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In search of missing time: A review of the study of time in leadership research

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ABSTRACT

Many studies describe leadership as a dynamic process. However, few examine the passage of time as a critical dimension of that dynamism. This article illuminates this knowledge gap by conducting a systematic review of empirical studies on temporal effects of leadership to identify if and how time has been considered as a factor. After synthesizing key findings from the review, the article discusses methodological implications. We propose that a computational science approach, particularly agent-based modeling, is a fruitful path for future leadership research. This article contributes to leadership scholarship by shedding light on a missing variable (time) and offering a novel way to investigate the temporal, dynamic, emergent, and recursive aspects of leadership. We demonstrate the usefulness of agent-based modeling with an example of leader-member exchange relationship development.

Introduction

As the scientific study of leadership evolves, the concept of time is increasingly discussed as a variable of interest. Scholars recognize that time plays a vital yet poorly studied role in the process of leadership. It takes time to become a leader, to enact leadership, and to be perceived by others as a leader (Day, 2014; Shamir, 2011). A number of leadership constructs (e.g., leader behavior, leader development, leader emergence, leader-follower relationships) involve temporal considerations. Examples include events (Ballinger & Rockmann, 2010), ordering (Casimir, 2001), time lags (Day, 2014), and proximal/distal outcomes (Day & Dragoni, 2015). Such temporal aspects reflect the processual nature of change and development associated with leadership (Gollub & Reichardt, 1987).

While many studies describe leadership as a dynamic process, few investigate with specificity the passage of time as a critical dimension of that dynamism (Bluedorn & Jaussi, 2008; Day, 2014; Shamir, 2011). This lack of consideration is reflected in both conceptual and methodological shortcomings of current leadership studies. In his theoretical paper, Shamir argued that "most empirical studies of leadership, including longitudinal field studies, [did] not contain much information about the effects of time on leadership phenomena" (2011, p. 307) and that leadership theories did not specify the time it would take for leader characteristics to have an effect on outcomes. Similarly, Kozlowski, Watola, Nowakowski, Kim, and Botero (2009) posited that even though current leadership theories captured process-like functions such as planning, organizing, monitoring, and acting, these functions were static in nature because the effects of leadership were not theorized to change over time. This gap is important because without addressing these temporal effects, we have few answers to questions such as when leader characteristics and behaviors can have an effect on follower attributes and organizational performance, whether perceptions of leaders are stable or how they change over time, how leader-member exchange relationships are developed and maintained, or how leaders themselves change and develop (Day, 2014).

Methodologically, the majority of leadership studies have been static, cross-sectional, and heavily rely on survey data (Dinh et al., 2014; Dulebohn, Bommer, Liden, Brouer, & Ferris, 2012; Kozlowski et al., 2009). In Dinh et al.'s (2014) content analysis, the authors found that among the 752 leadership articles published in core journals between 2000 and September 2012, the vast majority (74%) of theoretical research stressed compilation forms of emergence—"a fundamental change in qualities and functions of the sub-unit as aggregation from lower to higher levels occurs" (Dinh et al., 2014, p. 43). However, empirical studies utilizing compilational emergence only accounted for 27% of all quantitative research. They attributed this misalignment to researchers' failure to attend to important effects that time has on leadership and organizations, as well as failure to adopt research methods that better align with theory. However, even in longitudinal studies that do consider temporal effects, data are usually collected over two or three points in time to cover a time period of less than a year (Dulebohn et al., 2012). While longitudinal design helps to assess if and how much

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change has occurred, it does not contribute to theoretical advancement on the impact of time on leadership phenomena (Shamir, 2011). Longitudinal studies also fail to account for emergent phenomena that may arise through repeated interactions over time and risk type I and type II errors. Type I errors occur when too few (or insufficiently spaced) measures suggest a pattern that, when data are viewed over a longer time frame, reveal a much different pattern. Type II errors result when a study concludes no change occurred when in fact it did, however, a longer time scale was required to recognize it (Day, 2014).

In this review paper, we build on prior reviews (Bluedorn & Jaussi, 2008; Day, 2014; Fischer, Dietz, & Antonakis, 2017; George & Jones, 2000; Mitchell & James, 2001; Shamir, 2011; Shipp & Cole, 2015; Zaheer, Albert, & Zaheer, 1999) to systematically address the knowledge gap about the role of time in leadership. For example, we extend Shipp and Cole (2015) by considering methodological issues concerning the study of time in micro organizational research. We also build on Fischer et al.' (2017) discussion of leadership processes as a cause-mediator-effect logic, arguing that mediation studies represent only one way of studying temporal effects and that not every mediation study actually captures the flow of time (as supported by the authors' finding that only a third of quantitative-empirical studies included time lags). Our review also suggests that traditional statistical methods may constrain the field by imposing linear and variable-based ways of thinking, which are better suited for some kinds of research questions than others. As such, we review a smaller sample of leadership studies and address a methodological gap in the extant literature. Our review contributes to the leadership literature by drawing attention to the different areas of time that have been well- or not well-studied, as well as by shifting the focus of research design assumptions away from linear and to nonlinear, emergent thinking. We conclude by proposing a relatively new methodological approach (agent-based modeling) to overcome limitations of existing methodologies.

Systematic review and coding

Our review began with a search for original empirical leadership studies in the top management and psychology journals. Consulting Table 1 in Gardner, Lowe, Moss, Mahoney, and Cogliser' (2010) review, we selected *The Academy of Management Journal*, *Administrative Science Quarterly*, *Journal of Applied Psychology*, *Personnel Psychology, Journal of Leadership and Organizational Studies*, *Journal of Management*, *Journal of Management Studies*, *Journal of Organizational Behavior*, *Leadership*, *Leadership and Organization Development*, *Leadership Quarterly*, *Organization Science*, *Organization Studies*, *Organizational Behavior and Human Decision Processes*, and *Strategic Management Journal*. We excluded *The Academy of Management Review* because it does not publish articles with data. We also included the *Journal of Public Administration Review and Theory* to ensure that studies from all sectors (public, private, nonprofit) would be included. To identify studies that empirically capture the effects of time, we searched for *lead** as well as one of the following keywords in the title of the article: *chang**, *emerg**, *dynamic**, *time*, *temporal*, and *longitudinal*. To ensure that the studies contain data and analyses, we also searched if at least one of the following keywords was present anywhere in the article: *design*, *method**, *sample*, and *analy**. We limited our results to articles published in or before December 2016. This search yielded 122 results.

To further confirm that the articles we found were relevant to the purpose of this review, the two coauthors independently read the abstracts of the 122 articles and coded whether each article captured any kind of temporal effect of leadership. Expected agreement due to chance between the two coders was 52.58%; the authors agreed 96.72% of the time. Cohen's *Kappa* was higher than the commonly accepted threshold of 0.80 ($\kappa = 0.93$, $S.E. = 0.03$, $T = 10.31$, $p < 0.001$), suggesting that this agreement was substantially better than chance. The most common reasons for exclusion of articles were that they were either theoretical in nature or did not capture temporal effects. For example, many research studies on organizational change, transformational leadership, or the role of leadership during change initiatives contained the search keywords but did not examine the effects of time. Others measured independent variables, mediators, and dependent variables at the same time point. We then discussed and resolved differences in coding and proceeded to review the resulting 45 articles. Two of the papers each conducted two studies, bringing the total number of studies reviewed to 47. The reviewed articles are indicated with an asterisk in the References section.

We performed a content analysis following the process reported by Gardner et al. (2010) and Dinh et al. (2014). The first author and four undergraduate students independently coded these articles by journal

Table 1
Summary of content analysis.

Total number of studies	47	(45 articles, two of which each conducted two studies)				
(some categories below add up to more or < 47, e.g., articles using multiple analytical methods, addressing multiple units of analysis, etc.)						
Types of studies	Longitudinal Real time	Longitudinal Archival				
	43	4				
Time range	< =4 weeks	> 4 to < 12 weeks	12 to < 24 weeks	6–12 months	> 12–24 months	> 24 months
	7	8	8	3	6	15
Data collection Frequency	Two-wave	Three-wave	Four-wave	> 4		
	21	9	3	14		
Monge's typology	Continuity	Magnitude	Rate of change	Trend	Periodicity	Duration
Stated	1	1	0	2	0	0
Implied	6	0	1	20	0	0
Other temporal Dimensions	Pattern	Trajectory	Rhythm	Cycle	Oscillation	
	1	4	0	1	0	
Theory-based time lags	Yes	No				
	6	37				
Time conceptualized as	Focal Construct	Medium				
	1	46				
Analytical method	HLM	Growth curve	Other quantitative	Qualitative		
	21	3	19	14		
Stability assumptions Addressed	Yes	No				
	9	36				
Unit of analysis	Individual	Group	Organization			
	34	32	6			

name, year of publication, title, keywords (if available), authors, abstract, type of article, data collection timing, research method, analytical method, leadership theory categorization, level of analysis, form of emergence, type of model (independence, dependence, interdependence, hybrid),[1] theory and design match/mismatch, limiting assumptions, and effects of time using Monge's (1990) *Typology of Analytical Alternatives for Longitudinal to Static Research Designs*. Specifically, we examined whether time was addressed in terms of continuity, magnitude, rate of change, trend, periodicity, and duration. *Continuity* considers whether the variable has a consistent nonzero value, meaning that it exists in some form at all times (e.g., organizational climate, Joyce & Slocum, 1984). *Magnitude* describes the amount of the variable at each given point in time. *Rate of change* refers to how fast per unit of time the magnitude decreases or increases. *Trend* describes the long-term increase or decrease in a variable's magnitude; it can have either a positive or negative value. *Periodicity* refers to the amount of time between the repetition of the variable's values; there is no period without this regular repetition. *Duration*, associated with discontinuous time variables, measures the length of time the variable is at a non-zero value; a variable's duration may change over time (Monge, 1990). These six aspects of temporality should be addressed when building theory about a dynamic phenomenon (Monge, 1990). Agreement among the five coders was 92%. Coders reconciled discrepancies and the first author developed summary tables outlining the results of this content analysis.

Results of content analysis

The 45 articles reviewed covered 47 empirical studies. Below we describe findings for each dimension of our analysis. Table 1 presents numerical counts for each element.

Conceptualizations of time

The construct of time can mean different things to different people. In academic research, two predominant interpretations of time are standard clock time (Clark, 1985; Gurvitch, 1964; Mitchell & James, 2001) and subjective/psychological time (Mitchell & James, 2001). In our analysis, all 47 studies interpreted time as standard clock time (either explicitly or implicitly). This means that temporality was conceived as measurable, dividable (e.g., days, hours, minutes), homogeneous (each moment is the same), and moving from past to future (Shipp & Cole, 2015). None of the studies conceptualized time subjectively, i.e., as varying by individual or culture, moving multi-directionally (e.g., future to past), or passing at different rates for different people (Bluedorn, Kaufman, & Lane, 1992a; Mosakowski & Earley, 2000; Shipp & Cole, 2015; Shipp & Fried, 2014). This conceptualization of time as objective and clock-like mirrors previous analytical findings (e.g., Shipp & Cole, 2015).

The studies we reviewed also did not investigate participants' subjective perceptions of time as a phenomenon of interest, such as whether leaders were themselves future/past oriented (temporal depth) or how they directed followers' attention to past, present, or future (temporally focused, Shipp & Cole, 2015). This speaks to another fundamental aspect of temporal conceptualization: whether time is viewed as a contextual backdrop (the medium in which change transpires), or as a focal construct specifically studied as an independent, dependent, moderator, or mediator variable of the theoretical concept under investigation (Chan, 2014; P. Goodman, Lawrence, Ancona, & Tushman, 2001; McGrath & Tschan, 2004; Shipp & Cole, 2015). For example, some studies consider time as a variable to explain within-person change (Shipp & Cole, 2015; Sonnentag, 2012). Of the 47 studies, only one (Day, Sin, & Chen, 2004) conceptualized time as a focal construct element. In that study, time was modeled as a random effect, and leadership status (being named captain of a professional hockey team) was examined as a time-varying covariate.

In the remaining studies, time was referred to only in contextual terms, such as describing the duration of the study or other background information about the case to orient the reader to the context of the study. This is problematic, as understanding evolutionary processes requires detailed information about when and at what rate a phenomenon changes (Roe, 2008; Zaheer et al., 1999). Additionally in the reviewed studies, temporal considerations generally began with the advent of research studies. In keeping with Shipp and Cole (2015), these studies usually frame t_0 or t_1—when data collection begins—as the temporal starting point of the research phenomenon, neglecting how the participants' prior experiences might have affected their behaviors during the study. Such omissions relegate participants' histories and prior relationships as exogenous factors when in fact they may have relevance to the phenomenon being investigated (Johns, 2006; Rousseau & Fried, 2001).

Research designs and data collection

Of the 47 total studies, 43 were empirically conducted in real time. Four were retrospective, examining solely archival data. The length of the studies ranged from two weeks to over 25 years. Surprisingly, of highest frequency were studies that covered time periods > 24 months (n = 15), followed by studies that lasted from between four to less than twelve weeks (n = 8); 12 to < 24 weeks (n = 8); less than four weeks (n = 7); from one year to < 24 months (n = 6), and from 6 to 12 months (n = 3). Some of the longest real-time investigations involved the Fullerton Longitudinal Study (Gottfried et al., 2011; Oliver et al., 2011; Reichard et al., 2011) that tracked participants from age two into adulthood. Other long-term studies looked at change over decades in organizations using historic records (Chung & Luo, 2008; Pajunen, 2006). Many of the studies had multiple units of analysis, including individual (n = 34), dyadic/group (n = 32), and organization (n = 6).

Data collection frequency included two waves of measures (n = 21), three waves (n = 9), four waves (n = 3), and more than four waves (n = 14). As noted in previous research, two-wave studies are problematic as they can only portray change as linear, potentially missing oscillations or other curvilinear manifestations (Day, 2011). Of the 43 studies where data collection timing was a result of strategic design choices (versus being constrained by the availability of archival data), only six articulated the theoretical justification for the timing they selected to administer their measures. For example, Epitropaki and Martin (2005) provide a detailed discussion of the rationale for their choice (p. 663). The remaining 37 studies relied on data collection based on convenience, such as the start and ending of a university course term.

Similarly, how long a study was conducted was also based on researcher decisions, yet researchers typically did not state how alternate choices (longer or shorter study periods) might have affected the results. This represents an important gap because timescales (the measure of the temporal interval selected to test theory) may affect how constructs emerge and are interpreted by the researchers (Roe, 2014; Zaheer et al., 1999). Ideally, study windows should be grounded in how individuals themselves identify the temporal boundaries of when a phenomenon begins and ends (Chan, 2014; George & Jones, 2000; Mitchell & James, 2001). Further, we found little discussion how one temporal dimension (e.g., an academic year cycle) might have affected the smaller window (e.g., semester) of the study (Ancona & Chong, 1996).

Finally, few of the designs explicitly discussed specific temporal

[1] *Independence* models investigate single variables. *Dependence* models consider relations between two or more variables. *Interdependence* models examine relations within (versus between) variables. *Hybrid* models cover approaches that fit into more than one category (Monge, 1990).

dimensions such as duration, continuity, periodicity, rate of change, magnitude, or trend. This finding is perhaps not surprising given the previous discussion that few of the studies considered time as a focal construct. However, knowledge of such temporal elements is essential to build theoretical understanding of underlying patterns. These patterns can include the trajectory of a leadership construct, event, or process over time, their stability or instability, as well as growth or decline (Pitariu & Ployhart, 2010; Ployhart & Vandenberg, 2010; Roe, 2008; Shipp & Cole, 2015).

Only four studies explicitly addressed specific temporal elements, including trend (n = 2, Day & Sin, 2011; Oliver et al., 2011), continuity (n = 1, Gottfried et al., 2011), and magnitude (n = 1, DeRue, Nahrgang, & Ashford, 2015). However, almost half of the studies (n = 20) implied the development of a trend (e.g., the emergence of leadership, Côté, Lopes, Salovey, & Miners, 2010). Others implied continuity (n = 6) and rate of change (n = 1). Among these studies and those that did not highlight temporal effects, few acknowledged this shortcoming of the study. Notably, one (DeRue et al., 2015) explicitly stated as a limitation that it did not capture rate of change data. Similarly, Day and Sin (2011) acknowledged that some effects may manifest as later times beyond the scope of the study. This study was also notable because it was the only one to find a negative trend (decline in leadership capacity after 13 weeks, Day & Sin, 2011) while the other articles implied and found a positive developmental trend over time. In sum, our review supports Kenny (1975) that timing design issues were often determined by convenience rather than by theory-based rationale. Further, 21 of the studies had insufficient measurement frequency to capture potential curvilinear phenomena or time lag effects. Nine had insufficient frequency of measures to capture oscillations, as those require at least four data points.

Theoretical frameworks, analytical methods, and testing of causal relationships

The articles we reviewed tested 18 of the 23 thematic categories outlined in Dinh et al. (2014). Many of the studies addressed multiple theories. Theories most frequently studied were *leadership emergence and development* (category 14, n = 19), *neo-charismatic* (category 1, n = 12), *contextual leadership* (category 11, n = 10), *strategic leadership* (category 10, n = 6), *social exchange/relational theories*, e.g., LMX (category 3, n = 4), *information processing theories* (category 2, n = 3), *team leadership* (category 13, n = 3), and *identity based* leadership theories (n = 3). Theories that were not studied in our sample of articles were *diversity and cross-cultural leadership* (category 5), *power and influence* (category 9), *ethics/moral leadership* (category 15), *destructive leadership* (category 19), and *leader error and recovery* (category 22).

The theories were used to guide research design and testing of different types of change relationships (linear, nonlinear, etc.). While Mitchell and James (2001, p. 533) outline eight predominant models of causation, our review found that most studies fell into three types, all assuming linear temporal relationships. The first and most common design was to determine whether X causes Y, with X preceding Y. The second most common was X causing Y, where the relationship and effect on Y is presumed to be stable over time. The third most common was X causing Y, which then causes a changed X, which causes a changed Y (cyclical recursive causation). However, for this third category, few studies reached this level of evidence. To demonstrate causation, they would have to provide sufficient evidence that 1) X preceded Y in time (ordering); 2) the variance in X was associated with the variance in Y, and 3) that no other variables existed that might affect the X → Y relationship (Popper, 1959). Especially problematic were issues of mediated and moderated variables, often due to the lack of specificity about their temporal dimensions (e.g., rate of change, duration, continuity, etc.). Similarly, the issue of effects at multiple levels was also challenging (Goodman, 2000; Willett & Sayer, 1994). Additionally, most studies failed to address that *when* X is first measured is a paramount consideration, particularly for cyclical phenomena or dependent on a pre-existing condition or ordering (Mitchell & James, 2001).

On a positive note, most of the studies included acknowledgements about the limitations of causation they could state. For example, all the archival studies stated lack of generalizability as a limitation. Six of the studies specifically addressed their temporal limitations, and two stated concerns about potential bidirectionality of causation. However, several studies seemed to assume immediacy of effects when that assumption may not be accurate (Fischer et al., 2017). Similarly, for virtually all studies, the frequency of repeated data collection was not sufficient to capture incremental change that would be needed to truly understand how constructs fluctuate, evolve, and develop over time (Mitchell & James, 2001). Few of the studies empirically examined feedback loops, cross-lagged associations, and time lags that could determine interdependence of dynamic relationships, e.g., what is rate of change for both X and Y, and how that affects change in the X → Y relationship (Mitchell & James, 2001).

Additionally, only a few of the studies considered assumptions about the stability of a variable. From a theory-building standpoint, this is important because although stability is often assumed, it may in fact be rarer than the phenomenon of change (Roe, 2014; Shipp & Cole, 2015). To demonstrate stability, a study must assess for rate, magnitude, and possible reasons for change over time, and then consider the possibility of random error, systematic sources of error, and systematic change (Mitchell & James, 2001). Of the studies we reviewed, nine addressed such stability assumptions, while 36 did not.

Analytically, many of the studies employed multiple methods. Qualitative methods were used in 14 studies and quantitative methods were used in 43. The most common quantitative method was hierarchical linear modeling (HLM, n = 21), followed by other quantitative methods, e.g. regression (n = 19), and growth curve modeling (n = 3). For assessing change over time, HLM and latent growth methods are particularly effective for assessing how and when a variable changes over time (Mitchell & James, 2001). Four of the studies that employed these methods specifically discussed growth trajectories. Other temporality discussed included identification of patterns (n = 1, Chung & Luo, 2008) and cycles (n = 1, Denis, Lamothe, & Langley, 2001).

Type of change

A problem with process research in general is that change is often conceptualized as being sequential (Y follows X) and linear (e.g., more input of one variable will produce an equal amount of more X or Y through an input—mediator—output process model) (Mitchell & James, 2001). However, change can manifest in a variety of forms. Examples include nonlinear (the output produced is not proportional to the quantity of input); cyclical or oscillating; incremental or discontinuous; spiraling up or down; or exhibiting rhythms and patterns over time (Mitchell & James, 2001). As noted previously, these dynamic change models require deep knowledge and articulation of temporality, e.g., ordering. Mitchell and James (2001) posited that change can be characterized as having three phases: equilibration (when then causal relationship builds), equilibrium (when the relation between X and Y has stabilized), and entropic, when the relationship destabilizes and its causal associations become uncertain (Mitchell & James, 2001). In the studies we reviewed, three of the four studies that discussed trajectories included illustrations of the trend path. However, only one (Day & Sin, 2011) illustrated the magnitude of the causal relation, visualizing how its linkage of X to Y was moderated through stages of the causal cycle. In the next section, we relate the various types of change to the concept of emergence, arguing how computational science can bridge many of the issues identified in this analysis.

Discussion on temporal studies of leadership

Ancona, Goodman, Lawrence, and Tushman (2001) commented that arguably the largest obstacle to using a temporal lens in organizational research is the degree of difficulty in accounting for time. They state that four reasons contribute to this difficulty: (1) there is still little theory about time lags, feedback loops, and duration; (2) scholars in our field have not developed the tools needed to detect complex patterns within time series data beyond linear and quadratic forms; (3) we still are learning about how to choose temporal variables, and (4) we do not know when trigger events will happen, thus cannot "foresee" behaviors to study.

To address the first issue, our systematic review suggests that there are existing frameworks that can guide the conceptualization and investigation of time more robustly. These conceptual frameworks over the last three decades help researchers with better specify of temporal effects in general (Ancona et al., 2001; Bluedorn & Jaussi, 2008; Day, 2014; Langley, 1999; Mitchell & James, 2001; Monge, 1990; Shamir, 2011; Shipp & Cole, 2015; Van de Ven, 1992), yet such studies are still rare. Our content analysis found that only a few studies explicitly addressed Monge's temporal dimensions of continuity (n = 1), magnitude (n = 1), and trend (n = 2), leaving rate of change, periodicity, and duration unaccounted for. Only about a third of the 189 quantitative empirical studies about leadership process reviewed by Fischer et al. (2017) included time lags in their designs, and six in our review. The number of temporal studies in leadership remains limited until the late 2000's, almost two decades after the publication of Monge's typology (see Fig. 1). This phenomenon illustrates a chicken-and-egg dilemma. Empirical studies do not study time rigorously, resulting in no theoretical advancement being made regarding time lags. Subsequent studies then do not adopt theory-based time lags because no new information about appropriate timing was produced.

These observations bring us to the second issue raised by Ancona et al. (2001), which is the lack of methodological and analytical tools in organization science. Our review suggests that indeed, many leadership scholars tend to assume that the effects of predictors on mediators and outcome variables are immediate and stable. The cross-sectional studies reviewed typically collected those measures either simultaneously or soon after each other. Survey-based studies may have blurred the effects of time because: (1) retrospective survey responses may be susceptible to distortion, hindsight bias, and social desirability, (2) the leadership behavior being assessed was not specified within a particular time period, and (3) leadership questionnaires tended to ask respondents about behaviors as a whole, assuming that they are stable across time (Day, 2014; Fischer et al., 2017). These problems may reflect methodological determinism, where the research question was determined by the data to which a researcher had access (Monge, Farace, Eisenberg, Miller, & White, 1984). In empirical investigations of leadership, scholars often develop research questions and constructs based on available methodological tools rather than those most appropriate for the phenomenon under study, frequently for practical reasons (Day, 2014).

To remedy this situation, in the last 10 years or so, organizational scholars have made more and more efforts to identify methodological tools that can aid in this endeavor. These efforts range from advanced statistical techniques such as using simultaneous equation models, regression discontinuity, and difference-in-differences models (Antonakis, Bendahan, Jacquart, & Lalive, 2010) to using different research designs such as dynamic mediated longitudinal design (Pitariu & Ployhart, 2010; Ployhart & Vandenberg, 2010), experiential sampling (Trougakos, Hideg, Cheng, & Beal, 2014), and qualitative historiometric (Shamir, 2011). Kozlowski and colleagues have developed metatheoretical frameworks to study emergence in organization science (Kozlowski & Chao, 2012; Kozlowski, Chao, Grand, Braun, & Kuljanin, 2013). Notably, the *Academy of Management Journal*'s 2013 special issue on process included a group of exemplary research documenting how different organizational processes unfold over time (Langley, Smallman, Tsoukas, & van de Ven, 2013). On a theoretical level, these scholars agree that data that take into account time, processes, and dynamics are often longitudinal, rich, varied, and most importantly, nonlinear (Ancona et al., 2001; Langley et al., 2013).

We take a step further toward advancing the field of leadership by arguing the need to learn more from other disciplines and adopt novel tools to study time as dynamic, emergent, nonlinear, and complex. As opposed to twenty or thirty years ago, tools to study complex systems have been developed and adopted quickly in multiple social science fields such as sociology, psychology, anthropology, and political science. The field of management has been slow compared to other social science fields to adopt complex systems methods (Davis, Eisenhardt, & Bingham, 2007; Squazzoni, 2010). In the next section we make the case for why computational simulation in general, and agent-based modeling (ABM) specifically, is a powerful tool for advancing scholarship, and why it should be added to leadership researchers' toolkits to aid in the specification, design, and analysis of temporal effects. In particular, we explain how ABM addresses the four obstacles identified by Ancona et al. (2001): (1) ABM models can be programmed to include recursive processes (i.e., feedback loops) as well as duration of the researcher's choosing. The analytical insights generated then provide a foundational rationale from which to build and test theory; (2) ABM is capable of detecting and analyzing the step-by-step development of complex

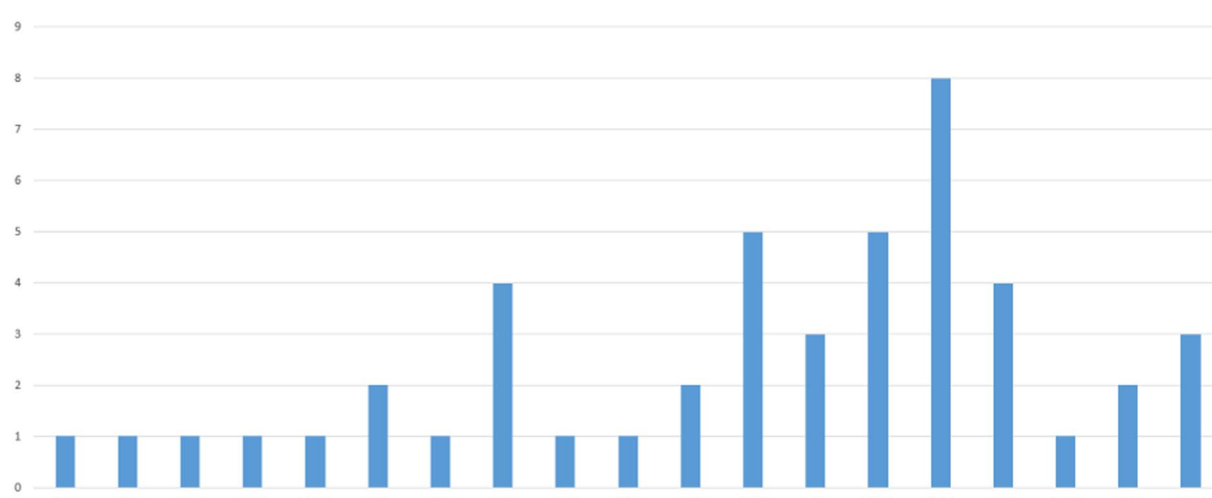

Fig. 1. Number of articles published by year.

Table 2
Methodological advantages of agent-based modeling (ABM).

Methodological issue	ABM contribution	ABM example(s)
Preventing type I and II errors (Day, 2014)	Data generated at each time step over sufficient time span enable accurate detection of patterns and change	Pattern identification of mental model convergence (Dionne et al., 2010); pattern-oriented modeling to detect adaptive behavior and system complexity in ecology (Grimm et al., 2005)
Illuminating multiple types of causation (Mitchell & James, 2001); effects across multiple levels (Goodman, 2000); mediator-moderator effects (Fischer et al., 2017)	Programming enables isolation/combination of selected variables, ordering of actions, and feedback dynamics; time series data document change process over time and scale	Leader and group effects in learning contexts (Black et al., 2006); hierarchical group decision optimization (Dionne & Dionne, 2008)
Endogeneity (Antonakis, Bendahan, Jacquart, & Lalive, 2014)	Enables isolation of variables through programming decisions. Variables can be added one at a time to examine their effects and interactions	Through isolation of external factors, found timing of an organization's promotional activities generated market diffusion (Delre, Jager, Bijmolt, & Janssen, 2007)
Lack of sufficient time series data (Dulebohn et al., 2012)	Models can be run for any length of time	Examined a 10,000 period run of a simulated stock market (LeBaron, 2001)
How to determine time lags (Ancona et al., 2001)	Computational experimentation can suggest temporal intervals to guide design of future empirical studies	Demonstrates amplification and correlation of variation over time (Parunak, Savit, & Riolo, 1998, p. 14)
Standard clock time vs. subjective time (Mitchell & James, 2001); monochronic vs. polychronic time (Bluedorn, Kaufman, & Lane, 1992b); relationships between nested temporal dimensions (Ancona & Chong, 1996)	Enables creation of different time cycles for individual or groups of agents	Agents each assigned their own time and rhythm. The researcher coupled these in various ways for experimentation and analysis (Fianyo, Treuil, Perrier, & Demazeau, 1998)
Understanding compilational emergence (Dinh et al., 2014)	Can reveal structure, patterned behaviors, and generativity that emerge from local-level interactions	Micro-*meso* interactions and effects of cognition and cohesion in work teams (Kozlowski & Chao, 2012)
Identifying temporal dimensions, e.g., duration, continuity, periodicity, rate of change, magnitude, trend (Monge, 1990); assessing stability (Mitchell & James, 2001)	Step-by-step iteration of time series data can reveal these temporal dimensions as well as equilibration, equilibrium, and decay/entropy of a phenomenon	Relational stability through strategic choice (Axelrod, 1987); development of human and social behaviors during emergency evacuation situations (Pan, Han, Dauber, & Law, 2007)

patterns within time series data such as cyclicity, not just linear and quadratic forms; (3) ABM enables clear separation of variables through programming, with the ability to cleanly add or remove parameters for experimentation by adjusting program code; (4) ABM enables researchers to schedule changes and observe their effects. This is a starting point to better understand trigger events and how those may affect leadership processes, thus providing insight into which behaviors and variables merit further study. Table 2 summarizes methodological issues regarding time identified from this and other reviews. It also outlines how agent-based modeling can address those issues and presents examples of relevant studies.

Agent-based modeling as a powerful tool to study temporal effects

Computational simulation is recognized as the third way of doing science in addition to quantitative and qualitative research methods (Axelrod, 1997). It is employed with increasing frequency in multiple fields of social science such as economics, sociology, anthropology, political science, and the behavioral sciences (Henrickson, 2004). Below, we elaborate on potential ways in which ABM can advance the study of leadership and time. Even though we focus explicitly on ABM, it is important to note that many of the advantages provided by ABM are also available with other types of computational simulation, such as system dynamics, genetic algorithms, and cellular automata. For a full review of these models and different research questions that they can each address, we recommend the work of Davis et al. (2007) and Harrison, Lin, Carroll, and Carley (2007).

Agent-based models are computational simulations that capture the behaviors and interactions of adaptive actors in a social system whose actions are interdependent upon one another (Macy & Willer, 2002). Each actor or "agent" in the system has unique properties while behaving according to some simple behavioral rules specified by the researchers. The outcomes of the system may include emergent properties generated by the interdependent behaviors among agents, often reflecting bottom-up instead of top-down processes (Axelrod, 1997). Like other types of computational simulation, ABM is capable of generating controlled computational experiments, systematically varying different input parameters, and reproducing those experiments over time and space. As such, ABM enables social scientists to "think about social phenomena in terms of processes that emerge from agent interaction and change over time" (Squazzoni, 2010, pp. 202–203). In the study of leadership, ABM has been used to study leader and group effects on context for learning (Black, Oliver, Howell, & King, 2006), hierarchical group decision optimization (Dionne & Dionne, 2008), shared mental model convergence and team performance improvement (Dionne, Sayama, Hao, & Bush, 2010), leadership emergence in face-to-face and virtual teams (Serban et al., 2015), and collective decision making and collective intelligence (McHugh et al., 2016). We use these studies as illustrative examples as we discuss below how ABM can contribute to the study of time in leadership research below.

While ABM is a simulation of what is observed in the real world, its goal is not to provide a completely accurate or realistic representation of real-world phenomena (Axelrod, 1997). Its strengths and objectives lie in enriching our understanding of key process mechanisms driving a particular phenomenon, often revealing that complex phenomena can arise from rather simple behavioral principles. Our contention is not that ABM will solve all of the problems in the study of time in leadership research outlined in our review above. Rather, we propose that ABM is a powerful tool to complement other research methods (quantitative and qualitative) to reveal and discern temporal effects in leadership phenomenon.

As will be explained below, the use of ABM in conjunction with other methods enables leadership researchers to be precise about the phenomena they are studying and to be able to ask different types of questions that traditional methods have not been able to answer. With this aim in mind, we now outline a number of ways in which increasing use of ABM as a methodology can push the study of time in leadership research forward theoretically. We first start with a specific example of how an ABM could complement an excellent empirical study in our review (Nahrgang, Morgeson, & Ilies, 2009) to extend the study's findings. We then discuss broadly four main theoretical contributions of ABM: to help researchers be precise about the mechanisms underlying their research phenomena, to ask different types of research questions, to specify temporal effects, and to connect different academic disciplines.

An ABM example

Nahrgang et al. (2009) used longitudinal design and growth-curve modeling to examine how leader-member exchange (LMX) relationships developed from their initial interaction through early relationship stages over eight weeks. They hypothesized that initial levels of LMX would be determined by extraversion, which facilitated more successful interactions, and agreeableness, which facilitated trust and cooperation among leaders and members. They expected that performance would positively relate to changes in LMX at later times. The authors collected data from leader-member dyads at seven time points: Extraversion and agreeableness were measured at time 0, prior to the initial interaction between leaders and members. LMX was measured at time 0, 4, 6, and 8, while performance was measured at time 3, 5, and 7. Results suggested that at time 0, leader agreeableness had a significant relationship with member's perception of LMX ($b = 0.12, p < 0.05$) while member extraversion had a significant relationship with leader's perception of LMX ($b = 0.07$, $p < 0.01$). Over time, member performance significantly predicted the change in leader's perception of LMX ($b = 0.20$, $p < 0.01$) and leader performance significantly predicted the change in member's perception of LMX ($b = 0.29$, $p < 0.01$).

We selected this study to demonstrate the use of ABM for a few reasons. This study has many strengths regarding temporal processes. The authors purposefully defined time 0 as the time before the research phenomenon (LMX) began, which made time 0 a meaningful start instead of a convenient sampling point. Data collected over seven time points enabled them to specifically examine the continuity, magnitude, and trend in LMX over time (see Fig. 1, Nahrgang et al., 2009, p. 262). At the same time, there are many ways that this study could be extended. Other than the well-defined start and end times, the chosen sampling time and time lag among them were not theoretically derived. The authors also noted that eight weeks was a relatively short period of time for LMX to reach the stage of mature partnership. They recommended that future research examine how critical events could change this relationship over time. Finally, like most other LMX studies, though only indirectly related to temporal effects, this study suffered from endogeneity issues. Because performance was the only measured predictor of subsequent LMX, it was not clear that the change in LMX actually reflected the effects of performance or that of other omitted causes.

An agent-based model would address these limitations to various extents. Building on the design and findings of this empirical study, an ABM could simulate a simple system containing a leader and a member interacting over continuous time steps. Doing so serves several purposes. First, it helps clarify the logical arguments made in the empirical study and creates a controlled setting in which causal relationships can be specified and tested. An essential part of this process is to define the temporal order of which event happens before which. As will be detailed below, if LMX at time $t + 1$ is specified as a function of performance at time t, then we can conclude that the change in LMX is caused by the change in performance instead of any other omitted cause X, simply because X is not built into the model. Second, an ABM allows us to create controlled experiments to vary different parameters in the model to examine what-if scenarios (Burton & Obel, 2011). For example, what would happen if the leader and the member were both high in extraversion? Both low in extraversion? One high and one low? Both high in extraversion yet low in agreeableness? Given that no other variables were theorized to cause LMX and performance (i.e., not built into the model), how would the effect sizes compare to those reported in the empirical study? Third, this ABM provides us with simulated scenarios of a time period that endures as long as we wish, even forever! This can help explore duration as well as questions such as how LMX would fluctuate in later stages of the relationship, ceteris paribus, how LMX would change if any of the parameters or their relationship changes, or how LMX would change in cases of critical events.

A simple agent-based model to capture this phenomenon is straightforward to build. It would consist of one leader agent and one member agent, forming a dyad. The leader and the member each have their own characteristics, namely extraversion, agreeableness, and perceived relationship with the other agent (LMX). Based on Nahrgang et al.'s (2009) hypotheses, extraversion leads to more frequent and more successful interactions, thus higher LMX. This would lead to the first behavioral rule: the higher an agent's extraversion, the more frequently it interacts with the other agent. Similarly, agreeableness was hypothesized to lead to higher initial LMX through cooperative behaviors, translating to the second behavioral rule: the higher an agent's agreeableness, the more it cooperates with the other agent. LMX, then, is defined as a function of agents' extraversion, agreeableness, frequency of interactions, and cooperations. Researchers can draw on central tendency statistics of extraversion and agreeableness as well as the effect sizes in the empirical studies as input parameters to build this simple model. They also have the choice to specify whether this effect only applies to the initial round (as hypothesized by Narhgang et al.) or continues to apply to subsequent rounds of interactions, with or without subsequent changes in magnitude or effect size. Performance parameters could be built in and specified similarly. This model could be specified to run for a particular time (e.g., 300 interactions), or could be set to run until a particular stopping condition is met (e.g., LMX reaches 0). Because the analysis of such a model is an inductive process, researchers could experiment with different scenarios and different parameters until they reach theoretical saturation. Results from this model would provide a full picture of how LMX plays out over time (see Fig. 2 for an example).

Charting the score of LMX (from both leader and member or either one) would show whether this variable consistently has a non-zero value (*continuity*), how large the effect is at any given time point (*magnitude*), and how fast this effect changes from one time point to the next (*rate of change*). The *trend* of how LMX changes over time could be identified visually (in cases of simple, linear changes) or by fitting a trendline into the chart (using polynomials or other nonlinear methods). Examining this chart would also indicate if there is any *periodicity* or *duration* associated with LMX (in this particular example, there is none). Taken altogether, this output suggests that LMX fluctuated in the first 70 interactions or so, then quickly stabilized for the next 160 interactions. As expected, when a critical event was introduced into the model at $t = 230$, this relationship exhibited substantial changes but quickly bounced back up to a level that is higher than before the critical event. This result is consistent with research that suggests that successful conflict resolution helps strengthen relationships (Jehn & Bendersky, 2003; Tjosvold, 2008). Combined with results from the empirical study, this simulation output suggests that while LMX may appear to stabilize quickly in the initial stage of the relationship, critical events in later stages can and will impose significant changes into this relationship. Therefore, it is important to capture these changes immediately after critical events happen, as well as during and after their resolution.

ABM helps researchers be precise about the mechanisms underlying their research phenomena

Kreps (1990) specifies several characteristics of a good formal model. First, it should be clear in explaining what it is about, the purpose it serves, and what contributions it makes. Second, it must be reproducible in the sense that it explicitly states all of its assumptions, behavioral rules, and procedures so the same model can be replicated by different teams of researchers. Third, it should be logical, presenting consistent

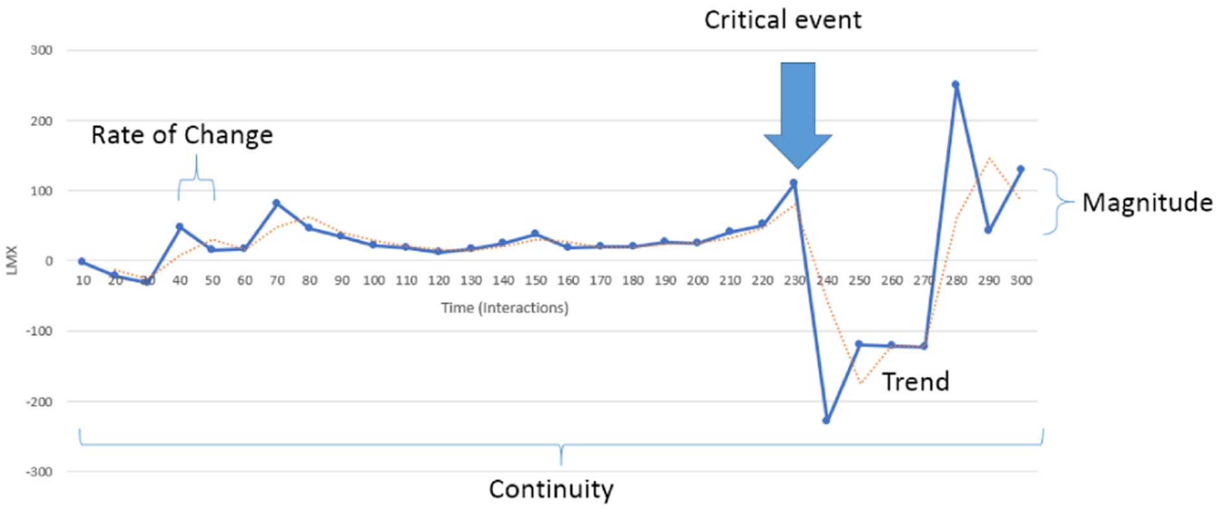

Fig. 2. An example of output from an agent-based model.

chains of logic among its different elements. Finally, it must transparently link certain values of input to certain values of output and, in so doing, reveal what mechanism or assumption is truly at the heart of a particular phenomenon. As a computational simulation tool, ABM embodies these characteristics because it operates precisely in the way it is programmed. In designing the model, researchers must make and document their deliberate decisions at every step in the process. To facilitate reproducibility, ABM best practices call for formal articulation of assumptions, behavioral rules, and procedures, such as the *Overview, Design concepts and Details* (ODD) protocol (Müller et al., 2013) or ODD + D protocol that includes human decision making assumptions (Grimm et al., 2010). To facilitate subsequent access and testing by other scholars, platforms such as OpenABM (CoMSES, 2016) and GitHub promote open access archiving of model code and protocols.

At the macro level, elements of building a good ABM include defining the model space, modeling the phenomenon, articulating what is and is not included in the model, and developing model input parameters. At the micro level, design elements include programming different types of agents into the model, specifying what characteristics differentiate the individual agents, how agents will behave, and the sequence of events in the model. By thinking through and articulating these choices, researchers demonstrate strong understanding of the research phenomena inside out by documenting and justifying their decisions. To accomplish this, researchers must master the literature surrounding their research questions and make decisions about behaviors. While these decisions often look like common sense, in fact this process helps identify issues that remain unaddressed in the literature.

For example, Black et al. (2006) used ABM to describe mechanisms underlying the development of group context-for-learning for different combination of leader and workgroup. They presented clear diagrams of how model parameters were calculated and how one influenced another (see pages 42–44). Their Fig. 2, for instance, specified that a follower agent's context-for-learning at any given time is the result of its previous round's context-for-learning (recursion), experiential learning factor, and the leader-directed learning factor. The latter two factors were further decomposed into smaller contributing elements, while the authors provided a clear theoretical explanation for how each element fit in the bigger system. Furthermore, in our example above, "high extraversion leads to more successful interactions hence stronger LMX" is often implicit in correlational survey studies. Specifying this chain of events allows researchers to have control over this causal relationship: *how much* extraversion leads to *how many* interactions, which then leads to *how much* LMX are all specified in the model.

Questions frequently asked by readers and reviewers include "Have you considered adding X to your model? How would adding X change your model?" Researchers must thoroughly understand their research phenomenon and its underlying mechanism to be able to explain that X can indeed be added to the model; however, it is not the central driving force of the particular phenomenon of interest. Axelrod (1997) emphasized that researchers must adhere to the KISS principle—acronym of the army slogan "Keep It Simple, Stupid." While the research phenomenon may be complex, the goal is parsimony—to explain the model as simply and with as few parameters and assumptions as possible. As Sterman (1991, p. 211) put it, "the art of modeling is knowing what to cut out, and the purpose of the model acts as a logical knife." Researchers can further elaborate on the simple model by adding another parameter or mechanism, resulting in other models with increasing complexity. This building block approach often produces multiple articles in a research program, one building upon another, and all trying to understand the research phenomenon from different angles (Harrison et al., 2007). In our LMX example, we could easily specify additional behavioral rules based on gender or personality similarity consistent with Nahrgang et al.'s (2009) supplemental analyses. We could also run a number of what-if scenarios with more parameters, each of which would contribute new insights to understanding how LMX unfolds over time. Yet they all begin with a simple model: one with two agents (a leader and a member) and three input parameters (extraversion, agreeableness, and performance).

ABM is not about perfectly reproducing reality, but about using a few parameters and simple behavioral rules to identify central mechanisms driving a particular complex phenomenon. Even though it does not (and should not) include every variable that one can think of, the fact that researchers know precisely what it includes and excludes and why, can strengthen the field by generating theories with high internal validity (Axelrod, 1997; Davis et al., 2007). Similarly, Axelrod (1997) stated that a simulation model should achieve three objectives: validity, usability, and extendibility. Good models have high internal validity—the programmer must be able to correctly build and execute the theoretical model and to distinguish between programming errors and unexpected emergent model outcomes. Good models must be usable, allowing researchers, reviewers, and readers to understand how it works and experiment with it. Good models are also extendible—leaving rooms for future researchers to add, modify, or build on to it to study related phenomena.

As Antonakis (2017, p. 4) observed in leadership theories, "on a very basic level there is a general lack of precision in definitions, assumptions, and in expounding on the variables constituting the theory as well as their causal impact." Many leadership models such as LMX or transformational-transactional leadership models suffer from endogeneity and confounding effects, in which it is unclear if, for example, LMX actually causes anything because LMX may be caused by the same omitted predictors variables that cause the outcomes (Antonakis, 2017). ABM's clear specification of constructs, measures, and assumptions can sharpen theoretical arguments and logic underlying a phenomenon (Carroll & Harrison, 1998). In such a controlled setting, a particular outcome can only be caused by what is included in the model, thus eliminating the possibility of confounding or spurious variables being present. Furthermore, causal relationships can indeed be inferred in ABM because researchers have clearly specified the causal mechanisms both in the theoretical justification and in the programming of the model.

ABM helps researchers ask different types of questions

In empirical studies, the types of research questions one can ask often depend on available methodologies (Monge et al., 1984). As the third way of doing science (Axelrod, 1997), ABM is a powerful tool that can be applied for multiple research purposes beyond hypothesis testing and proposition generation. Its uses include, but are not limited to, prediction of relationships among variables, proof of exploratory phenomenon existence, discovery of unexpected consequences, explanation of research processes, critique of theories, prescription of new methods of organizing, and empirical guidance for new empirical strategies (for more details of ABM usage, see Axelrod, 1997; Harrison et al., 2007). However, it is important to note that ABM adds the most value to a research problem if it is used with abductive logic (the logic of potentiality) as its main way of reasoning (Addis & Gooding, 2008; Lorenz, 2009; Peirce, 1994). The concept of abduction was brought into pre-modern scientific paradigms by Charles Peirce (1839–1914), who proposed it to be a distinctively different means of logical inference from deduction and induction. While deduction draws predictions of effects from known causes and mechanisms, and induction examines causes and effects to infer mechanisms, abduction is a tool to generate new hypotheses about causes from observed effects and mechanisms (Peirce, 1994, p. 5.171 & 7.218). In our LMX example, a deductive inference is: *Successful interactions between leaders and members lead to higher LMX* (mechanism). *The leader and the member successfully interacted* (cause). *Therefore, we predict that their LMX is high* (effect). An inductive inference would be: *We observe that leaders and members successfully interacted* (cause) *and their LMX levels become higher after the interactions* (effect). *Therefore, we believe that successful interactions between leaders and members lead to higher LMX* (mechanism). An abductive inference would be: *We know that successful interactions between leaders and members lead to higher LMX* (mechanism). *We observe that LMX levels are high* (effect). *Therefore, we suspect that leaders and members successfully interacted* (cause). Fig. 3 illustrates the difference between deductive, inductive, and abductive logic.

In modern day social science, abduction is often needed when there is a surprise in collected data, when there is no appropriate existing explanation for an observed phenomenon, and when researchers have to make new discoveries to connect seemingly unrelated results and components that drive those results (Reichertz, 2007). In other words, with abduction, researchers search for what *causes* an interesting observed effect. Pierce (1994, p. 5.189) characterized abduction as, “the surprising fact, C, is observed; but if A were true, C would be a matter of course. Hence, there is reason to suspect that A is true.” Researchers who are familiar with grounded theory have actually been exposed to this line of thinking. While the grounded theory approach taken by Glaser (2002) is more purely inductive in insisting that the coded themes emerge from the data directly, Strauss and Corbin's (1997) approach is more abductive in the sense that it allows researchers to ask “what if” questions and to modify themes during the coding process (Reichertz, 2007). Translated to the context of ABM,

> an unexplored emergent phenomenon of some complex social system is observed and agent-based model of corresponding complex system is then constructed. If multi-agent simulations lead to growing of the emergent phenomena, then there is a reason to suspect that assumptions of the model are correct. (Halas, 2011, p. 108)

Rather than being constrained by data access issues that may risk methodological determinism (e.g., a study design based on what data can be collected empirically), ABM allows for testing of potential relationships between variables. This simulation process can be considered as a thought experiment (abduction) to explore the possible dynamics of the system. Thus, ABM's use of three types of logic could be broadly categorized as examining *what-is*, *what-might-be*, and *what-should-be* (Burton, 2003; Burton & Obel, 2011). *What-is* includes a description and an explanation: ABM can predict the relationship among variables, or explain the processes by which an outcome emerges because of agent behaviors and interactions (Harrison et al., 2007). *What-might-be* examines possibilities, boundaries, or alternative explanations. When done in this way, ABM can indicate that an unlikely event could indeed happen (along with the associated parameters that causes the event), or help researchers discover unexpected consequences of simple interactive processes (Harrison et al., 2007). This is often the most exciting part of doing ABM, as it provides researchers with unlimited space to conduct controlled thought experiments with endless possibilities.

Our LMX example above illustrated a number of different *what-might-be* (i.e., “what-if”) questions that researchers could ask in such a basic model. Other questions that might be applicable to other aspects of LMX include “What would happen if the leader's goal is to build the best LMX relationships with followers over a short period of time?”, “What if his/her goal is to build sustainable long-term LMX relationship?”, “What if LMX has a seasonal pattern?”, “What if LMX only matters at *time 1* then remains constant no matter what happens?”, etc. In fact, frequently used techniques to analyze an agent-based model include trying extreme values of the model input parameters, looking for striking or strange patterns in the model output, or even exploring unrealistic scenarios (Railsback & Grimm, 2012). Finally, *what-should-be* entails choosing the best option out of different alternatives. This

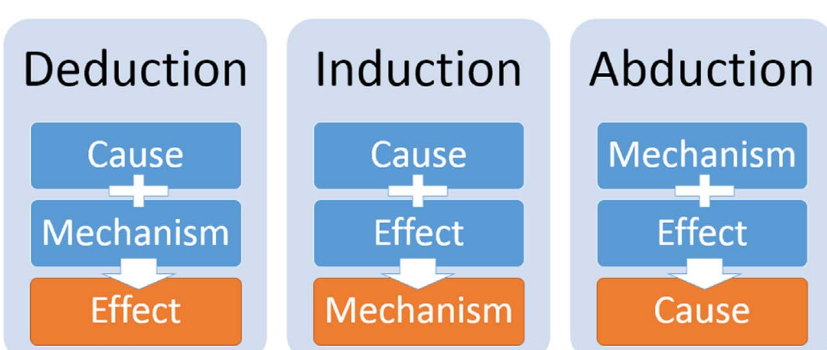

Fig. 3. Deduction, induction, and abduction; adapted from Lorenz (2009).

could happen if the objective of ABM is to offer a critique of different theoretical frameworks explaining a common phenomenon, to suggest a better method of organizing, or to generate alternative empirical strategies to test a theory or model (Harrison et al., 2007).

ABM can be coupled with and triangulated against other methods in several ways. Burton and Obel (2011) provided a detailed account of how previous studies in organizational science have used computational simulation to complement other kinds of data. These complementary approaches include design structure matrix of existing organizations (Carroll, Gormley, Bilardo, Burton, & Woodman, 2006), human subject laboratory study (Burton & Obel, 1988), case study (Lin, Zhao, Ismail, & Carley, 2006), confirmatory empirical field study (Lenox, Rockart, & Lewin, 2010), and longitudinal field study (Long, Burton, & Cardinal, 2002). Our LMX example is one way ABM could be used in conjunction with empirical survey designs.

In the field of leadership study, two notable examples have been published in recent years. Studying how team virtuality and density of social network ties set boundary conditions to the emergence of leadership in work teams, Serban et al. (2015) simulated an agent-based model about the process of leadership emergence through discussion based on four individual characteristics—cognitive ability, personality, self-efficacy, and comfort with technology. This ABM enabled them to examine the interaction effects between team type and other predictors of leadership emergence over time as team processes and social networks changed. They then used quasi-experiment and lab experiment to empirically test the findings from their ABM. Similarly, McHugh et al. (2016) coupled ABM with content-coded field data to document how individual and collective intelligence leads to quality of collective decisions. Although these authors did not explicitly examine the effect of time, they noted from their simulation results that the consensus model of decision-making takes much longer than other methods to arrive at a collective decision but also produced substantially better quality decisions compared to independent contribution methods.

ABM helps researchers specify temporal effects

Multiple theoretical frameworks exist to guide the study of temporal effects, such as Monge's (1990) dimensions of dynamic analysis, Mitchell and James's (2001) eight configurations of theoretical relationships and time, and most recently, Fischer et al.'s (2017) alternative temporal configurations of leadership. Time can be studied explicitly or implicitly (Shipp & Cole, 2015) and manifest as mediating mechanisms (Fischer et al., 2017), as rich, unfolding processes (Langley, 1999), as emergent phenomena (Waller, Okhuysen, & Saghafian, 2016), or as part of a complex adaptive system (Uhl-Bien, Marion, & McKelvey, 2007). As a methodological tool, ABM helps leadership researchers study time in all these ways, both abductively and deductively in the design phase of a model and inductively as they carry out analysis of model results.

ABM is inherently about process and emergent phenomena resulting from complex interactions of multiple agents continuously over time. When designing such a model, researchers must clearly specify the process mechanisms (a.k.a. "behavioral rules") that dictate the behavior of the agents in the model. Hence, they have a choice of specifying how different kinds of effects play out temporally across repeated interactions. All elements of temporal studies specified by Monge (1990) can and should be addressed in this process: Is one effect continuous or discontinuous? How large is the effect? How does it change over time? How quickly does it change? How long does this continue? Even when the researcher is studying a simple phenomenon, such as in our LMX example, the effects of extraversion on LMX (through interactions) are consistent over time, this assumption is explicitly stated, executed, and understood.

Perhaps a more important and promising use of ABM is as a theorizing tool to help researchers perform multiple thought experiments and develop simple theories of how temporal effects may manifest in the research phenomenon before they go out and collect data in the field. Output results from agent-based models are most often time series data, in which time elements such as continuity, magnitude, trend, rate of change, periodicity, and/or duration are present. Analysis of this data may reveal important initial insights into when things happen, when a specific change in X leads to change in Y, as well as the temporal effects of different patterns of behavior within the complex system. This data may also be scrutinized using nonlinear dynamic analyses such as phase space reconstruction, recurrence quantification analysis, fractal and multifractal methods, sample entropy, or wavelet transform (Richardson, Paxton, & Kuznetsov, 2017) to reveal nonlinear patterns beyond the reach of traditional statistical methods. The presence of a nonlinear pattern of behavior over time may prompt the need to collect data at more than three points in time. After identifying critical points along the temporal spectrum, researchers can then develop a theory about time lags and feedback loops and better design an empirical study and generate hypotheses to test the proposed theory. Clarity from the model design process and insights from the model results can help inform researchers about how to choose temporal variables, when to collect data, how many time points may be necessary, and when to anticipate a change in the phenomenon of interest before or after a critical trigger event. Returning to our LMX example, had our illustrative graph been the generic patterns from 10,000 iterations of the model, it would have suggested that data should be collected at minimally six time points: time 0, 70, 230, 240, 270/280, and 300.

Another example is Dionne and Dionne's (2008) comparison of how four types of leadership—participative leadership, individualized leadership, LMX, and an ideal control condition—contributed to effective decision-making in small groups. Their simulation results demonstrated that groups with participative leadership produced the best decision (closest to the optimal decision in the ideal control condition) initially, but decision quality improved more slowly after about 2,000 interactions. After the halfway point of the decision making process (about 5,000 interactions), all decision groups failed to make any more improvement in their decision, signaling a diminishing return effect on any decision made after this point. This finding has important implications for both research and practice. Researchers studying group decision-making may consider collecting data at three time points at the minimum: at one-fifth of the way through, half way through, and finally at the end of the decision-making process. In practice, group leaders should recognize the initial plateau point in the group's decision and conclude soon after to avoid wasting additional time on not making much further progress.

Another way that ABM can advance the study of temporal effects in leadership is to consider stability explicitly, such as speculating if certain effects would hold or change over time after experimental or empirical data have been collected and analyzed. For example, Serban et al.'s (2015) simulation of leadership emergence over time demonstrated that among the main and interaction effects found to be significant in their experimental and quasi-experimental studies, some were stable while others changed over time. The authors were also able to conduct post hoc analyses to show that leaders emerged faster in face-to-face teams than in virtual teams, and that this difference started to be eliminated after about 125 discussion rounds. Another notable example is Park, Sturman, Vanderpool, and Chan' (2015) study combining meta-analysis and simulation to understand how LMX develops over time.[2] The authors meta-analyzed extant literature concerning LMX, job performance, and justice perception to derive a correlation matrix, simulated a dataset that conformed to this correlation matrix, and then projected how this dataset would grow or change over time. This study was able to extrapolate possible long-term effects of LMX, job performance, and perceived justice and show that the effect of

[2] Even though this study did not specifically use ABM, its approach could be transferred to an agent-based model, allowing diverse agents to interact.

justice on LMX becomes weaker in more mature stages of LMX, but the effects of LMX on performance and justice become stronger over time.

Although ABM alone has the potential to address many challenges in the study of time and to provide crucial additional insights regarding time lags and feedback loops, we are not suggesting that it is appropriate for all usage. The biggest criticisms of ABM, like other simulation methods, are and will always be that it is just "fake data" simulated from a computer program, and that agent-based models lack the nuanced complexity of real world phenomenon. In fact, ABM is most powerful when coupled with other research methods, as outlined in the section above. A well-developed model has limitless potentials to add values to the extant literature. We resonate with Day's (2014, p. 48) assertion that "if we could adopt intensive, inductive approaches in addition to theoretically driven, deductive tests it would potentially offer greater possibilities for better understanding the effects of time on leadership while helping to build better theory."

ABM helps researchers connect different disciplines

Sometimes, the lack of existing theory, empirical evidence, or analytical methods in one field can be addressed by borrowing from and building on work from a different field. ABM embraces an interdisciplinary approach because it examines problem-driven research phenomena broadly. We argue that in leadership studies, ABM can advance our understanding of the effects of time in leadership by drawing on knowledge and process-based analytical methods from other fields. For example, while most statistical tools in organizational studies and the social sciences in general, rely on linear models, normal curves, and significance testing, ABM and dynamic modeling make other analytical approaches possible. These include pattern recognition, identification of hidden order (e.g., fractality), and order that depends on and emerges from repeated interactions and coupling of variables. Examples of pattern-based studies from other fields include communication (Foster, 2004), learning (Kelso, 1995), psychology (Koopmans, 1998; Smith & Thelen, 2003), biology (Gavrilets, Auerbach, & van Vugt, 2016; Gavrilets & Fortunato, 2014), and ecology (Grimm et al., 2005).

Implications and conclusion

This review examined 47 leadership studies published in the field's top journals. The purpose of the review was to assess how time has been studied as a factor in leadership. Building on prior reviews, we examined a sample of studies that longitudinally investigated leadership processes (e.g., emergence, development). Our findings indicate that while time is acknowledged as an important component in the construct of leadership, the methods employed generally do not capture fundamental aspects of temporality, including duration, continuity, periodicity, rate of change, magnitude, and trend. This supports the possibility that leadership process studies may be hampered by methodological determinism (Day, 2014; Monge et al., 1984). Our review also found that issues such as stability and time lags were insufficiently addressed. As discussed previously, these gaps have serious implications for theory building. We acknowledge as a limitation that our sample did not include published leadership studies appearing in journals beyond our list or that used other keywords. Rather than broad coverage, our aim was to examine a small, rigorously peer-reviewed sample in detail.

Our literature review makes several contributions to the study of leadership. First, our content analysis fills an identified gap in the literature by systematically examining how time has been considered in empirical research studies of leadership, shedding light on the extent to which these studies attended to specific temporal aspects of change processes, and identifying methodological challenges. Second, we have outlined a promising new approach, computational science, to overcome some of these issues. Similar to how the development of non-Euclidean geometry expanded analytical capacity that subsequently made possible Einstein's novel theory development (Polanyi, 1958), we have argued here that the emergence of new tools creates new research possibilities. Agent-based modeling is a promising new tool to reshape and open up new pathways in leadership theory, research, and practice.

Benefits of ABM include its ability to provide step-by-step documentation of the change process, enabling time series decomposition and the depiction of linear and/or nonlinear change in the time series data (Lee et al., 2015). ABM provides the analytical capacity to specifically describe, understand, and explain the role of time in the process of leadership. It can conceptually link multiple levels, building from the micro-level of individual assumptions to macro-level patterns that emerge in a population (Smith & Conrey, 2007). Additionally, ABM accommodates and documents the recursive process of agents' mutual influencing, specifically tracking the role of time in that change. This can yield insights into transformations such as those that occur between proximal and distal leadership development (Day & Dragoni, 2015). By developing computational evidence to better understand leadership as a process that emerges through interactions over time, ABM research also holds promise for developing computationally-supported hypotheses for scientific testing, as well as practical insights to promote more effective practice of leadership.

Moving forward

The gaps identified in this analysis highlight the urgent need for future research to more effectively articulate and investigate the relationship between time and leadership. While arguing for the use of a computational science approach, it is important to acknowledge and make explicit some potential impediments to the adoption of this methodology. Even though ABM is easily learned by novices (Wilensky & Rand, 2015), that alone will not ensure its enactment. A change in mindset is also needed.

Specifically, scholars and research education programs must shift from an implicit bias for linear deterministic scholarship rooted in variable-based analysis to an appreciation for the logic and value of complex systems and dynamics thinking. This shift entails embracing a constructivist worldview and process ontology, e.g., abduction (Langley et al., 2013; Locke, Golden-Biddle, & Feldman, 2008). It also requires understanding and becoming conversant in the vocabulary and epistemology of complexity dynamics, including concepts like emergence, self-organization, self-similarity (e.g., fractals), attractors, and phase space (Rickles, Hawe, & Shiell, 2007). Gaining this new understanding may assist in better ways to identify time lags and assess stability. Second, beyond learning ABM methods and programming, scholars also need to learn and teach nonlinear research design, encouraging interdisciplinary collaboration with scholars in other fields that already employ this approach. This may be difficult due to the training and acculturation in our field. Finally, scholars and the field of management will need to overcome common misconceptions (and perhaps even biases) about modeling, e.g., that it is not realistic enough. As we have made clear above, the goal of modeling is not to replicate reality but rather, through the exploration of potentialities (e.g., developing parsimonious formal models with as few parameters and behavioral rules as possible), to generate new insights and explanations of what is observed in reality.

For our field to shift, systemic barriers must also be acknowledged and resolved. At the micro level, we encourage editors and reviewers to be open-minded and practice intellectual humility (Yanow, 2009), asking questions and calling on peer expertise from other fields as necessary. Publishing venues such as *Journal of Artificial Societies and Social Simulation*, PLoS ONE, and other open-access journals may be fruitful options to disseminate small exploratory studies that show promise of elucidating temporal insights. At the macro level, we resonate with the observations by Shipp and Cole (2015) and Fischer et al. (2017) that the tenure-track system does not encourage people to explore and conduct time studies. Therefore, it is essential to build up a critical mass of scholars who are committed to this shift. We are grateful to the scholars whose work we build on here for galvanizing that mass. We call on readers to continue and expand this trajectory.

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