Introduction

Colette Hoption, Amy Christie
and Julian Barling

As we move into the 21st century, we are witnessing enormous changes in the workplace that deeply affect the people that work in them, such as changes in how, when, where, and with whom they work. In addition, workforce characteristics are changing in terms of how young employees start working and at what age employees then choose to stop working. Other seismic changes include the embedding of new information technologies into the fabric of our organizations (and, indeed, our lives), as well as often-intangible knowledge replacing observable units of production as the currency of work. These changes have occurred in public and private sector organizations, the organized and non-unionized sectors, non-profit businesses and family businesses. Structural changes too have deeply affected individuals in organizations, whether it be the push toward being 'leaner and meaner,' hence the downsizing and restructuring of many formerly large organizations, or toward growing into global behemoths through merging and acquiring other organizations. Furthermore, these massive changes in the nature of work pose considerable and new challenges to work performance. Subsequently, we question whether the understanding of work, and people at work, generated over the past century, remains relevant to the workplace and workforce today.

This is a propitious moment to stop and gather our thoughts as to the origins, current status, and of equal importance, future directions of organizational behavior – and it is to these issues that this Handbook is directed.

This Handbook highlights the major topics in the field of micro-organizational behavior (OB). In many important respects, the intent underlying this Handbook differs somewhat from many other Handbooks. Traditionally, Handbooks focus mainly on integrating past and current knowledge. In contrast, the editors of this volume have specifically invited our contributors to be retrospective and prospective in their orientation. The authors explore leading research in their domain over the past several decades, and look toward future research that will lead us into the next decade.

Second, the chapters in most existing Handbooks are authored by leading

academics and scholars who have left their footprint on the field. While respecting the value of accumulated wisdom in our choice of authors, we were also guided by the need for the field of OB to be able to generate new knowledge relevant to the changing environment. Therefore, emerging scholars who are already making substantial contributions to the field join long-standing experts in authoring this Handbook.

A third difference between this and other Handbooks is especially relevant from the perspective of this chapter. Many introductory chapters are authored by leading authorities who have earned the right to provide a retrospective account of the field. Consistent with the goal of looking both backward and forward in this Handbook, the authorship of this chapter is constituted differently. One academic already relatively long-in-the-tooth (Barling) joins with two new scholars (Hoption and Christie), and together they speculate about where micro-OB could or should be headed. The rest of this chapter, which considers conceptual and methodological issues, and their confluence, is the result of this collaborative effort.

ORGANIZATIONAL BEHAVIOR IN THE FUTURE

We open this discussion with a consideration of five different conceptual directions that hold considerable promise for pushing the limits of our understanding of micro-OB. We begin at the individual level of analysis, speculating about the potential benefits of incorporating a social neuroscience perspective into the study of behavior in organizations. Thereafter, we go beyond the individual and turn our focus to relational issues in the study of OB. We then discuss organizational and societal influences on individual behavior in organizations. Last, we underscore the need to extend our focus beyond the oft-asked questions of *what* effects can be expected to emerge from different organizational phenomenon and *why*, and raise the importance of understanding and predicting *when* such effects might be expected.

Two consistent themes cut across these five different areas. First, individual behavior in organizations does not occur in isolation; instead, an individual's behavior in organizations is both influenced by, and exercises influence upon biological, relational, organizational and societal factors. Second, historically, theory and method were often viewed as separate, with one the 'means' and the other the 'end,' as is evident in the title of Signorelli's (1974) article 'Statistics: Tool or master of the psychologist?'. In contrast, as will be seen in the ensuing discussion, there is now a greater rapprochement between the two, and because of methodological and statistical advances, empirical answers to our questions about each of the five conceptual directions are possible.

OB MEETS SOCIAL NEUROSCIENCE

The 20th century saw many significant advances in our understanding of human behavior. Broadly speaking, most of these advances derived from research that focused broadly on either social *or* biological processes. Some of the most substantial advances in our understanding of individual behavior over the past two decades have come from the field now known as social neuroscience, which merges the social and the biological, using knowledge about how broadly-based biological processes activate social interactions and behaviors (Cacioppo et al., 2007). The breadth and depth of the lessons already learned make it imperative that the methods used in, and the knowledge derived from, social neuroscience no longer escape the purview of those wishing to expand our understanding of micro-OB. With only very few exceptions (for example, Arvey et al., 1989; Heaphy and Dutton, 2008; Nicholson, 1998), some of which are discussed in the following paragraphs, explanations of individual behavior in organizations rarely rely upon knowledge of biological processes. Yet three specific examples will be sufficient

to demonstrate the enormous potential from merging OB with social neuroscience.

First, an intriguing question that continues to bedevil social and behavioral scientists is the extent to which particular behaviors are either learned, or inherited. Empirical research contrasting the roles of genetic and social factors in behavior was undoubtedly stimulated by the classic studies conducted by Bouchard and his colleagues on identical twins reared apart (see Bouchard et al., 1990). In a series of studies, they identified the extent to which genetic factors influence cognitive ability, personality, anti-social behavior and psychopathology (see Baker et al., 2007; Bouchard et al., 1990; McGue and Bouchard, 1998).

In an early application to OB of the methodology of studying identical twins reared apart, Arvey et al. (1989) demonstrated that approximately 30 per cent of the variance in job satisfaction was accounted for by genetic factors. While the implementation of this methodology is not without its problems (Cropanzano and James, 1990), it has also been extended to the study of leadership. Given recent findings within the burgeoning field of social neuroscience (see Cacioppo et al., 2007), the question of whether leaders are 'born or made' is often raised. Not surprisingly, several studies have identified the genetic and environmental contributions to leadership emergence (Arvey et al., 2006; Arvey et al., 2007). Despite these advances, genetic and biological effects on the development of leadership behaviors deserve attention and robust empirical examination. The need for this is accentuated when even recent calls for more integrative theory building on leadership make limited reference to the roles of genetic and biological factors (Avolio, 2007), and future calls for additional research on social neuroscience do not identify leadership development as a possible focus (Cacioppo et al., 2007).

Second, and related, a consistent finding from the broad field of social neuroscience might offer some additional ideas for our understanding of leadership, and in particular abusive leadership behaviors and entrepreneurship. The possible influence of biological factors on aggression (and related behaviors such as social dominance, social control, and risk propensity; Dabbs, 1992) has attracted considerable empirical attention and support. In brief, this research, conducted in the field and in experiments, shows that higher levels of testosterone predispose individuals to increased aggression and related behaviors. Consistent results are found in studies where naturally-occurring levels of testosterone are measured, as well as in studies where testosterone is manipulated directly (van Honk and Schutter, 2007). Furthermore, testosterone has also been linked to entrepreneurship. White et al., (2006) contrasted two groups in an intriguing study: one with a substantial history of experience in the creation of new business ventures (entrepreneurs), and the other with no such experience (non-entrepreneurs). They showed that entrepreneurs evidenced higher testosterone levels than their non-entrepreneur counterparts.

As with Arvey et al.'s (1989) research pointing to variance explained in job satisfaction by non-contextual factors, pursuing a social neuroscience approach might lead to an understanding of the genetic factors inherent in leadership behaviors, and an appreciation of just how much of a leader's behavior is under (or beyond) individual or organizational control. In addition, if biological and genetic factors explain meaningful variance in leadership emergence and behaviors, what other core organizational behaviors might be similarly influenced?

Third, an approach that honors the assumptions of social neuroscience would extend its search for the causes of organizational behaviors beyond the social realm, suggesting that some of the explanations for behavior in organizations might well emerge very early in an individual's life. One example of this phenomenon emerges from a recent study on the predictors of individuals' counterproductive behavior in organizations – a frequent topic of study, as evident from Robinson's chapter in this volume, and one which has primarily emphasized the role of

situational factors and individual differences. Recently, Roberts et al. (2007) examined the extent to which counterproductive workplace behaviors might be predicted by factors outside of the workplace. Their longitudinal study carried out over 23 years showed that being diagnosed with a conduct disorder by the age of 11, but no later than 18 years of age, was a significant predictor of subsequent counterproductive behaviors within the organization (although having a criminal record was not a significant predictor). These results are intriguing; genetic factors have been implicated in the development of conduct disorders (Slutske et al., 1998), and early conduct disorders persist into adulthood (Campbell, 1995).

While each of the three examples presented are specific, the reason for drawing attention to them is more general: Our understanding of the causes of individuals' behaviors in organizations can be expanded in innovative ways and directions by embracing the methods and lessons from social neuroscience.

THINKING RELATIONALLY

To understand the way in which work is now suffused by relationships, and just how much this situation has changed, we need look no further than our own work: While single-authored publications were the norm several decades ago (and in many situations even a requirement for tenure), it is now rare in our field to find instances of single authorship and this handbook itself, our introduction and the majority of the chapters are no exception! Further evidence of the relational nature of work (if any is needed) is how, throughout our collaborations with others, our own unique ideas are shared freely and frequently, and on reading the end product, it often becomes difficult to tease apart original ownership of ideas. How do we know what was self- versus other-initiated? Mead (1934) earlier termed this phenomenon 'a conversation of gestures,' and its prevalence and importance provides the foundation for conducting relational research.

Mead's (1934) conversation of gestures illustrates the dilemma of understanding and predicting most organizational phenomena: What behaviors can we study and understand without accounting for 'the other'? Even demographic variables such as age and gender are studied relationally. As one example: it is not being female per se that is associated with negative work experiences, but rather, being female in a male-dominated work environment that is associated with such experiences (see Gutek and Morasch, 1982). Many additional variables in OB lend themselves equally to a relational lens, including workplace aggression, leadership, discrimination, emotions and power and politics, each of which is considered in separate chapters in this Handbook.[1]

Aquino and Lamertz's (2004) model of workplace victimization exemplifies the move toward relational models in OB. As they cogently argue, 'by proposing a relational approach, we are directly exploring the idea that such behavior must be understood as a function of a relationship that develops between a victim and perpetrator rather than solely by their individual attributes' (Aquino and Lamertz, 2004: 1024). While this statement might seem self-evident, most research on workplace aggression and violence (as but one example) proceeds by focusing separately on either the victim or the perpetrator. Aquino and Lamertz (2004) are moving beyond this towards an understanding of how factors associated with the victim, the perpetrator and the relationship between the two, give rise to victimization. Several other examples of existing relational research help to illustrate its potential value to an understanding of behavior in organizations.

First, with few exceptions, traditional attempts to understand the effects of leadership proceed as if the presence or absence of followers was largely irrelevant. Recognizing the active role of followers in what is still referred to as the leadership process invites a relational perspective. Meindl et al. (1988) initially begged this question when addressing the 'romance of leadership.' Later, Klein and House (1995) invoked a relational perspective

in their conceptualization of charisma: the leader may be charismatic, but charisma can only surface and flourish if and when the follower is open to charisma. More recently, dyadic-based leader-follower studies have ensued (for example, Dvir et al., 2002; Dvir and Shamir, 2003). These dyadic studies enrich our understanding of the nature and effectiveness of leadership, because they draw attention to the leader's behavior *and* the follower's active participation in interpreting (and modifying) that behavior.

As documented in the relational model of victimization and the aforementioned leadership research, other workplace behaviors may also benefit from a relational perspective. For instance, the relational lens can be used when studying courageous acts in the workplace; heroic (or altruistic) behaviors are often conceptualized solely in terms of individual differences of the altruistic individual. Yet our understanding of such behaviors (which include phenomena such as empathy and compassion, bystander non-intervention and whistle blowing) may be further understood by understanding the relationship between the 'hero' and the 'rescued' rather than examining 'altruistic behavior' in isolation.

Some of the most captivating illustrations of Mead's (1934) conversation of gestures derive from instances where explicit intentions to influence or affect another individual are absent. Research findings from beyond the confines of OB have provided support both for positive and negative emotional contagion. For example, emotional contagion is evident when merely looking at others' happy faces induces more happiness in oneself (Wild et al., 2001), and when being around depressed individuals increases one's own depressive symptoms (Stevens and Prinstein, 2005). In a similar vein, recent research has demonstrated how indirect witnesses to displays of interpersonal injustice react negatively along with the victim (De Cremer and van Hiel, 2006). Extending the findings of emotional contagion and vicarious learning, might it be possible that other anxieties, fears or frustrations, can be understood relationally? Similarly, might optimism and hope be equally 'contagious'?

From the perspective of thinking relationally, perhaps the greatest insights stand to be gained from the study of marital and romantic relationship processes and interactions. Unfortunately, a common practice in the social sciences is to become specialized in one's own areas, at the expense of understanding and benefiting from conceptual and methodological advances in other areas. From a science that emphasized self-reports in the mid-twentieth century, marital research has gained substantial predictive power by incorporating interview, observational and self-report data, and sequential and time-series analyses (Gottman and Notarius, 2002). Like other areas within OB (for example, workplace aggression; Barling, 1996) where our understandings have been enhanced by lessons from marital research, our understanding of relational issues in organizations may benefit meaningfully from the techniques used in marital research, the study of which may be more likely given the thoughts shared by Gottman (2007) in a recent *Harvard Business Review* interview.

In conclusion, many phenomena long seen as core parts of OB derive special meaning from one's position relative to others. For example, holding more or less power in an organization (group or team), enjoying relatively more or less control (see Dupré and Barling, 2006), and being of higher or lower status (Aquino et al., 2001), necessitate relational approaches to theory and research. Embracing such a relational approach to the study of micro-OB would provide the opportunity to consolidate and extend our understanding of individuals' behaviors in organizations.

INDIVIDUALS ARE EMBEDDED IN ORGANIZATIONS

'Open systems' theory was widely-accepted as an organizing framework for understanding individuals and organizations from the middle of the 20th century. Within this framework, organizations were seen as being inseparable from the societies in which they existed, and

the influence between organizations and the individuals within those organizations was reciprocal. Despite the conceptual promise offered by an open-system perspective, the practice of research was not necessarily affected; research in the latter part of the 20th century continued to study employee behavior in isolation.

Now, organizational scholars are again recognizing the complex interdependence of individuals, groups and organizations, both conceptually and methodologically. To this end, there has been a veritable surge in multilevel theory and methodology. It would be redundant for us to reiterate calls for multilevel research which are comprehensively described elsewhere (for example, Kozlowski and Klein, 2000); instead, we raise three issues for consideration in multilevel research that would plausibly expand our knowledge of individual behavior in organizations.

First, whether for the sake of conceptual simplicity or methodological feasibility, multilevel research has usually examined basic instances of individuals embedded within teams that exist within organizations. In sharp contrast, the reality is that many individuals hold multiple competing roles within organizations (Beehr and Glazer, 2005) and across different social systems (e.g., as reflected by work-family conflict; Bellavia and Frone, 2005; also see the chapter by Grzywacz and Butler in this volume). With the increasing number of individuals who hold different part-time jobs in different organizations (Barling and Gallagher, 1996; Gallagher, this volume), or who moonlight (Inness et al., 2005), the complexity of the interdependencies within and across levels requiring attention becomes apparent.

Our second question deals with the reciprocal influences within these multiple levels of analysis. Most often, research has investigated top-down processes, where upper-level phenomena are presumed to influence lower-level phenomena (Kozlowski and Klein, 2000), such as team efficacy influencing individual performance. As is often the case, the way in which we structure our research questions reflects philosophical assumptions, not all of which are necessarily intended. In this case, top-down approaches ascribe a relatively passive role to employees. While recognizing obvious power imbalances in organizations, we suggest that it would be more appropriate to ascribe a greater agentic role to individuals in the many ways in which they actively structure their environments (Bandura, 2005). Consistent with Mead's (1934) conversation of gestures, we might even ask where this process begins and ends? Chen's (2005) model of newcomer adaptation to new team settings is instructive in this sense. Finding that individual performance was both a product of initial team performance, and a driver of future team performance, Chen suggests that 'the assumption underlying the hypothesized model is that newcomer adaptation is a longitudinal process that evolves within persons (newcomers) over time, and that newcomer adaptation both influences and is influenced by certain individual and team constructs' (p. 102). While exploring the intricacies of reciprocal effects across levels are no doubt challenging, doing so will produce knowledge that more accurately reflects the active role that individuals play in interacting with and creating their environments.

Third, we also encourage greater attention to the nature of higher-level variables, such as groups. Commonly, within-group agreement has been a requirement central to multilevel research; groups in which agreement is not reached are discarded from the research. However, the disagreement that exists within groups is equally interesting. We illustrate this with two examples. Roberson and Colquitt (2005) discussed how team-level justice can be characterized by shared or dissimilar perceptions of justice, the former being more closely related to team effectiveness. Likewise, Ng and Van Dyne (2005) showed that teams with greater variation in the level of helping behaviors were associated with worse team performance. However, this study goes further, showing that this effect emerged irrespective of the mean levels of helping behavior. Both of these studies point to the possibility that the differences in perceptions and behavior within groups may

be just as informative as the within-group similarities.

Theoretical and methodological advances in multilevel modeling will no doubt increase in the future, and confronting the questions asked here have the potential to contribute to an ever-expanding body of knowledge about behavior in and of organizations.

ORGANIZATIONS ARE EMBEDDED IN SOCIETY

> 'Few people are capable of expressing with equanimity opinions which differ from the prejudices of their social environment. Most people are even incapable of forming such opinions.' Albert Einstein.

Ignoring Einstein's insights in general would surely be inadvisable; ignoring the implications of this particular observation for understanding the impact of the social context on the individual employee would result in a truncated appreciation of individuals' behavior in organizations. In this section, we focus on the influence of societal events and trends on employees, as well as the significance of social context on the individual employee. Specifically, we discuss corporate social responsibility, community and natural disasters, and sleep.

Organizations' attempts to recognize social issues are frequently captured under the umbrella of 'corporate social responsibility' (CSR), which 'reflect[s] the organization's status and activities with respect to its perceived societal obligations' (Dacin and Brown, 1997: 68). Most of the studies focusing on CSR have been conducted at the organization level (e.g., Bird et al., 2007); only rarely is empirical research on this topic investigated at the employee level. Employees remain the face of faceless organizations, and the success of any CSR initiative is dependent on the extent to which employees identify with the values expressed in the CSR initiative, and are willing to go beyond normal job expectations to make the initiative a success. Anecdotal evidence of individual employee involvement in CSR initiatives exists. As one example, in the summer of 2007, employees at Lush, an organization specializing in handmade cosmetics, served customers naked to protest against the damaging effect of packaging on the environment (a frequent focus for CSR is environmental sustainability). Employees covered parts of their bodies with an apron that invited the question 'Ask me why I'm naked.' Ignoring the potential risks of sexual harassment and interpersonal injustices, Lush employees showed their identification with the company's values and ethics. Not only did Lush workers prompt questioning from the public, but they were willing to deal with any criticisms their behaviors raised.

Existing OB theories are already well-placed to provide an understanding of the employee identification with organizational values that would make CSR initiatives possible. Whether it be the 'collective sense of mission' and going 'beyond self-interest for the good of the group' (Bass and Riggio, 2006) inherent in transformational leadership, the sense of identification that is core to organizational commitment (Meyer and Allen, 1997), or the task significance and meaning that individuals seek in their work (Hackman and Oldman, 1980), these issues have been at the core of OB for several decades.

Moving beyond established organizational behavior theories will also invite new avenues for research. For example, Andersson et al. (2007) found that hope and gratitude were associated with pro-social behaviors in organizations, as well as stronger beliefs in social responsibility. Compassion might fulfil a similar predictive role in involvement in CSR. Confronting such possibilities might help involvement of CSR initiatives in the burgeoning move towards positive organizational scholarship (see the chapter by Dutton and Glynn in this volume).

Recognizing how societal forces influence the individual in other ways can also advance our understanding of OB. Organizations and their employees have long been subjected to the vicissitudes of natural and technological

disasters (Baum et al., 1983). Consequently, it should not be surprising that research exists focusing on disasters, as well as on individual differences such as resilience (for example, Bonanno et al., 2006) and coping strategies (e.g., Tucker et al., 2002) to account for employee well-being. However, organizations and their employees have also suffered from acute violence perpetrated both by organizational insiders and outsiders (e.g., partners of employees exacting revenge at the worksite; disgruntled customers; Kelloway et al., 2006), as well as major terrorist attacks such as those of September 11, 2001 (Inness and Barling, 2005), all of which may include among their consequences some effects on the experience of work and individual well-being. As is evident (e.g., Beehr and Glazer, 2005; and Jex's chapter in this volume), a mountain of research exists on chronic work stressors. In contrast, acute societal traumas such as 9/11, critical moments that have lasting effects on organizations and their employees, have attracted much less conceptual and empirical attention (Pratt and Barling, 1988) despite indications that disasters such as 9/11 might be associated with subsequent employee absences (Byron and Peterson, 2002), and that levels of community violence influence levels of employee aggression at work (Dietz et al., 2003). Focusing on leadership during the tragedy of 9/11 has also led to an understanding of the role of compassion in times of existential crisis (Dutton et al., 2002).

Third, just how individual-level employee behaviors might be affected by societal events can be gleaned from a focus on sleep trends. According to the National Sleep Foundation, the amount of individuals who endure less than six hours of sleep per night has steadily increased based on data gathered from 1998 to 2005 (National Sleep Foundation, 2005). The perils of sleep deprivation have been well documented, including compromised safety (e.g., Muecke, 2005), physical health (e.g., Babkoff et al., 1989), and mental health (e.g., Morin et al., 2006). The phenomenon of *karoshi* referring in Japanese to death from overwork (Kanai, 2006) may be equally relevant in this regard (e.g., Uehata, 1991). There is no reason to expect these trends will reverse themselves in the near future, and therefore, just how societal changes in sleep influences the nature and experience of work (e.g., shift work), as well as safeguards at work to protect against sleep deprivation, warrant future research.

In summary, given the continuing and increasing interdependence of organizations and societies, the attempts made by organizations to influence society, and the disturbances from the external environment on organizations and their members will likely continue. As a result, any explanation of individual-level employee behavior that ignores these external events will likely result in an narrow perspective of OB. Accordingly, an opportunity exists for research in this area to enhance our understanding of the interconnection between the environment in which organizations function and the individual employee.

TIME AND TIMING IN OB RESEARCH

The very idea that time is a core issue in OB research would come as no surprise to anyone who has completed a first-level course in the behavioral sciences, where the notion that temporal ordering is a necessary condition for causal inferences is likely introduced early and discussed often. The attention given to this issue in classic methodology texts (e.g., Shadish et al., 2002) would further reinforce this notion. Moreover, how many of us have not received feedback from reviewers on a manuscript submitted that did not in some way question whether longitudinal data were needed, whether the longitudinal data presented were sufficient, or whether the temporal periods between data points were adequate? Our raising of this issue is not simply an attempt to reinforce the methodological importance of considering these issues in research; this is appropriately and extensively dealt with elsewhere (Shadish et al., 2002). Instead, we introduce this section to refine and extend existing knowledge, and to invite new conceptual questions. In that

sense, time will not only be used to disentangle cause-and-effect relationships, but become an issue worthy of scholarly attention itself.

Perhaps an appropriate way to begin this discussion is to point a finger at research that one of us was involved in more than two decades ago (Barling et al., 1987). In the study, the authors were presented with the opportunity to study the effects of a major explosion that took place in a dynamite factory just outside Johannesburg, South Africa which resulted in the deaths of 14 employees. The organization involved invited them to study the effects of the explosion on employee attitudes and mental health of survivors, helped them to construct two quasi-experimental control groups, and allowed them to collect data within two weeks of, and again two months following the disaster. Remarkably, no negative effects on employee attitudes or mental health were revealed, and these findings were explained in terms of the likelihood that employees believed that there was no longer any threat to their personal well-being, as a result of which the stressor was no longer present. With the benefits of hindsight, we might offer a different set of explanations. What if we had not only measured the outcome so soon after the disaster, but also one year later? Might it be possible that the results would then have suggested that the earlier pattern of relief for having survived the disaster was replaced by later post-traumatic stress disorder? Just when we collected the outcome data – which was dictated entirely by organizational realities rather than conceptual criteria – might have been critical in generating a potentially erroneous set of conclusions. We are by no means alone in this phenomenon. The appropriate timing of any measurement is an issue that continues to plague OB research, and it is frequently determined by opportunity rather than conceptual design.

Research on the mental-health benefits of respites from work further illustrates the importance of time. Westman and Eden (1997) demonstrated that job stress and burnout can be relieved through vacations, but the benefits of any respite from work dissipated over a short period of time. Specifically, burnout was reduced during and immediately following a vacation, but returned to its pre-vacation levels within three weeks of returning to work. Had Westman and Eden (1997) conducted their post-respite assessment at only one of these time periods, or perhaps at any other period, different conclusions might have been reached.

Such examples highlight how assumptions about time permeate and influence OB research. Other core areas of micro-OB are beset with similar issues. To illustrate, we provide an example based on transformational leadership research – today's most prominent theory of leadership (see Judge et al.'s chapter in this volume). Most of the research on transformational leadership uses either self- or other-reports based on various forms of the Multifactor Leadership Questionnaire (Bass and Avolio, 1995). Invariably, respondents are asked to report on how frequently their leaders display various leadership behaviors across time, often a period of six months. Averaged measures of leadership are then created based on these aggregated reports. Not only are such methods susceptible to memory biases and distortions, but they also make an assumption that merits attention. The inherent assumption is that leadership behaviors are highly stable across time, which rationalizes measures of leadership based on aggregated reports. Yet even individuals who indicated that they have charismatic or transformational leaders often report verbally that these leaders do not display these behaviors 'all the time,' but rather episodically. This is consistent with recent theoretical suggestions that leadership is reflected in critical moments (Tucker et al., 2006; Avolio and Luthans, 2005). To capture these critical 'moments' and take a different approach to studying leadership, researchers could pursue the methodological approach of daily studies which have been conducted in other areas of OB such as daily stress (e.g., Story and Repetti, 2006).

So far we have focused on methodological considerations of time; however, research can also benefit from explicit conceptualizations of temporal effects. While research in OB is

frequently focused on asking whether effects emerge, ascertaining *when* any effects abate is of equal importance. Team-based research may show the greatest advancement in this respect. Gersick's (1988, 1989) punctuated equilibrium model shows that time is critical in shaping group development, which is marked by two phases, the first of which is characterized by little progress toward the group's goals, with the second yielding considerable effort toward task completion. These two phases are separated by a midpoint transition, occurring when approximately half of the group's allotted time has passed. At this point, group members' awareness and perceptions of time shift, and accordingly, they change their behaviors. The punctuated equilibrium model illustrates the importance of timing for groups, and suggests that researchers must be cautious of how relationships could differ during various stages of a team's shared tenure. For instance, Harrison et al. (1998) showed that the effects of demographic diversity on group outcomes weakened as groups spent a greater amount of time together as a team; however, the opposite was true for diversity based on deep-level attitudes that are revealed over time.

Our understanding of teams has benefited from a conceptual focus on time and timing in other ways as well. For example, creativity may only have positive team implications when it emerges *prior* to a team's transition point, at a time when individuals are willing to embrace it (Ford and Sullivan, 2004). Likewise, the benefits of shared team experience exist, but there is an optimal amount of shared experience, after which negative performance effects occur – a process referred to as knowledge ossification (Berman et al., 2002).

Not directly confronting the issue of time and timing presupposes that any effects that emerge do so immediately and permanently which is an unlikely situation. We have argued that asking conceptual questions of time and timing can protect against such presuppositions and enhance our theories of behavior in teams. Furthermore, we suspect that the scarcity of such questions in the current literature derives from the methodological challenges inherent in handling longitudinal data. Fortunately, newly-embraced statistical techniques available to micro-OB researchers now enable us to ensure that our understanding of individual behavior in organizations is not a function of the methodological tail wagging the conceptual dog.

Before concluding this section, we raise one final consideration about the conceptualization of time in existing OB research. The effects of time urgency, time management, and deadlines (e.g., Waller et al., 2001), as well as perceived control over scheduling, have all received considerable empirical scrutiny (e.g., Barton, 1994). Additionally, the effects of time away from work, whether in the form of breaks (Galinsky et al., 2000), vacations (e.g., Westman and Eden, 1997) or absenteeism (see Johns, this volume) have been extensively studied. We suggest, however, that what has been lacking from this research is a sufficient appreciation of cross-cultural differences in the understanding and influence of time. Research needs to account for the way in which time is conceptualized within OB research, wherein time as a linear, limited resource, is culturally bound (e.g., Adair and Brett, 2005). Consistent with recent research on national differences in the experience of work stress (e.g., Liu and Spector, 2005), and the general move toward organizational research that is more responsive to cross-national differences (see the chapter by Gardner and Early in this volume), greater understanding of OB will emerge from using a broader lens to conceptualize time.

In summary, individuals are inseparable from their personal and cultural histories, and future trajectories, making time an inherent property of individual behavior in organizations, and warranting a temporal lens to study OB (Ancona et al., 2001).

CONCLUSION

Each of the chapters in this Handbook takes a very specific approach in reflecting on the

history, current status and future possibilities of the specific area under consideration. In this introduction we have raised several questions that cross all of the areas considered. Confronting these questions enhances our understanding of individual behavior in organizations, and in turn, reveal meaningful and new directions for OB research.

NOTE

1. See the chapters by Cortina and Berdahl, Judge et al., Nkomo, Grandey, and Lux et al. respectively.

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