

The **Organizational Frontiers** Series

# Modern Research Methods for the Study of Behavior in Organizations

Edited by **Jose M. Cortina**  
and **Ronald S. Landis**

ROUTLEDGE



A Publication of the Society for  
Industrial and Organizational Psychology



“When I first heard of the idea for this book a few years back, I was truly excited. Now that it's finished, I'm truly amazed. Professors Cortina and Landis not only identified a set of topics that will move organizational research forward, but also recruited some of the most knowledgeable people in the world to write on them. This book needs to be required reading in any research methods course oriented toward the organizational sciences. It will truly get students to think about research design issues very differently.”

—Robert Vandenberg, University of Georgia,  
Professor of Management, Past Editor,  
*Organizational Research Methods*

“Cortina and Landis bring a wide range of research methods that are not familiar to I/O psychologists to the attention of this community. Their introductions of techniques such as catastrophe theory, social network analysis, latent class analysis, Petri nets, and experience sampling (to name only a few of the techniques described in this volume) will add breadth and depth to the toolbox of I/O scientists and practitioners alike.”

—Kevin R. Murphy, Colorado State University

“Scientific progress accelerates when newer methodological approaches allow for the novel examination of enduring issues. I am confident that the methodological approaches described in this wonderful volume will lead to advancements in many important domains for years to come.”

—Herman Aguinis, Kelley School of Business,  
Indiana University



# Modern Research Methods for the Study of Behavior in Organizations

The goal for the chapters in this SIOP Organizational Frontiers series volume is to challenge researchers to break away from the rote application of traditional methodologies and to capitalize upon the wealth of data-collection and analytic strategies available to them. In that spirit, many of the chapters in this book deal with methodologies that encourage organizational scientists to reconceptualize phenomena of interest (e.g., experience sampling, catastrophe modeling), employ novel data-collection strategies (e.g., data mining, Petri nets), and/or apply sophisticated analytic techniques (e.g., latent class analysis). The editors believe that these chapters provide compelling solutions for the complex problems faced by organizational researchers.

**Jose M. Cortina** is a Professor in the Industrial/Organizational Psychology program at George Mason University. His recent research has involved topics in meta-analysis, structural equation modeling, significance testing, and philosophy of science, as well as predictors and outcomes of emotions in the workplace. He currently serves as Editor of *Organizational Research Methods* and is a former Associate Editor of the *Journal of Applied Psychology*. Dr. Cortina was honored by SIOP with the 2001 Ernest J. McCormick Award for Distinguished Early Career Contributions, by the Research Methods Division of the Academy of Management with the 2004 Robert O. McDonald Best Paper Award, and by the *Organizational Research Methods* Editorial Board with the 2012 Best Paper Award. He was also honored by George Mason University with a 2010 Teaching Excellence Award and by SIOP with the 2011 Distinguished Teaching Award.

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# Modern Research Methods for the Study of Behavior in Organizations

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*The Editors dedicate this book to their advisor, Neal Schmitt. Many people rave about their advisors. Actually, most complain, but a few rave, and with good reason. A good advisor gives time and expertise without any expectation of receiving anything in return. But Neal wasn't a good advisor. He wasn't a great advisor. Neal Schmitt was simply the best possible advisor. He was (and is) a great scholar and teacher to be sure, but the quality that distances him from all others was his absolute commitment to putting students first. Neal has had dozens and dozens of advisees (he is quite old, you know), and every single one of them that we know felt that Neal ALWAYS prioritized them over all of his many other commitments. We can never pay you back Neal. We can only offer our gratitude and esteem.*

*Plus the occasional book dedication.*

**Jose M. Cortina**  
*Fairfax, Virginia*

**Ronald S. Landis**  
*Chicago, Illinois*



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# About the Editors

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**Jose M. Cortina** is a Professor in the Industrial/Organizational Psychology program at George Mason University. Professor Cortina received his Ph.D. in 1994 from Michigan State University. His recent research has involved topics in meta-analysis, structural equation modeling, significance testing, and philosophy of science, as well as predictors and outcomes of emotions in the workplace. His work has been published in journals such as the *Journal of Applied Psychology*, *Personnel Psychology*, *Psychological Bulletin*, *Organizational Research Methods*, and *Psychological Methods*. He currently serves as Editor of *Organizational Research Methods* and is a former Associate Editor of the *Journal of Applied Psychology*. Dr. Cortina was honored by SIOP with the 2001 Ernest J. McCormick Award for Distinguished Early Career Contributions, by the Research Methods Division of the Academy of Management with the 2004 Robert O. McDonald Best Paper Award, and by the *Organizational Research Methods* Editorial Board with the 2012 Best Paper Award. He was also honored by George Mason University with a 2010 Teaching Excellence Award and by SIOP with the 2011 Distinguished Teaching Award.

**Ronald S. Landis** is Nambury S. Raju Endowed Professor in the College of Psychology at Illinois Institute of Technology. Prior to IIT, he was Professor of Psychology and founding Director of the University of Memphis Center for Organizational Research and Effectiveness (UMCORE) at University of Memphis. He has also served on the faculty at Tulane University, where he was awarded the Tulane President's Award for Excellence in Graduate and Professional Teaching in 2004. He is a Fellow of SIOP and was honored by the *Organizational Research Methods* Editorial Board with the 2012 Best Paper Award. He has primary research interests in the areas of structural equation modeling, multiple regression, and other issues associated with measurement and the prediction of performance. His work has been

published in several leading journals, including *Organizational Research Methods*, *Organizational Behavior and Human Decision Processes*, *Personnel Psychology*, and the *Journal of Applied Psychology*. He is currently an Associate Editor of the *Journal of Business and Psychology* and a former Associate Editor of *Personnel Psychology* and serves on the editorial boards of *Organizational Research Methods*, *Journal of Management*, *Personnel Psychology*, *Human Performance*, and the *Journal of Applied Psychology*. He received his Ph.D. from Michigan State University in 1995.

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# About the Contributors

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**Michael D. Coovert**, upon completing his doctorate at the Ohio State University, joined the industrial/organizational faculty at the University of South Florida, where teaching factor analysis and structural equation modeling are among his responsibilities. One summer, when working as part of a research team whose purpose was modeling behavior and interactions in combat information centers on Naval warships, he was stumped, as none of his traditional methodological tools could capture behavior across time and at multiple levels of abstraction. Not wanting to pump gas for the summer, he reached back into his training in computer science and pulled out graph theory and Petri nets to apply to the problem.

**Richard P. DeShon**, Ph.D., is Professor and Chair of Organizational Psychology at Michigan State University. His research interests are in the areas of dynamic models, motivation and self-regulation, measurement theory, performance measurement, and selection and classification based on individual differences. He is currently an Associate Editor for the *Journal of Applied Psychology* and is on the Editorial Boards for *Psychological Methods* and the *Journal of Management*. He is a Fellow of the Association for Psychological Science and winner of the Ernst J. McCormick award for early career contributions to industrial and organizational psychology. He received his Ph.D. from the University of Akron.

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**Le Zhou** is a Ph.D. student at the University of Florida. Her research interests include leadership, team, and employee training and development. She is also interested in quantitative methods in organizational behavior research (e.g., longitudinal data analysis, computational modeling).

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# Series Foreword

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Know thy methods! It's a must. We all need them—an indispensable tool of our profession. If there is something we all use, need, and apply as scientists and practitioners, it is methods. Methods are what make or break our studies, experiments, interventions, or practical actions in the labs and the field. Methods are at the core of our science and practice—that is why we all should know our methods. We need to know their strengths, limitations, and applicability. We need to know what they do for us, as well as what they won't do. We need to know how they help us with external and internal validity of our studies and interventions. We need to learn new and emerging methods to deal with our ever-changing research and practice. And so this volume is much welcomed and, more importantly, much needed.

Jose and Ron have rightfully described this volume as “transforming our field by transforming methods” and have assembled a set of chapters illustrating that. Our theories and constructs are changing, and so our methods must change as well. Topics range from longitudinal growth modeling to qualitative research to Petri nets to synthetic environments. This volume contains a set of transforming chapters to help us answer questions about our science and practice and to increase our methodological toolbox. There is food for thought and tools for graduate students and for seasoned scientists and practitioners, something for all of us. Remarkable.

On behalf of the Editorial Board of the SIOP Organizational Frontiers Series—thank you Jose and Ron (as well as your collaborators) for creating this one-of-a-kind addition to our series. A much needed volume that will enhance how we think and execute our science and practice. Well done Jose and Ron!

Eduardo Salas, Ph.D.  
*SIOP Organizational Frontier Series Editor*  
*University of Central Florida*





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# Preface

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We wanted to create this volume because we believe that advances in research methodology play a crucial role in the development of our field. Cutting-edge methods can, and should, invigorate and inform our science. For many researchers, applying newer, and often more sophisticated, techniques can be daunting. This, in part, arises from trying to understand the “guts” of a particular analysis from the rather limited information often provided in typical journal articles. Along with this, researchers may not see how particular techniques can be used to study their particular substantive questions. In that spirit, we challenged our contributors to provide specific, detailed examples that will give researchers the confidence to use techniques that they might otherwise avoid.

Descriptions of each contribution are contained in our introductory chapter, but it suffices to say that we were lucky enough to have contributors not only accept our invitations to explain these vanguard methods, but also to provide clear road maps for those interested in applying said techniques to their own research. In short, the chapters in this volume provide fabulous treatments of a variety of measurement, design, and statistical topics. We are supremely confident that these chapters will stimulate the enhanced use of the focal techniques and be a wonderful reference source for interested researchers.

If you find one or more chapters to be especially useful to you and/or your students, we would be thrilled to hear from you. If you have complaints, contact the authors.



# 1

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## *Introduction: Transforming Our Field by Transforming its Methods*

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*Jose M. Cortina and Ronald S. Landis*

Those who study human behavior in organizations confront a plethora of challenges. In order to meet these challenges, researchers sometimes employ complex measurement or analytic techniques, without necessarily knowing how, or even if, they serve the researcher's purposes. Although there are many ways in which human-computer interaction has changed for the better, the ability to collect or analyze data without knowing what one is doing is not one of them. What we need is a sort of methodological prism that breaks our techniques into their component parts, allowing us to understand how they fit together.

Our goal for the chapters in this book is to challenge researchers to break away from the rote application of traditional methodologies and to capitalize upon the wealth of data-collection and analytic strategies available to them. In that spirit, many of the chapters in this book deal with methodologies that encourage organizational scientists to reconceptualize phenomena of interest (e.g., experience sampling, catastrophe modeling), employ novel data-collection strategies (e.g., data mining, Petri nets), and/or apply sophisticated analytic techniques (e.g., latent class analysis). We believe that these chapters provide compelling solutions for the complex problems faced by organizational researchers, problems that, if left unaddressed, might leave us on the dark side of the moon.

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## TOO MANY COOKS, TOO FEW APPLIANCES

The methods that we use to collect data necessarily influence (and constrain) the way that we conceptualize organizational phenomena. As a result, scientific advancements are limited, to the extent that we continue to rely on the same old methods to study new problems. Imagine a chef who wishes to make a tasty meal. If the chef is given only, say, a deep fryer with which to work, culinary options become necessarily limited. Although it is certainly true that the deep fryer will be useful for making some dishes, the chef will be in trouble if he or she would like to poach an egg [Editor's note: *Do not* drop an egg into a deep fryer unless you enjoy third-degree burns]. As anyone who grew up in the deep south can tell you, the chef operating in the deep fryer-only kitchen will come to view available dishes primarily through the lens of this tool [Editor's note: If you find yourself in Louisiana, avoid the fried green salad]. On the other hand, if the chef is provided with a range, oven, grill, wok, etc., a much wider variety of dishes can be conceptualized and executed. The same is true for the organizational researcher who operates in, say, the "OLS regression kitchen." If ordinary least squares (OLS) serves as the only methodological tool, the researcher will come to view organizational problems through the OLS lens. Although many wonderful dishes can be made with OLS regression, many others cannot. One must limit oneself to the prediction of continuous dependent variables whose errors are uncorrelated, using variables that are, or can be, converted into interval level variables. One must restrict oneself to the study of phenomena that change in a continuous fashion over time. At a broader level, one must restrict oneself to phenomena that are sufficiently understood that one knows which questions to ask (i.e., quantitative as opposed to qualitative research). It is only when the list of tools is augmented that the list of topics can be expanded.

Of course, we have no desire to denigrate OLS regression. Indeed, there are still many social scientists who work in the even more rustic analytic kitchen in which analysis of variance (ANOVA) is the technique of choice. When confronted with the horror that is a continuous predictor, these poor devils either artificially categorize it, resulting in nonlinear, non-random measurement error, or relegate it to (additive) covariate status in ANCOVA. They need their blender to frappé, but, alas, it only has on/off. And don't get us started on what is happening in the t-test galley.

The chapters compiled in this book help organizational researchers to become aware of, and appreciate, the tools that are hiding in the methods pantry. The authors of these chapters not only provide descriptions of these contemporary methodologies, but also provide examples of how they may be applied to organizational phenomena. In particular, we believe this latter aspect of each chapter may be this volume's greatest asset. Frequently, we see researchers get excited by particular techniques, only to become frustrated because they do not see how the methods can be applied to their own work. The authors of the chapters in this volume have taken care to provide this information.

A second theme that we have attempted to integrate in the current collection of chapters is that of organizational research as increasingly complex and challenging. As a field, we study phenomena that are typically directly unobservable, temporally volatile, and in contexts that often do not permit tight, experimental control. Thus, despite the claim that ours is a field of "soft" scientists, we hope that the current chapters convince you that our field can apply rigorous methodologies for studying organizational behavior, and that, through the use of these methods, our field can further develop as an applied science that meaningfully contributes to the understanding of modern organizational phenomena and problems.

We also want to emphasize that statistics and methods are as vibrant and vital a research area as any substantive one. Both of us have had interactions with colleagues, the nature of which will be familiar to many readers of this book:

*Colleague:* So, tell me, what is your primary research area of interest?

*Jose/Ron:* Research methodology.

*Colleague:* That isn't an area of research.

To us, this type of interchange reflects a conceptualization of methods as immutable (read: stagnant) and leads to a cookbook application of old techniques that constrains theoretical development and knowledge creation. We believe the chapters in this volume challenge that view of methodology and, instead, convey the important contributions made by those working in the area.

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## ADVANCED, NOT MAGICAL

In choosing authors and topics for this particular volume, we had certain principles in mind. First, we wanted chapters on cutting-edge topics and authors with the expertise to write them. Second, we wanted the chapters to inform and educate readers about the nature and relevance of particular techniques and tools through clear summaries and reviews. Third, we wanted the chapters to provide sufficient information to allow the reader to adapt the techniques to his or her own research. All too frequently, beneficial methodological techniques are not adopted, because researchers don't have a clear road map for application. Finally, we wanted the chapters to prompt researchers, not only to apply newer techniques (when appropriate), but also to challenge status quo thinking about particular organizational phenomena. As a result, we specifically asked the contributors to identify cutting-edge issues with respect to particular methods that will serve to stimulate future substantive research. Our contributors have provided such a resource, and we trust that the following chapters will serve as catalysts for significant advances in the organizational sciences.

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## CONNECTING THE PRESENT (AND FUTURE) TO THE PAST

More than a decade has passed since the publication of the most recent volume in the *Organizational Frontiers Series*, devoted to research methods. Since the publication of that volume (Drasgow & Schmitt, 2002), our field has seen an explosion of interest in, and use of, advanced measurement, design, and analysis techniques. At the time that Drasgow and Schmitt went to press, many of the techniques that now seem common were either in their infancy (e.g., latent growth modeling (LGM), grounded theory, response surface methodology) or so uncommon in the organizational sciences as to be unworthy of inclusion in a volume on methodology (e.g., catastrophe modeling, latent class analysis, experience sampling). Indeed, the Drasgow and Schmitt volume was instrumental in solidifying researchers' understanding of many advanced methodological techniques, which in turn led to these techniques being more commonly and appropriately used.

We believe that our field is now poised to take another important step down the path of sophisticated methodological techniques. In recent decades, techniques have emerged that, not only improve our ability to collect data and to evaluate the data that we collect, but also provide researchers with the freedom to develop more nuanced theory. Instead of exploring LGM at a broader level, as did David Chan in his excellent and crucial chapter from the Drasgow and Schmitt volume, we assert that our field is ready to explore extensions of LGM (Ployhart and Kim), as well as pitfalls that are well understood in other fields but new to ours (Braun, Kuljanin, and DeShon). We must go beyond a descriptive treatment of grounded theory and explore the latest in case studies, textual analysis, and other quantitative methods (Gephart). We must acknowledge the existence of nonmonotonic relationships and more explicitly consider discontinuous relationships with techniques such as catastrophe modeling (Guastello) and discontinuous growth modeling (Ployhart and Kim). We should move beyond recognizing that some organizational phenomena involve hierarchical structures and parallel processes and more appropriately model these contexts (Coover), as well as more explicitly consider individuals as part of larger systems (Kalish). In short, it is time for our field to explore the next frontiers of research methodology. Some of these frontiers may represent fine-tuning of our techniques, but others (e.g., catastrophe modeling, experience sampling) have the potential to turn our field on its ear and, indeed, have already done so (e.g., Guastello, 1988; Ilies, Scott, & Judge, 2006; Guastello, 2007).

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## ORGANIZATION OF THE VOLUME

This book is divided into two parts: *Statistical Analysis* and *Research Design and Measurement*. In the first chapter of the *Statistical Analysis* part, Guastello describes catastrophe theory and the analyses that accompany it. Many of us have heard of catastrophe theory (or at least have heard of related concepts such as chaos theory), but few of us have taken steps toward applying it to our research in organizations. This is a terrible shame, because so many organizational phenomena are likely to conform to catastrophe models. In fact, we believe that our field is on the cusp (if you will) of a “catastrophe revolution,” and those who join it early



will be remembered for (and credited with) having changed our field for the better.

Catastrophe models describe discontinuous phenomena, that is, phenomena that involve sudden “catastrophic” change. For example, Guastello (1987) suggested that, for low levels of task variety, there is a monotonic, positive relationship between ability and performance, whereas, for high levels of task variety, there is a discontinuous relationship between ability and performance such that performance is stable and low for lower ability levels but tends to jump “catastrophically” at some middle level of ability, with the jump point being tied to the reward system. The jump is consistent with the tenets of insight learning, in which an “a-ha moment” creates a qualitative change in knowledge. As another example, Guastello (1988) showed that, for large workgroups, accidents are monotonically and positively related to environmental hazards. For small workgroups, however, accidents are discontinuously related to hazards, such that accident rates are stable and low for low-hazard groups, stable and high for high-hazard groups, and jump catastrophically at some mid level of hazard. One reason for this is that small groups tend to be more cohesive, and this cohesiveness creates a cyclical process that causes accident rates to “shift gears” at some level of environmental hazard.

Guastello and his colleagues have used catastrophe theory to explain a wide variety of organizational phenomena, and yet few other researchers have done so. We suspect that the reason is that most organizational researchers are intimidated by the abstruse mechanics of catastrophe modeling. In Guastello’s chapter in the current volume, he provides a detailed and approachable description of catastrophe modeling and its application. We cannot imagine a better presentation of this material and believe the chapter will serve as foundation for “catastrophically” influencing our field for the better.

In the second statistical-analysis chapter, Ployhart and Kim tackle random coefficient models (RCMs). These authors focus their attention on a surprisingly underutilized application of RCM, namely *dynamic* or *time-varying predictor models*. Although RCM and LGM have become quite common in organizational research, it is relatively rare to see research in which Level 1 predictors vary over time. And yet, as Ployhart and Kim put it, “wouldn’t it be exciting to see research showing how changes in knowledge acquisition relate to changes in job performance over time?” We know from cross-sectional research that those with greater amounts

of knowledge tend to have better performance evaluations, but, because dynamic predictor models have not been applied to the problem, we don't actually know if one's performance increases as one's knowledge increases! Ployhart and Kim explain the mechanics of dynamic predictor models (including latent growth models), their data requirements, the pitfalls associated with such models, and the strategies that can be used to avoid these pitfalls.

These authors also discuss extensions of the standard dynamic predictor RCM. First, they discuss *lagged growth models*, in which data points are lagged in time to reflect hypothesized causal sequences. Collecting data in this way, as the authors explain, allows one to address problems that are common to dynamic models, such as reciprocal causation and spurious relationships. Second, these authors describe *autoregressive latent trajectory (ALT) models*. In ALT models, change over time in a given variable is estimated after controlling for previous levels of the variable (i.e., the autoregressive element). As the authors point out, ALT models reflect the axiom that the best predictor of future behavior is past behavior.

Third, Ployhart and Kim discuss *nonlinear* and *discontinuous growth models*. Nonlinear growth models capture change over time as a nonlinear function of time. For example, we know that knowledge acquisition does not change in a linear manner over time, so why should its effects be modeled as if it did? Discontinuous growth models can be used to model phenomena that do not change in a monotonic manner. Indeed, discontinuous growth models are very similar to the catastrophe models described in the Guastello chapter.

Finally, Ployhart and Kim describe *between groups change models*. In such models, grouping variables are used to distinguish different clusters of change patterns. For well-defined groups, multiple group LGM is quite useful. For less defined or unknown groups, latent class analysis or, more broadly, mixture modeling can be used. In short, if you want to understand the latest in RCM with time-varying predictors, this chapter is a must.

Social network analysis, as described in the third statistical chapter, by Kalish, holds great promise for researchers interested in modeling social influence and communication in organizational contexts. No matter their formal structures, services provided, or products generated, organizations are fundamentally social systems. Individual employees interact with customers, colleagues, subordinates, supervisors, and myriad others through the formal and informal aspects of contemporary jobs.

Unfortunately, we organizational scientists frequently choose to simplify these complex relationships and, all too frequently, study individuals in isolation, or at best as members of collectives, and attempt to explain behavior through a somewhat static lens. Social network analysis provides us with opportunities to uncover how individual relationships (dyads, triads, etc.) are formed, influence individual behavior, and ultimately change and dissolve.

One would not likely choose to study a family by individually surveying the children and presuming that these individual perceptions fully capture the complexity of the family dynamic. Even if we were to take a higher-level perspective and consider the children as a “team,” we are still likely to miss important dyadic relationships between the children and/or the parents. Similarly, organizational researchers should not ignore the contextual aspects of modern organizations. These contexts shape individuals and their interactions with one another through formal policies, structures, rules, and informal norms. Social network analysis allows researchers the opportunity to more fully model such contexts and to capture important, “bottom-up” processes that are not easily assessed through traditional techniques (e.g., hierarchical linear modeling (HLM)).

Upon reading the Kalish chapter, one will have a clear understanding of why social network analysis is an important tool. One is also left with a profound appreciation of the tremendous research opportunities that await those interested in studying networks. Of equal importance, Kalish provides user-friendly examples of how to apply social network analysis that will provide the necessary grounding for individuals interested in applying these techniques. Advances in theory are, to some degree, constrained by available methodological and analytic tools. Kalish demonstrates this by illustrating how social network analysis, not only allows researchers to more accurately model individual processes, but also allows for, and encourages the development of, more sophisticated theories about how individuals within a system are connected with one another.

Wang and Zhou explore the latest issues and applications of *latent class analysis* (LCA) in the fourth statistical-analysis chapter. As mentioned earlier in this chapter, variable-centered statistical methods such as multiple regression have been, and continue to be, invaluable tools for organizational researchers. Despite the wide applicability of these analytic techniques and their obvious relevance for answering particular research questions, variable-centered methods are ill equipped to answer questions of intra-

individual differences. For such questions, researchers instead rely on person-centered approaches, such as cluster analysis. As Wang and Zhou's chapter points out, our ability to answer more sophisticated research questions is greatly expanded when the variable-centered and person-centered approaches are combined in latent class procedures. More recently, LCA has been extended further so that class membership can be based, not only on patterns of scores on variables, but also on factors such as item response patterns and changes over time.

The extensions of LCA discussed in this chapter are *mixed-measurement item response models*, *growth mixture modeling*, and *mixture latent Markov modeling*. Wang and his colleagues have written some of the seminal work on these procedures (e.g., Wang & Bodner, 2007; Wang & Chan, 2011). This chapter not only explains the nuts and bolts of these procedures, but also illustrates why and how they are applied. After a discussion of LCA and how it differs from more traditional clustering techniques (i.e., theory driven, ML estimation), Wang and Zhou describe mixed-measurement item response models (MM-IRT), which are combinations of LCA and IRT models. In traditional IRT models, variability in item parameters between specified groups can be examined by testing for measurement equivalence among pre-specified groups. MM-IRT can be used to identify heterogeneity in item parameters, which can then be attributed to membership in latent classes. The technique can also be used to compare models with different numbers of latent classes.

Wang and Zhou then discuss growth mixture modeling (GMM). LGM typically involves the identification of growth parameters that describe the growth curves that exist for a given set of data. GMM is a combination of LGM and LCA that allows for the identification of groups of subjects that have similar growth curves. Latent class variables can then be used to explain this variability in growth curves. These authors also describe mixture latent Markov modeling (MLMM). The term "Markov chain" is used to describe response patterns on a categorical variable across time. As the authors put it, a Markov chain reflects the changing status of a respondent on a discrete variable, which is traditionally modeled with latent transition analysis. Of course, just as is the case with growth curves, it is possible for different groups of respondents to have different transition patterns over time. MLMM can be used to identify such groups and to link latent classes to other covariates, thus providing the categorical variable analog to GMM.

For each of these three extensions of MCA, Wang and Zhou provide the mathematical underpinnings of the approach, parameter estimation methods, and model testing methods. Any researchers who are interested in identifying latent classes of item responses or response patterns over time, and/or who wish to link such class membership to other covariates will find this to be an indispensable chapter.

In the fifth statistical analysis chapter, Braun, Kuljanin, and DeShon describe their work on some of the pitfalls of growth modeling. For reasons also discussed in relation to the Ployhart and Kim chapter, growth modeling has become increasingly common, as our field has come to recognize the importance of intraindividual variability and individual trends over time. Although growth modeling is relatively new to the organizational sciences, it has been common in other fields (e.g., economics) for some time. These fields have discovered important dangers associated with growth modeling research of which the organizational sciences are relatively unaware. In the present chapter, Braun et al. investigate stochastic trends in growth models, focusing particularly on the “random walk.”

A random walk is a longitudinal trend that is comprised entirely of random error that cumulates over time. The problem is that random walks are very difficult to distinguish from the deterministic trends that we typically hypothesize and hope to find in our growth models. That is, it is entirely possible to hypothesize a certain trend, collect longitudinal data, and find evidence of trends that seem to be deterministic and supportive of hypotheses, but are in fact due only to the cumulative effect of errors across time. The authors explain the nature of these random walks and describe the various techniques that allow one to identify, and to some degree correct for, them.

The final chapter of the first section, by Jeff Stanton, examines the use of data mining techniques in organizational research. Is there truth in the sentiment that one can have “too much of a good thing?” We suspect that the answer when the good things are data is, generally, “No.” In fact, many of us pine for larger samples. Increased access to large datasets affords organizational researchers with opportunities that have traditionally been unavailable. These opportunities, however, are accompanied by challenges that many of us have not been trained to confront. Stanton’s chapter describes the opportunities and problems associated with extremely large datasets and provides a road map for researchers interested in studying organizational phenomena using these resources.

Given the nature of data mining, many of us may be wary of, if not hostile toward, the application of such exploratory techniques. We have been conditioned to view confirmatory techniques as “real” science and are all too happy to leave exploratory techniques to the tea-leaf readers. We do this, however, to our own detriment. Although criticisms of “dust-bowl empiricism” are well targeted to particular elements of our scientific history, we must be careful to distinguish the practice of drawing confirmatory conclusions through exploratory, bottom-up techniques from using such techniques to generate research questions that can be used as jumping-off points for future studies.

Indeed, it has been argued quite convincingly that our obsession with developing and “confirming” novel theories has damaged the field. For example, Gray and Cooper (2010) suggest that this obsession has led to an incoherent literature. In a related vein, Edwards and Berry (2010) commented that increases in methodological precision have led, not to a refinement of hypotheses, but merely to an increased capacity for confirming that which we want to confirm. Gephart (this volume) urges us to explore, to learn before we set about confirming anything.

Exploratory techniques, in particular the types of analysis described by Stanton, afford us the opportunity to base our theories, in part, on observation. As Stanton notes, “there could be important reservoirs of social and behavioral knowledge that remain untapped unless more organizational researchers become comfortable with data mining tools.” We couldn’t agree more. Perhaps there is a researcher out there right now who, through data-mining techniques, is poised to uncover “Moneyball”-type principles applied to traditional organizational settings. The possibilities are certainly exciting, and, when the movie rights are sold, we will all wish that we had the foresight to take advantage of this underutilized methodology.

Of course, data analysis of any kind is pointless without good research design. The first chapter in the *Research Design and Measurement* section, by James, LeBreton, Mitchell, Smith, DeSimone, Cookson, and Lee explores the latest in personality-measurement techniques. Personality measurement is a cornerstone of research in organizational settings. Despite the widely acknowledged limitations of self-report measures, surprisingly few alternatives are available for assessing personality. For almost two decades, research has been building on a promising alternative to self-report measurement: conditional reasoning. James and colleagues’

chapter provides another success story for conditional reasoning, with this one having as its focus the measurement-of-power motive.

For more than a decade, James and colleagues have reported successful development of personality measures based on principles of conditional reasoning. In short, conditional reasoning is based on the notion that people want to believe they make choices rationally (James, 1998). In order to accomplish this, people rely on reasoning processes (i.e., justification mechanisms (JMs)). People tend to favor certain types of behavior and, in turn, develop JMs that support these behaviors. In turn, because individuals differ on various personality dimensions, people express different behaviors across situations. Further, even when the same behavior is expressed, individuals will have different reasons (JMs) as a function of individual differences on various latent variables. The term “conditional” reflects the idea that what is justifiable behavior in a particular situation is fully dependent upon the person choosing the behavior.

In the present chapter, James and colleagues apply principles of conditional reasoning to the general area of leadership, and the power motive in particular. Of particular interest, James et al. carefully distinguish the power motive from “toxic” applications of power. One can certainly appreciate that a given individual may have a desire for power, but not abuse that power if given the opportunity to do so. Alternatively, another individual with the same motive may act aggressively when given the chance. As the authors note, it is truly unfortunate that the power motive has been cast as the villain in the latter case above. Doing so has retarded research progress in this area.

One might well ask the question, “Why are there not more examples of conditional reasoning measures in our literature?” The answer is not that such measures are unreliable, invalid, or in any way psychometrically weak. Instead, the reason would appear to be the heavy lifting required to develop such tools. We find this to be an unfortunately reality. Our field should not be daunted by the time commitment required for the development of conditional reasoning measures. Indeed, we hope this chapter serves as a stimulus for personality researchers in our field to devote time and effort to the development of similar measures.

In the second research-design chapter, Gephart offers a modern review of qualitative methods. This is certainly not the first treatment of qualitative methods, but it is one of the best in terms of explaining (to those who might otherwise be skeptical) why qualitative methods represent a valuable

class of methodologies and how they should be used. Specifically, Gephart discusses the various paradigms that underlie qualitative research, such as post-positivist, critical theory/research, and interpretive and offers organizational science applications of case studies, interviews, observational approaches, document analysis, computer-aided interpretive textual analysis, and grounded theory from each paradigmatic perspective (insofar as this is possible).

Several of the studies described by Gephart are particularly noteworthy as exemplars of applications of qualitative techniques in the organizational sciences. For instance, Graham (1995) and Barker (1998) reported ethnographic studies that explored different aspects of team-based management systems. In a nutshell, through interviews and observations, both authors found that team-based systems were associated with many counterintuitive consequences, not the least of which was less individual agency than is typically the case in traditional top-down systems. These sorts of finding certainly might pave the way for targeted quantitative research, but they would have been difficult to produce with quantitative research, because such research requires one to know which questions to ask ahead of time. The ethnographic approaches of Graham and Barker allowed the nature of the phenomena under observation to emerge as the phenomena unfolded naturally, and this nature turned out to be rather different than anyone (including Graham and Barker) might have expected. Ethnography also requires a level of immersion by the researcher that is seldom present in quantitative research (e.g., Graham was at a West Lafayette automobile plant for 6 months), without which the requisite detail is unlikely to be apparent.

As examples of grounded-theory applications, Gephart describes some of his own work (Gephart, 1975, 1978). These papers describe a grounded theory exploration of a graduate student organization in turmoil. Gephart used initial observations of interactions among organization members to form initial questions that he then addressed by searching through records of prior organization activities. The answers to these questions provided the basis for more targeted data collection, with the result being a deep understanding of the genesis of the forced removal of the organization's leader.

As with the previously mentioned ethnographic examples, the Gephart examples show how a grounded theory approach can yield detailed information about a specific phenomenon, and do so in a way that wouldn't



be possible with a quantitative study. In particular, one wouldn't know which questions to ask, and of whom. It would be difficult to argue that there are no organizational phenomena about which we know very little. Indeed, new phenomena are frequently identified. For example, two of our students are examining cell-phone-app usage in the workplace as a coping mechanism for workplace stress (Kim & Niu, in progress). The present chapter does an excellent job of explaining how qualitative methods can set us on the road to understanding hitherto unseen phenomena, as workplaces change, new technologies emerge, and organizational systems evolve.

The next research-design chapter, by Dimotakis, Ilies, and Judge, describes the use of *experience sampling methodology* (ESM) in organizational research. A theme from several chapters in this volume is that the previous decade has seen a dramatic shift from cross-sectional to longitudinal designs. One of the most prominent approaches has been ESM, in which measurements of variables thought to vary within persons are taken at regular intervals, at specific times, or in response to environmental triggers.

Dimotakis et al. explain the various types of ESM design (signal based, interval contingent, and event contingent), as well as the ways that these different types can be combined. The authors are able to do this in a particularly compelling way because they can use examples drawn from their own work for nearly every sort of design. They also discuss the technologies that can be used to implement these designs, as well as the difficulties that must be overcome for various design-technology combinations.

Through reading this chapter, one certainly learns a great deal about using ESM designs. Perhaps more importantly, however, the authors' use of examples demonstrates the degree to which the results of ESM studies force us to look at even the most mainstream phenomena in a completely novel way. Until relatively recently, our field conceptualized many variables as between-person variables (i.e., stable within person), even though an argument could be made that they are more appropriately conceptualized as within-person variables. Thanks to applications of ESM designs by the authors and their colleagues, we now know that much of the variance in variables such as job satisfaction (Judge & Ilies, 2004), quality of co-worker interactions (Ilies, Johnson, Judge, & Keeney, 2011), organizational citizenship (Ilies et al., 2006), workplace deviance (Judge, Scott, & Ilies,

2006), workload (Ilies, Dimotakis, & De Pater, 2010), work–family conflict (Ilies et al., 2007), and emotional labor (Judge, Woolf, & Hurst, 2009) is within-person variance, and that this variability overlaps with within-person variance in other important variables.

To take one example, Ilies et al. (2006) reported that (1) nearly one third of the variance in organizational citizenship was within person; (2) slightly more than one third of the variance in job satisfaction was within person; (3) within-person variability in satisfaction explains within-person variability in citizenship; and (4) agreeableness, a stable trait, moderated this relationship such that agreeable people were more consistent in their citizenship, with the result being that their citizenship was less influenced by their job satisfaction. In retrospect, these results make perfect sense. A person tends to be a better citizen on days on which the person is satisfied with his or her job, agreeable people are more likely to engage in citizenship, and their citizenship is less governed by their ephemeral job attitudes than is the case for those low in agreeableness. Prior to this study, citizenship was almost always studied as a between-person variable (i.e., a person is a good citizen or not). Through the use of ESM, Ilies et al. showed that people who are typically good citizens could, on some days, be bad citizens, and that this within-person variability can be explained by job attitudes. What does this mean for the hundreds of primary studies and dozens of meta-analyses in which citizenship was treated as a between-person variable? At the very least, it means that those studies missed part of the story. How many more organizational variables are out there waiting for their within-unit components to be discovered via ESM?

In the next chapter in the research-design section, Dietz, Bedwell, Oglesby, Salas, and Keeton describe *synthetic task environments* (STEs). An STE is a combination of task and medium in which fidelity is higher than in a typical lab task, but control is higher than in a typical field study. Although it is possible to design an STE oneself, significant investments and resources are required in terms of programming skill (for computer tasks), construction (for noncomputerized tasks), or both. As a result, adaptation or customization of off-the-shelf tasks (COTS) to one's purposes is far more common. For this reason, Dietz et al. devote most of their attention to these COTS tasks.

STEs come in many different forms. After describing the principles that determine the quality of STEs in general, Dietz et al. discuss *games*, *simulations*, *microworlds*, and *virtual worlds*. For each form of STE, the

authors describe how they have been, and can be, used to study organizational phenomena, as well as the challenges posed by the use of each form.

A *game* in this context is an artificial, interactive activity and has a specific, goal-driven purpose in a specific context. Alternatively, a *simulation* is also interactive, goal-driven, and contextualized, but involves more realistic activities and/or more complex process models. Because the distinction between games and simulations is not always clear, the authors treat them together. An example of a game/simulation that can be used to study organizational phenomena is *SimCity*. *SimCity* is a decision-making game in which the participant plays the role of city planner, using survey data, crime data, etc. to make decisions about the city's development. Games/simulations such as *SimCity* are especially useful for studying contexts that are generally unavailable (the authors give the example of landing on the moon) or contain too much danger to study directly (such as reacting to a nuclear plant emergency or flying a plane). Thus, they can be used to study, for example, shared mental models in dangerous contexts without putting individuals at risk.

The authors next discuss microworlds, which are “computer-based platforms that simulate a complex work environment” and permit the active exploration of that environment. Microworlds differ from games/simulations in that the activities that they offer are less regimented, allowing the participant more freedom regarding what they do and how they do it. This makes them especially useful for the study of emergent phenomena. For example, the microworld *C3 Fire* requires teams of participants to execute the extinguishing of a wildfire, but does so in a relatively unstructured environment in which team members must gather information without even knowing which pieces of information are needed. In this way, phenomena such as norm formation, emergent leadership, and development of shared mental models might be studied.

Finally, the authors discuss virtual worlds. Virtual worlds typically have the structure of games, but they are unique in that they can allow entry through the Internet from anywhere in the world, by as many people as required. Moreover, the virtual world continues to function and change, even if a participant leaves. Thus, if that person returns, s/he returns to a different world. Virtual worlds such as *Second Life* are used by many organizations for various functions, and they should be particularly useful for studying complex, long-term phenomena. For example, many

organizations use virtual worlds for recruiting purposes. They allow job seekers to acquire information, either directly or through an interview with a virtual representative. Thus, virtual worlds can be used to identify the information sources that job seekers find most useful at various stages of the recruitment process, and to do so in an environment that has fewer demand characteristics than a typical lab study. The applications are virtually (pun intended) limitless.

After reading the Dietz et al. chapter, one is left with two lasting conclusions. The first is that STEs offer enormous advantages over more traditional data-collection platforms and provide unlimited possibilities that have yet to be fully explored. The second is that relatively few organizational scientists are using STEs to study organizational phenomena. Given the first conclusion, the lack of application is truly unfortunate. We are confident that this chapter will encourage researchers to more fully exploit this largely untapped methodological resource.

Perhaps no technique in the industrial and organizational (I/O) psychology literature is as well established as job analysis. Most of us received extensive exposure to job analysis through graduate training and are well versed in the benefits of a high-quality job analysis. One may, in fact, reasonably argue that job analysis represents one of the most important contributions of I/O psychology to contemporary organizations. Given this state of affairs, there are obviously no methodological frontiers or challenges for job analysts. Or are there?

Despite the notable strengths of job analytic methods for identifying critical job tasks and required individual attributes (i.e., knowledges, skills, abilities, and other characteristics (KSAOs)), such information often cannot fully capture the complexity of contemporary jobs. In Chapter 12, Coover describes how the application of *Petri nets* provides an opportunity to model tasks that occur in an asynchronous fashion, incorporate hierarchical job structures that involve parallel activities, and include both individuals and collectives (e.g., teams). Such dynamism and complexity are neither well captured through traditional job analysis nor well described graphically through traditional flowcharts.

Coover provides a brief summary of the historical background of Petri nets that provides a clear sense of why this method has been more commonly applied in the study of chemical processes and software design, but explains why organizational researchers should more seriously consider applying these tools. Through his examples, Coover refers to successful

applications of Petri nets to organizational settings in his own work (e.g., Coover, Salas, Cannon-Bowers, Craiger, & Takalkar, 1990) and identifies opportunities for other applications. Similar to how structural equation modeling (SEM) has forced researchers to be more specific with regard to expected relations and nonrelations between variables of interest, Petri nets allow researchers to propose and test models with even greater complexity. Also similar to SEM, the quality of the inferences drawn from Petri nets is fully dependent upon the components included (and not included) in the model. Because the visual palette for creating Petri nets may not be known to many organizational researchers, Coover provides an invaluable and accessible primer for developing models.

Coover also explicitly describes the application of Petri nets to two common organizational contexts. In the first example, he describes how Petri nets can be used to represent a three-person team operating in a dynamic environment in which individuals have unique expertise and access to information and must make decisions in parallel. The second example describes how Petri nets can be used to understand a team working aboard an air force AWACS. Because this team performs missions that have identifiable stages, the individual members have well-defined roles and responsibilities, and events may necessitate reacting to unexpected events, so that traditional workflow or job analytic methods are likely to provide an incomplete picture of the crew's task performance. As Coover notes, Petri nets are not just useful for describing particular systems, but also provide information that can be used to redesign any aspect of the system.

When organizations update technology or workflow, or otherwise redesign jobs, great care and attention are frequently given to the process or program itself. Such redesign efforts, however, are not as frequently accompanied by an understanding of how the technology will in fact be used by individuals. Petri nets would appear to be a promising tool that could be used in conjunction with traditional job analysis techniques to develop a more thorough understanding of job demands, individual skills required, and processes through which individuals currently interact. Perhaps even more interesting are those applications in which jobs do not yet even exist. This methodology would appear well suited for assisting with such future-oriented job analysis efforts (Schneider & Konz, 1989; Landis, Fogli, & Goldberg, 1998). After reading this chapter, the reader is left with an enthusiasm for using Petri nets when traditional methods

provide an incomplete picture, and, in the words of the author, “watch your model come to life!”

In Chapter 13, Adis and Thompson discuss the application of neuroimaging techniques to organizational research. Our field generally claims to be less interested in scores on manifest variables than in the latent factors that cause them, and yet we pay little attention to the neurological causes of the cognition and behaviors that drive our field. In fact, there has long been hostility in our field toward neuroimaging techniques, just as there seems to have been hostility in the neuroscience community toward behavioral research. We tend to see neuroimaging researchers as being obsessed with pretty color images, while they see behavioral researchers as ignoring the root causes of the very things that they purport to care about. It is long past time for this rift to close, and the Adis and Thompson chapter shows us why and how to do it.

These authors explain how three neuroimaging techniques can be used to study behavior and cognition in the workplace. Although there are many neuroimaging techniques, Adis and Thompson focus on *structural MRI* (MRI), *functional MRI* (fMRI), and *electroencephalography* (EEG). The two MRI techniques are based on the observation that different parts of the brain have not only different functions but also different magnetic properties, owing to differences in water concentration (MRI) or oxygen-rich blood (fMRI). MRI uses differences in magnetic properties to assess the volume of different types of brain matter. These differences, in turn, can be linked to different behavior patterns. For example, DeYoung and colleagues have linked differences in gray-matter density in areas associated with reward sensitivity to extraversion (DeYoung & Grey, 2009; DeYoung et al., 2010). Others have linked creativity to the dopamine systems of the prefrontal cortex (Takeuchi et al., 2010). This work moves us toward brain-based theories of many of our most important individual-difference variables.

One of the limitations of MRI is that it is not dynamic in nature. fMRI, on the other hand, can be used to examine neurological responses to stimuli. Active neuronal cells use oxygenated blood, and this blood usage (or, rather, its immediate consequences) is detected in an fMRI. In other words, fMRI detects the parts of the brain that are particularly energized at any given point in time. Although fMRI has not really been used in organizational studies, many researchers have suggested that it might be used to identify the parts and actions of the brain that distinguish effective

leaders from ineffective leaders (e.g., Rock & Schwartz, 2006; Peterson, Walumbwa, Byron, & Myrowitz, 2009) or to uncover the driving forces behind organizational citizenship behavior (OCB) (e.g., empathy; Marsh, Kozak, & Ambady, 2007).

Two nontrivial problems with MRI and fMRI is that they are physically cumbersome and very expensive. A single scan costs about \$500 (at least currently) and requires that the subject lie motionless in an enclosed space. Although EEG is not as precise, more freedom of movement is allowed, and it costs about \$10 per subject and tracks neuronal activity in real time. Again, various possibilities exist for the use of EEG in organizational research. The authors explain, for example, that EEG can be used to detect neuronal responses to errors and goal interference. Thus, EEG might be useful in providing feedback to participants in error-based training (i.e., where the committing of errors is desirable).

The authors provide many examples of areas of interest to organizational scientists that are appropriate for study through neuroimaging. We suspect that the reader will be able to imagine applications in his/her own areas of interest. In any case, I/O will have to come to the party sooner or later. Why wait?

The final chapter, by Dudley-Meislahn, Vaughn, Sydell, and Seeds, may strike some readers as the most heretical of all. If one were to suggest to organizational scientists that they rethink the measurement of knowledge, they might very well react with, "If it ain't broke, don't fix it." As Dudley-Meislahn et al. recognize in this chapter, knowledge-based assessments have been largely unchanged for many decades. This is owing, in part, to the relative ease of use and generally strong psychometric properties of existing measures. Although many traditional measures have been refined to take advantage of technological innovations (e.g., computer-based administration and/or adaptive testing formats), the fundamental approach to knowledge assessment has been quite consistent since at least the time of Goddard and his immigrant screening tools at Ellis Island.

The ideas presented in the Dudley-Meislahn et al. chapter do not offer a condemnation of traditional knowledge measures. Instead, these authors advocate the possible benefits of expanding our methodological toolkit when assessing knowledge. Dudley-Meislahn et al. first provide a summary of how and why we measure knowledge using the methods that we do. This brief review provides a foundation upon which they propose two

alternative techniques. Drawing from research in other areas, Dudley-Meislahn et al. then describe the *construct-generation* and *idea-generation* methodologies.

Construct generation is predicated on the assumption that individuals construct, and continually revise, personal theories that help them make sense and meaning of the world around them. The complexity of one's personal construct theory relative to a particular domain should, therefore, be useful in predicting how that person will behave in that domain. Although much of the reviewed research in support of this methodology is drawn from clinical researchers, the constructs that are targeted by these measures (e.g., interpersonal skills) are often highly relevant for organizational scientists. For example, interpersonal construct complexity has been empirically linked with communication skills (e.g., Burleson & Caplan, 1998), perspective-taking ability (e.g., Kline, Pelias, & Delia, 1991), nonverbal decoding ability (e.g., Woods, 1996), and social perception skills (e.g., Burleson, 1994). Construct complexity has also been related to effectiveness on the job, particularly for those in management-level positions (e.g., Sypher, 1984).

Because this method has often been applied with a focus on interpersonal constructs in general, Dudley-Meislahn et al. caution that tailored applications may be necessary when interest is related to other knowledge domains. Indeed, one of the presented examples, based on the lead author's own work, describes the adaptation of construct generation to measuring interpersonal construct knowledge specific to Reserve Officers' Training Corps (ROTC) cadets. Along with the clear review and summary of research using this technique, this example serves as a road map for how one can apply construct generation to similar knowledge domains.

A related methodology that Dudley-Meislahn et al. review is idea generation. Although this technique has often been applied in the measurement of divergent thinking, examples of the application of idea generation by organizational scientists, though encouraging, are still rather limited. As a field, we may be conditioned to view measures of divergent thinking as exclusive to situations in which creativity is the primary construct of interest. As noted by Dudley-Meislahn et al., however, such measures may also be linked to organizationally relevant variables such as sales strategies and performance (e.g., Sujana, Sujana, & Bettman, 1988), leadership knowledge (Mumford, Marks, Connelly, Zaccaro, & Johnson, 1998), and helping behavior (Dudley & Cortina, 2008).



If the only benefit of applying idea generation were to increase the pallet of alternative formats for knowledge assessment, we would certainly do well to explore this method in more depth. On top of this, however, Dudley-Meislahn et al. also present some evidence (Dudley & Cortina, 2008) that, at least in one context, this technique provides incremental validity beyond more traditional measures of helping behavior. If altering our conceptions of what knowledge measures “should” look like allows us to explain even more variance in important criterion variables, we should be eagerly exploring methods such as idea generation.

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## SUMMARY

The request that we made to the authors of the chapters in this book was to explain and justify the use of a particular methodological technique. Further, we asked each author to illustrate the technique through examples that would easily allow a reader to see how the technique could be applied to her or his own work. We believe the authors have responded to both of these requests even better than we dared hope. In isolation, each of the chapters should serve as a fantastic resource for those interested in learning more about these techniques.

Taken as a set, several common themes emerge from these chapters. First, our interest in intraindividual change has been, and will continue to be, buoyed by advances in analytic techniques such as LGM. Several chapters (Wang and Zhou, Ployhart and Kim, Dimotakis, Ilies, and Judge, and Braun, Kuljanin, and DeShon) provide excellent treatments of associated techniques, advances in knowledge that have accrued through their use, and potential issues to which we should be attentive when modeling change. Second, we see parallels between the ideas presented in the Dudley-Meislahn et al. chapter and those presented by James et al. In both of these chapters, the authors’ challenge the inertia that, in one case, seems to keep our personality measurement constrained to self-reports and, in the other, cultivates the belief that the methods we use to assess knowledge cannot be improved.

Third, our methodological pantry is greatly enhanced when we look outside the boundaries of our field to work performed in other areas. Several of the contributions (Dudley-Meislahn et al., Adis and Thompson,

and Coovert) describe methodologies with roots clearly outside of traditional organizational research. Additional chapters (Guastello, Kalish, and Stanton) describe methods that, though slightly more familiar to organizational researchers, are still not widely used by our field. In an increasingly multidisciplinary world, we hope that our field continues to adapt techniques and philosophies that have proven successful in others.

Finally, from these chapters, it is clear that most of our contemporary analytic techniques require advanced statistical software and/or modeling tools. Although these tools are not necessarily intuitive for many researchers, the examples provided in the included chapters should give researchers a resource upon which to build. Related, widespread accessibility of computing technology has been, unfortunately, underutilized in organizational research. The chapters by Dietz et al. and Stanton, in particular, describe methodological tools that more fully exploit these technological opportunities and advances. We will certainly not be able to put the computing genie back in the bottle, nor would we want to. Instead, we should follow the lead of these authors and fully embrace what the virtual world has to offer for our field.

In conclusion, we could not be more pleased with the contributions contained in this volume. We believe they summarize the current, and future, methodological tools that will play an important role in shaping our field in the coming decade. We hope that these chapters encourage organizational scientists to continue to push boundaries, challenge conventional thinking, and view our methodologies as vibrant and evolving.

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