions (e.g., Lanaj et al., 2019, 2022; Song et al., 2018). For example, in a positive leader self-reflection intervention (Lanaj et al., 2019), participants were randomly assigned to either the control or the intervention condition on a daily basis across ten workdays (five days for each condition). Thus, this study demonstrated a high level of dynamism because of the repeated manipulations and measurements. In this study, however, instructions of the manipulation were delivered at a fixed time, and thus there was no customization. More generally, although the few existing within-person experiments have captured momentary states in the individual, these states (or changes therein) have not themselves influenced the delivery or tailoring of the intervention support or manipulation.

Thus, here, we propose that OBHR researchers should consider adding to their repertoire a more dynamic and customizable design, the JITAI, which incorporates momentary factors to both deliver intervention options (i.e., just-in-time) and select them (i.e., adaptation). Next, we articulate various benefits of using JITAIs in organizational settings in terms of both theoretical and practical considerations. The motivation for applying JITAIs is grounded in the idea that momentary factors play an important role in determining whether an intervention is necessary and beneficial.

For example, momentary intervention support is likely to be most efficacious for constructs that exhibit appreciable within-person variance (McCormick et al., 2020). Researchers can consider applying JITAIs to align intervention support according to likely time- frames in which the construct of interest is likely to change or be changed. Furthermore, some constructs may exhibit generally low base-rates but nonetheless occur on a momentary basis and, when they occur, exhibit high severity (e.g., anger, sexual harassment). In these cases, a JITAI may be more effective as a “booster shot” to be deployed periodically in addition to, rather than instead of, traditional interventions. To this end, in Figure 2, we provide a flowchart to illustrate the circumstances under which JITAIs would be a good candidate when designing an OBHR intervention.

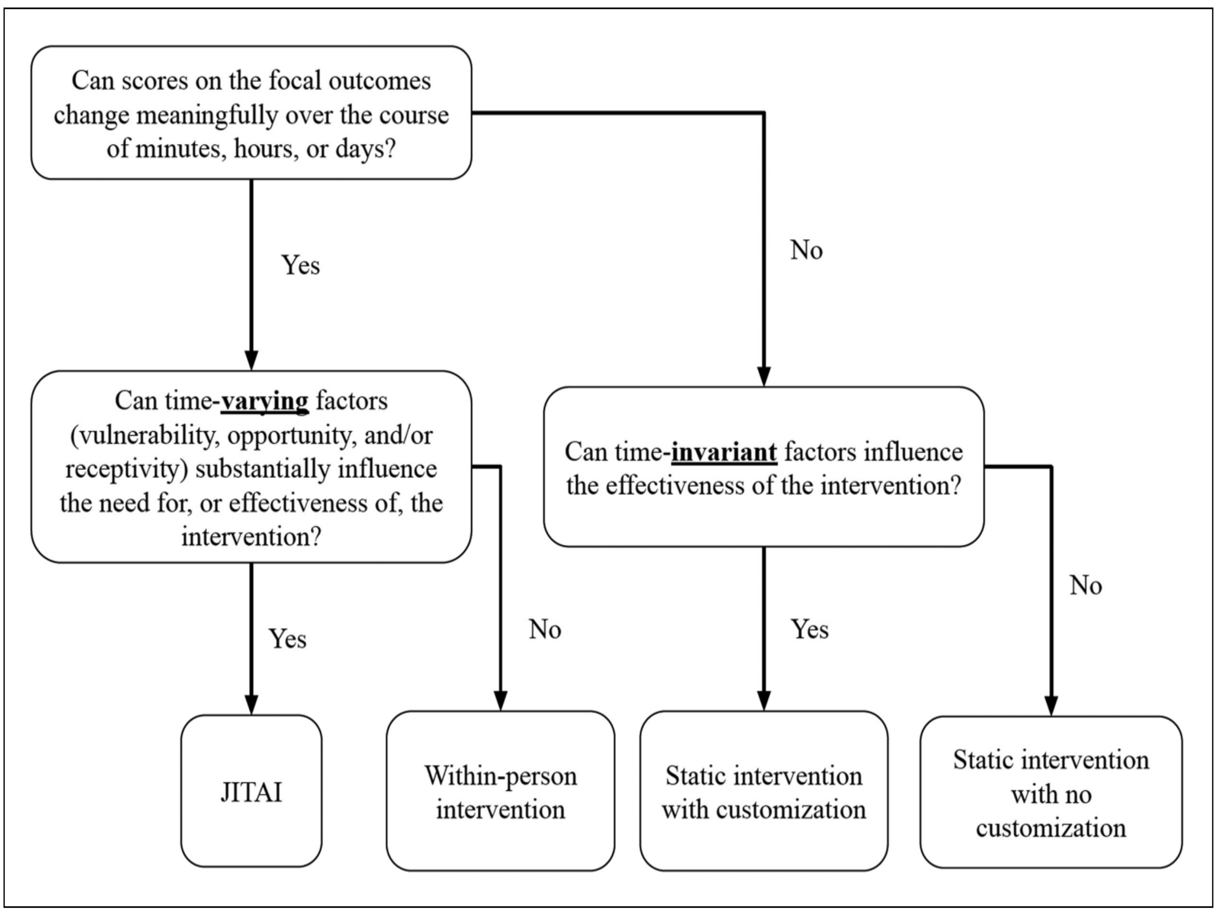

Figure 2. Flowchart that illustrates when to apply a just-in-time adaptive intervention (JITAI) versus other types of interventions.

The Design, Development, and Evaluation of JITAIs

The JITAI design framework allows for intervention support to be delivered at the moment when it is most needed, theoretically maximizing intervention effectiveness (and adherence) and minimizing participant burden. This section describes (1) the fundamental design elements of a JITAI, (2) the three-stage process of preparing, optimizing, and evaluating a JITAI, and (3) an example of an OBHR JITAI for workplace emotion regulation.

Design Elements of a JITAI

The flow and fluctuation of psychological states and behaviors is punctuated by moments when a person is particularly vulnerable to negative change or by moments characterized by opportunity for positive change (Nahum-Shani et al., 2018). For example, when an affective event occurs at work, a state of core affect may shift to a full-blown emotional episode that redirects attentional focus, detracts from task performance, and causes fatigue (Weiss & Merlo, 2020). The JITAI consists of six design elements that allow for successful intervention during these moments (i.e., before the emotion episode ensues): Decision points, tailoring variables, intervention options, decision rules, proximal outcomes, and distal outcomes (see Table 2 for definitions and operationalizations in example health behavior and hypothetical OBHR research).

Decision points are the time points where researchers decide to send an intervention and decide which intervention option (i.e., type of intervention) to send. The timeframe of decision points should align with the variability of the outcome of interest and the feasibility of the research design. For instance, sedentary behaviors can be measured every 30 min or even continuously through sensor data, whereas emotional states can be measured a few times per day through self- report. Assessments of the target state or behavior can be fixed (e.g., three times per day with a fixed schedule) or random (e.g., three random prompts per day between 8:00 am and 6:00 pm). The decision points can be determined by the researcher ("push" interventions) or self-initiated by the participant ("pull" interventions).

In either case, these moments (points) where change is most likely to occur are embedded in a person's natural environment (Hormuth, 1986). As such, they depend on context, concurrent states, and stable traits (e.g., Horstmann et al., 2021). Each of these related factors may be operationalized as tailoring variables, which can be time-varying or time-invariant. Tailoring variables help determine when intervention support should be provided (Nahum-Shani et al., 2018). For example, some individuals may be especially adept at regulating their emotions and may therefore require less frequent intervention than other individuals (Joseph & Newman, 2010). Similarly, some affective events may be less significant or less difficult to react to and may therefore be less important candidates for intervention than other events (Frijda et al., 1989).

If intervention support should be provided at a given decision point based on these tailoring variables, then it is also necessary to determine which *intervention option* to provide (Nahum-Shani et al., 2018). Because of the JITAI's reliance on mobile technology, a single study can contain a number of intervention options that vary in content, length, intensity, and so forth, so as to ensure that the support matches the needs of the moment (Kelly et al., 2012). There exist many emotion regulation strategies, for example, and some may be more effective than others in a given context (English et al., 2017). All the decision points, tailoring variables, and intervention options are then codified into a set of *decision rules* (i.e., if-then statements) to ensure that support is provided systematically according to specific values and thresholds of these design elements (Nahum-Shani et al., 2018).

Altogether, these design elements are intended to work in concert to produce changes in *proximal outcomes*, which are the mechanisms by which more *distal outcomes* may be affected (Nahum-Shani et al., 2018). Much behavioral and psychological change is gradual, resulting from the formation of habits and a perceptual association between behaviors and contexts (Steinhart et al., 2019; Versluis et al., 2016). Thus, to achieve an "ultimate" distal outcome, a JITAI is primarily intended to target malleable, proximal outcomes such as the underlying mechanisms, temporal precedence of media- tors, or state expressions of the distal outcome itself (Nahum-Shani et al., 2015; Steinhart et al., 2019).

Table 2. The Six Key Elements JITAIs, With Examples

		Examples from health behavior research		Hypothetical examples from OBHR	
Element	Definition	Stress management (Smyth & Heron, 2016)	Alcohol abuse (Gustafson et al., 2014)	Leader development	Sexual harassment prevention
Decision points	Moments when an intervention option may (or may not) be delivered	Three semirandom prompts each day (morning, afternoon, and evening)	Passively tracking distance to high-risk locations (e.g., bars frequented by participants)	One prompt each month to subordinates during a leader's first year on the job	One prompt each month for a year
Tailoring variables	Time-varying (states, situations) or time-invariant (traits) information that may inform individualization of intervention support	Self-report on negative mood, stressful events, and stress severity	High-risk locations identified by the participants	Subordinates' reports of leadership effectiveness from different perspectives (e.g., initiating structure, consideration, instrumental support, emotional support, family support, and recovery support)	Information from company calendar on time and location of company events
Intervention options	Possible treatments that might be employed at any given decision point	Interventions on stress appraisal (16 prompts) and relaxation (11 prompts)	Alert sponsor OR provide nothing	Interventions on leadership knowledge, skills, abilities, and other components (Day, 2000; Lanaj et al., 2019) in different domains (e.g., initiating structure, consideration, instrumental support, emotional support, family support, and recovery support)	Interventions on sexual harassment knowledge, identification skills, and prevention strategies
Decision rules	Formal operationalization of when, how,	If stress or negative affect ≥ 1 SD	If a patient nears a high-risk location,	If the mean of monthly leadership	If a company event is scheduled at

	and what intervention support will be provided based on these previous JITAI elements	above the person-centered mean on the relevant construct, then send a microintervention immediately and another one 10–20 min later	then initiate an alert asking the patient if he or she wants to be there and provide just-in-time support	effectiveness score rated by subordinates ≤ a cutoff, then send an intervention immediately to the leader	a higher-risk time (e.g., outside regular work hours) or place (e.g., outside the workplace), then send an intervention to attendees the day before the event
Proximal outcomes	The short-term or immediate goals of the intervention (e.g., mediators, other indicators, short-term measure of a long-term outcome)	Decreases in the frequency of stressors reported, stress severity, negative mood, and cortisol levels; an increase in positive mood	Fewer risky drinking days, assessed halfway through the intervention, at the end of the intervention, and four months after the intervention	Improvements in leaders' affective-, cognitive-, and skill-based learning outcomes (Lacerenza et al., 2017), assessed monthly during the leader's first year on the job	Increases in reactions, knowledge, identification of sexual harassment skills, attitudes, and perceived organizational intolerance of sexual harassment (Roehling & Huang, 2018), assessed every 3 months for one year
Distal outcomes	The long-term or ultimate goals of the intervention	Improvements in health behaviors (eating frequency, alcohol consumption, smoking, sleep quantity, and sleep quality)	Greater abstinence and fewer negative consequences of drinking, assessed halfway through the intervention, at the end of the intervention, and four months after the intervention	Increases in leadership effectiveness (e.g., team performance and employee well-being), assessed every 3 months during the leader's first year on the job	Fewer sexual harassment complaints, both in general and stemming from company events in particular, assessed at the end of the year (and compared to the previous year)

Note. JITAIs unlike many conventional interventions, are brief, lasting for a maximum of a few minutes at a time. OBHR = organizational behavior and human resources; JITAIs = Just-in-Time Adaptive Interventions.

The Development of a JITAI

The development of a JITAI prototypically consists of three phases: Preparation, optimization, and evaluation (Collins, 2018).[3] *Preparation* involves the conceptual framing and theoretical development of the JITAI. The intervention is then tested through the latter two phases of *optimization*, which corresponds to the MRT and maximizes the within-person internal validity of the intervention, and *evaluation*, which corresponds to the RCT and maximizes the between-person internal validity of the intervention. Altogether, these developmental phases help ensure that a JITAI will be effective in achieving its intended outcomes while also maximizing external validity due to its field-based implementation. We discuss each of the three stages in turn, and illustrate them with an emotion regulation intervention example. See Table 3 for a summary and comparison of the MRT and RCT designs (along with the EMA design). See Table 4 for a checklist to develop a JITAI study.

Preparation. The preparation of a JITAI begins with the conceptual grounding of the intervention within a theory of human behavior that elucidates the mechanisms and timeframes by which a person will engage in more or less of a given behavior over time (Nahum-Shani et al., 2015; Spruijt-Metz & Nilsen, 2014). The JITAI's design elements may then be chosen and aligned to change theoretically relevant proximal outcomes and, ultimately, distal outcomes (Nahum-Shani et al., 2018).

Key Decisions in the Preparation Phase. At this phase, there are several key decisions that must be made with respect to the design of the intervention. First, *what kinds of and how many intervention options should be provided?* Before conducting the full JITAI study, researchers can run a pilot study to examine the effectiveness of the intervention options for the intended outcomes by including manipulation checks and discriminant validity checks. If researchers find that the intervention options cause changes in the intended constructs (manipulation check) and do not cause changes in the unintended but plausible alternative constructs (discriminant validity check), the construct validity of the intervention options will be supported. If the manipulation check does not demonstrate any improvement in the intended outcomes, researchers can consider redesigning the intervention options to better target the intended outcomes. If the intervention options do demonstrate positive changes in the intended outcomes, but also demonstrate changes in unintended outcomes, researchers can consider either including these unintended outcomes as control variables in the full JITAI design or else redesigning the intervention options to better target *only* the intended outcomes.

Table 3. A Summary of and Comparison Among EMA, MRT, and RCT Designs.

	Research Design		
Comparison Factor	EMA	MRT	RCT
Key features	• No randomization • No manipulation	• Within-person randomization across decision points (i.e., occasions) • Has manipulation: All participants receive intervention and control conditions at different decision points (i.e., occasions)	• Between-person randomization • Has manipulation: Participants in the intervention group(s) receive interventions
Strengths	• Observation of naturally occurring relationships (less intrusive)	• High internal validity at the within-person level through within-person random assignment • Within-person causal inference using the causal excursion model (which can provide unbiased estimates of causal effects)	• High internal validity at the between-person level through between-person random assignment • Practical benefits in the JITAI group where the most suitable intervention support is delivered when most needed by the participant, based on time-varying and time-invariant variables
Weakness	• Lower internal validity (e.g., endogeneity) • Demand effects • Sample selection bias • Compliance issues • Costa	• Intrusive manipulation • Demand effects (when the control condition is a “provide nothing” condition) • Sample selection bias • Compliance issues • Costa	• Intrusive manipulation • Demand effects (when the control condition is a no-intervention condition) • Treatment selection bias (1. when there is a “pull” intervention option; 2. when intervention support is personalized, such that participants with certain

			characteristics or under certain situations only receive certain intervention support) • Compliance issues • Cost (As an intensive intervention, the JITAI may be a costly endeavor, in terms of time and money, for researchers to design and for participants to complete.)

Note. WCLS (Boruvka et al., 2018) for continuous time-varying outcomes. EMA = Ecological Momentary Assessment; MRT =microrandomized trial; RCT = randomized controlled trial.

[a]Yet, because it is a within-person design, the necessary sample size is smaller, and therefore the cost is lower, than that for a between-person (or rather multilevel) design to achieve the same power in examining within-person effects.

Table 4. A checklist for Researchers to Design JITAI Studies.

Design phase	Key considerations	Solutions
Preparation	1. Determine the need for a JITAI	
	a. Is there sufficient within-person variability in the construct of interest?	Look at relevant theories and available empirical evidence that articulate the timeframes over which the construct varies/changes. If theories or empirical evidence suggest insufficient within-person variability in the construct, consider taking an approach that is at a lower level of dynamism (vs. a JITAI).
	b. Are there moments of vulnerability (to experience adverse outcomes), opportunity (for a positive change in the construct), or receptivity to an intervention?	Consult relevant theories and empirical evidence to find whether there are any time-varying factors (e.g., vulnerability, opportunity, receptivity) that may influence the need for, or effectiveness of, an intervention. If no theory or evidence is available, researchers can consider conducting cognitive interviews to inform the decision.
	2. Determine the components for a JITAI	
	a. Does a JITAI study need the full design, including both the MRT and the RCT? Are there constraints that would suggest a quasiexperimental (vs. true experimental) design?	If there are insufficient resources for both the MRT and the RCT, consider focusing on the study that is best aligned with the research goals. If the study aims to infer causal relationships, an MRT study is needed. If the study aims to compare the JITAI intervention with other traditional interventions, or if the study is more focused on real-world impact than causal

		relationships, an RCT design is recommended. However, when it is not feasible to conduct an RCT study, quasiexperimental designs can be considered. If quasiexperimental approaches are adopted, consider using propensity score matching.
	b. Should the JITAI be developed as a standalone intervention? Or should the JITAI be appended to a pre-existing intervention?	Either approach could work, depending on the research goals. If the latter, then the JITAI may be designed as a "booster shot" to enhance training transfer and/or target within-person elements of a pre-existing intervention.
	3. Operationalize the design elements of a JITAI	
	a. Decision points. (1) When and how frequently should decision points be placed? (2) Should they be placed at a constant rate across the entire study?	(1) Decision points should be placed commensurately with the temporal nature of the outcome of interest (i.e., when change is most likely or possible) as well as in light of concerns regarding participant burden. (2) Decision points can be placed at either a constant rate or at a declining/increasing rate such that there are fewer/more decision points in the later portion of the study. To prevent participant fatigue, consider placing fewer decision points in later portions of the study if the intervention is especially intensive or burdensome.
	b. Tailoring variables. Decide whether tailoring variables are needed in a JITAI study. (1) If the intervention should be tailored, what are the focal time-varying and time-invariant tailoring variables that should be tested as moderators of intervention effectiveness? (2) What considerations determine whether the participant is available and/or receptive to the intervention?	(1) Consider whether the amount, type, or effectiveness of the intervention depends on any time-invariant or time-varying individual or contextual factors. For example, do emotionally stable individuals require fewer emotion regulation interventions? Or are cognitive reappraisal JITAIs more effective when people work on more complex tasks than less complex tasks? If any factors are identified, consider measuring time-invariant tailoring variables in the baseline survey and measuring time-varying tailoring variables in the preintervention assessments. (2) Consider operationalizing "availability" as a tailoring variable by collecting information from the context of the sample (e.g., work schedules, upcoming events), including items regarding a person's location and/or interaction partners, and measuring psychological states of receptivity (e.g., affect).
	c. Intervention options. (1) How many and what type of intervention options should be prepared? (2) How should the control condition be designed to facilitate fair comparisons?	(1) Consider including some variety of intervention options not just to prevent participant habituation and boredom but also to target the proximal and distal outcomes in more than a single fashion. Consider also whether one kind of intervention option would be more appropriate for one proximal outcome

		versus another, as well as whether the intervention options are theorized to act similarly on proximal versus distal outcomes. (2) The control condition should be designed to be equivalent in "strength" to the intervention option(s), so consider matching the type and duration of the control activity with that of the intervention options or else using a static intervention.
	d. Proximal and distal outcomes When should proximal and distal outcomes be measured?	Proximal outcomes should be assessed as close to the intervention as possible temporally, and distal outcomes should be assessed at times when change is theorized to have cumulated as well as at later times to measure whether the intervention effect "fades out" over time.
	4. Plan the intervention delivery	
	a. Which platform should be used to deliver interventions or assessments?	Many possible tools may be used to deliver interventions and collect data (e.g., surveys can be emailed or texted automatically using scheduling applications or survey platforms). Prioritize those that are easily and inexpensively accessible and that allow for maximal flexibility (e.g., in survey design, automation, etc.).
	b. Should a self-initiated "pull" intervention (akin to a "panic button") be included?	If participants are aware of the vulnerable moments, consider including a self-initiated "pull" intervention.
MRT	1. Design an MRT study	
	a. Baseline measurements	(1) Consider measuring time-invariant factors (e.g., individual differences and chronic/stable situational factors) in an initial baseline survey before the intervention portion of the study. (2) In most cases, consider measuring the preintervention level of the focal outcomes and time-varying factors before each microintervention session.
	b. Control conditions	Consider including one or multiple control conditions. Some options for control conditions include "provide nothing" (not optimal), neutral activity, or active alternative intervention.
	c. Within-person randomization	In most cases, randomize all conditions at a decision point regardless of the preintervention measure. In other words, time-varying factors or time-invariant factors should not be related to the probability of being assigned to a condition. However, within-person randomization may not be feasible on certain occasions. For example, when it is raining heavily, a message to encourage participants to walk outside is not suitable, and thus the probability of participants being available for this

		intervention option is related to weather (a time-varying factor).
	d. Probability of getting each condition	Consider balancing statistical power (i.e., between intervention option[s] and control) with participant burden. If there is no available empirical evidence suggesting whether one intervention option is better than others, use equal probability for all options. Conversely, consider giving one condition a higher probability if prior research has shown that one intervention option is theoretically or empirically more effective than the others. From the standpoint of power, a balancing ratio (i.e., 50:50) will achieve decent statistical power.
	e. Sample size	Conduct an a priori power analysis for MRT (see the tool by Liao et al., 2016) to determine the necessary sample size requirements to appropriately power the MRT study.
	f. Study length	The study length varies considerably across existing MRT studies. To determine study length for a planned study, consider the timeframe over which the intervention effects are expected to unfold, the costs of the intervention, and participant fatigue and habituation.
	2. Data analysis for an MRT study	
	a. Data analytic method	Use the causal excursion model (Qian et al., 2021) to analyze MRT data (e.g., MRTAnalysis R package).
	b. Centering	In the causal excursion model, centering is only needed for the intervention variables. For example, the dummy variable of intervention or not is centered on the probability of receiving an intervention condition vs. a control condition. The dummy variable of receiving a certain intervention option is centered on the probability of receiving this intervention vs. all other conditions.
RCT	1. Design an RCT study	
	a. Baseline measurements	(1) Consider measuring time-invariant factors in an initial baseline survey in all groups before the entire intervention program. If the outcomes of interest are at the between-person level, also include the preintervention measurement of outcomes in the initial baseline survey. (2) Consider measuring time-varying factors before each microintervention session (or equivalent control condition). If the outcomes of interest are at the decision-point (i.e., within-person) level, also include the preintervention measurement of the

		momentary outcomes before each microintervention or control activity.
	b. Control groups	Consider including one or multiple control groups. Potential options for control groups include an EMA-only group, a traditional static intervention group, a within-person intervention group without customization based on time-varying or time-invariant factors, and a waitlist control group. When there is difficulty in including a control group, researchers can at least use a pre-post intervention design to assess the effectiveness of the JITAI intervention.
	c. Between-person randomization	Consider randomizing individual participants into intervention or control groups. When it is not feasible to randomize participants to different groups (e.g., because of team membership), randomization at a collective (e.g., team or organization) level is acceptable.
	d. Sample size	Determine the sample size a priori through power analysis. For multilevel power analysis, which aids in the within-person aspects of between-person (or rather multilevel) models, readers can refer to multilevel modeling literature and tools (e.g., Lafit et al., 2021; Scherbaum & Pesner, 2019). For power analysis for solely between-person models (e.g., t-tests, F-tests, multiple regression), readers can refer to tools such as G*Power (Faul et al., 2007) and the pwr R package (Champely, 2020).
	2. Design the intervention group	If an MRT study is conducted before the RCT study, design the intervention group of the RCT study based on the results of the MRT study.
	a. Decision points	Decrease the number of less effective decision points and/or increase the number of more effective decision points. For example, if an afternoon decision point in the MRT study is more effective than a morning decision point (e.g., more likely to detect a moment of vulnerability, opportunity, or receptivity), include more decision points in the afternoon when designing the RCT. We do note that these decisions may be highly participant-specific (e.g., as a function of individual differences) and job-specific (e.g., as a function of job characteristics), suggesting that tailoring variables (see the next row) may impact decision points as well. If there is a significant trend in effectiveness of the intervention throughout the intervention program period, adapt the decision point schedule (via machine learning algorithms

		or manually) to that trend to maximize effectiveness and minimize participant burden. For example, if intervention becomes less effective over time due to habituation to the intervention options, schedule fewer decision points in the second half of the intervention period.
	b. Tailoring variables	Use the statistically significant time-varying and time-invariant moderators from the MRT and the decision-priority perspective (Collins, 2018) to determine tailoring variables for the RCT.
	c. Intervention options	Use the statistically significant time-varying and time-invariant moderators from the MRT and the decision-priority perspective (Collins, 2018) to determine tailoring variables for the RCT.
	d. Proximal and distal outcomes	Consider the level of the outcome(s) of interest when measuring the proximal outcome(s). (1) if the proximal outcome of interest is at the decision-point level, it should be captured right after each microintervention session. (2) if the proximal outcome of interest is at the between-person level, it should either be aggregated from the decision-point level measurements or else measured at the end of the entire intervention program. Distal outcomes are typically measured at the day level or the between-person level.
	3. Data analytic methods	
	a. If the outcome of interest is at the decision-point level	The correlated random effects approach (CRE; Mundlak, 1978; see also Antonakis et al., 2021) can be adopted to account for the nested nature.
	b. Centering	If CRE is used, centering is not recommended for both Level-1 and Level-2 variables.
	c. If the outcome of interest is at the between-person level	Independent-sample t-tests (if there are two groups), analysis of variance (ANOVA; if there are two or more groups), or regression (e.g., means-as-outcomes regression; Raudenbush & Bryk, 2002) can be used to compare the outcomes between groups.

Note. MRT = microrandomized trial; RCT = randomized controlled trial; CRE = correlated random effect; ANOVA = analysis of variance; JITAI = Just-in-Time Adaptive Intervention. Note that not every item applies to all JITAI studies. We recommend that researchers go through this checklist, and if an item does not apply to the study, answer not applicable.

Moreover, the provision of several intervention options is emphasized by the JITAI literature to prevent participant boredom and habituation but also to improve participants' skills through different mechanisms (Nahum-Shani et al., 2018). In addition, the researcher may choose to provide intervention support from different intervention options at different ratios (e.g., one intervention option may be administered twice as often as another), depending on participant burden as well as theory. Beyond the theoretical and practical importance of including a variety of intervention options, it is possible to test the effectiveness of intervention options that vary in theoretical mechanism, design principle, and/or "strength" versus each other, as well as versus a control condition. It is here that the researcher must also design a control condition that is procedurally and distributionally equivalent to intervention conditions to allow for fair comparisons with the intervention options (Cooper & Richardson, 1986). Some typical control options include a neutrally valenced activity (e.g., Lieberman et al., 2011), a decontextualized, static treatment or manipulation (Steinhart et al., 2019), or "provide nothing" (as is commonly done in health behavior research; Nahum-Shani et al., 2018). Researchers should however be aware that providing nothing as a control condition may introduce the potential for demand effects (Lonati et al., 2018), and thus researchers should con- sider including other control conditions as well or instead. To reduce demand effects, the control condition should be as similar as possible to the intervention condition in terms of activity type and duration.

A second set of decisions that must be made is, *how should vulnerability, opportunity, and receptivity be considered in adapting interventions?* When designing a JITAI, time-varying factors related to vulnerability, opportunity, and receptivity can be considered in sequence. That is (1) is the person currently susceptible to negative outcomes (vulnerability) or to positive changes (opportunity)? (2) If so, then would the person currently be receptive to receiving and using a specific intervention (receptivity)? If the answer is "yes" to both questions, intervention support should be provided. Although we propose this general guideline in considering states of vulnerability, opportunity, and receptivity, one of the main challenges for creating effective JITAIs is to identify these states. When preparing a JITAI, researchers can make decisions about the above states based on theories, prior empirical evidence, practical considerations, and data from pilot studies. Nahum-Shani et al. (2023) proposed analyzing intensive longitudinal data to answer questions about states of vulnerability, opportunity, and receptivity in JITAIs. For example, machine learning algorithms can be used to explore the extent to which a constellation of dynamic and static factors measured at time *t* predict the state of vulnerability (e.g., likelihood to smoke) at time $t + 1$ (Nahum-Shani et al., 2023).

Third, *when should decision points be planned, and how many decision points should be used?* Researchers must consider the timeframe along which the focal phenomenon is theorized to fluctuate (McCormick et al., 2020). For example, whereas boredom is a relatively frequent affective experience throughout workdays and is aversive but of generally low-grade severity (Chin et al., 2017), anger may be infrequent but severe (Gibson & Callister, 2010). The number and timing of decision points (as well as the "strength" of intervention options) depends on timeframe considerations such as these. Moreover, researchers may consider reducing the number of decision points over the course of the study, assuming that participants may benefit less over time from the same frequency of decision points as their regulatory skills increase and/or habituate to the intervention (Nahum-Shani et al., 2018).

Fourth, *when should proximal and distal outcomes be assessed?* The proximal outcome should be measured as close as possible to the intervention delivery to capture the maximum change in variance due to the manipulation (Mitchell & James, 2001; Nahum-Shani et al., 2018). Proximal outcomes may serve as mediators of the causal effect from the intervention to distal outcomes, with distal out- comes themselves being assessed near enough to the proximal outcomes to capture change due to the intervention as well as later in time to evaluate the potential "fadeout" of the intervention effect. Additionally, proximal outcomes and distal outcomes can exist at multiple levels of analysis and timeframes: A momentary emotional state, a postepisodic emotion, a day-level emotional experience, or a month of emotional experience. Proximal outcomes should by definition be nearer in time to the intervention than distal outcomes, but precise details may vary greatly.

Fifth, *how should intervention support be tailored?*[4] Tailoring variables may be time-varying, such that individuals who report an especially high or low level of a transient psychological state or task characteristic may be more or less likely to receive the intervention, and/or time-invariant, such that all individuals who score at or above a certain level of an individual difference variable or who experience a certain level of a work demand are more likely to receive a given intervention option. Availability is one such time-varying variable that has been highlighted in the health behavior literature (Nahum-Shani et al., 2018). Tailoring variables may be assessed actively through questionnaires at each decision point, or they may be assessed passively through sensor data (e.g., location- tracking, physiological data). For example, Bae et al. (2018) used a combination of passively assessed data via participants' smartphones (time, movement patterns, communications, and psycho- motor impairment) and surveys to build a model that was 90% accurate in predicting low versus high risk of binge drinking. Another consideration is whether tailoring should be accomplished manually or automatically. Reinforcement learning algorithms are one option that has been discussed in the JITAI literature for adaptive tailoring of decision rules at the momentary level using both self- reported and passively assessed sensor data (Goldstein et al., 2017; Gonul et al., 2019). However, manual tailoring is an option employed by many JITAI studies (Perski et al., 2022; Wang & Miller, 2020). This may be accomplished through collecting pilot data, measuring participants for a pretest period to derive individual decision rules for each person, or establishing separate between- person treatment conditions depending on responses in an initial baseline survey.[5]

Finally, *how should the JITAI be administered?* The JITAI approach allows for the integration of automation in many respects, but such automation is not a necessity. Broadly available survey plat- forms (e.g., Qualtrics) as well as emailing and text messaging systems may be used to randomize participants and set up intervention/survey delivery to minimize manual labor. More advanced features such as automatic adaptation and the integration of reinforcement learning algorithms may require more advanced technology, which may be actualized through mobile application development and by allying with application developers and/or human-computer interaction researchers. Researchers who plan to integrate passive assessment may consider utilizing devices such as wear- able trackers (see Chaffin et al., 2017, for a review). Furthermore, although this is considered best- practice for EMA studies in general (Fisher & To, 2012), we especially recommend that researchers carefully train participants on survey procedures and provide support throughout the study (e.g., scheduling online check-ins, creating a training website or fact sheet). This may reduce participant confusion and attrition while also providing opportunities to cultivate more cooperative relationships with participants that may benefit the study (Larson & Csikszentmihalyi, 1983).

In addition to the above key decisions, researchers should also consider (1) whether the JITAI should be deployed as the focal intervention or as an in situ “booster shot” to improve the efficacy of a static intervention (e.g., Demerouti et al., 2019), (2) the balancing of the cost of the JITAI versus its optimal design, and (3) whether intervention support should include exclusively “push” components designed by the researchers or should additionally include “pull” components that allow participants to initiate intervention support when they deem it necessary (Miri et al., 2019).

An Illustrative Example. We turn now to an illustrative example. Due to the prevalence, momentary nature, and impact of work-related emotions (Weiss & Cropanzano, 1996), the development of a JITAI for emotion regulation at work is of high theoretical and practical relevance to OBHR. In pre- paring this JITAI, we begin by grounding the intervention in AET (Beal, 2015; Weiss & Cropanzano, 1996), which emphasizes immediate and cumulative within-person event → emotion → behavior linkages and therefore orients the JITAI towards effectively harnessing and influencing these link- ages where and when they occur. Moreover, because our focus is the regulation of these elicited emotions, we draw on Gross’s (1998) process model of emotion regulation, which describes the different kinds of regulatory strategies as well as their temporal enactment in relation to the emotion itself. Finally, to incorporate context as an aspect of the JITAI preparation, we note that recent emotion regulation research has emphasized that one strategy may not always be “better” than another per se; instead, effective strategy use is likely to depend on individual differences, situational cues, goals, and the emotion itself (McRae et al., 2012; Scott et al., 2020). Thus, this preparation phase provides conceptual framing to operationalize the design elements of an emotion regulation JITAI.

We begin by selecting positive and negative emotions as proximal outcomes to be measured at the momentary level and job performance and well-being variables as distal outcomes to be measured at the end of the day, theorizing that immediate change in proximal outcomes will mediate and cumulate to longer-lasting change in distal outcomes. We can also examine these distal outcomes at various points after the intervention portion of the study has concluded, so as to test whether the intervention effect "fades out" over time (Westman & Eden, 1997).

To operationalize decision points, we can obtain participants' work schedules and the specific days/times when and the specific work locations in which participants report being in particularly positive and negative moods and/or performing particularly well or poorly. Alternatively, given the importance and frequency of employee performance episodes (Merlo et al., 2018), we can use an event-contingent design (Beal, 2015) to ask participants to self-report the beginnings and ends of performance episodes–and we can deliver the intervention at those times. We may also include a small number of random decision points throughout the day. This ensures that the JITAI is personalized and timely but is also able to account for unforeseen occurrences.

For intervention options, we may desire to provide a variety of regulatory strategies as well as compare the efficacy of such strategies, and so we may include both antecedent-focused strategies (e.g., situation selection and situation modification) and response-focused strategies (e.g., cognitive reappraisal, attentional deployment, and response modulation) to determine which strategy is most effective and when (Gross, 1998; Webb et al., 2012). As is common in emotion regulation research, an appropriate control condition may involve a neutrally valenced activity (Lieberman et al., 2011) or an instruction to regulate emotions "naturally" in response to the stimulus or situation of interest (Halperin et al., 2013). For tailoring variables, we may select trait neuroticism and/or trait affectivity (John & Gross, 2007) as time-invariant moderators as well as task characteristics (e.g., difficulty; Vahle-Hinz et al., 2021) as time-varying moderators, generating empirical evidence for their influence on intervention effectiveness as moderators (via the MRT) with which we may be able to create decision rules. Decision rules may already be derived in this stage based on literature review (e.g., meta-analytic evidence may suggest that an intervention be delivered at higher levels of task difficulty such as Mean + 1 SD), but this may be infeasible and therefore decision rules may instead be created following the MRT, which we discuss next.

Optimization Phase (MRT). JITAIs are complex interventions that rely on a number of design elements and potential levels of each variable, making the optimization phase a critical step (Collins, 2018). Indeed, in many cases, OBHR theory may be insufficiently rich (i.e., with respect to time and context) to ground the intervention, meaning that the JITAI may benefit inductively from insights in the data during this phase (Shipp & Cole, 2015; Spruijt-Metz & Nilsen, 2014). Thus, the primary aim of the optimization phase is to empirically validate the intervention's capacity to influence proximal outcomes and to do so by maximizing within-person internal validity.

The primary experimental vehicle by which this optimization phase is facilitated is the MRT, which is characterized by repeated within-person randomization of participants into intervention and control conditions at each decision point (Liao et al., 2016). Thus, the MRT enables causal inferences regarding the effectiveness of the JITAI by treating it as an exogenous rather than endogenous variable, providing evidence for the JITAI's capacity to influence proximal outcomes as well as the moderating effects of time-varying and time-invariant factors (Boruvka et al., 2018; Klasnja et al., 2015). Here, the researcher tests the JITAI by examining (1) whether the intervention has an effect on proximal outcomes and which intervention option is more or less effective, (2) whether the intervention changes in effectiveness over time (while accounting for autoregressive effects), and (3) which contextual tailoring variables are especially important in creating decision rules (i.e., for whom and when should the intervention be most effective; Walton & Wilson, 2018). Below, we illustrate how the MRT can address these issues.

The Design of MRTs. An MRT design typically involves three assessment components: baseline assessment, intervention delivery, and poststudy assessment. In the baseline assessment, researchers assess time-invariant factors (e.g., individual differences) and demographic variables. These time- invariant factors can be used as moderators in examining which tailoring variables are important for whom. Information collected in the baseline assessment can also be used to determine when individual participants are likely to be available or unavailable to receive the intervention.

The intervention delivery likely constitutes the bulk of the MRT design, and it is at each delivery of an intervention option (at a decision point) where participants are microrandomized at the within- person level. At each decision point, participants will be randomized into an intervention or a control condition, and they may also be required to respond to measures immediately before (e.g., to capture time-varying moderators and the preintervention level of the proximal outcomes) and after (e.g., to capture the postintervention level of the proximal outcomes). Thus, the intervention portion involves a within-person, pre-post, field experimental design. By measuring the proximal outcome right after the intervention options, the MRT may provide crucial construct validation of the experimental manipulation by the measurement of proximal outcomes (Chester & Lasko, 2021; Lonati et al., 2018).[6] It is important to note that distal outcomes are more so the focus of the RCT in the evaluation phase, but researchers may also consider including these outcomes in the MRT to evaluate them in an exploratory sense (e.g., including day-level outcomes). Finally, the MRT's focus on the within- person level also makes economical use of statistical power, requiring fewer participants and thus being less costly. Researchers may use the power analysis tool created by Liao et al. (2016) during the planning phase of the MRT.

In the poststudy portion, researchers can measure distal outcomes and ask participants to provide feedback on the intervention as a whole as well as on each intervention option. The success of a JITAI depends on its perceived usability and acceptability on the part of the participant (Steinhart et al., 2019). In this respect, it may be worthwhile for researchers to collect qualitative and quantitative data about participants' experiences with the intervention and participants' self-reported evaluation of its usefulness (Ben-Zeev et al., 2014; Hormuth, 1986).

Once data collection is complete, researchers may conduct a number of analyses to summarize the empirical evidence for the effectiveness of the intervention as well as to optimize the intervention.[7] First, by comparing the pre-post change in proximal outcomes between the intervention and the control conditions, researchers can examine whether the intervention as a whole has an effect on proximal outcomes. Moreover, by comparing the pre-post change in proximal outcomes between each intervention option, researchers can further examine which intervention option is more or less effective. If researchers find that one option is more effective than the rest, researchers can increase the probability of occurrence of that intervention option in the subsequent RCT in order to maximize the overall intervention effect. Second, by including time as a predictor in examining the change in proximal outcomes, researchers can explore whether the intervention changes in effectiveness over time. If the effectiveness of a particular intervention option decreases over the course of the daily intervention portion of the JITAI, researchers can consider reducing intervention support associated with that intervention option (vs. others) toward the end of JITAI. Third, researchers can examine the moderating effect of time-invariant factors (e.g., individual differences and chronic job situations) in the relationship between the intervention condition (as well as individual intervention options) and proximal outcomes. A significant moderating effect means that the intervention (or an intervention option) is particularly (in)effective for a certain group of participants, as defined by the moderator. Accordingly, when designing the subsequent RCT, researchers can customize the frequency of decision points or probability of a certain intervention option for the identified group (i.e., between-person customization). Similarly, researchers should test the moderating effect of the time-varying factors (e.g., time of day, task type, location, mood) in the relation- ship between the intervention condition (as well as individual intervention options) and proximal outcomes. By doing so, researchers can customize the intervention frequency or the probability of intervention options based on time-varying factors (i.e., within-person customization). Moreover, data from the poststudy evaluation may be useful to supplement optimization decisions and examine whether participant evaluations bear a similar pattern to empirical intervention effectiveness.

Although typical benchmarks of statistical and practical significance may be utilized when evaluating empirical findings (as described above), the researcher should consider adopting a decision- priority perspective (Collins, 2018). Here, the emphasis is on the practical ways in which the JITAI may be optimized and improved against several decisions: Which intervention options to include or exclude, whether decision points should be changed (e.g., differently timed, fewer or more decision points), whether tailoring should be done via states or traits, and so forth (Collins, 2018). Of related importance is that these decisions must be made with potentially imperfect evidence rather than waiting for the cumulative scientific process (Collins, 2018). Specifically, the researcher must decide which component to include using hypothesis testing but potentially with an altered Type I and/or Type II error rate, effect sizes, participant feedback or self-reported evaluations, and the theoretical relevance and real-world practicality of the component itself (Collins, 2018). This perspective is especially important to optimization for a subsequent RCT, given commonly noted issues of shrinkage in regression-based analyses (i.e., results in the MRT may not replicate at the same magnitude in the RCT due to overfitting to the MRT sample; Wherry, 1975). In sum, optimization decisions may be made based on several sources of information gleaned from an MRT, such as effect sizes, participant adherence and engagement, and tailoring variables (Klasnja et al., 2019; Qian et al., 2022a).

An Illustrative Example. We now turn once again to our illustrative example. With the design elements of the emotion regulation JITAI prepared, we may proceed to testing and subsequently optimizing it in an MRT. Consistent with the preparation phase, we may decide to structure the MRT around the performance episode as the focal decision point with proximal outcomes of emotions and episodic job performance. In the baseline assessment, then, we will measure time-invariant moderators of neuroticism and trait affectivity.

For intervention delivery, we will tie the decision point to participant-reported beginnings and endings of performance episodes, asking participants to report these and then randomizing every participant to the intervention or control condition at that time. We will set the randomization probability of intervention delivery at these decision points to 40:60 for the control and the intervention conditions, respectively, because we wish to maximize power but also to prevent participants from becoming fatigued with the intervention (Klasnja et al., 2019). Moreover, we will include four intervention options—one each for situation modification, reappraisal, attention deployment, and emotion suppression—hoping to provide variety to participants while also substantively comparing the effectiveness of each intervention option. The randomization probability for the options will be set at 15:15:15:15 to evenly distribute statistical power among them.

Furthermore, we will examine two “types” of decision points so as to determine which is most effective (i.e., to retain for the subsequent RCT): (1) Providing regulatory support at the reported end of an episode to aid participants in recovering from performance exertion and preserve emotional resources for subsequent performance (i.e., interepisodic), and (2) providing regulatory support after a random interval of time has elapsed since the reported beginning of an episode (e.g., 20–40 min) to help participants regulate themselves during the performance episode (i.e., intraepisodic). We will randomize which event-contingent performance episodes are treated as which type of decision point, and we will measure proximal outcomes of emotions and episodic performance at the reported end of the episode. Here, we will test whether the intervention (vs. the control) can influence proximal outcomes in general, and we will also compare the effect sizes of the intervention between decision points. In doing so, we may also test for (1) The overall effectiveness of the "suite" of intervention options (i.e., different emotion regulation strategies), (2) the relative effectiveness of each intervention option, and (3) changes in effective- ness over time. These tests may reveal whether, for the subsequent RCT, intervention support should only be provided at one type of decision point, whether less effective intervention options should be removed entirely or set at a lower randomization probability, whether more effective intervention options should be set at a higher randomization probability, and whether there should be fewer decision points and/or a lower randomization probability of the intervention condition or individual intervention options if effectiveness decreases over time (Walton et al., 2018). Each of these findings may allow for the intervention options and decision points to be optimized for the RCT.

Then, we may proceed to testing a number of research questions related to the customization of the JITAI and the decision rule that determines intervention delivery. The focal tailoring variables are time-invariant (i.e., neuroticism and affectivity measured at baseline) and time-varying (i.e., task characteristics measured at decision points). Moderation tests may reveal that interventions in the subsequent RCT should be delivered only, or at greater frequency, during performance episodes that are characterized by a particularly challenging task, or that the intervention is more effective for people who score higher in neuroticism. Moreover, we may find that there are differences in moderating effects for each intervention option, such that, during difficult tasks, situation modification is especially ineffective whereas reappraisal is especially effective. These findings may inform decision rules governing intervention delivery in the subsequent RCT.

Evaluation Phase (RCT). Finally, having established that the JITAI can influence proximal outcomes and having optimized it to do so, the third phase commences and involves the *evaluation* of the JITAI to influence distal outcomes at the between-person level in an RCT (Collins, 2018).

The Design of RCTs. It is the RCT experimental design, involving between-person randomization to intervention and control conditions as well as measurement of preintervention and postintervention estimates, which maximizes internal validity at the between-person level. The researcher may compare the JITAI against various control conditions, such as a no intervention, EMA-only condition or a comparable "standard" (static) intervention. This phase allows for testing the full behavioral model and examining whether the JITAI's influence on proximal outcomes—depending on certain levels of time-invariant and time-varying variables—ultimately changes the distal outcome of interest.

Whereas the MRT is intended to generate fundamental knowledge regarding the within-person mechanisms by which the intervention gives rise to changes in proximal outcomes while maximizing within-person internal validity, the RCT tests whether the JITAI demonstrates not only comparative effectiveness versus other intervention types and controls—thus maximizing between-person internal validity—but also whether the tailored JITAI condition demonstrates superior effects in an ecological field setting, thus maximizing external validity. In maximizing external validity, the RCT is able to demonstrate the scientific value of the JITAI as well as its practical value to the user. Indeed, one of the fundamental goals of JITAIs is to conclude with a tool that may improve organizational life through individual employees and/or through use as an HR in situ support tool (Antonakis, 2017). A well-conducted RCT demonstrates the external validity of the intervention and its capacity to achieve distal outcomes by targeting proximal ones, and it is the pairing of the MRT design with the RCT design that allows for maximum confidence in the utility and fruitfulness of a JITAI.

An Illustrative Example. Before concluding our discussion of the RCT, we turn once again to our illustrative example. Alongside the optimized JITAI condition, we will include three control conditions: the first without treatment at all and involving only event-contingent EMA measurement, the second with static intervention such as a one-time, half-day emotion regulation workshop paired with event-contingent EMA measurement, and the third with an alternative within-person intervention that instead targets attentional mechanisms (e.g., such as promoting interepisodic work breaks to promote recovery; Trougakos et al., 2008) by which the same proximal and distal outcomes might be achieved. In doing so, we intend to provide a strong and fair test of the optimized JITAI. Participants will be randomly assigned at the between-person level to the intervention condition or one of the three control conditions. Once we have collected the data, traditional hypothesis testing may be conducted to assess the effectiveness of the intervention versus these control conditions.

Quasiexperimental Designs

Although we have emphasized between-person (condition) random assignment via an RCT, it is important to note that this may be infeasible, as is often the case in OBHR research (Grant & Wall, 2009).[8] In such cases, we recommend that researchers consider adopting quasiexperimental methods and conducting a pseudo-RCT, such as utilizing propensity scores to statistically match participants who were most likely (based on responses to relevant measures) to be assigned to intervention or control condition and then only conducting comparative analyses between matched participants (Connelly et al., 2013). Moreover, researchers should also carefully consider statistical control variables for plausible alternative explanations (Shadish et al., 2002), through a method such as the hierarchical iterative control approach (Spector et al., 2019; though see also Carlson & Wu, 2012). Furthermore, the experimental control afforded by the MRT may help to offset concerns regarding internal validity in a subsequent quasiexperimental evaluation phase that replaces the RCT. However, if an MRT, too, is infeasible, then conducting a multitrial laboratory experiment to compare the effectiveness of each intervention and control condition may be a good alternative to the

MRT.

A Summary of the Potential Contributions of JITAIs for OBHR

Here, we summarize the areas where we believe JITAIs have the most potential impact for OBHR research. First, as noted above, JITAIs are particularly amenable to constructs involving within-person variance. As such, JITAIs can capture and capitalize on ephemeral and changing states in key organizational outcomes of interest in OBHR (e.g., emotions, cognitions, and job performance facets; McCormick et al., 2020; Podsakoff et al., 2019). In this sense, JITAIs align the levels of analysis and timeframes of research designs with existing theories that emphasize within-person dynamism. A misalignment between the temporal unfolding of a phenomenon and attempts to measure (and, here, change) it can result in erroneous conclusions (Mitchell & James, 2001).

Second, because a JITAI can involve a within-person field experimental design (i.e., the MRT), the use of JITAIs allows for drawing causal conclusions about relevant within-person organizational theories. Specifically, a JITAI involves (1) random assignment to intervention or control conditions both within persons via the MRT design, and between persons via the RCT design and (2) manipulation of the independent variables. In doing so, researchers can provide a more rigorous investigation of causal relationships in organizational theories.

Third, JITAIs can add nuance to within-person organizational theories by examining causal mechanisms. Capturing immediate, proximal outcomes (e.g., emotions) plays an important role in understanding the mechanisms through which the intervention works to influence distal outcomes (e.g., job performance).

Fourth, from a more applied perspective, JITAIs have the potential to improve the effective- ness of OBHR interventions for employee and organizational functioning. Specifically, JITAIs capitalize on the temporal processes of momentary psychological states and behaviors: States of vulnerability to adverse outcomes, opportunity to accept positive changes, and receptivity to receive, process, and utilize the intervention support. Moreover, JITAIs have a high level of mundane realism by capitalizing on natural, real-life situations to facilitate the transfer of training.

Literature Review on JITAIs in Health Behavior Research

Thus far, JITAIs have mainly been applied to health- or lifestyle-related behaviors (e.g., Hardeman et al., 2019; Smyth & Heron, 2016). To illustrate how JITAIs could be used in OBHR, we first review the use of JITAIs in health behavior research. Through this review, we demonstrate how participants' momentary personal and/or situational factors can be incorporated when designing JITAIs. These momentary factors play an important role in both health-related behaviors (e.g., urge to engage in unhealthy behavior) and organizational phenomena (e.g., intention to engage in CWB). Thus, we use this review as an opportunity to show what research still needs to be done in the OBHR context.

JITAIs in Health Behavior Research

Although JITAIs in health behavior research are themselves conducted in multiple domains (e.g., stress, anxiety, alcohol use, weight control, and sedentary lifestyle), they share some similarities in design features. For example, they include decision rules to determine when to initiate interventions and which intervention option to deliver. Relatedly, the decision rules are based either on time- varying (i.e., momentary) personal factors through self-report measures (e.g., scores on a stress scale) or on time-varying situational factors (e.g., proximity to a stressful location, monitored passively through GPS). In some JITAIs, participants can either receive an intervention option from a mobile application or self-initiate intervention (the equivalent of a "panic button"). To demonstrate these design features, we provide an example in health behavior research below.

In a JITAI designed to support sedentary adults in getting more activity throughout the day (Klasnja et al., 2019), participants received messages about walking or other antisedentary activities across five decision points each day for six weeks. At each decision point, HeartSteps (the intervention platform) delivered contextually tailored activity suggestions to participants. However, these suggestions were only delivered if participants were considered "available." HeartSteps determined availability using passive data (i.e., passively monitored by a device) from the smartphone-based accelerometer (vs. data actively reported by the individuals). Activity recognition algorithms were employed to identify unavailable moments for treatment, such as when the individual was driving, walking, running, or had just completed a physical activity within the last 90 seconds. Participants were also told they could access their activity graphs and suggestion history at will on the application. Step count data from the tracker showed that step counts on average increased by 14% in the 30 minutes after an activity suggestion (vs. no suggestion). This effect was not evenly distributed over the course of the study. There was a strong effect at the beginning of the study (step count increased by 66%). However, the effect diminished linearly over time at 2% per day during the 6-week study. Table 2 includes two more examples of JITAIs in the health behavior domain to demonstrate the key design elements of JITAIs.

Effectiveness of JITAIs in Health Behavior Research

Having shown how JITAIs are designed and implemented in health behavior research, we turn now to the effects of JITAIs on health outcomes. Typically, compared to conventional, static intervention designs, JITAIs have been shown to be feasible, usable, and acceptable to participants (e.g., Ben-Zeev et al., 2014; Pulantara et al., 2018). For example, Pulantara et al. (2018) developed a JITAI (iREST application) delivered via a mobile health platform to treat insomnia. Findings from in-application questionnaires indicated that participants rated the application as highly usable.

Meta-analyses have also shown that JITAIs are more effective than their non-JITAI counterparts in health behavior domains. Versluis et al. (2016) conducted a meta-analysis on the effects of JITAIs on mental health (e.g., depressive symptoms, anxiety, quality of life, stress, acceptance, and relaxation). Results indicated a medium to large average effect on mental health from preintervention to postintervention (Hedges' g = 0.73, 95% CI = [0.56, 0.90], $p < .001$) among within-subject studies (k = 33, n = 1,156). The average effect in between-subject studies (k = 13, n[JITAI condition] = 454, n[control condition] = 522) was considered small to medium (Hedges' g = 0.40, 95% CI = [0.22, 0.57], $p < .001$). In another review, Loo Gee et al. (2016) conducted a meta-analysis on the effect of JITAIs on anxiety. Results showed a small to medium difference between JITAIs and control conditions in reducing anxiety symptoms

(Cohen's $d = 0.32$, 95% $CI = [0.12, 0.53]$, $p = .002$, $k = 9$). Most recently, Wang and Miller (2020) conducted the most comprehensive meta-analysis of JITAIs across different health behavior domains (e.g., healthy diet, mental health, addiction, weight loss, and physical activity). Their results documented moderate to large effect sizes of JITAIs compared to waitlist-control conditions (Hedges' $g = 1.65$, 95% $CI = [0.72, 2.60]$, $k = 9$) and non-JITAI treatments (Hedges' $g = 0.87$, 95% $CI = [0.41, 1.32]$, $k = 21$) in between- subject studies. Moreover, the average effect size of JITAIs from preintervention to postintervention was considered medium to large (Hedges' $g = 0.79$, 95% $CI = [0.50, 1.08]$, $k = 13$). In sum, JITAIs have shown considerable promise in health behavior research.

However, the meta-analytic findings are based on primary studies with appreciable variation in research designs. To evaluate the quality of primary studies, we examined various design factors in the primary studies included in the health behavior JITAI meta-analyses: That is, whether preintervention baseline measures were included, whether control groups were included, whether an RCT design was used, whether the control groups were no-intervention or active control groups, and whether an MRT study was included. To contextualize JITAIs in organizational research, we further coded three OBHR intervention meta-analyses (Karabinski et al., 2021; Knight et al., 2017; Vanhove et al., 2016) that reported the above design factors in their primary studies, so as to compare the design (and the quality) of the primary studies meta-analyzed in the health behavior JITAIs to those of the primary studies in comparable OBHR intervention meta-analyses.[9] The direct comparison allows us to evaluate the extent to which (1) the health behavior JITAIs can be viewed as directly relevant to OBHR and (2) effect sizes from the health behavior JITAIs can be viewed as practically significant in an OBHR context.

In general, the primary studies in the health behavior JITAI meta-analyses are of good quality. Results showed that 40.98% of primary studies in the three health behavior JITAI meta-analyses included baseline estimates (vs. 46.94%[10] of the OBHR intervention primary studies in the three OBHR intervention meta-analyses, $z = -0.63$, $p = .529$); 70.49% of health behavior JITAIs included a control group (vs. 87.21% OBHR interventions, $z = -2.51$, $p = .012$); and 60.61%[11] of health behavior JITAIs adopted an

RCT design (vs. 37.21% OBHR interventions, $z = 2.30$, $p = .021$). Based on the results of z-tests for proportions across the three comparisons (one of which was nonsignificant, one of which favored OBHR research, and one of which favored health behavior research), we conclude that health behavior JITAIs do not differ substantially in quality or rigor from the status quo in organizational intervention design.

Among health behavior JITAI studies that included a control condition, 37.21% of the studies used a no-intervention control versus an attention control group (e.g., active treatment control, placebo control; 69.77%),[12] limiting the ability to control for demand effects. Of note, though, most health behavior JITAIs did not include an MRT design to (1) establish causal relationships or (2) inform the decisions for the follow-up RCT design. Thus, researchers should be cautious about the potential for endogeneity bias when interpreting the results of health behavior JITAIs. For example, without an MRT design, these JITAI studies may be vulnerable to treatment selection bias, a source of endogeneity bias. That is, if treatment varies on the basis of momentary or stable individual and/or contextual factors or individual choice, the treatment is not an exogenous manipulation (Hill et al., 2021). We note, however, that such endogeneity concerns are also prevalent in many OBHR intervention designs.

To summarize (1) meta-analyses in health behavior reveal JITAIs to be effective and (2) our comparison of design features from primary studies included in health behavior JITAI meta-analyses and primary studies included in comparable OBHR intervention meta-analyses suggests that the health behavior JITAIs are relevant to OBHR and that their results are practically significant in an OBHR context.

Differences Between JITAIs in OBHR and Health Behavior

Next, we review three aspects of the OBHR context that differ from a health behavior context. We note that, because there do not yet exist any JITAIs in organizational settings, the following discussion focuses on *potential* JITAI studies and overall design factors in OBHR.

First, whereas health behavior JITAIs may be largely designed for individual usage, OBHR JITAIs have the potential to operate at multiple levels of analysis: The individual employee level, to be sure, but also, potentially, the work team level and/or the organization level. Moments of vulnerability (to negative change) or opportunity (for positive change) may depend in large part on situational forces emanating from higher levels of analysis. Moreover, outcome variables of interest may exist at higher levels of analysis. For instance, at a large organization with multiple, geographically dispersed work units, upper management's concern about sexual harassment during social events such as each unit's annual holiday party may provide an opportunity for intervention, with unit-level sexual harassment complaints from the holiday party representing the proximal outcome variable and unit-level sexual harassment climate three months after the holiday party representing the distal outcome variable for the intervention.

Second, training is a systematic and important practice in many organizations (Aguinis & Kraiger, 2009), making the JITAI in an OBHR setting a promising supplement to existing training programs. Specifically, JITAIs could be attached to organizational training programs to augment traditional "classroom" training with time- and situation-specific "booster shots," thereby enhancing the transfer of training at the within-person level (Huang et al., 2017). For instance, traditional "classroom" training on how to repair shop-floor machinery could be supplemented by a JITAI, activated by a QR code on a specific machine, which provides a just-in-time refresher on how to repair that particular machine. This application of JITAIs is not completely novel to OBHR (rather, it is potentially relevant to all domains involving extensive training or education), but it represents an application highly relevant to OBHR and one where, given the dearth of such research, JITAIs in OBHR can provide an important "proof of concept."

Third, and most generally, the situational forces in organizational settings are likely to be stronger than those in many other life domains. Indeed, Davis-Blake and Pfeffer (1989) have argued convincingly that we should consider "organizations *as* strong situations" (emphasis added).

Moreover, it is perhaps no coincidence that recent research on situational strength is disproportionately situated in organizations (see, e.g., Dalal et al., 2020b; Keeler et al., 2019; Meyer et al., 2010). This suggests that there is both great promise in studying JITAIs in OBHR (e.g., due to the possibility of harnessing existing situational forces), but also great peril if appropriate care is not taken (e.g., due to the possibility of situational confounds that serve to either attenuate or artifactually amplify JITAI effects). These above features may serve to differentiate OBHR JITAIs from health behavior JITAIs.

Data Analysis for JITAIs

Although this paper focuses on the promise of JITAIs as a research design for the organizational sciences and practices, we nonetheless also briefly cover statistical issues of particular relevance to JITAIs in the MRT and RCT designs. Our description is aimed at providing high-level guidelines for OBHR researchers.

Microrandomized Trial

Data Structure. Given that the MRT is a within-person experimental design wherein the processes of change are tracked over time, one of the most salient characteristics of the data is a hierarchical structure wherein multiple intervention decision points and the corresponding preintervention and postintervention measurements are nested within each person. The data structure of the dataset contains an intervention variable (e.g., a categorical variable with 0 being a control condition, and 1 being an intervention condition), time-varying factors (e.g., location, time), and preintervention and postintervention outcomes in each row, along with participant identifiers and individual differences (also see Beal & Weiss, 2003, for a review of multilevel data structure).

Evaluation of Causal Effects. Causal effects in the MRT study can be tested with the causal excursion model (Boruvka et al., 2018; Qian et al., 2021, 2022a). The excursion effect is a contrast between the potential outcome under the intervention condition and the potential outcome under the control condition at each decision point for each individual (Cohn et al., 2023; Dempsey et al., 2020). Whereas mixed-effect models (Laird & Ware, 1982; Raudenbush & Bryk, 2002) are most commonly used for modeling the time-varying *association* between two or more variables in EMA studies, such models may result in biased causal effect estimates when there are time-varying, endogenous covariates (Qian et al., 2021). This is the case in an MRT study, where the intervention effect at a certain time point is dependent on one's history in the entire study process such as previous outcomes or treatments (Qian et al., 2021; Shi et al., 2022). By averaging and controlling for all previous intervention/control effects when comparing the difference between the intervention and control conditions at a given time point, the causal excursion model provides a more accurate estimate of the causal effect (Dempsey et al., 2020; Qian et al., 2021).

The causal excursion model estimates causal effects using the weighted and centered least-squares (WCLS) estimator (Boruvka et al., 2018) for continuous time-varying outcomes. This estimation method includes centering the intervention indicator(s) on its randomization probability and weighting the estimating function using the inverse probability weighting (Boruvka et al., 2018).[13] Centering is not necessary for the rest of the variables. By centering on the randomization probability, the WCLS estimator makes the model robust against model misspecification (Boruvka et al., 2018).[14] Moreover, the WCLS estimator is similar to multilevel models in that it takes into account the nested nature of the data and autoregression in outcomes (Klasnja et al., 2019). For binary outcomes, researchers should use the estimator of the marginal excursion effect (Qian et al., 2021). For a more thorough description of the causal excursion model, see Qian et al. (2021, 2022a, 2022b).

The first analysis using the causal excursion model with the WCLS estimator is to examine the average causal effect across all decision points and covariates (Qian et al., 2022a). Secondly, this causal excursion model allows for further modeling moderation effects by including moderators (e.g., time, day, location, emotions) and the interaction between the intervention variable and moderators in the model (see Qian et al., 2022a, for example moderation models). The moderating effects shed light on the tailoring of the JITAI, such that a significant moderation effect suggests that the intervention program or a certain intervention option is more or less effective in certain situations. Thirdly, the causal excursion model can examine not only the immediate effect on the proximal outcomes but also lagged effects (Boruvka et al., 2018), so as to examine the carry-over or delayed effect of the intervention.

Randomized Controlled Trial

Although the RCT design is a between-person design, researchers need to consider the potential hierarchical structure of the dataset when the focal outcomes are the proximal outcomes right after each intervention session. This is because the multiple intervention supports and repeated measures of preintervention and postintervention outcomes are nested within each person (or the unit of interest), as described in the design section. In this case, multilevel models should be adopted to account for the nested nature of data (McNeish & Kelley, 2019; Raudenbush & Bryk, 2002). Failure to correctly model the unobserved variation due to the hierarchical structure may result in endogeneity bias (Antonakis et al., 2021; Bliese et al., 2020). Specifically, the assumption in random-effect models that the random intercept is uncorrelated with the regressors is often violated (Antonakis et al., 2021), which may lead to biased estimates. This is the case in EMA studies (though rarely acknowledged), and it is the case in JITAI RCTs as well. We recommend that researchers use the correlated random effects approach (CRE; Mundlak, 1978), which can relax the assumption by adding the group mean(s) of the Level-1 regressor(s) in the regression model. Simulation studies showed that CRE approaches outperform the random effects approaches with group-mean centering on the Level-1 variables and are more efficient in small samples (Antonakis et al., 2021). Thus, we recommend using the CRE approach as the default in analyzing the multilevel data from the RCT study.

Centering (either grand-mean or group-mean centering) is not necessary for analyzing the RCT data. CRE approaches can be viewed as an alternative to group-mean centering in the fixed-effect model because they can accomplish the same function of producing a within-person effect free of endogeneity bias while also providing more flexibility in modeling (Antonakis et al., 2021). Grand-mean centering may lead to biased estimates (Antonakis et al., 2021); thus, grand-mean centering Level-2 variables should be avoided.

To capture the difference in outcome changes between groups (i.e., the residualized change), researchers can regress the postintervention outcome at the decision-point level (Level 1) onto the pre- intervention measurement of the outcome at the decision-point level (Level 1) and its group (here: Person) mean (Level 2), as well as the condition (e.g., intervention vs. control) membership at the between-person level (Level 2). Time-invariant factors can be entered into the equation at the between- person level, whereas time-varying factors can be entered at the within-person level (as in the multilevel model used in EMA studies). Because an extensive discussion on multilevel modeling is beyond the scope of this paper, readers should refer to recent literature on the topic (Antonakis et al., 2021; González-Romá & Hernández, 2022).

When the focal outcomes are measured at and/or after the end of the entire intervention program (e.g., stress level measured at the end of the intervention portion of the study and then again three months later) or are best operationalized at an aggregated level (e.g., frequency of stressors reported or total instances of CWB over the entire study period; i.e., Level 2 outcomes only), independent- samples *t*-tests (if there are two groups), analysis of variance (if there are two or more groups) or regression (e.g., means-as-outcomes regression; Raudenbush & Bryk, 2002) can be used to compare the outcomes between groups.

General Discussion

Methodological Considerations

We have delineated the advantages of JITAIs, provided a selective review of the JITAI literature in non-OBHR areas (e.g., health behavior research), provided examples of what JITAIs might look like in OBHR, and advocated that OBHR researchers should adopt the JITAI design. However, no method is without its limitations. Here, we discuss methodological aspects broadly related to validity threats that must be considered when designing a JITAI.

Threats to Validity: Endogeneity, Demand Effects, Selection Bias. First, depending on the particulars of the JITAI design used, endogeneity bias may or may not remain a concern in inferring causal relationships. Endogeneity occurs when a predictor correlates with the unexplained residual of the outcome in a predictive model (Hill et al., 2021). MRT designs (or multitrial laboratory experiments) that involve within-person microrandomization and experimental manipulation are particularly helpful in mitigating endogeneity bias. However, when random assignment is not feasible, the intervention condition is not truly exogenous (i.e., a real exogenous manipulation) in the relationship with the outcomes because some omitted variables may influence the assignment of the intervention condition (e.g., Antonakis et al., 2010, 2014; Podsakoff & Podsakoff, 2019). For example, in an intervention condition of an RCT design, the within-person customization on the basis of participant vulnerability, opportunity, and receptivity is likely to result in increased effect sizes associated with the RCT. However, it also generates treatment selection bias, a source of endogeneity bias (Hill et al., 2021). Thus, customization comes with intended positive consequences but also unintended negative consequences.

We therefore provide some solutions to address endogeneity bias in cases where an MRT (or multi trial laboratory experiment) using random assignment is not possible. Prior to turning to the solutions, however, we note in passing that, although our discussion naturally focuses on JITAIs, endogeneity bias is even more of a problem in EMA studies than JITAIs.

One solution is to use instrumental variable designs to correct for endogeneity in the relationships between intervention and outcomes by leveraging an instrumental variable to which participants are randomly assigned or "as-if" randomly assigned. As-if random assignment is the assignment process that is independent of factors related to the outcome or that is unaffected by self-selection into intervention or control conditions (Dunning, 2012; Sieweke & Santoni, 2020). The instrumental variables should (1) be exogenous (uncorrelated with other causes of the outcomes except for the intervention), (2) influence the assignment of the intervention, and (3) have no relationship with the outcome except through the intervention. Moreover, the regression discontinuity design can be used when the selection of intervention is determined by a cutoff or threshold in a continuous variable (e.g., + 1 SD above the mean level of negative emotion; Calonico et al., 2019; Hill et al., 2021). Specifically, this method assumes that the observations just below and just above the cutoff scores have similar scores on the omitted variables. However, these observations are categorized by the decision rule as being in an intervention group or not, based on falling below or above the cutoff scores. Thus, the observations around the cutoff score can be viewed as random assignments to the intervention or control condition. Accordingly, researchers can test the effect of the intervention versus the control condition (or no intervention condition) on the outcomes among these observations as if in a true randomized design.

Second, concerns over reactivity and demand effects are endemic to within-person EMA studies and traditional between-person experimental studies (Barta et al., 2012; Beal, 2015; Shadish et al., 2002). As such, these effects must also be considered in the context of a within-person experiment such as a JITAI. For example, including manipulation-check questions and repeated measures of proximal outcomes in a JITAI may cause demand effects. In this regard, an MRT (or, failing that, a multitrial laboratory experiment) may be especially useful as a pilot study to test the effectiveness of the intervention to mitigate concerns over demand effects (Eckerd et al., 2021). That is, by testing the intervention at the within-person level, including the same measures and procedures in the intervention and control conditions, and by maximizing internal validity through microrandomization, the researcher may have greater confidence that there is a causal effect of the intervention. Beyond the MRT, there are a number of options that can be used to mitigate reactivity, for instance, including a control condition that is similar to the intervention options in terms of key aspects of design (e.g., duration, activity type), providing a greater variety of intervention options that also vary in strength, changing the randomization probability to provide certain intervention options or the control condition(s) more frequently, and/or reducing the number of decision points overall or over the duration of the study itself (Nahum-Shani et al., 2018). Researchers should also minimize the transparency of the JITAI's objective and/or measure and control for trait social desirability (Eckerd et al., 2021).

In addition, in the RCT, it may be useful to design between-person control groups (i.e., conditions) that explicitly allow for comparisons with respect to demand effects, such as an ESM-only control group, a control group that receives random intervention or control options (vs. based on tailoring variables), a control group that receives the JITAI intervention but over a briefer period of time, a control group that only receives "standard care," and so forth. Underscore the importance of matching demand strength across treatment conditions as well as of incorporating objective or non-self-report outcome measures to reduce demand effects. Finally, if an MRT and/or random assignment in the RCT is infeasible, researchers may consider utilizing propensity scores as dis- cussed in the quasiexperimental design literature (Connelly et al., 2013).

Third, although the microrandomization of intervention options at each decision point in the MRT design eliminates selection bias at the within-person level, it is possible that the high degree of personalization and adaptation inherent to the JITAI design results in the confound of treatment selection bias (Hill et al., 2021). We therefore echo the recommendation from Nahum-Shani et al. (2018) that “push” intervention options may be preferable in general versus “pull” intervention options because participants may not be fully aware of when they may benefit most from an intervention, and this may help to reduce concerns over selection bias. If a “pull” intervention is included in a JITAI, the researcher may need to first test the intervention as a corresponding “push” option to provide strong evidence that selection bias is not a concern (Klasnja et al., 2015). It is also true that, in some JITAIs with “pull” interventions, there is still a suite of “push” interventions to which participants are randomly assigned. In this case, treatment selection bias is not a concern. Additionally, a researcher may randomize intervention delivery to test time-varying moderators that may serve as tailoring variables (Collins, 2018). However, if an MRT cannot be conducted, JITAI researchers seeking to customize the intervention may be well-served by establishing three comparison groups: The fully customized JITAI group, the uncustomized within-person intervention group (i.e., intervention but no personalization with time-varying variables), and the control group (e.g., EMA-only, or “standard care”). Thus, in general we recommend that researchers seeking to personalize interventions do so incrementally and through efficiently utilizing microrandomization where possible. Moreover, researchers should report the results with caution when an MRT is not feasible.

We recommend that researchers report the effectiveness of JITAIs in the RCT study with additional information about selection options for each participant (i.e., “if…then…” decision rules).

Practical Issues. Finally, there are a number of practical issues that researchers must consider in implementing JITAIs. One issue is maintaining participant compliance and mitigating and addressing attrition over the course of the study. A lack of participant adherence to the intervention is a critical barrier to implementing intervention programs and achieving behavioral changes (e.g., Lemstra et al., 2016). The JITAI is a generally intensive method and presents a number of barriers to compliance. We therefore discuss solutions, some of which are extensions of solutions discussed in the EMA literature (given that EMA is also an intensive method, albeit less so than the JITAI). First, measures should be taken to mitigate participant burden in the design of the intervention (e.g., design brief interventions, minimize the required number of items and measures or the required number of decision points; Gabriel et al., 2019; Nahum-Shani et al., 2018), and the researcher should also ensure that a cooperative relationship is cultivated with participants and that there is sufficient clarity in terms of participation expectations (Larson & Csikszentmihalyi, 1983). Second, organizations may be resistant to JITAIs being tested on their employees, so trusting and cooperative relationships must be cultivated not just with individual participants but also with the organizations in which they are employed (e.g., van Roekel et al., 2019). Feedback reports may be offered (with anonymized and aggregated data) to provide useful information to organizations and to incentivize their agreement to participate. Third, the more detailed, within-person data required by some JITAIs (e.g., physiological data, temporal patterns of location and activity) are likely to raise concerns about employee privacy (Bhave et al., 2020). Because the “privacy calculus” of employees is highly con- textually determined (Bhave et al., 2020), future research could examine how best to balance participant privacy with externally planned design features (Nahum-Shani et al., 2018) within different work contexts. These methods may help to maintain compliance and decrease attrition.

The issue of missing data is endemic to EMA research (Gabriel et al., 2019) and has received attention in the organizational literature (Newman, 2014), but in an experimental design attrition —especially for nonrandom reasons—is a threat to internal validity that must be carefully considered throughout the developmental phases of a JITAI. In the MRT, researchers may consider explicitly testing for missingness as a reflection of fatigue or boredom with the intervention (Nahum-Shani et al., 2018). Including questions in the poststudy evaluation to assess user experience can provide insights as well. In both the MRT and RCT, attrition should be analyzed (e.g., comparing rates between treatment and control, comparing attrition between different decision points, and testing for patterns; Shadish et al., 2002). Moreover, analyses may be conducted using the instrumental variable approach described above (Sieweke & Santoni, 2020), and intention-to-treat analyses (Gupta, 2011) may also be conducted in the RCT.

An issue related to missing data is sample selection bias. When the observed data do not represent the full population of interest (e.g., individuals self-select into a study, as is generally the case in OBHR research in general, let alone EMA studies and JITAIs in particular), sample selection bias occurs, and the full range of the outcome is not available. If unmeasured variables affect the probability of selection into a sample, the study has an endogeneity concern. For example, in an OBHR context, even when employees within an organization are randomly assigned to an intervention condition and a control condition, the employees have all been selected into that organization. Sample selection bias is also an issue when certain participants are more responsive than others due to non- random reasons (e.g., differing levels of conscientiousness; Hill et al., 2021). Thus, researchers can estimate how the intervention affects the job outcomes only for those employees and responses the researchers can observe. Sample selection bias should be addressed once more through appropriate research design, first and foremost, but also through data-analytic means, such as the Heckman selection model (Certo et al., 2016; Hill et al., 2021) and statistical weighting corrections (Cortes et al., 2008). Furthermore, if sample selection bias is a concern in JITAI studies, researchers should be cautious when generalizing the results to the population.

Conclusion

The extensively documented within-person variability in many OBHR-relevant constructs has yet to be harnessed effectively by dynamic interventions in the OBHR literature. In the current paper, we presented the JITAI as an intervention framework well-suited to this purpose, reviewed JITAI research on OBHR-adjacent topics in the health behavior literature and discussed methodological features of JITAIs from both descriptive and prescriptive perspectives.

In an effort to be thorough, we have introduced several considerations in the current paper. However, we would also be remiss not to emphasize that a JITAI is in many ways simply an extension of an EMA design. Researchers familiar with EMA research will find JITAIs quite straightforward to understand and use.

In the final analysis, well-designed JITAIs exhibit considerable potential as interventions that improve performance and well-being in workplace settings while simultaneously providing causal tests of OBHR theories of the antecedents of performance and well-being. Stated differently, well- designed JITAIs occupy the coveted “Pasteur’s Quadrant” (Stokes, 1997), that is, they are highly important not just to practical application in organizations but also, simultaneously, to fundamental scientific knowledge. Consequently, in our view, OBHR researchers and practitioners would be well advised to incorporate JITAIs into their repertoire.

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Supplemental Material

Supplemental material for this article is available online.

Notes

1. Shim et al. (2021) is a recent illustration of the application of ecological momentary interventions to the domain of positive psychology. That paper focuses on the practical application vs. theoretical aspects of ecological momentary interventions. For example, it does not mention the MRT or discuss how ecological momentary interventions have the potential to test causal claims. Thus, the scope and focus of the Shim et al. (2021) paper are different from those of our review.

2. Given that the literature review focuses on dynamic, within-person experimental manipulation, the key- words (e.g., within-person, dynamic, intraindividual) associated with the search almost certainly yielded OBHR experimental research with a disproportionately high level of dynamism and customization. Thus, the reported statistics are almost certainly conservative, such that actual dynamism (and customization) in OBHR is lower than estimated here.

3. We acknowledge that such a sequential design process may be infeasible in some applications of the JITAI technique in OBHR contexts.

4. A JITAI need not be tailored extensively or at all if tailoring is infeasible or not of focal interest. Instead, the influence of such tailoring variables on proximal outcomes and/or intervention effectiveness may be assessed empirically, through moderator tests, to provide empirical evidence for future research. In other words, rather than delivering interventions only at certain levels (e.g., high or low) of the tailoring variables, the intervention delivery could be made non-contingent on these variables and the effect of the intervention could subsequently be examined for differences in effectiveness as a function of the levels (e.g., high vs. low) of these variables.

5. It is important to note that our discussion here assumes that the researcher is the primary agent in determining decision rules and which participant receives which intervention at what time. This is to prevent the biasing of results due to selection of treatment (Shadish et al., 2002) as well as to ensure that treatment does not rely on participants' perceptions, which may be inaccurate (Steinhart et al., 2019). If "pull"

intervention options (i.e., those initiated by the participants themselves) are desirable, researchers may first test equivalent “push” intervention options (i.e., those initiated by the researchers) in an MRT and then adapt them to the corresponding “pull” options in the RCT. Moreover, in the case of “pull” interventions, although the individual is initiating interventions for themselves, within-person randomization may still be utilized to

randomly assign the participant to one of several intervention options to ensure internal validity.

6. If researchers are concerned that repeated measurement of proximal outcomes at every decision point may exert a demand effect on participants (Lonati et al., 2018), we recommend randomizing which proximal outcome is measured at which decision point to reduce potential habituation.

7. We note that MRTs are a possible and desirable, though not necessary, precursor to RCTs. In some cases, intervention support may be relatively straightforward and require little optimization or piloting: JITAI booster shots appended to standard interventions or trainings, adaptation of previously validated materials (e.g., at the between-person level) to the within-person level, incorporation of minimal or no tailoring variables, and/or the availability of thorough and time-sensitive theorizing or empirical data. Moreover, if the primary interest of the researcher is at the level of the intervention as a whole (vs. a control or an alternative treatment) rather than the level of the individual intervention options (vs. a control or other intervention options), then evaluation of the JITAI with the RCT design may be prioritized. However, when the researcher is developing a novel JITAI and a full-fledged MRT is infeasible (e.g., time, finances, participants), we recommend piloting and relying on subject matter experts to maximize the likelihood that the intervention will work as intended, as well as including manipulation checks in the RCT.

8. It is likely that there will be lower barriers to randomization in the MRT, given the within-person nature of intervention and control conditions: each person in the MRT will receive the intervention at least some of the time.

9. Because there do not yet exist any JITAIs in organizational settings, the OBHR interventions were traditional interventions versus JITAIs.

10. This percentage (46.94%, or 23 of 49) is based on two of the three OBHR

meta-analyses because the third OBHR meta-analysis, Vanhove et al. (2016), did not report information about baseline measures. The other percentages reported are based on the primary studies from all three OBHR meta-analyses (*k* = 86).

11. This percentage (60.61%, or 20 of 33) is based on two of the three health behavior JITAI meta-analyses because the third health behavior meta-analysis, Wang and Miller (2020), did not report any information about the use of an RCT design. The other percentages reported are based on the primary studies from all three health behavior JITAI meta-analyses (*k* = 61).

12. Four studies included both a no-intervention control group and an attention control group, and thus they were counted in both categories, leading to the overall percentage of the two categories being over 100%. Another two primary studies were coded as including an unspecified control group, and thus they were not counted into either category.

13. When the randomization probability is constant across all the decision points in an MRT (which is the case for most MRT studies), researchers can set weight as the availability of receiving an intervention at each decision point (1=available, 0=not available). If all individuals are available at all the decision points, researchers can set weight equal to 1 for all the decision points.

14. Note that when the randomization probability is constant over time and across individuals, centering is not necessary for providing robustness.

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