Strengthening Mediation-Based Management Research: Translational Value, Statistical Power, and Covariate Decisions

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Abstract

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The goal of my dissertation is to address the gap between research and practice in management scholarships. Despite decades of methodological innovation and theoretical advancement, many organizational research findings fail to translate into actionable guidance for practitioners. Conversely, questions faced by managers are not well represented in academic research. This disconnect enlarges the research-practice gap.

To bridge this gap, my dissertation pursues two goals:

1. Advancing methodological rigor by investigating the statistical foundations of commonly used models in organizational research.
2. Improving practical applicability by clarifying the conditions under which these methods yield reliable, interpretable, and generalizable insights for both scholars and practitioners.

At the core of this project is a simple but important set of research questions:

- *How much statistical power do researchers typically have when testing mediation models, which are among the most widely used approaches in organizational research?*
- *How do design choices, such as sample size, covariates, and measurement assumptions, shape the validity and interpretability of mediation findings?*
- *What implications do these methodological considerations have for bridging the gap between academic research and organizational practice?*

By addressing these questions, my dissertation aims to provide both researchers and practitioners with clearer guidance on when mediation models are sufficiently powered, when conclusions can be trusted, and how study design decisions affect both scientific rigor and practical relevance.

The dissertation is organized into three papers, each addressing a different aspect of this problem:

- Chapter 1 develops the conceptual foundation for understanding translation in management science.
- Chapter 2 examines the statistical power of mediation models through large-scale simulations.
- Chapter 3 investigates how covariates affect the power and interpretability of mediation findings.

Together, these studies provide a systematic evaluation of mediation research in management, clarify the methodological conditions under which findings are robust, and offer practical recommendations for conducting and interpreting mediation analyses.

Acknowledgments

"Using no way as way, having no limitation as limitation."
—Bruce Lee

"It doesn't matter how beautiful your theory is, it doesn't matter how smart you are. If it doesn't agree with experiment, it's wrong."
—Richard Feynman

These two quotes have shaped my thoughts, my philosophy, and my approach to doing science. They remind me to remain open-minded in method, disciplined in evidence, and humble before data.

I would like to express my sincere gratitude to my dissertation chair, Chris, for his guidance, patience, and support. You have been more than an advisor to me. In many ways, you have been an academic father, even if my age makes that biologically questionable. More importantly, your support helped me through one of the most difficult periods of my life.

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This Ph.D. journey has taught me persistence, patience, and humility in doing science. I learned much more than I expected — not only about my area of research, but also about how to think, how to struggle, how to revise, and how to grow into adulthood.

Finally, I want to acknowledge ChatGPT, my AI assistant, and Eunice, my personal assistant, for serving as my research assistant, teaching assistant, writing partner, and secretary throughout this process. To be clear, I did not use it to simulate or generate the data, nor did I rely on it to replace statistical judgment. Rather, it helped me organize ideas, revise writing, debug code, prepare teaching materials, and move forward when the work felt overwhelming.

This dissertation is the product of many conversations, many revisions, many failures, and many small acts of support. I am grateful to everyone and everything that helped me arrive at this moment.

CHAPTER ONE
A Theory of Translational Science in Management

by
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Chapter 1

The science-practice gap refers to the disassociation of the knowledge produced by academics and the knowledge implemented by practitioners (Aguinis et al., 2020; Cascio & Aguinis, 2008). The science-practice gap has been a concern for multiple decades (e.g., Anderson et al., 2001; Bansal et al., 2012; Beyer & Trice, 1982; Cober et al., 2009; Kozlowski, 2017), and is now considered one of the Grand Challenges for management researchers (Avolio, 2017; Banks et al., 2016). Many researchers have discussed evidence-based practice as the key means to close this gap (Briner & Rousseau, 2011; Byrne et al., 2014; Gubbins & Rousseau, 2015; Hodgkinson, 2011; Kieser et al., 2015; Rousseau & Gunia, 2016; S. Rynes, 2007). Evidence-based practice could facilitate cooperation between researchers and practitioners, applying organizational science findings to companies (Lawler & Benson, 2022).

Unfortunately, the science-practice gap has proven difficult to close, with several researchers noting persistent and problematic barriers to evidence-based practice (Bartlett, 2011; Hambrick, 1994; Kulikowski, 2022; Salas et al., 2017; Tihanyi, 2020). These barriers are categorized into four disconnections: 1) lack of communication between researchers and practitioners (Banks et al., 2016; Hodgkinson & Rousseau, 2009; Hughes et al., 2011; Weathington et al., 2014); 2) lack of co-involvement between researchers and practitioners (N. Anderson et al., 2001; Briner & Walshe, 2013; Hodgkinson, 2011; Shapiro et al., 2007); 3) goal incongruence between researchers and practitioners (Markides, 2007; Pfeffer & Sutton, 2006; Rousseau, 2006; Rynes, 2007; Rynes et al., 2018; Spender, 1989); and 4) disbelief or misunderstanding towards academic research findings (Bartlett, 2011; Boatman & Sinar, 2011; Briner & Walshe, 2013; Silzer & Cober, 2011). This persistence of the problem is despite specific recommendations and efforts targeting those barriers made across decades, such as using storytelling techniques in research writing (Allen et al., 2020; Gubbins & Rousseau, 2015; Kozlowski, 2017), providing support for academics to write customized and “readable” research results for different stakeholders (Shapiro et al., 2007), and including practitioners in the publication review process (Alonso et al., 2016; Geimer et al., 2020).

Over the past few decades, the biological sciences have wrestled with similar issues and settled on translational science as a potential solution (Butler, 2008; Littman et al., 2007; Stingl, 2015; Wehling, 2015b). In contrast to management literature discussions of “translational science” which focus on simply translating findings from technical language used by academics to a language more suited to practitioners (Hambrick, 1994; Hodgkinson & Rousseau, 2009), the biological sciences use the term “translational science” to refer to a much deeper and more systematic set of processes to travel the steps required to justify moving from bench to bedside (e.g., Collins, 2011; Moore et al., 2011; Stroncek et al., 2015). One would never expect to see the medical community conduct a rigorous single study and immediately leap into deploying the treatment globally. Such a practice would be seen as irresponsible and unethical (Schork, 2015), and likely to violate regulations from the Food and Drug Administration (Center for Drug Evaluation and Research, 2014). In contrast, scholars often conduct a single study, publicize broadly the results, and recommend that organizations take action (Geletkanycz & Tepper, 2012). Making this comparison with the biological sciences presents an uncomfortable possibility: perhaps the science-practice gap in management persists in part because very little management research has traveled through the translational process for actual implementation.

As important as this insight is, it begs an important question: What should translational science look like in management? Perhaps a simple solution would be to apply existing translational science recommendations from the biological sciences to management. However, the conceptual and methodological gaps between the biological sciences and the organizational sciences makes moot much of the existing translational science framework in biology to the context of management. First, medical research employs animal testing and in vitro research early in the process and switches to human samples in later phases (Czekanska et al., 2012;

Wehling, 2015b). Second, medical research requires collaboration with not only practitioners (i.e., doctors), but also other stakeholders, such as pharmaceutical companies for drug development (Butters et al., 2006; Tao & Xu, 2009; Woodley, 2008), biomedical engineering companies for new device development (Collins, 2011; Johnson et al., 2017), and government organization for regulation (e.g., U.S. Food and Drug Administration; Goldman et al., 2013; Hampel et al., 2010; Woodcock & Woosley, 2008). Indeed, regulators have a powerful role in driving how translational science is conducted when a proposed intervention has sufficiently gone through the translational science process to be considered for approval for broad deployment (Claiborne & Olson, 2012). In order to close the science-practice gap, we need a tailored model of translational science in management.

Accordingly, the purpose of Chapter 1 is to develop a theory of translational research in management. To do so, I draw inspiration and relevant ideas from the existing translational science literature as well as existing research in management on uncertainty reduction. I begin by identifying the processes from that literature which are irrelevant to translational science in management. I then adapt those which are relevant to translational science in management. Moving beyond mere adoption, I add additional steps, drawn in part from the existing management literature on the science-practice gap and in part from the uncertainty literature in management and decision science, and arrange the concepts in a process model of translational science in management. In so doing, I describe how management knowledge and uncertainty evolves as it goes through the translational science process. Moreover, I highlight what each step in my translational process model accomplishes for various organizationally-relevant stakeholders. The resulting theory of translational science in management serves as a useful anchor for researchers to begin closing the science-practice gap, as well as fertile ground for further debate and development of ideas on how to improve the degree to which our beloved organizational sciences are positioned to make tangible impacts on real world outcomes.

EVIDENCE-BASED PRACTICE AND THE SCIENCE-PRACTICE GAP

Evidence-based Practice (EBP) uses multiple sources of evidence, including practitioner expertise, local contextual evidence (e.g., structure of an organization), rigorously evaluated research evidence, and perspectives of stakeholders (Briner et al., 2009; Rousseau et al., 2008; Rousseau & Gunia, 2016), to inform managerial decisions (Rousseau & McCarthy, 2007). EBP facilitates communication (Shapiro et al., 2007) and collaboration (Latham, 2007) between researchers and practitioners. Practitioners are not always effective at articulating their current and future needs (S. L. Rynes et al., 2007), and researchers often struggle to interpret those needs in ways that translate into actionable research. Moreover, there exists an important “middle group” of scholar-practitioners, individuals who are embedded in both research and practice (e.g., Adam Grant, Wayne Cascio), who serve as translators and connectors across the two communities. This bridging role highlights that the research–practice relationship is not a seamless loop but a dynamic and often imperfect process of negotiation, translation, and adaptation (Banks et al., 2016), and ultimately engage in critical evaluation through systematic reviews and meta-analyses (S. L. Rynes et al., 2018). This study builds on this recognition, focusing on where and how methodological rigor can strengthen the credibility and applicability of research findings, thereby narrowing the gap between research and practice.

Scientists aim to conduct rigorous research (N. Anderson et al., 2001), which is relevant to practitioners’ needs (Kieser et al., 2015), and then seek to persuade managers to learn principles drawn from management research (Rousseau & McCarthy, 2007). Ultimately, engaging in relevant (and interesting) research bring positive influences to individuals and groups in organizations (Gubbins & Rousseau, 2015).

Despite these aims of EBP, organization scientists “need to better engage the business world and close the research-practice gap” (Roberson, 2022, p. 207). This need extends beyond businesses to include government agencies, educational institutions, healthcare systems, and nonprofit organizations, as well as private industry. The gap refers to the dissonance between academic research and practice (Bansal et al., 2012). Although management scholars often conduct rigorous research, scholars often fail to translate and integrate the findings directly into guidelines for practitioners. Some organizational scientists see the science-practice gap as overblown (Shapiro et al., 2007). Nevertheless, the Practitioner Needs Survey (Cober et al., 2009) revealed that management researchers are concerned that much of organizational research cannot be implemented in organizations (Beyer & Trice, 1982; Markides, 2007). I build from this perspective that management research is still falling short of the goal to EBP.

Both researchers and practitioners are important components of the science-practice gap, highlighting that they live in different “thought worlds” (Cascio, 2007) which has limited the practice of EBP (Bansal et al., 2012). Moreover, the interests of scholars and that of practitioners are misaligned. Researchers are driven to publish in journals that value certain criteria (Banks and McDaniel 2011), including high-impact factors (Aguinis, Ramani, et al., 2017) and subjective interest from the authors' perspectives (Tihanyi, 2020). Academics may not value practitioner-oriented articles (A. S. Davis et al., 2020). Current promotion and tenure system tend to neglect translational research (Shapiro et al., 2007). Finally, there is a knowledge transfer barrier: research is written in a scientific language is inaccessible to practitioners (D. J. Cohen, 2007).

Practitioners also contribute to the gap. They often lack interest in scholarly research, assuming that as irrelevant (Pearce & Huang, 2012; Rousseau, 2006), or not credible (Hughes et al., 2011; S. L. Rynes et al., 2002). They often see practice is ahead of academic research (Silzer & Cober, 2011). They rely on their own idiosyncratic experiences instead (S. L. Rynes et al., 2018).

Some scholars have proposed some solutions regarding the research-practice gap. This could include modifying the existing career model (Kieser et al., 2015; Kulikowski, 2022) and research procedures (Banks et al., 2016; Gubbins & Rousseau, 2015; Rousseau, 2006) to better align with the goals of EBP. Incentives (A. S. Davis et al., 2020; Hambrick, 1994; Vermeulen, 2005), including promotions, awards, or other forms of recognition can be designed to encourage scholars to bridge the gap (Banks et al., 2016; Dutton, 2005). Researchers can address practical problems by conducting systematic reviews (Briner et al., 2009; Rousseau et al., 2008). Researchers can attract practitioners by hosting conferences (Hambrick, 1994) and translational publications (A. S. Davis et al., 2020; Latham, 2007). Scholars can collaborate with practitioners (Briner et al., 2009), and provide support for practitioners who are willing to implement EBP (Rousseau, 2007) .

Some of these solutions are potentially promising in curbing the science-practice gap. However, these solutions have various levels of success, with some unlikely to be implemented in the future. There is no evidence that the science-practice gap in management is closing. For example, despite the rise of open-access publishing and practitioner-oriented outlets, surveys of managers continue to show low reliance on academic journals as sources of evidence for decision-making (Rynes et al., 2002). Some even believe the gap is getting larger (Banks et al., 2016). These solutions are likely partial solutions at best. An important part is missing: a discussion of the more fundamental shortcomings of management research in academia and practice in industry. To begin, I look outside of management.

TRANSLATIONAL SCIENCE IN THE BIOLOGICAL SCIENCES

U.S. President Franklin D. Roosevelt recognized the value of science and technology for development. He requested the director of the Office of Scientific Research and Development, Vannevar Bush to develop a model for scientific development (National Academy of Sciences, 1978). In Bush's (1990) model, government invests in basic research, and private corporations translate it into commercial products. Depicted in Figure 1, this model has been implemented successfully in basic sciences (Crowley & Gusella, 2009).

Insert Figure 1, about here

Biomedical scientists define translational science as a discipline drawing basic research findings from laboratories (Littman et al., 2007; Wehling, 2015a) to clinical human application (Butler, 2008; Gilliland et al., 2019), and ultimately into treatment for patients (Rubio et al., 2010; Woolf, 2008). For example, scientists have studied aging and found that moderate dietary restriction is positively correlated with lifespan in organisms such as yeast, worms, and rodents (Fontana et al., 2010). However, finding this effect in some organisms does not necessarily indicate the same effect in human beings. Thus, scholars moved to species more genetically similar to humans (Anderson et al., 2009). Translational scientists then extended this program of research to specific human patients (High & Kritchevsky, 2015).

Basic medical research serves as a guide to medical professionals when it comes to raising significant questions (D. L. Sackett, 1997). Missing insights from basic research findings can mean missed opportunities to improve patient health (Chalmers et al., 2014). Translational biological science can be a useful example for management scholars because it has facilitated the integration of basic, patient-oriented, and population-based research into public use (Rubio et al., 2010).

According to the National Institutes of Health (NIH), there are five steps in translational biological science (*Translational Science Spectrum*, 2015): basic research (T0), translation to humans (T1), translation to patients (T2), translation to practice (T3), and translation to communities (T4). We use this spectrum as a theoretical framework for understanding how scientific knowledge moves from discovery to use. T0 basic research is "the systematic study directed toward greater knowledge or understanding of the fundamental aspects of phenomena and observable facts" (Office of the Federal Register, 2012). In the biological sciences, T0 research discovers basic information about diseases and biological processes (Collins, 2011; Karlsson, 2015; Plebani et al., 2015; Wehling, 2015b, 2015c). It typically includes animal studies that precede human studies (Bryda, 2013; Van Norman, 2019), as well as in vitro research examining biological processes captured from living tissue samples kept alive outside of the body from which they were drawn (Balouiri et al., 2016; Hirsch & Schildknecht, 2019). Although T0 studies are foundational, they are not directly implemented into practice. Instead, they create the knowledge base that later stages of translation must convert into human studies, patient care, clinical practice, and community-level impact. This distinction is theoretically important for management research because many methodological insights similarly begin as basic knowledge about how evidence is produced, evaluated, and accumulated. Like T0 research in the biological sciences, methodological research in management may identify foundational problems, such as low statistical power or replication failure, but these insights will not improve research or practice unless they are translated into usable research norms, tools, and decision rules.

T1 transits from bench to bedside through early human clinical trials to evaluate safety in a small group (Fort et al., 2017; *The Basics*, 2015; Zarbin, 2020). Following this, in T2 research, researchers shift their attention to investigate effectiveness of an intervention and establish clinical guidelines (Fort et al., 2017). Large groups of people (1000-3000) are recruited to compare the new drug/treatment to current standard's safety and effectiveness (Van Norman, 2016). The goal in T2 is to prove the usefulness of a new intervention with objective data and obtain approval from the Food and Drug Administration (FDA) (Woodcock & Woosley, 2008).

T3 seeks to translate to practice by implementing the new intervention under realistic conditions (i.e., dissemination research) (Ross-Hellauer et al., 2020). T3 research integrates the new interventions into existing programs (i.e., implementation research) (Khoury et al., 2007; Westfall et al., 2007). T3 often evaluates long-term safety and effectiveness of FDA-approved drugs or treatments on the market (*Types and Phases of Clinical Trials*, 2020). Researchers explore clinical questions and research gaps through the trial results (Curran et al., 2012).

For community translation, T4 looks at population health outcomes (*Translational Science Spectrum*, 2015), and seeks to improve global health. Outcome research describes, interprets, and predicts the impact of influences, especially interventions, on final endpoints important to decision makers (Khoury et al., 2007). Macro-level research studies well-being, mortality, and morbidity in population predicted by geographic and demographic variables, while micro-level research aims to improve patient-provider decision making in clinics (Lipscomb et al., 2004). This translational process opens the door to further develop new interventions, highlighting a feedback loop back to another cycle of T0 research.

TRANSLATING TRANSLATIONAL SCIENCE

Translational science has been especially successful in biological sciences, and evidence-based practice draws its central ideas from the medical field (Pfeffer & Sutton, 2006). Unfortunately, some aspects of translational science as developed within the biological sciences do not apply well to the field of management. I highlight these important differences from three angles: sources of data, regulations, and stakeholder differences.

One key difference is that the sources of data differ. Translational science in the biological sciences typically starts with research conducted on non-human animals (Plebani et al., 2015, p.22), and then systematically proceeds to larger and more representative samples (Mergenthaler & Meisel, 2015, p.86). Animal models are often considered a crucial first step in translational science in the context of biology research, such that it would be considered unethical to research on humans. Recently, the United States Environmental Protection Agency made a call for researchers to change from animal models to vitro testing models (Paul Friedman et al., 2020). Engaging in vitro research allows scientists to elucidate the mechanisms and pathways in biological processes, and evaluate the safety and potential toxic effects of a new potential treatment before testing in animals or humans (Institute of Medicine (US) and National Research Council (US), 2005). In vitro research receives less attention for its ethical concerns because it minimizes harm to living organisms (Polli, 2008). A particularly striking example of this type of research is lung-on-a-chip research (Huh, 2015), in which a microphysiological system can replicate a functional unit in a lung. In management research, research conducted on non-human animals would almost always be considered irrelevant at best, and perhaps even absurd enough to jeopardize the credibility of the research program. There is no convincing analog of a “lung-on-a-chip” in management research. Human behavior in organizations is influenced by a myriad of psychological, social, economic, and environmental factors (e.g., Fugate et al., 2004). Much of the first phase of translational biological sciences does not apply well to management research.

Regulation in biological sciences, especially in clinical trials, and regulation in management research differs. Management research has regulation in the form of institutional review boards (i.e., boards which review ethical issues concerning the conduct of research). The purpose of these boards is to look after the well-being of research participants. The purview of institutional review boards has no authority over how research findings might be implemented outside of the research context. In contrast, biological research has additional regulatory bodies to navigate. The Food and Drug Administration (FDA) regulates biological research involving the use of pharmaceuticals and devices to protect the rights, safety, and welfare of participants in clinical trials (Glickman et al., 2009). Much of translational science in the medical field aims to address these forms of oversight, which do not have an analogue in management research.

Rather than suggesting that management research involves fewer stakeholders than biological science, we argue that the two domains differ in how stakeholder influence is organized. Biomedical translation is often mediated through formal institutions such as regulatory agencies, clinical trial systems, health care organizations, insurers, and public health authorities. Management translation also involves many stakeholders, including scholars, journals, business schools, consultants, HR professionals, executives, employees, labor groups, policymakers, technology vendors, and shareholders. However, management research is typically translated through less formalized channels, such as consulting, executive education, practitioner-oriented publications, internal organizational decision-making, and managerial norms. Therefore, the central issue is not stakeholder breadth but the relative absence of standardized translational infrastructure in management research.

Due to these differences, the exact protocol for translational medicine cannot be directly applied to translational management. Several steps of the traditional translational science process are irrelevant and even potentially problematic for translational EBP. Specifically, animal experiments and in vitro studies in basic research are best removed from translational management. The nature of human interactions is different from that of animals, so taking animals as samples is not appropriate (Premack, 2007). The role of regulation in management research is different from biological research as management scholars are not typically required to prove that a new procedure or method is harmless, unlike biological sciences. The process of translating to human research does not apply to management.

Translational management operates distinctly from its medical counterpart, particularly in its relationship with regulation. While translational medicine operates in tandem with regulatory bodies like the FDA, ensuring that research meets stringent safety and efficacy guidelines for patient care, translational management is not beholden to such oversight. Instead, its primary responsibility is to the ethical considerations of stakeholders and the efficacy of managerial practices. This distinction is not a diminution of the rigor or significance of management research but rather an acknowledgment of the unique context within which it functions. In contrast to medical interventions, management interventions are typically seen as further removed from direct health risks and not subject to the same degree of regulatory vigilance. The core of translational management lies in ensuring that research is actionable and grounded in real-world rather than meeting regulatory benchmarks. This focus allows for more agile adaptation to the rapidly evolving landscape of management practices.

Beyond these important differences, I posit that articulating steps in a translational process is not sufficient to have a truly compelling translational science of management. The steps are a means to an end which is the basis of a theory of translational management. A theory of translational management should describe how knowledge changes through the process of translation in a manner which enables useful translation for end users (i.e., practitioners).

I posit that knowledge is translated through different states of uncertainty. New management knowledge often begins in a high state of uncertainty when mechanisms, boundary conditions, effect sizes, implementation requirements, or practical consequences are not yet well understood. Through translation, these uncertainties can be reduced, allowing research to become more usable for practice and helping to close the science-practice gap. However, uncertainty is not uniform across all areas of management or psychology. Some theories, including self-determination theory, social learning theory, and Pygmalion theory, have accumulated strong theoretical and empirical support and have already reduced substantial uncertainty about motivation, learning, modeling, expectations, and performance. These theories demonstrate that translation is possible and that well-developed research programs can provide practitioners with more actionable guidance. At the same time, even well-supported theories may retain uncertainty when applied to new settings, populations, technologies, or organizational problems. Thus, my model does not assume that management knowledge is uniformly uncertain. Rather, it proposes that translation involves identifying and reducing the specific uncertainties that limit responsible implementation. ~~Through~~

A THEORY OF TRANSLATIONAL MANAGEMENT

Scholars produce research which is intended to be of use to practitioners. However, managers must navigate uncertainty in applying this research. Uncertainty in some cases is relatively minimal. This enables practitioners to implement the principles gleaned from the research. For other lines of research, the uncertainty is so great that practitioners are broadly deterred from implementing any meaningful interventions, even if scholars believe in the usefulness of the findings and conclusions. It is uncertainty from the practitioner's perspective which is especially relevant to translational science of management. Although there will never likely be complete consensus about the uncertainty of the degree to which they should implement a given principle drawn from management research, I posit that there may still be some meaningful degree of social consensus regarding uncertainty which will be relevant in determining the translatability of management. Figure 2 reveals my conceptual model.

Insert Figure 2, about here

Uncertainty

Within the organizational science, researchers have delineated several forms of uncertainty. The first is radical uncertainty. This type of uncertainty limits the ability to form a precise question after observing a phenomenon (Knight, 1921). McMullen and Shepherd (2006) suggested that under extreme levels of radical uncertainty, individuals might be unaware of or overlook certain phenomena. Entrepreneurs often necessitate in-depth understanding and exploration in rapidly changing environments (Khandwalla, 1976). Under severe radical uncertainty, business owners may ignore potential technologies and miss out on opportunities. The movie rental industry started to blossom in early 1980s because home video systems became the standard equipment of an American family. Mainstream movie companies used rental store as their business model for decades, and ignored the potential of technologies such as DVD and internet (Raffaelli et al., 2019). Netflix changed its business model by offering DVD rental shipping services online and faded other competitors out of the market (Snihur & Zott, 2020).

Two other forms of uncertainty which are relevant to each other are creative uncertainty and state uncertainty. Creative uncertainty occurs when the research question’s objective is clear, but the method of addressing it remains ambiguous (Knight, 1921). Practitioners experiencing this are uncertain about which practice to implement (Packard et al., 2017). For example, a manager wants to market a new product to maximize its sales and acceptance among a target audience. There are numerous approaches they can consider, such as traditional advertising (e.g., television, and radio), digital marketing, and event promotions, but the team, under high creative uncertainty, is unsure about the best method to reach their target audience (Cooper, 1990).

State uncertainty refers to the difficulty in predicting an intervention's effects due to uncontrollable factors (Milliken, 1987). This type of uncertainty prevents the ability to ascertain a proposed treatment's impact or underlying mechanism. It is characterized by a lack of holistic understanding about the situation of implementation. For instance, senior managers may be unsure about how differences in employee pay influence thoughts and actions related to disengagement (Williams et al., 2006). Factors outside their control, like perceived job roles, perceived contributions, and discrepancies between actual and perceived compensation, can make it challenging for them to gauge the effects of pay disparity.

Another form of uncertainty is effect uncertainty. Effect uncertainty entails the inability to predict the consequences of a specific action in a given scenario. Individuals are not able to replicate the same effects in different samples. From Milliken's (1987) framework on effect uncertainty, I define effect uncertainty in translational management as the inability to predict a treatment's effects on a broader group (e.g., managers, and followers). Organizational scientists tend to emphasize theoretical boundary conditions and external validity when demonstrating generalizability, while researchers in other disciplines focus on effects across multiple operationalizations, samples, and dependent variables (DeCelles et al., 2019; Pierce & Aguinis, 2013). Scholars often cannot generalize findings to everyone based on a few studies, nor do scholars know if the same study can be replicated in different operationalizations (Hideg et al., 2020). Management research has been primarily focused on the United States thus far. It is often not appropriate to apply the findings from these studies to other nations (Chen et al., 2017). Differences in institutional, philosophical, and cultural values may lead to various behaviors (Barkema et al., 2015). For example, a manager seeks to implement a new policy globally, but it could be challenging to foresee the effectiveness due to language and cultural gaps (e.g., Tenzer & Pudelko, 2017).

Complexity uncertainty refers to the inability to predict behaviors or outcomes of a complex system when considering multiple simultaneous variables and interactions (Simon, 1966; Townsend et al., 2018; Zack, 1999). An individual implementing two or more interventions simultaneously may fail to obtain the effects due to complex psychological processes (Vygotsky & Cole, 1978, p.4). Scholars may not know which treatment is the best among existing solutions. Although researchers can draw conclusions via meta-analysis (Huy Le et al., 2007), a meta-analysis including both highly rigorous and poorly designed studies decreases the meaningfulness of the conclusion (Hunt, 1997, p.42; Rosenthal & DiMatteo, 2001). Scholars cannot rely purely on statistical methods (i.e., meta-analysis) to draw an integrative conclusion.

Scholars need not abandon existing treatments when bringing up a new treatment. Much like how medical professionals may prescribe multiple medications, managers may implement multiple interventions. The medical literature directs a lot of attention toward drug interactions (Center for Drug Evaluation and Research, 2020), whereas the organization science literature largely overlooks this issue. For example, mindfulness meditation (Hafenbrack, 2017) and ethics training (Kish-Gephart et al., 2010) are both empirically supported means to reduce counterproductive workplace behavior. But how well do these interventions work together? Are they redundant, do they undermine each other, might they enhance each other? Under conditions of high complexity uncertainty, combining these practices may yield an unknown range of possible outcomes.

Timing is another potentially important but generally overlooked factor (e.g., Ancona et al., 2001; Chan, 1998; George & Jones, 2000; Mitchell & James, 2001). My understanding is limited regarding the duration of effect of an intervention, as well as the point when an intervention starts to become effective. Despite the rising popularity of longitudinal management studies in recent years, establishing causality remains a challenge. This is because researchers often do not measure the duration of effects precisely (Shamir, 2011). This lack of information about how different treatments interact over time undermines generalizability..

Implementation uncertainty refers to the unpredictability involved in applying an empirically supported plan, intervention, or practice in a specific organizational setting (Rice et al., 2008).

In the field of management, this issue overlaps with the transfer-of-training and applied management knowledge literature. Baldwin and Ford (1988) define positive transfer as the degree to which trainees apply trained knowledge, skills, and attitudes to the job, generalize them beyond the training context, and maintain them over time. This literature has long recognized a “transfer problem,” although broad claims that only 10% of training transfers to the job should be treated cautiously because later scholars have criticized that figure as an under-evidenced estimate rather than a precise empirical benchmark. More directly relevant evidence comes from Saks and Belcourt (2006), who found that training professionals from 150 organizations estimated that 62% of employees applied training immediately after training, 44% after six months, and 34% after one year. These findings suggest that implementation uncertainty is not simply a question of whether an intervention is supported by prior evidence, but whether knowledge can be transferred, adapted, and sustained in a new organizational context. Meta-analytic evidence further indicates that transfer is shaped by trainee characteristics, training design, motivation, transfer climate, supervisor support, and measurement timing (Blume et al., 2010). Similarly, research on applied management knowledge suggests that even experienced managers may possess conceptual knowledge of effective management practices yet struggle to apply that knowledge in context (Baldwin et al., 2011). Thus, implementation uncertainty in management should be understood as a central mechanism in the science-practice gap: evidence may exist, but practitioners still face uncertainty about fit, fidelity, adaptation, sustainability, and effectiveness in their own organizations.

Translational Management and the Evolution of Uncertainty

Importance of Uncertainty Reduction

Management ultimately seeks to make a difference in practice. As much as managers want to apply interventions, they are concerned about uncertainties in implementation (De Meyer et al., 2002). Translational management is the key to bridging this science-practice gap (e.g., Cohen, 2007; Rousseau et al., 2008; Rynes et al., 2007). Translational applied psychologists aim to systematically reduce uncertainties, so that managers can apply them confidently.

In the following paragraphs, I discuss how organization psychologists can help managers meet the goal of reducing uncertainty. This includes holistic combination of observations and theories, systematic evaluation of interventions, accurate effect measurement, detailed analysis of experimental and actual ecologies, and close monitoring of implementation. I develop a translational model of management research which can fundamentally address the science-practice gap discussed. I begin with a discussion of radical uncertainties in discovery.

Radical Uncertainty and Discovery: Combining Daily Observations into Theories

Radical uncertainty entails a balanced incorporation of both theoretical ideas and observations. This type of uncertainty limits the ability to form a precise question after observing a phenomenon (Knight, 1921). McMullen and Shepherd (2006) suggested that under extreme levels of radical uncertainty, individuals might be unaware of or overlook certain phenomena. Managers often require in-depth understanding in rapidly changing environments (Khandwalla, 1976). Under severe radical uncertainty, managers may miss opportunities for innovation. Traditional brick-and-mortar retailers dominated the shopping landscape for years, largely ignoring the potential of e-commerce and online shopping platforms. Amazon, emerging initially as a modest online bookstore, revolutionized its approach by providing a diverse array of products and prioritizing personalized online shopping experience and efficient home delivery services. This retail strategy gradually eclipsed many established retailers who had failed to recognize and adapt to the shift (Evans & Wurster, 1999).

To address this form of uncertainty, I posit that researchers must engage in discovery. Discovery refers to the act of uncovering or finding new information, insights, or understanding that was not previously known or fully comprehended (Cambridge Dictionary, 2023). In management science, it is defined as a way that provides insight into new, unappreciated, and mis-appreciated aspects of phenomenon (Locke, 2011) Discovery involves the recognition and understanding an observed phenomenon as a problem. Kodak's situation is a classic example of a missed discovery opportunity in the business world. Despite inventing digital photography technology in 1975, Kodak failed to uncover and understand the full potential and implications of this innovation for the photography industry. They did not realize that the general public would appreciate the ease of sharing digital media, as opposed to the challenges of sharing hard copies. Meanwhile, companies like Sony and Canon recognized this consumer preference and capitalized on the digital photography wave, leaving Kodak behind (Wu et al., 2014).

Discovery differs from a straightforward application of existing science theories in that scientists recognize a phenomenon as a problem with the possibility of a solution. Before Tesla developed electric cars, the automotive industry largely overlooked the latent potential after addressing the limitations of car battery capacity. Traditional industry perspectives were constrained by the limitation of car batteries itself, and people in that industry failed to discover the potential behind addressing this shortcoming. Discovery kickstarts the model for translational management because it takes vision to overcome obstacles and turn a problem into an opportunity. In the sense of translational management, scientists systematically combine observations into theories, thereby allowing them to present the interventions as a solution.

Translational management seeks to identify phenomena by drawing inspiration from daily observations and theories in management literature. Without a scientific way to posit experimental questions, scientists run the risk of over-emphasizing any salient daily observations or any single theories that may scholars have implicit biases for. In the organizational behavior literature, organizational scientists have been trying to capture ideas for interventions through multiple perspectives. Some try to observe business operations during consulting or fieldwork (Banks et al., 2016; Briner et al., 2009; Hambrick, 1994; Huy Le et al., 2007; S. L. Rynes et al., 2007). Some let their subconscious mind take over (Bargh & Morsella, 2008) to capture the "aha" moment for new concepts (Kounios & Beeman, 2009).

At the same time, researchers tend to agree with full evaluation of former ideas from theories and past experiments, to existing managerial practices to understand what would likely work, what will not, along with the reason why (Zajonc, 1965). In this sense, the two perspectives are not contradictory: daily observations and intuitive insights can provide the starting point for idea generation, while systematic evaluation provides the safeguard that ensures only the most robust and useful interventions are advanced.

After discovering potential interventions, scientists start to design and measure methods of intervention. Managing this subsequent stage of state uncertainty and creative uncertainty allows scholars to understand what can happen in their design and proposed solutions.

State Uncertainty, Creative Uncertainty and Basic Research: What CAN Happen?

State uncertainty entails the difficulty in predicting the effect of an intervention, whereas creativity uncertainty entails the dilemma between choosing the best method of intervention. State uncertainty refers to the difficulty in predicting effects due to uncontrollable factors (Milliken, 1987). This type of uncertainty prevents the ability to ascertain a proposed treatment's impact or underlying mechanism. It is characterized by a lack of holistic understanding about the situation of implementation. Senior managers may be unsure about how pay differences influence thoughts and actions related to disengagement (Williams et al., 2006). Factors outside their control, like perceived job roles, perceived contributions, and discrepancies between actual and perceived compensation, can make it challenging to gauge the effects of pay disparity.

In contrast, creativity uncertainty arises when managers or researchers are aware of multiple possible intervention strategies but are uncertain which will be most effective. For example, Welsh and Ordonez (2014) found that subconscious ethical and unethical priming could reduce dishonesty when participants are unmonitored. However, we cannot control the exact priming effect as people may have biased moral prototype (Reynolds, 2008). Those biased individuals may not regard such events as (un)ethical incidents. Liang et al. (2012) discovered that the antecedents of promotive voice and prohibitive voice are different. It may be due to individual differences in regulatory focus (Higgins, 2002). Promotion-focused employees may engage in greater promotive voice because of achieving ideals and possibilities mindset, whereas prevention-focused employees may engage in greater prohibitive voice because of avoiding losses mindset. The main idea is that scientists may miss some uncontrollable factors in our basic research. The goal is to understand what this intervention does and whether it has an effect.

On the other hand, creative uncertainty arises when the goal of a research question is well-defined, yet the approach to tackle it remains unclear (Knight, 1921). When faced with creative uncertainty, practitioners find themselves unsure about which method or practice to adopt (Packard et al., 2017). For instance, managers want to boost their followers' job performance. Due to the variety of potential interventions available, managers under high creative uncertainty cannot find the optimal implementation. To examine how the perception of employer's fairness shapes employees' behavior, researchers may collect data repeatedly across days or weeks. Scholars can examine it by averaging levels of fairness, but we can also examine it by measuring its variance. Matta et al. (2017) showed that justice variability exacerbated the positive, daily relationship between general workplace uncertainty and stress. An entrepreneur may have a revolutionary product idea but is uncertain about the best business model. They want to choose one but are uncertain what the next model should be or how to create it. This creates a vicious cycle: great ideas and bold bets cannot and do not emerge (Venkataraman, 2004).

To address these forms of uncertainty, I posit that researchers must engage in basic research. Basic research is a type of scientific investigation that is conducted without a specific practical application in mind (Bush, 1990). Its primary aim is to gain a deeper, more comprehensive understanding of the fundamental aspects of nature and its laws. This form of research aims to explore and explain the world, rather than solve specific practical problems. Basic research is defined by the National Institute of Health as “a systematic study directed toward greater knowledge or understanding of the fundamental aspects of phenomena and of observable facts without specific applications towards processes or products in mind.”

This is the second step of my translational model because this evolves around basic research paradigms, and no longer only about ideas. As researchers make the leap from an idea to a research design, we come to understand the importance of knowing clearly what the intervention can do and explore potential improvements. Researchers need to first garner a thorough understanding of the intervention they are proposing. Basic research is crucial for comprehending the prospective impact of an intervention. As I explore ways to reduce state and creative uncertainty, researchers confirm these effects are theoretically reproducible across environments. Further uncertainty emerges as scientists explore the results of their intended intervention in specific scenarios. This instability is called effect uncertainty. Recognizing and reducing effect uncertainty is key to achieving success in subsequent stages.

Effect Uncertainty and Translation Across Contexts: What WILL Happen?

Effect uncertainty entails the inability to predict the consequences of a specific action in a given scenario. Individuals are often not able to replicate the same effects across contexts. From Milliken's (1987) framework on effect uncertainty, I define effect uncertainty in translational management as the inability to predict a treatment’s effects on a broader group.

To address implementation uncertainty, I posit that researchers must engage in translation across contexts. Translation across contexts refers to the process of examining whether knowledge, strategies, findings, or practices developed in one setting can be meaningfully adapted to another setting while accounting for differences between those settings (Crandall & Sherman, 2016; Lykken, 1968). his process is central to reducing uncertainty about generalizability. A finding may be empirically supported in one sample, industry, country, or organizational context, but practitioners still face uncertainty about whether the same effect will hold in their own setting, among their own employees, and under their own constraints.

This issue is especially important in management because organizational interventions are rarely implemented in contexts identical to those in which the original evidence was produced. For example, Kim et al. (2023) found that employees' clothing affected self-esteem and subsequent task and relational behaviors in a South Korean sample drawn from four industries. The inclusion of multiple industries provides some evidence of generalizability, but additional uncertainty remains regarding whether the effect would operate similarly across cultures, age groups, occupational groups, or organizational norms. Similarly, Engel et al. (2023) concluded that women are less interested than men in applying for startup jobs. Their use of both Dutch job seekers and U.S.-based Prolific participants represents a useful step toward testing generalizability across contexts. However, uncertainty remains about whether these findings would generalize to other labor markets, national cultures, types of startup ecosystems, or applicant populations not well represented in online panels.

These examples are not meant to suggest that any single study should be expected to test every possible boundary condition. Rather, they illustrate why translation across contexts is a cumulative process. Individual studies can provide initial evidence, but broader translational confidence emerges when a literature tests whether effects replicate, generalize, or change across theoretically meaningful contexts. Thus, translation across contexts reduces implementation uncertainty by helping researchers and practitioners distinguish findings that are broadly portable from those that are context-dependent. ~~so.~~

As noted (DeCelles et al., 2019; Pierce & Aguinis, 2013), scholars tend to emphasize theoretical boundary conditions and external validity when demonstrating generalizability, whereas researchers in other disciplines focus on effects across multiple operationalizations, samples, and dependent variables. We often cannot generalize research findings to everyone based on a few studies, nor do we know if the same study can be replicated in different operationalizations and populations (e.g., gender, ethnicity, and age) (Hideg et al., 2020). Management research has been primarily focused on the United States thus far. However, it is often not appropriate to directly apply the findings from these U.S. studies to other nations (Chen et al., 2017). Differences in institutional, philosophical, and cultural values across countries may lead to various behaviors (Barkema et al., 2015). For example, a manager may seek to implement a new policy globally, but it could be challenging to foresee the effectiveness of the policy due to language and cultural difference (e.g., Tenzer & Pudelko, 2017).

Translating laboratory practices is challenging, especially in terms of selecting a workplace for implementation. To reduce any sampling or selection bias, researchers can randomize their study trials in a hybrid manner. Hybrid experimental design in randomized controlled trials (Nahum-Shani et al., 2022) makes use of advanced wireless technologies and human-delivered components. This is particularly useful for longitudinal studies, where researcher-participant interactions are maintained (e.g., weekly, monthly, yearly). The use of smartphone apps and other applications allows participants to report in the desired point in time, condition and location. Participants are allowed to answer prompts in their office, or at night without the need to commute. Real-time responses can be collected. This dual-lens approach provides a comprehensive temporal perspective, significantly curtailing effect uncertainty.

My research in this stage ensures the new intervention works invariantly across contexts. Scientists are one step closer to successful translation of management intervention to the workplace. However, there are likely similar interventions in literature experimented for such scenarios waiting for us to pick up the best one or integrate multiple interventions as a new one. This type of uncertainty involves the evaluation and possible integration of related interventions. It advances from the previous stages where the scientist's experimentational intervention is the sole focus to another level where scientists would consider holistically other interventions done in their target contexts. This complexity gave rise to the name of the next uncertainty: complexity uncertainty. I discuss complexity uncertainty further in the next section.

Complexity Uncertainty and Translation Across Intervention Ecologies: Will It Fit?

Each company has a unique ecology. Complexity uncertainty refers to the inability to predict behaviors or outcomes of a complex system when considering multiple variables and interactions (Simon, 1966; Townsend et al., 2018; Zack, 1999). Implementing two or more interventions simultaneously may fail to yield beneficial results due to complex processes (Vygotsky & Cole, 1978, p.4). Environmental or interaction effects may be the reason (Erez & Grant, 2014).

It should become clear that translating the same intervention across companies is no simple task. There may be configurations that boost, suppress, or even cancel out the intended effect. A company has its own demographics, culture, and existing practices. There likely already exists a myriad of policies before the new proposal is used. This adds another layer of complexity to gauging the precise outcomes when managers implement the intended scheme.

This stage follows the interaction between existing interventions to the newly developed ones. Because this is no easy task, I name the science-practice gap in this area the complexity uncertainty. The question in this stage is: in which intervention ecologies will this work?

To address this uncertainty, I posit that researchers must engage in translation across intervention ecologies. Translation across intervention ecologies refers to the process of applying findings from basic research or controlled environments to more complex environments, considering the unique characteristics and complexities. This involves adapting new research-based interventions to existing literature or practices. The existing policy of the organization should be measured and accounted for when examining the effects of the new intervention.

Before implementation, scholars can run computation simulations for possible outcomes and behaviors in complex environments (i.e., workplace). These simulations, rooted in computer models, act as a mirror to reality. They distill the intricacies of the real world into core mechanisms and principles (Davis et al., 2007). Such tools have found resonance in management research (e.g., Bruderer & Singh, 1996; Kroeck et al., 1983; Vancouver et al., 2010). These studies underscore the value of simulations in drawing insights from assumptions and scenarios (Scandura & Williams, 2000). For example, Luan and colleagues (2019) simulated 1728 task environments to show that managers could make better decisions using heuristics than using rational strategies (i.e., logistic regression). Simulations can unlock a richer understanding of convoluted systems, all while keeping the cost minimal (Shaw & Ertug, 2017).

Comparing present laboratory studies with similar research emerges as an effective guide in determining whether a study design can be valid in the real world. This has been supported by multiple translational studies in the field of medicine (K. Armstrong, 2012; Conway & Clancy, 2009; Iglehart, 2009; Naik & Petersen, 2009, 2009; Sox & Goodman, 2012; Sox & Greenfield, 2009; Sullivan & Goldmann, 2011), where diagnostic tests or screening procedures are evaluated in different ways. From these comparisons, medical professionals are positioned to recognize the best methods. Comparing various options, such as different treatments for specific conditions (e.g., comparing medication, surgery, and lifestyle modifications for treating diabetes), determines the relative effectiveness of different medical treatments, interventions, or strategies.

Comparing interventions and present practices in companies provides great insight into experiments and their ecological validity. It is essential as researchers can gauge their studies' relative effectiveness in practice. Envision a study design where participants are randomly assigned to diverse conditions (i.e., new vs. established interventions). These conditions can be integrated, allowing for a synthesis of interventions on each participant (Nahum-Shani et al., 2022). Scholars can employ hybrid studies to better understand the effects (Nahum-Shani et al., 2022). The rapid progression of wireless technologies, exemplified by devices like smartphones, offers a wider reach and impact of interventions. Such technologies afford us the luxury of tailoring interventions with greater finesse and agility.

In traditional research settings, each cohort is observed in a fixed period, and intervention length is pre-determined. With a rotational hybrid design, scientists can categorically group participants based on varied time frames with assignments delivered to participants' digital devices. The length of the intervention can change. This allows a more efficient collection of data and more precise tailoring of study design according to the dynamic changes at work. Researchers can track real-time changes of emotions by sending participants daily or weekly prompts. Researchers can pinpoint the time when employees face rejection and record associated emotions without having to make an appointment in the laboratory.

Diminishing complexity uncertainty allows scientists to gauge interactions between interventions and existing workplace issues. Scientists are now a step away from full implementation: will managers agree to implement it?

Implementation Uncertainty and Implementation Science: Will Managers Do It?

Implementation uncertainty is defined as the unpredictability of implementing a plan, strategy, or design (Rice et al., 2008). After previous steps, researchers may still face this final task: implementation uncertainty, as managers may still opt not to implement the intervention.

One common reason behind this is internal competition of ideas. Should scientists pitch their intervention to one manager, the intervention may not be adopted because the target of the pitch simply was not the person in charge. In other cases, even though scientists persuade managers successfully about the feasibility of the intervention, the managers' ideas may be discounted according to their relative scope of power among other managers on the same level. Pitching to the right person, in this case, holds the key to the actualization of the intervention.

Unpredictability is central to implementation uncertainty. Even when a company decides to implement a new inventory management system to reduce waste and improve efficiency, there are unpredictable factors such as reliance on former policies. The actual implementation may encounter uncertainties such as resistance from employees who are comfortable with the old system, unforeseen technical issues, or supplier disruptions that were not accounted for initially.

There are structural barriers to implementing organizational interventions where stakeholders face myriad levels of bureaucracy within the practice to put the new policies in place (Banks et al., 2021). A big part of implementation uncertainty in terms of company structures involves establishing a new norm, where employees transition from an older to a new set of common practices. The structure and hierarchy of the company interact with the formation of norms and communities where employees overcome the strangeness one may experience when adjusting to a new intervention, despite having continued working in the same workplace.

Implementation uncertainty is shaped by the nature of the organizational context in which a practice is introduced. In stable and routine contexts, managers may face relatively low uncertainty because goals, workflows, roles, and performance standards are already well established. In contrast, implementation becomes more difficult in volatile, uncertain, complex, and ambiguous, or VUCA, contexts. In these environments, managers may be unsure whether an evidence-based intervention will fit existing routines, whether employees will accept it, whether partial implementation will create unintended consequences, or whether the practice will remain useful as conditions change. Incentives and resources are therefore important because they reduce the perceived risk of implementation. Even when managers believe that an evidence-based practice could benefit the organization, they may hesitate to adopt it if failure could harm their role, team performance, or the company. Organizations can reduce this uncertainty by providing dedicated implementation teams, formal coaching, regular evaluation, feedback systems, and clear communication about how the new practice aligns with the organization's strategy and business model. Managers are more likely to implement and sustain interventions when they operate in a supportive climate with positive social interactions and shared norms around change (Banks et al., 2021).

Incentives contribute to implementation and adherence. To avoid malicious competition, incentives including pay raises, team-level awards, and public recognition promote detailed implementation. Decision-makers are more motivated to lead with new interventions, as opposed to drawing shortcuts or aiming only to pass evaluations. Incentives promote organizational citizenship as individuals feel more valued. Allocating resources for recognition of members can develop a culture of innovation (Banks et al., 2021), especially during stressful times with short-term targets. Employees are rewarded for pushing interventions correctly, alleviating burnout.

Another prominent deterring factor against implementation is personal biases from managers due to a lack of awareness or understanding (Mueller et al., 2012). Adopting changes can be scary, even when given the confidence to do so. Tables, graphs, and figures can provide objective evidence to the effectiveness of the intervention. Still, personal beliefs and insecurity can be a huge deterrence to successful implementation (Rousseau, 2006). When an organization increases its corporate social responsibility (CSR) initiatives, it often enhances how shareholders perceive the moral integrity of the top management team. This can lead to greater shareholder support for the compensation of these top executives (Fehr et al., 2021). Still, managers might not fully grasp the benefits of CSR (Kim, 2022), see CSR as a burden (Vogel, 2007), or simply hold skeptical views about scholarly research (Hafenbraedl & Waeger, 2018) . This may lead to reluctance in adopting CSR practices, limiting potential positive impacts of CSR on compensation.

Addressing the perceived uncertainties of managers is crucial, and we must pay attention to the empirical uncertainties inherent in organizations. Even within a single practice, there exist layers of hierarchy and power dynamics across the same level of management. While the ultimate objective remains to seamlessly integrate novel practices with existing ones, it is essential to validate this integration through preliminary studies. Hybrid experimental designs can be among the best methods to discover the interaction effect and confirm that the new practice will not deter current management practices (Nahum-Shani et al., 2022). Hybrid experimental designs allow companies to minimize the risks and costs of new implementations. Practitioners should provide information, ranging from demographics to organizational structures, to scholars. Collaborative data-sharing facilitates pilot studies, allowing scholars to distill insights and minimize unforeseen uncertainties (Wagenmakers et al., 2023). Successful implementation allows managers and researchers to pinpoint barriers, curate strategies to navigate challenges, and rigorously implement and evaluate these strategies, making requisite modifications to ensure success (Grol & Grimshaw, 2003). Successful collaboration ensures no conflict between existing and new management practices. Practitioners can scale up and apply it with adjustments across teams.

Initiating training and monitoring effectiveness are important. Through management courses and trainings, employees can be acquainted with the new implementation, ensuring clarity and dispelling any misconceptions (e.g., Heshmati & Csaszar, 2023; Lacerenza et al., 2017). It is important to note that completing all steps of the model is paramount to successful implementation. Reducing only some uncertainties does not result in optimal implementation, as oversight will result in partial application and inaccurate measurement of effects.

DISCUSSION

The existing EBP conversation suggests that journals should broaden their evaluative criteria to account for practicality (Antonakis, 2017; Rousseau et al., 2008; Tihanyi, 2020), tenure systems should incentivize contributions from translational research (Shapiro et al., 2007), and encourage translating complex academic language for practitioners (Banks et al., 2016; Hambrick, 1994; Hodgkinson & Rousseau, 2009; Shapiro et al., 2007). Current discussions lack a systematic process that applies research in real-world (e.g., Collins, 2011; Moore et al., 2011; Stroncek et al., 2015).

My article demonstrates the uncertainties from the initial discovery stage to the final implementation stage. I propose solutions to reduce uncertainties, allowing teams to follow the guidelines and implement research. I hope this chapter catalyzes future translational research and helps to bridge the science-practice gap.

Theoretical Implications

Integrating evidence-based practice research with uncertainty research presents solutions to reducing the science-practice gap (e.g., Banks et al., 2016, 2021).

Enhancing Relevance of Practice Theories to Managers

Translational research ensures that theoretical constructs closely align with the complexities of the real world. The reduction of effect and complexity uncertainty eliminates potential errors during transition and takes into account organizational demographics. This increases the usability of theories in managerial decision-making.

Translational research promotes the concept of evidence-based practice, where managerial decisions are based on research and empirical evidence, rather than intuition or conventional wisdom (Banks et al., 2016, 2021; Rousseau, 2006). Theories suggest that the biggest barrier deterring practitioners from translational management is uncertainty. Researchers can help guide managers implement the right intervention. Managers relying on intuition is the consequence, instead of the cause of the science-practice gap (Aguinis et al., 2010; Banks et al., 2016; Cascio, 2007; D. J. Cohen, 2007; Rousseau, 2006; Rousseau et al., 2008). Strategic reduction of radical uncertainty and creative uncertainty helps to systematically focus the intervention. Managers and scientists can join their efforts in implementation for a specific practice. I propose that an uncertainty framework can lead to more productive implementation.

Actively Refining and Developing New Management Theories

Translational management research begins with applying existing theories to practical, real-world business. Researchers can gather empirical data. This data reflects the complexities and dynamics of actual operations, which may not be fully captured in theoretical models. When theories are tested in real-world settings, certain assumptions may not hold, or there might be variables not accounted for. Researchers can modify and refine existing theories. This iterative process involves adjusting theoretical models to better align with realities.

The focus on empirical evidence in translational research ensures that both practice and theory are grounded in solid data. Applying theories in global and cultural contexts enables researchers to understand how management principles vary. Breakthroughs often happen with unexpected results or novel contexts. This understanding can lead to the development of more universally applicable theories or context-specific adaptations of existing models.

Strengthening Theoretical Flexibility of Theories

Translational research is deeply rooted in empirical evidence. By continually testing and refining theories based on real-world data, these theories remain flexible and relevant. Having empirical grounds allows theories to be adapted in response to new findings or changing conditions. The close alignment with practical, real-world challenges ensures that theories developed through translational research are more adaptable to various settings.

Translational research helps in developing theories that can adapt to the implications of new technologies. This adaptability is crucial for theories to remain relevant in an increasingly digital and interconnected business environment. It is important for translational applied psychologists to emphasize the integration of feedback loops in implementation science, where theories are continuously tested based on ongoing application and feedback. This process inherently builds flexibility into the theoretical models. Modern organizations operate in complex and uncertain environments. Translational research fosters the development of theories that can accommodate this complexity and uncertainty, rather than oversimplifying or ignoring it, leading to more nuanced and adaptable models. Translational management research brings ethical and social considerations into the development of theories.

Future Research

Future research should focus on identifying gaps at each stage in a more detailed manner, as scientists work to advance interventions. There will be more data on implementing evidence-based practices, requiring consistent follow-up from scholars.

Developing a Feedforward and Feedback Loop

As future research examines translational management, there are likely intermediary steps that will further aid this process. What could researchers do to modularize that process? Agile and flexible approaches in the implementation of practices could involve iterative implementation processes and ongoing adjustments based on feedback and changing conditions. Research could focus on methods for involving employees in decision-making, fostering a sense of ownership, and ensuring their commitment to new initiatives.

Aside from reflecting on existing theories, translational management can develop a systematic set of measures to investigate its active influence. This includes identifying key performance indicators, and evaluation methods that accurately reflect the outcomes. The ethical and social implications of implementation, especially those that significantly alter work environments and employee roles, is another scope of future research.

Investigating Long-Term Effects of Implementation

Conducting longitudinal studies to understand the long-term effects and sustainability of implemented practices is essential in moving the field of translational management forward. Longitudinal studies are designed to track changes and outcomes over an extended period. This is crucial in management contexts where the full impact of an implementation may not be immediately apparent and might evolve over time.

These studies assess whether the impact of an implemented practice is sustained over the long term. They can reveal whether initial improvements are maintained, or still evolving. Researchers can identify patterns and trends that might not be evident at first. Longitudinal research is essential for evaluating the long-term return on investment (ROI) of management practices, considering not just immediate benefits but also longer-term impact. We can assess how different factors moderate or mediate the implementation. Utilizing advanced data analysis tools, such as machine learning, can enhance the efficiency and depth of longitudinal studies.

Establishing Best Practices and Benchmarks

A best practices framework and benchmarks should incorporate feedback from both academics and practitioners to ensure their relevance to changing business landscapes. A well-defined framework provides a clear and structured approach to conducting translational management research. Researchers can more effectively translate their findings into actionable strategies, leading to more informed decision-making and enhanced overall performance. Benchmarks play a critical role in setting standards and expectations.

The establishment of a best practices framework facilitates a common understanding among researchers and practitioners, promoting effective collaboration. Standardization ensures that findings are more comparable and transferable across contexts. This universality is key to globalized business, where organizations operate across cultural and geographical landscapes.

Practical Implications

Translational management research influences the content and methods of education. By integrating the latest findings, the gap between academia and practice is narrowed.

Bridging the Science-Practice Gap

A key aspect of translational management is collaboration between researchers and practitioners. Researchers gain insights into practical needs, while practitioners gain access to the latest findings and theoretical advances. This helps refine theories to make them more relevant and demonstrates their practical utility, encouraging their adoption. Currently, most of the burden of implementation is on practitioners. If we were to better calibrate the advice to practitioners about the scope of intervention effect, practitioners could be better informed in decision-making.

Promoting Diversity and Inclusion of Interventions

Organizational management research should take into account diversity and inclusion elements in workplaces (Henrich et al., 2010). Translational research often involves working in diverse cultural and geographical contexts, which can introduce global perspectives into management theories, making them more universally applicable. The reduction of effect and complexity uncertainty requires scientists to simulate or hold pilot studies in different groups of people to collect and compare results. Reducing effect and complexity uncertainties uncovers biases interventions have. By applying and testing theories in various cultural settings, translational research can help managers and organizations develop greater cultural competence.

Translational research brings together academics and practitioners. This collaboration can lead to more inclusive research agendas and practices, as practitioners bring in real-world insights about diversity and inclusion challenges. This approach helps in countering biases and stereotypes that may otherwise influence managerial decisions. Translational research can help in identifying and addressing systemic barriers to inclusion within organizations. By applying theoretical insights to real-world, researchers can uncover how policies, practices, and cultural norms may inadvertently perpetuate inequalities, and suggest ways to rectify these issues.

Effective Decision-making and Organizational Performance

By grounding management practices in research and evidence, translational management research enhances the quality of decision-making in organizations. Managers can make more informed decisions based on what has been empirically proven to work, rather than relying solely on intuition or conventional wisdom. The application of rigorously tested theories can lead to improved organizational performance. The practical focus of translational management research allows for the development of customized solutions tailored to specific organizational contexts and challenges.

Conclusion

The model of translational management outlines empirical uncertainties in each stage of intervention development. Skipping oversteps in the model will run the risk of failed or problematic implementation because uncertainties in each stage accumulate and snowball into increasing complications in later stages. Though challenging and often time-consuming, my model provides a step-by-step guide to ensure fruitful implementation of interventions in various managerial practices.

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FIGURE 1

Translational Medicine Process

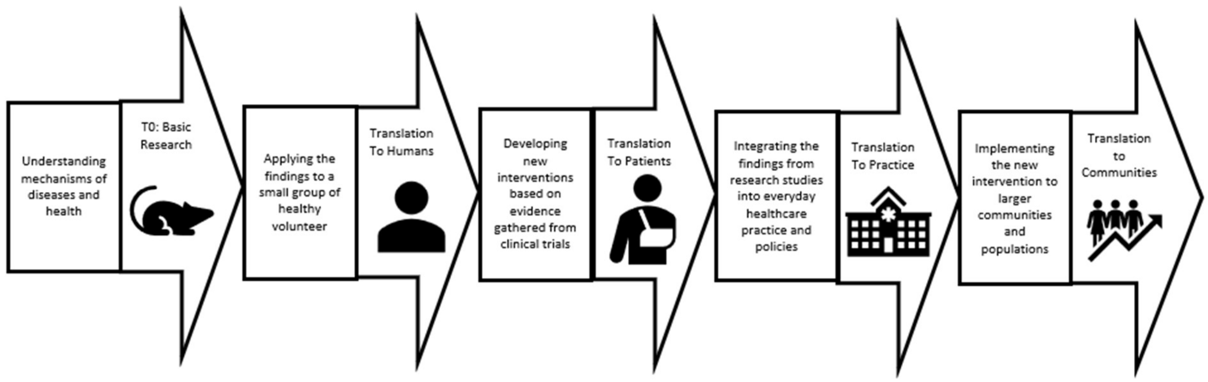
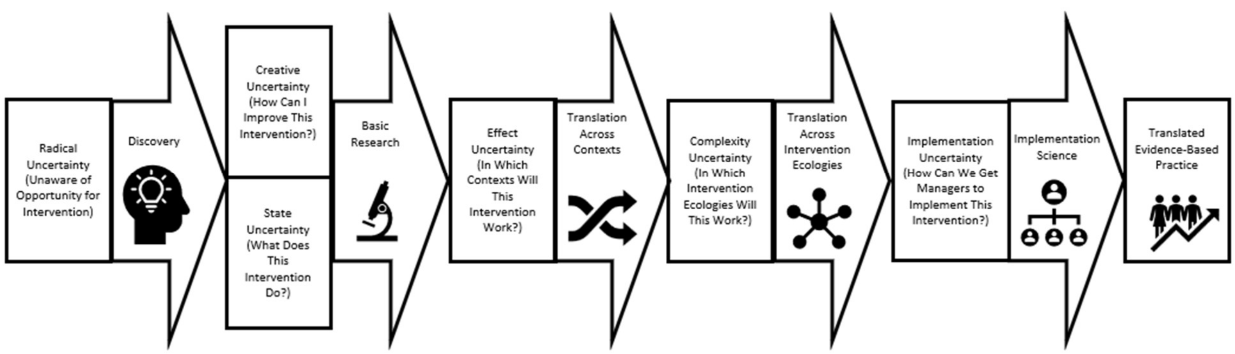


FIGURE 2

Translational Management Process



CHAPTER TWO
Powering Mediation: A Comprehensive Analysis of Statistical Power in Mediation Models

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Chapter 2

Mediation is a fundamental concept in management, enabling scholars to uncover and explain the underlying mechanisms that connect two related constructs (Aguinis, Edwards, et al., 2017; Wood et al., 2008). By introducing an intervening construct, known as the mediator, researchers can clarify how a predictor exerts its influence on an outcome. This process allows for a deeper examination of theoretical causal mechanisms, offering scholars the opportunity to test existing explanations. Mediation analysis is thus critical for advancing theory by providing a more nuanced understanding of how effects unfold within organizational contexts. Thus, empirical tests of mediation—and more relevant to the current chapter, analytical tools for drawing inferences about the results of mediation tests—are key to advancing and improving organizational theories.

Structural equation modeling (SEM) (e.g., G. W. Cheung & Lau, 2008; M. W. L. Cheung, 2007; Sardeshmukh & Vandenberg, 2017) and regression-based techniques, such as Baron &

Kenny approach (1986), the PROCESS model (Hayes, 2012), and the Sobel test (Preacher & Hayes, 2004, 2008), are the most commonly used analytical methods for testing mediation models. These techniques allow scholars to estimate the indirect effect of an independent variable on a dependent variable through a mediator, often relying on statistical significance and confidence intervals for inference (Wood et al., 2008). However, conducting these tests effectively requires adequately powered studies. Unfortunately, power analysis in mediation models is a neglected topic. This is particularly concerning when testing models with complex structures or smaller sample sizes, where statistical power may be insufficient to detect indirect effects (Hollenbeck et al., 2006). Insufficiently powered studies can potentially increase the probability of Type II errors (Aguinis, 1995; Button et al., 2013; Schmidt, 1996), which is particularly problematic because a scientific field can only flourish by reliably detecting real effects that exist, rather than overlooking them due to low power.

Based on my review of 1184 articles published between 2021 and 2023 in the Academy of Management Journal, Administrative Science Quarterly, Journal of Applied Psychology, Personnel Psychology, Organizational Behavior and Human Decision Processes, and Organization Science, I identify three especially important flaws in current mediation testing practices. First, many studies assume sufficient statistical power without explicitly justifying how they determine the required sample size for different hypotheses. This is especially the case for tests of indirect effects. Often, researchers assume that power analysis for main effects or interaction effects calculated using G*Power (Faul et al., 2007, 2009) is applicable to other effects, such as mediation, without proper validation (Moshagen & Bader, 2024). Second, when testing mediation models with multiple outcomes, many studies use inappropriate methods (e.g., PROCESS is only for models with one outcome, whereas the proposed model has multiple outcomes) to estimate effects simultaneously, which increases the risk of Type I errors (Feise, 2002; Streiner, 2015). Third, the effect of covariates on the statistical power of mediation models is often ignored entirely, despite the fact that including control variables in a simple mediation model can potentially reduce the power and thus increases Type II error (Shen et al., 2024). The current literature has discussed the effects of covariates on models lacking mediators (Antonakis et al., 2010; Becker, 2005; K. D. Carlson & Wu, 2012; Sturman et al., 2022), but the assumption that these effects extend to mediation models has been challenged (Shen et al., 2024)..

The purpose of this chapter is to introduce an improved approach to testing mediation models, with a particular focus on power analysis. The proposed approach addresses current issues by providing researchers with tools and guidelines to better estimate the statistical power of common mediation models used in organizational research. My research allows researchers to estimate the necessary sample size and compute power for various hypotheses before data collection. Additionally, the approach enables power estimation for models with multiple outcomes and those incorporating control variables. In short, my proposed approach addresses each of the three limitations I note in the paragraphs above. Moreover, based on a comprehensive review identifying a commonly used model in organization science research, I not only provide a publicly accessible tool for researchers to use my preset models, but also allow researchers to tailor the code to fit their specific models in R, making it adaptable to diverse research designs.

The Role of Power Analyses in Theory Testing

Power analysis is a statistical tool that helps researchers determine the likelihood of detecting a true effect when it exists (Ferguson & Ketchen, 1999; Scherbaum & Ferreter, 2009). Four critical components must be considered: effect size, sample size, the significance level (α, usually set at 0.05), and statistical power ($1 - \beta$, typically aimed at ≥ 0.80) (e.g., Cashen &

Geiger, 2004). Conducting a power analysis before a study allows scholars to estimate the sample size needed to achieve a desirable probability of detecting a hypothesized effect.

Without power analyses, researchers risk using a poorly chosen sample size, which can damage the credibility of the field. Specifically, studies may be either underpowered or overpowered, each presenting unique challenges. Underpowered studies primarily increase Type II error and produce imprecise estimates. In the presence of publication bias, flexible analyses, or selective reporting, low power can also increase the false-discovery proportion among published significant findings and exaggerate the magnitude of statistically significant effects. Type I errors occur when a true null hypothesis is incorrectly rejected. Underpowered studies, often characterized by small sample sizes, lead to greater variability in the data (Alasuutari et al., 2008). This increased variability can cause significant results to overestimate the true effect size due to sampling error (Altman, 1980; Halpern, 2002). Type II errors occur when a false null hypothesis is not rejected. High variability in the data makes it harder to distinguish the effect of the independent variable from random fluctuations. Consequently, the observed effect size may appear smaller than the true effect size, reducing the likelihood of detecting a significant result. When the true effect size is small (e.g., $d \leq 0.2$), studies with insufficient power are particularly prone to failing to detect the effect (Maxwell, 2004, p. 160). Small effect sizes require larger sample sizes to be detected, as their influence on the outcome is less pronounced. Studies with high levels of statistical power (e.g., > .90) may involve diminishing returns, as additional precision often requires substantial increases in sample size. In many fields, where research funding is drawn from student tuition, donations, or government support, such resources might be allocated more efficiently across multiple projects. This creates important opportunity costs in the form of other potentially valuable research that is not conducted because of insufficient resources (Cohen, 1992; Maxwell, 2004). Thus, the answer is not always that more participants are better.

Faulty power analyses often result from a mismatch between the model being used to analyze the data and the model assumed by researchers to estimate statistical power. Researchers may make incorrect assumptions, such as over- or underestimating effect sizes or selecting a statistical model unsuited to the data structure. An especially important example of this is using a single-equation model (i.e., no mediation) for a system of equations (e.g., mediation models). These errors increase the risks of both Type I and Type II errors. For example, a t-test power analysis might suggest that 80 participants are sufficient, leading to an underpowered study with a higher chance of missing important indirect effects. A mismatch between the analysis model and the power model can lead researchers to choose sample sizes that are inadequate for the effects actually tested. This primarily reduces sensitivity and precision. If the same study also involves multiple unadjusted tests, model-searching, or selective reporting, the probability of false-positive claims may increase.

Minimizing Type I and Type II errors through well-powered studies is crucial for reliable hypothesis testing. Power analysis promotes a transparent and reproducible approach to theory testing, enhancing the credibility of research findings and strengthening confidence in theoretical frameworks.

Literature Review in Management

Literature Search & Inclusion Criteria

I conducted a literature search to gather studies employing mediation models. To ensure inclusivity, I collected all research articles published between 2021 and 2023 in the Academy of

Management Journal, Administrative Science Quarterly, Journal of Applied Psychology, Personnel Psychology, Organizational Behavior and Human Decision Processes, and Organization Science. The search identified 1184 peer-review articles. I did not rely on keyword searches (e.g., “indirect effect,” “mediation,” or “mediates”) when downloading articles from the database, since mediation tests are sometimes reported in supplementary analyses or secondary models without being explicitly labeled. Instead, I systematically screened the full set of articles from these journals within the specified timeframe.

I carefully reviewed each of the 1,184 articles, including supplementary materials when available, to assess their relevance. Specifically, I examined the hypotheses and methods sections to determine whether a mediation model was employed. Studies that did not include mediation models were excluded, resulting in a final sample of 442 articles. At this stage, I also used targeted keyword searches within each article (e.g., “mediation,” “indirect effect,” “mediates”) and inspected figures or model diagrams to ensure that mediation analyses were not overlooked.

Coding

Stage 1 coding. The coding process involved several stages to account for the varying structures and variables in mediation models. Since journal articles in managment often present multiple studies, I began by coding the structure of each mediation model. For example, if a study proposed a model comprising a predictor (X), a mediator (M), and an outcome (Y), I coded it as Model 4, following Hayes's PROCESS Model framework (Hayes, 2017). Given the common practice among researchers to test multiple pathways within a model, I coded only the most complex model presented when path analysis was used, except in cases where studies employed PROCESS and researchers conducted separate analyses. For example, a researcher proposing a serial mediation model might first test the simple mediation effect (e.g., PROCESS

Model 4) before proceeding to test the serial mediation effect (e.g., PROCESS Model 6). Additionally, I documented the analytical methods used, such as PROCESS models, multilevel modeling, multilevel structural equation modeling (MSEM), structural equation modeling (SEM), and ordinary least squares (OLS). Through this initial coding stage, I identified 791 mediation models across the studies.

Stage 2 coding. I coded various aspects of study reporting, including descriptive statistics, effect size (categorized as not reported, uncertain, unstandardized, standardized, or both) and regression/path analysis coefficients (categorized as not reported, uncertain, unstandardized, standardized, or both). While regression coefficients can be considered a form of effect size, I distinguished them because authors often report indirect effects or total effects separately in mediation tables (sometimes without reporting the underlying path coefficients). Since my later power computations required access to the regression/path coefficients themselves, I coded these separately from effect size reporting. I also noted the reporting of standard errors or deviations (classified as not reported, uncertain, or included) and the handling of control variables (not reported, unclear, or included). Additionally, I documented whether a path analysis model was included (with distinctions for unstandardized coefficients, standardized coefficients, or unknown) and whether an indirect results table was presented. I further categorized each testing model type, such as moderated mediation, serial moderated mediation, or parallel mediation with multiple dependent variables, grouping them under similar PROCESS Models (e.g., Model 4, Model 6, Model 83). This categorization was crucial for identifying the most commonly used models and developing practical tools and templates for researchers. In total, I coded 1127 testing models in this stage.

Stage 3 coding. I focused exclusively on simple mediation studies (i.e., PROCESS Model 4) to investigate model power. I chose not to examine more complex mediation models, as simple mediation models require the least sample size to achieve adequate power, making them a more accessible starting point (Sim et al., 2022). Instead of coding the proposed models, I documented instances where authors performed a simple mediation test. I coded correlations between the independent and dependent variables (X → Y), the independent variable and mediator (X → M), and the mediator and dependent variable (M → Y), along with the standard deviations of these variables (X, Y, M). Additionally, I noted the analytical approach used. For example, if an author used the PROCESS framework to analyze a mediation model with three dependent variables separately, I coded this as three distinct studies, since PROCESS can only handle one dependent variable at a time. In this stage, I coded 598 testing models.

Results

For transparency and reproducibility, the data for review and code used in this chapter can be accessed at OSF. Data was simulated and analyzed using R, version 4.4.1 and the package lavaan, version, version 0.6-19. This study's design and its analysis were not preregistered.

Stage 1 coding. I identified 442 articles out of 1,184 from the journals reviewed that included at least one mediation model. However, the distribution of these articles is uneven across journals. Among the 442 articles, the *Journal of Applied Psychology* published the most (177), followed by *Organizational Behavior and Human Decision Processes* (105), the *Academy of Management Journal* (63), *Organization Science* (51), *Personnel Psychology* (37), and *Administrative Science Quarterly* (9). For this count, I defined "research articles" as empirical studies reporting original data analyses. Editorial letters, commentaries, and meta-analyses were excluded.

When considering the proportion of mediation model articles relative to the total number of research articles in each journal, *Organizational Behavior and Human Decision Processes* published the highest proportion (105/184 = 57.1%), followed by the *Journal of Applied Psychology* (177/333 = 53.2%), *Personnel Psychology* (37/77 = 48.1%), the *Academy of Management Journal* (63/217 = 29.0%), *Organization Science* (51/290 = 17.6%), and *Administrative Science Quarterly* (9/83 = 10.8%).

These results reveal that mediation models are more likely to be implemented in journals focused on micro-level management research.

Stage 2 coding. I discovered that not all testing models matched their proposed designs. Although many models followed Hayes's PROCESS Model framework, several included multiple parallel mediators or dependent variables for which Hayes's PROCESS Model is not suitable. Some researchers still used PROCESS to analyze their data. For example, a researcher may run PROCESS Model 4 twice when proposing a simple mediation model with two dependent variables. Researchers ran separate tests rather than testing the model simultaneously. Additionally, I observed instances where different statistical tests were used on the same dataset, such as examining interaction effects before testing for moderated mediation. This led to 184 mis-tested models, inflating the risk of Type I errors. When running repeated analyses on the same data, using 0.05 as a criterion for obtaining significant results is not meaningful (e.g., Armstrong, 2014; McEwan, 2017; Sedgwick, 2012). I analyzed all 1091 testing models, including 73 theoretical models. The top 10 testing models are listed in Table 1, with the remainder in Appendix 1 at OSF. The simple mediation model (Model 4) (41.9%) was the most frequently implemented, followed by the moderated mediation model (Model 7) (31.0%).

Insert Table 1, about here

Stage 3 coding. I conducted two main analyses using Stage 3 codes (see Appendix 2): power analysis and an assessment of analysis usage. As previously described in the coding section, this stage focused on simple mediation models, including variations such as models with two dependent variables, parallel mediation, and models with two independent variables. I utilized the power analysis application developed by Schoemann et al (2017). By inputting the sample size, correlations, and standard deviations from my coding, I calculated the power for each study. Although it is well known that a posteriori or "post-hoc" power analyses are generally not recommended for interpreting the results of individual studies (e.g., Gillett, 1994; Griffith & Feyman, 2021; Yuan & Maxwell, 2005), my aim is different. Specifically, I seek to assess the statistical power of studies in this domain as a means of understanding typical practices and their implications for mediation analysis. Similar approaches have been utilized in other fields, such as neuroscience and psychology (see Button et al., 2013). However, I were only able to compute power for 276 out of 551 models, because the remaining lacked sufficient information[1].

Notably, 79% (438 out of 551) of the studies did not provide an explanation for their sample size estimates. Of the 21% that did, only one study used the appropriate mediation power analysis reference (Fritz & MacKinnon, 2007). The rest cited G*Power to perform power analyses, often assuming a medium effect size of 0.5 (r) (J. Cohen, 1992) without adequate justification despite evidence that the average effect size (r) in psychological studies is around 0.25 (Kühberger et al., 2014). While some may argue that power analysis is a fundamental step

[1] I captured the essential information from the paragraphs, table, and path analyses.

that researchers may have completed but chosen not to report, I provide evidence suggesting this is not the case.

My review indicates that organizational studies are generally underpowered. Across the 276 mediation tests analyzed, the average statistical power was 0.53, and the median was 0.60. Moreover, 190 tests failed to meet the commonly accepted power threshold of 0.80 (see Figure 1). Even among the studies with sufficient power, 70 studies had power over 0.90, suggesting that perhaps more resources were devoted to these studies than might have been warranted, considering the opportunity costs. However, it is worth noting that large sample sizes can enhance the reliability, replicability, and credibility of findings, particularly for testing important hypotheses where a greater than 80% probability of detecting the hypothesized effect is desirable. For underpowered studies, the average sample size was 283.2 (SD = ± 178.5) participants. For studies with adequate power, the average sample size was 415.7 (SD = ±132.4) participants. As expected, power was positively correlated with sample size (e.g., Faul et al., 2007, 2009; Scherbaum & Ferreter, 2009).

Insert Figure 1, about here

Power analysis for mediation models

My review revealed that mediation models are prevalent in organizational studies, yet researchers often fail to conduct appropriate power analysis before data collection. This oversight can lead to inconclusive results, limit the generalizability of findings, and waste resources due to a heightened risk of Type II errors.

To discuss the importance of power analysis in mediation models, I focus on the simple mediation model presented in Figure 2 (Baron & Kenny, 1986; MacKinnon, 2007). This model can be expressed in two regression equations:

$$M = i_1 + aX + e_1 \ldots (1)$$

$$Y = i_2 + c'X + bM + e_2 \ldots (2)$$

Scholars can assess three possible effects in a mediation model: the indirect effect (i.e., a*b), the direct effect (i.e., c'), and the total effect (i.e., c = a*b + c') (e.g., Preacher & Hayes, 2004, 2008). Current mediation power analysis tools primarily focus on estimating the power of the indirect effect (e.g., Fritz & MacKinnon, 2007; Qiu & Qiu, 2018; Schoemann et al., 2017b; Thoemmes et al., 2010; Zhang, 2014). Although these tools are excellent for research questions centered solely on the indirect effect, they often overlook power analysis for other hypotheses.

Moreover, addressing power analysis for mediation models alone is not enough for organizational scientists. My research frequently includes moderators to specify when or for whom an effect occurs. For example, first-stage moderated mediation (i.e., Process Model 7) introduces a moderator between the independent variable (IV) and the mediator, which can either strengthen or weaken the effect. This model can also be expressed using two regression equations:

$$M = i_1 + a_1X + a_2W + a_3X * W + e_1 \ldots (3)$$

$$Y = i_2 + c'X + bM + e_2 \ldots (4)$$

Compared to the regression equations for simple mediation, two additional terms (a_2W and a_3X*W) are introduced in the regression on the mediator. Based on my review, I find that researchers are most commonly interested in five key effects: the indirect effect (a_1*b), the moderated indirect effect (a_3*b), the indirect effects conditional on moderator ($a_1 + a_3$* [average of W ± standard deviation of W]*b), and the total effect (c' + a_1*b). I will investigate the impact of these moderated effects on mediation power analysis in subsequent sections.

In management research, scholars often use the causal steps approach (Baron & Kenny, 1986) to test mediation models and to develop hypotheses grounded in theory (Aguinis, 1995; Mathieu et al., 2008; Wood et al., 2008). For example, the first hypothesis might posit a positive relationship between the independent variable (IV) and the dependent variable (DV); the second, a positive relationship between the IV and the mediator (ME); the third, a positive relationship between the ME and the DV; the fourth, that the ME mediates the relationship between the IV and the DV; and the fifth, that a moderator (MO) strengthens the indirect effect.

In practice, beyond deriving hypotheses from theoretical frameworks, researchers must consider which analytical approaches to implement. For example, the indirect effect may be significant even if the direct relationship between the IV and DV is not, especially when using structural equation modeling (e.g., Aguinis et al., 2017). In the following sections, I will illustrate how the power of the direct effect, indirect effect, total effect, and moderated indirect effect can vary, using Monte Carlo simulations (Donnelly et al., 2023; Xu et al., 2024; Zhang, 2014).

Insert Figure 2, about here

I conducted a series of Monte Carlo simulation studies to explore the relationship between sample sizes and standardized regression coefficients across 11 mediation models derived from a systematic literature review. Unlike analytic approximations or post-hoc analyses, Monte Carlo simulations make it possible to isolate the effects of specific design factors and evaluate their impact across multiple model forms.

To ensure reproducibility and provide additional methodological clarity, the simulation code, including specific parameter settings and data generation procedures, is publicly available in Appendix 3. Readers can refer to this repository for comprehensive details and to replicate my findings.

For models without moderators, I used the simulateData function from lavaan to generate data under the assumption of multivariate normality. For models involving moderators, I simulated data using rnorm to create interaction terms, because simulateData does not support generating data with interaction effects. This approach ensured the generated data aligned with the theoretical structures of the respective models. Importantly, all simulated data for the models were initially simulated under the assumption of multivariate normality, with interaction terms subsequently constructed from these variables.

Each power analysis involved 3,000 replications. Power was calculated for the structural equation modeling of regression coefficients of the indirect effect (e.g., a and b for the simple mediation model) at values of 0.15, 0.39, and 0.59 simultaneously, while I set the direct effect coefficient (c') to 0, 0.15, or 0.39. These coefficient ranges align with values used in prior simulation studies (e.g., Fritz & MacKinnon, 2007; Sim et al., 2022; Thoemmes et al., 2010). Sample sizes ranged from 100 to 1,000 participants, adjusted for model complexity, with more complex models such as serial mediation requiring larger sample sizes to achieve adequate power (≥0.8).

For illustration, I focused on the simple mediation model and the moderated mediation model, as these models are the most intuitive to understand and among the most popular in organizational research. Readers can refer to Appendix 4 for results on additional models. For the simple mediation model, I calculated the power of specific effects (i.e., direct, indirect, and total effects) by varying sample size and regression coefficients. My findings revealed that the power of these effects changes across conditions. For example, when the sample size is 600 and the regression coefficients (a, b, and c') are 0.15, the power of the indirect effect, direct effect, and total effect are 0.82, 0.95, and 0.99, respectively. When $c' = 0$, the rejection rate for the direct effect was .052, which is an empirical Type I error estimate rather than power.

Focusing on the power of the indirect effect, I found that a simple mediation model with regression coefficients of 0.15 across variables requires a sample size of 600 to achieve ~0.8 power. By contrast, a serial mediation model with two mediators requires a sample size of 800, and a serial mediation model with three mediators requires 1,000 participants to attain similar power levels.

For the moderated mediation model, I calculated the power of specific effects, including the direct, indirect, total, moderated indirect, and conditional indirect effects based on the moderator. The setup was identical to that of the simple mediation model. However, the addition of a moderator results in power values for the direct, indirect, and total effects of a moderated mediation model that differ slightly from those of a simple mediation model. For example, with a sample size of 600 and regression coefficients for the direct, indirect, and moderating effects set at 0.15, the power of the indirect effect, direct effect, and total effect for the moderated mediation model were 0.84, 0.95, and 0.99, respectively. These values are similar to those of the simple mediation model (0.82, 0.95, and 0.99). This suggests that the sample size requirements for moderated mediation models are comparable to those of simple mediation models. However, researchers may sometimes focus on groups that score higher on the moderator (+1 SD), as per the analysis of simple slopes (Hayes, 2017). In this case, the power increases to 0.94, making the study overpowered (i.e., Power > 0.9) if this specific effect is the primary focus. This highlights the importance of tailoring sample sizes to the particular effects of interest to avoid unnecessary resource expenditure.

In conclusion, as most research studies report their findings using either structural equation modeling or PROCESS models which can test all hypotheses simultaneously (Hayes, 2017), it is critical to conduct power analyses tailored to each hypothesis. Properly accounting for these differences ensures that studies are neither underpowered nor overpowered, leading to more reliable and interpretable results.

Control variables

In management research, control variables, derived from theoretical frameworks, are used to rule out alternative explanations (Bernerth & Aguinis, 2016; Mändli & Rönkkö, 2023; P. R. Sackett & Mullen, 1993). Empirically, control variables help isolate the theorized relationship by accounting for confounding influences, allowing researchers to test hypotheses with greater precision and increased statistical power (Aguinis et al., 2021). For example, in examining the relationship between leadership style (independent variable) and job performance (dependent variable), factors such as organizational size, job tenure, or educational level may independently influence job performance. Including such covariates is essential to minimize extraneous influences and focus on the unique effect of the predictor, ensuring alignment with theoretical propositions.

While the use of control variables enhances theory testing by reducing extraneous variance, their improper inclusion can increase the risk of both Type I and Type II errors (Becker, 2005; Mändli & Rönkkö, 2023; Sturman et al., 2022). Type I errors may occur when control variables share variance with the independent variable (IV) but not the dependent variable (DV).

In such cases, the control variable can artificially alter the relationship between the IV and DV, increasing the likelihood of incorrectly concluding that a significant relationship exists when it does not (i.e., a false positive). Importantly, this represents an error of inference, falsely rejecting the null hypothesis. Similarly, failing to include relevant control variables can inflate the explained variance in the DV (Becker, 2005; Spector & Brannick, 2011). Type II errors, on the other hand, can arise when control variables unnecessarily reduce the variance associated with the IV, often due to errors in their selection or application (Sturman et al., 2022). Additionally, control variables can produce suppression effects, where significant results emerge even when the IV has little to no true effect on the DV (MacKinnon et al., 2000; Li, 2021).

Much of the existing literature focuses on single-equation models (which do not have mediators), and the assumption that these effects generalize to mediation models has recently been questioned (Shen et al., 2024). Shen and their colleagues (2024) demonstrated that including a control variable can significantly reduce the power of a simple mediation model under conditions of moderate to strong positive correlations between the mediator and the covariate (e.g., 0 to 0.7). This raises the question of whether the same power reduction effects occur in more complex mediation models.

To explore the impact of covariates on the power of mediation models, I conducted simulations by varying the regression coefficients of the covariate on both the mediator and the DV (e.g., 0.1, 0.3, and 0.5). Using the simple mediation model as an example, the regression formula can be expressed:

$$M = i_1 + aX + d_1 CV + e_1 \ldots (5)$$

$$Y = i_2 + c'X + bM + d_2 CV + e_2 \ldots (6)$$

Each condition in the model without a control variable includes nine combinations of d_1 and d_2 , derived from three selected values for each condition. For instance, with a sample size of 600 and regression coefficients of a, c', and b set at 0.15, I simulated nine scenarios to evaluate the impact of a covariate. Given the potential for simulation durations to extend significantly—ranging from a few days to over a year (e.g., a simple mediation model with four control variables could involve $3^4 = 81$ combinations) —I limited my exploration to three different sample sizes. These sizes were chosen to ensure at least one scenario achieved sufficient power (i.e., 0.8) based on prior simulations using low regression coefficients and no control variables.

The complete simulation results are available in Appendix 5. Surprisingly, the simple mediation model was the only common model used by organizational scholars that exhibited a decreased power phenomenon when a covariate was included (Shen et al., 2024). For other mediation models, the inclusion of a covariate did not statistically alter the power of any effects of interest, regardless of whether the regression coefficients were small or large. In other words, adding a control variable did not affect the power of other mediation models. Since management research often includes multiple covariates due to existing literature or theoretical considerations, I strongly recommend that researchers conduct power analyses that incorporate theoretically meaningful covariates to anticipate and prevent unexpected results.

A Toolkit and Best Practices

Existing approaches for sample-size planning for mediation models have exclusively focused on planning for power. Existing resources, such as powerMediation (Vittinghoff et al., 2009), bmem (Zhang, 2014; Zhang & Wang, 2013), mc_power_med (Schoemann et al., 2017), semPower (Moshagen & Bader, 2024), and WebPower (Zhang, 2018), offer significant utility for researchers conducting power analyses for mediation models. However, each of these tools is limited in specific ways, such as handling models with covariates, or addressing more complex (in)direct effects beyond predefined paths. While these tools provide valuable insights, there remains a lack of a unifying framework or guideline to teach researchers how to conduct power analyses effectively across a variety of mediation contexts.

Insert Table 2, about here

Having highlighted some of the issues in power analyses in the context of mediation, multiple dependent variables, and control variables, and the limitation of existing tools, I now move on to solutions. Second, in this section I present a comprehensive toolkit for conducting power analysis. I provide a step-by-step guide to help researchers estimate the power of different hypotheses simultaneously, tailored to their specific models. Given the infinite ways to conceptualize and hypothesize a model, I offer customizable templates for readers to adapt. There are three key points to keep in mind:

1. The toolkit uses standardized regression coefficients, chosen for their simplicity. For readers interested in using correlations and standard deviations, these terms can be derived using the formula $\beta = R_{xx}^{-1} r_{xy}$ ... (7) (Bollen, 2014). However, deriving correlations from regression coefficients can become challenging as models grow more complex, requiring a solid understanding of systems of equations.
2. Sample size can be a constraint, especially in field studies. Although researchers may derive sophisticated models from theories, this does not guarantee that the model can be tested empirically with sufficient power. If a researcher's study relies on a field study population with limited sample sizes, they should consider simplifying the model—for

instance, reducing a serial mediated moderation with parallel mediators to a simple mediation model. Otherwise, the study risks being underpowered, leading to spurious or unreliable results.

3. Customization of code to fit a given model is essential. There is no one-size-fits-all solution, especially for models that deviate from PROCESS models (e.g., multiple dependent variables, multiple control variables, or multiple first-stage mediators in a serial mediation model). As noted, the power of the indirect effect in a serial mediation model is typically lower than in a simple mediation model when sample sizes and regression coefficients are comparable. In management research, models are often tested incrementally across different studies, each of which requires a distinct power analysis. Moreover, each hypothesis within a testing model may necessitate multiple power analyses.

Instructions

For transparency and reproducibility, the templates in this chapter can be accessed at OSF. Data was simulated and analyzed using R, version 4.4.1 and the package lavaan, version, version 0.6-19. This study's design and its analysis were not preregistered.

First, researchers should identify a template that aligns with or closely resembles their testing model. Mediation models can generally be categorized into broad types based on common structures observed in prior studies. For example, two prevalent categories include: (1) mediation without a moderator, or (2) mediation with moderator(s). These categories are not exhaustive but represent higher-order groupings that encompass many subcategories of models. For models in the first category, researchers can often pick one of the provided templates and customize them as needed. For instance, researchers could consider a testing model where one independent variable (IV) influences two mediators, which in turn influence three dependent variables (DVs), as illustrated in Figure 3. The closest template available in this chapter is Process Model 4 (2ME-2DVs). For simplicity, I used the basic Process Model 4 template for illustration purposes (see Figure 4a). I then adjusted the coding to match the structure shown in Figure 3 (see Figure 4b).

Readers will need to add equations to their models in a manner similar to path analysis. For the proposed model with two mediators and three DVs, 11 equations are required to fully specify the model, compared to just three equations for a simple mediation model. Additionally, if covariates are included, the code must be modified further by incorporating the control variables into each equation (see Figure 4c).

Insert Figure 3, about here

Insert Figure 4a, 4b, & 4c, about here

For models involving interaction effects, please refer to templates such as Process Model 7, Process Model 14, or Process Model 83, as the simlateData function from lavaan cannot simulate interaction effects (Rosseel, 2012). When the simlateData function is not applicable, additional steps are required to simulate data. Using a simple moderated mediation model (e.g., Process Model 7) as an example, the steps are as follows:

1. **Generate Variables**: Create each variable individually (e.g., X ← rnorm(n)), except for the dependent variable(s) and mediator(s). For this study, I simulate data under the assumption of a multivariate normal distribution, consistent with the assumptions of structural equation modeling (Bollen, 2014). Furthermore, I assume that all variables are

continuous, and all regression coefficients are standardized to enhance comparability and interpretability.

2. **Define Equations with Error Terms**: Similar to models without interaction effects, define the equations for your model. However, for interaction terms, include error terms (ζ) in the equation (i.e., include rnorm(n, sd =1)).
3. **Integrate Variables into a Dataset**: Combine all variables into a simulated dataset (e..g, sim_data <- data.frame(X = X, W = W, CV = CV, M1 = M1, Y = Y)).

Insert Figure 5, about here

The instructions for models with interaction terms also apply to models without interaction terms. Readers are free to choose the most suitable methods for their specific models, but for models with interaction terms, it is essential to follow the steps outlined above. To conclude, readers should modify the templates whenever discrepancies arise between their testing models and the provided templates, as my review identified 160 unique models from 945 studies.

For sample size determination, readers can specify multiple values (e.g., sample_sizes <- c(459, 700, 800, etc.); see Appendix 6). Based on my simulations, which assume all coefficients are small (i.e., 0.15), I offer the following rule of thumb: a mediation model with one mediator requires a sample size of 600 to achieve sufficient power (~0.8), a serial mediation model with two mediators requires 800, and a serial mediation model with three mediators requires 1,000. I recommend using these values as a starting point for the first iteration if the model is similar to those described.

Next, researchers need to estimate standardized regression coefficients for the relationships between variables. Instead of relying on the 'traditional practice' of assuming a medium effect size (d = 0.5), I provide three alternative approaches:

1. **Pilot Test**: Pilot data can inform feasibility, measurement quality, variance estimates, and plausible effect-size ranges, but pilot effect estimates are often imprecise. When possible, effect-size inputs should be based on meta-analytic evidence, well-powered prior studies, or a sensitivity grid spanning conservative, expected, and smallest-effect-of-interest values. Although it is true that the sample size in pilot tests is often small and may be underpowered, this approach is still preferable to skipping pilot tests altogether. If the coefficients are consistent across pilot samples, a researcher can confidently use them to simulate power.
2. **Literature Review**: Management research sometimes involves field studies where resources are limited, making pilot tests in the target population impractical. In such cases, conducting a literature review is the next best option. By reviewing prior studies, a researcher can identify relationships between variables and directly apply the reported coefficients to their analysis. If exact matches for the variables are unavailable (e.g., for novel constructs), it may be reasonable to consider using related constructs. For example, if no studies examine the relationship between authentic leadership and creativity, one could use findings on the relationship between transformational leadership and creativity as a proxy. In many cases, organizational studies report either standardized coefficients or a mix of standardized and unstandardized coefficients. To convert unstandardized coefficients into standardized ones, multiply the unstandardized coefficient by the ratio of the standard deviation of

the independent variable to that of the dependent variable (i.e., $\beta = b * \frac{SD_X}{SD_Y} \ldots (8)$).

For example, if regressing a mediator on a dependent variable, the numerator would be the standard deviation of the mediator.

3. **Educated Guess from My Review**: If resources for pilot tests are unavailable and no relevant literature exists, a researcher can reference the findings of my review. Given the general underpowered nature of many organizational studies, I recommend conservatively assuming small effect sizes (±0.15 or less) for unknown relationships. This approach should be treated as a last resort, as not all effects are small and this assumption may limit precision.

For researchers interested in using correlations to determine power, but who prefer not to derive the relationship between regression coefficients and correlations using formula (8), they can use cor(sim_data) to generate a correlation table from the simulated data. It may take a few iterations to find the appropriate values.

Next, a researcher should define the mediation model for analysis (e.g., Figure 6). Similar to step 1, this entails delineating the paths of the model. In a simple mediation model, there are three paths to define: a (the effect of X on M), b (the effect of M on Y), and c (the effect of X on Y). These coefficients (a, b, and c) represent the parameters you will estimate, and they can be renamed for clarity if needed.

After defining the paths, a researcher should specify the effects of interest. Using a simple mediation model (Process Model 4) as an example, the most commonly reported effects are the indirect effect (i.e., a*b), direct effect (i.e., c’), and total effect (i.e., a*b + c’). As noted in the review section, organizational studies often follow the causal steps approach (Baron &

Kenny, 1986), where hypotheses are developed sequentially—from the main effects between variables to the mediation effects. To fully delineate the proposed model, researchers might add two additional effects to Figure 6:

1. The effect of X on M: $effect\_XonM := a$.
2. The effect of M on Y: $effect\_MonY := b$.

Insert Figure 6, about here

Researchers can compute power after extracting the p-values of specific effects during the simulation process (see Figure 7). Unlike post hoc analysis, this approach involves repeating the simulation 3,000 times. Power is then calculated by determining the proportion of p-values less than 0.05 across the total number of replications (i.e., "reps" in Figure 7). To begin, readers must modify the code to capture the p-values for the effects they aim to analyze. For example, if additional main effects are included (as described earlier), the following steps should be taken:

1. Add variables to store the p-values at the beginning of the script:
   a. $effect\_XonMpval\ <-double(reps)$
   b. $effect\_MonYpval\ <-double(reps)$
2. Capture the p-values for the specified effects during the simulation:
   a. $effect\_XonMpval[i]\ <-pe\$pvalue[pe\$label\ ==\ "effect\_XonM"]$
   b. $effect\_MonYpval[i]\ <-pe\$pvalue[pe\$label\ ==\ "effect\_MonY"]$
3. Calculate the power of these effects by storing the results in the session:
   a. $Power\_effect\_XonM\ <-1\ -\ sum(effect\_XonMpval\ >\ 0.05)\ /\ reps$
   b. $Power\_effect\_MonY\ <-1\ -\ sum(effect\_MonYpval\ >\ 0.05)\ /\ reps$

By following these steps, researchers can calculate the power of each effect and ensure that their analysis adequately accounts for the statistical significance of key relationships.

Insert Figure 7, about here

For researchers who modify the templates, it can be helpful to verify whether the simulated coefficients align with the values specified in the model. For instance, if one is interested in the coefficient of X on M (i.e., a), one can add the following line to the section of the template that extracts p-values:

$$coeff\_a[i] \ < - \ pe\$est[pe\$label \ == \ "a"]$$

Additionally, including the following line in the section that stores the power result enables the calculation of the average of the simulated coefficient:

$$coefficient\_of\_a \ < - \ sum(coeff\_a) \ / \ reps$$

Once these modifications are made, a researcher can run the script and obtain results. Two scenarios are possible:

1. If the power of the targeted effect exceeds 0.8 (e.g., Power > 0.8), the study is adequately powered, and no further iterations are necessary.
2. If the power is below 0.8, it is possible to adjust the sample size and re-run the simulation. For example, as shown in Figure 8, when a = 0.10, b = 0.30, and c = 0.05, the power of the indirect effect is only 0.20 with a sample size of 600. Increasing the sample size to 800 may raise the power to an acceptable level. This iterative process continues until the targeted effect achieves sufficient power.

If a research question focuses solely on the total effect, Figure 8 demonstrates that a sample size of 600 is sufficient to achieve 0.80 power for the total effect. For researchers who wish to export the results to an Excel file, they can modify the path in the last line of the script to specify your desired storage directory.

Insert Figure 8, about here

Additionally, when including covariates in a model, it is important to assess the power differences between a model with covariates and one without covariates. Although Shen et al. (2024) demonstrated the suppression effect of including a single covariate in a simple mediation model, the current literature does not provide enough evidence to generalize this finding to other models (e.g., simple mediation with multiple covariates). Therefore, I strongly recommend conducting a power analysis beforehand to account for these potential variations.

A Demonstration

For transparency and reproducibility, the data for demonstration used in this study can be accessed at OSF. Data was simulated and analyzed using R, version 4.4.1 and the package lavaan, version, version 0.6-19.

This section provides an example with hypotheses for power analysis and highlights the differences between my power analysis tool and G*Power (Faul et al., 2007, 2009). While G*Power is designed for commonly-used statistical tests, it does not yet account for the complexity of mediation models. However, my review revealed that many organizational scientists use G*Power to estimate the power of mediation models, leading us to compare its capabilities with my tool.

To demonstrate, I delineate a parallel simple mediation model with three dependent variables (see Figure 3). In this hypothetical study, my research team examines the impact of employees' psychological safety (independent variable, IV) (e.g., Edmondson, 1999) on three outcomes: turnover intention (DV1) (e.g., Tett & Meyer, 1993), job performance (DV2) (Iaffaldano & Muchinsky, 1985), and prohibitive voice (DV3) (e.g., Liang et al., 2012). The mediation occurs through two variables: moral attentiveness (Reynolds, 2008); and trust (McAllister, 1995). Based on this setup, I hypothesize:

Direct Effects

Hypothesis 1a. Employee psychological safety is positively associated with trust

Hypothesis 1b. Employee psychological safety is positively associated with moral attentiveness.

Direct Effects

Hypothesis 2a. Trust is negatively associated with turnover intention.

Hypothesis 2b. Trust is positively associated with job performance.

Hypothesis 2c. Trust is positively associated with prohibitive voice.

Direct Effects

Hypothesis 3a. Moral attentiveness is negatively associated with turnover intention.

Hypothesis 3b. Moral attentiveness is positively associated with job performance.

Hypothesis 3c. Moral attentiveness is positively associated with prohibitive voice.

Direct Effects

Hypothesis 4a. Employee psychological safety is negatively associated with turnover intention.

Hypothesis 4b. Employee psychological safety is positively associated with job performance.

Hypothesis 4c. Employee psychological safety is positively associated with prohibitive voice.

Indirect Effects

Hypothesis 5a. Employee psychological safety is negatively associated with turnover intention, mediated by trust and moral attentiveness.

Hypothesis 5b. Employee psychological safety is positively associated with job performance, mediated by trust and moral attentiveness.

Hypothesis 5c. Employee psychological safety is positively associated with prohibitive voice, mediated by trust and moral attentiveness.

Total Effects

Hypothesis 6a. Employee psychological safety has a total effect on turnover intention, accounting for both the direct effect of psychological safety and the indirect effects through trust and moral attentiveness.

Hypothesis 6b. Employee psychological safety has a total effect on job performance, accounting for both the direct effect of psychological safety and the indirect effects through trust and moral attentiveness.

Hypothesis 6c. Employee psychological safety has a total effect on prohibitive voice, accounting for both the direct effect of psychological safety and the indirect effects through trust and moral attentiveness.

I selected the template **Parallel Process Model 4 2DVs** (Appendix 3C) to conduct the power analysis due to its similarity to the proposed model. Before modifying the model, it is essential to determine the regression coefficients for the variables. In total, 11 coefficients need to be identified through pilot studies, a review of the literature, or educated guesses (see Table 3, first and second columns). Since this is a simulation study involving hypothetical constructs, pilot tests cannot be conducted to determine these coefficients. Instead, I relied on values derived from literature or educated guesses.

Insert Table 3, about here

I used Google Scholar to collect standardized regression coefficients, beginning with relationships that are well-documented in the literature and moving to less established relationships. Since the relationships between psychological safety and (a) turnover intention, (b) job performance, and (c) prohibitive voice are well-supported in prior research, these coefficients were relatively easy to obtain, especially from studies providing standardized regression coefficients. For example, the standardized regression coefficient for c_2 (i.e., regressing psychological safety on job performance) was 0.21 (Chughtai, 2022; p. 6941), and for c_3 (i.e., regressing psychological safety on prohibitive voice) was 0.19 (Liang et al., 2012; p. 84), respectively. The standardized regression coefficient for c_1 (i.e., regressing psychological safety on turnover intention) was -0.29, converted from an unstandardized coefficient reported by Hebles et al. (2022).

For coefficients related to trust, I referred to trust literature. I identified coefficients for a_1 (i.e., regressing psychological safety on trust), b_1 (i.e., regressing trust on turnover intention), and b_2 (i.e., regressing trust on job performance) as 0.23 (Chughtai, 2022), -0.23 (Costigan et al., 2011), and 0.38 (Colquitt et al., 2007), respectively. Unfortunately, despite borrowing from the trust literature, I could not find a reported coefficient for b_3 (i.e., regressing trust on prohibitive voice), so I conservatively assigned a value of 0.15.

The remaining coefficients related to moral attentiveness were not available in the literature. Based on the hypothesized direction of relationships, I used +0.15 for positive relationships and -0.15 for negative relationships. These values are listed in the third column of Table 3.

Based on social learning theory (Bandura, 1977) and research on team learning (Edmondson, 1999), more tenured or highly educated members can influence team norms around voice and psychological safety by serving as role models or signaling expectations. For demonstration purposes, and having shown how coefficients could be determined, I assumed that all coefficients for the control variables were 0.1.

The next step involved modifying the template to fit the model. Scripts for the models with and without control variables are provided in Appendix 6A & 6B. For the initial iteration, I selected sample sizes of 400, 600, and 800. These choices were based on prior simulation studies indicating that a sample size of 600 achieves sufficient power (i.e., >0.8) when all coefficients for direct and indirect effects are 0.15 (i.e., small effects). I computed power for each hypothesis, including sub-hypotheses (e.g., Hypothesis 2 involves three sub-hypotheses due to the three dependent variables). For demonstration purposes, I included all possibilities, resulting in 26 power calculations.

Selecting an appropriate sample size for the first iteration can save time. For example, it took the first author 22 minutes to simulate power for the model without control variables across three sample sizes and 30 minutes for the model with control variables (10 additional coefficients) when run simultaneously. The simulation time increases significantly as more theorized variables or complex models are included.

The results are presented in Table 5a (without controls) and Table 5b (with controls). First, many hypotheses achieved sufficient power (>0.8) even at a sample size of 400 (e.g., H2a and H2b). However, some hypotheses remained underpowered even at a sample size of 800 (e.g., H3 and H4). When examining effects holistically (i.e., across multiple outcomes), researchers must pay extra attention to such details. Second, adding covariates did not significantly affect power (±0.03). Confidence intervals for power estimates can be calculated using formula (9) to compare the results between the two tables. Ultimately, additional iterations may be required for larger sample sizes (e.g., 1,000, 1,200, 1,400, etc.).

*Comparing with G*Power*. To calculate power using *G*Power*, I used t-tests (Means: Difference between two independent means [two groups]) to simulate the required sample sizes for 0.8 power. I ran the analysis using effect sizes of $d = 0.2$ (small effect size) and $d = 0.5$ (medium effect size), with a standard alpha error probability of 0.05 and power of 0.8 (Cohen, 1992, 2016). *G*Power* determined that sample sizes of 788 and 128 were needed for $d = 0.2$ and $d = 0.5$, respectively. However, my simulations revealed that even a sample size of 788 was insufficient for testing the mediation model. This demonstrates that G*Power is not suitable for mediation models.

Conclusion

This chapter highlights the role of rigorous power analysis in management for mediation models. My findings underscore the prevalence of underpowered studies in the field, which not only jeopardizes the reliability of research findings but also leads to less efficient use of resources across programs of research projects. Furthermore, I demonstrated the limitations of commonly used tools like G*Power, which fail to adequately address the complexities of mediation models and provided an alternative solution through my customizable toolkit. By addressing these methodological gaps, this chapter contributes to the advancement of best practices in organizational research design.

My toolkit offers researchers a solution for estimating power in models that include multiple mediators, moderators, and covariates, enabling the simultaneous testing of multiple hypotheses. This approach enhances the reliability and validity of findings, which are crucial for theory development in management. Also, the use of this toolkit encourages a systematic approach to study design, allowing researchers to design well-powered studies that are more likely to produce replicable and meaningful results.

I recommend that researchers adopt a three-pronged approach to determining regression coefficients: *conducting pilot studies, leveraging literature reviews, and using conservative estimates* when data are unavailable. Additionally, I advocate for the integration of power analyses into the research process as a standard practice and suggest including power analysis results in manuscripts to promote transparency and reproducibility in the field.

Despite the contributions, my toolkit nevertheless has limitations. My toolkit is not designed for multilevel models and models using experience sampling methods. I recommend this as an important step for future research efforts.

In conclusion, by providing a step-by-step framework and customizable templates for power analysis, this chapter equips researchers with the tools to design well-powered studies.

These efforts not only enhance methodological rigor but also contribute to the development of robust and credible theories in management, ultimately advancing the field as a whole.

Implications for Theory Development

The implications of power analysis extend beyond methodological planning to the way researchers frame and develop theory. Organizational scholars often construct increasingly complex models by adding multiple mediators, moderators, outcomes, and covariates to capture the nuances of a phenomenon. Although such complexity may appear theoretically comprehensive, each additional pathway introduces another effect that must be estimated with sufficient precision. Conducting model-specific power analyses during theory development can therefore help researchers distinguish the mechanisms that are central to their theoretical explanation from those that are secondary or exploratory. When the available sample cannot adequately test every proposed pathway, researchers may need to narrow the scope of their claims, simplify the model, or divide the theoretical framework across a sequence of studies. Power analysis should not determine which theories researchers pursue, but it can encourage greater alignment among the theory, hypotheses, research design, and strength of the conclusions. In this way, power analysis becomes not only a statistical procedure but also a tool for developing more focused, falsifiable, and credible organizational theories.

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TABLE 1

Most Common Testing Models in Organization Science Research

Similar Process Model	**Testing Model**	**Frequency**	**Percentage**
4	1IV-1ME-1DV	256	23.46%
7	1IV-1MO-1ME-1DV	180	16.50%
4	1IV-2ME-1DV	54	4.95%
6	1IV-1ME1-1ME2-1DV	52	4.77%
7	1IV-1MO-1ME-2DV	39	3.57%
7	1IV-1MO-2ME-1DV	38	3.48%
4	1IV-3ME-1DV	28	2.57%
4	1IV-1ME-2DV	22	2.02%
7	1IV-1MO-2ME-2DV	17	1.92%

TABLE 2

Overview of Software Packages for Power Analysis for Mediation Models in R

Package Name	Summary	Design Limitations
powerMediation	Primarily supports basic mediation models (i.e., Process Model 4) and applies to various regression methods, including linear, logistic, Poisson, and Cox proportional hazards models.	Does not support estimating sample size for customized models or models including covariates.
bmem	Focused on mediation models, with the added capability to accommodate non-normal data with skewness and kurtosis.	Does not support models that include covariates.
mc_power_med	Provides a graphical interface for power analysis and sample size estimation for simple and serial mediation models (e.g., Process Model 4 and Process Model 6).	Limited to default indirect effects (e.g., cannot estimate indirect paths like X → M2 → Y in serial mediation). Custom model sample sizes cannot be estimated, and covariates are unsupported.
semPower	Enables power calculation for mediation models, including more complex scenarios involving latent variables.	Requires advanced modifications or expertise for specific or complex use cases.
WebPower	Allows users to estimate sample sizes for a variety of predefined mediation models (e.g., Process Models 4, 7, 8, 14, 15, 58).	Does not support models with covariates or custom model specifications.

TABLE 3

Model Specifications of Figure 3

Coefficient	Relationship	Proposed Coefficients
a1	IV on M1	0.23
a2	IV on M2	0.15
b1	M1 on Y1	-0.32
b2	M1 on Y2	0.38
b3	M1 on Y3	0.15
b4	M2 on Y1	-0.15
b5	M2 on Y2	0.15
b6	M2 on Y3	0.15
c1	IV on Y1	-0.29
c2	IV on Y2	0.21
c3	IV on Y3	0.19

TABLE 4

List of the Hypothesized Effects

Hypothesis	Path Coefficients
1	sum of 1a-1b
1a	a1
1b	a2
2	sum of 2a-2c
2a	b1
2b	b2
2c	b3
3	sum of 3a-3c
3a	b4
3b	b5
3c	b6
4	sum of 4a-4c
4a	c1
4b	c2
4c	c3
5	sum of 5aa-5cb
5aa	a1*b1
5ab	a2*b4
5ba	a1*b2
5bb	a2*b5
5ca	a1*b3
5cb	a2*b6
6	sum of 6a-6c
6a	4a + 5aa + 5ab
6b	4b + 5ba + 5bb
6c	4c + 5ca + 5cb

TABLE 5a

Power Analysis of the Demonstration without Covariates

Power/Sample Size	H1	H1a	H1b	H2	H2a	H2b	H2c	H3	H3a	H3b	H3c	H4	H4a
400	0.999	0.984	0.848	0.687	1.000	1.000	0.846	0.413	0.850	0.845	0.847	0.233	1.000
600	1.000	0.999	0.955	0.845	1.000	1.000	0.956	0.563	0.959	0.958	0.953	0.324	1.000
800	1.000	1.000	0.985	0.927	1.000	1.000	0.985	0.692	0.986	0.986	0.991	0.420	1.000

Power/Sample Size	H4b	H4c	H5	H5aa	H5ab	H5ba	H5bb	H5ca	H5cb	H6	H6a	H6b	H6c
400	0.983	0.951	0.764	0.978	0.522	0.980	0.508	0.735	0.509	0.518	1.000	1.000	0.995
600	0.999	0.996	0.938	0.999	0.822	0.999	0.828	0.931	0.834	0.713	1.000	1.000	1.000
800	1.000	0.999	0.984	1.000	0.945	1.000	0.946	0.981	0.950	0.818	1.000	1.000	1.000

TABLE 5b

Power Analysis of the Demonstration without Covariates

Sample Size	H1	H1a	H1b	H2	H2a	H2b	H2c	H3	H3a	H3b	H3c	H4	H4a
400	0.999	0.985	0.843	0.685	1.000	1.000	0.856	0.398	0.858	0.835	0.846	0.244	1.000
600	1.000	0.999	0.953	0.842	1.000	1.000	0.954	0.564	0.959	0.959	0.954	0.332	1.000
800	1.000	1.000	0.984	0.927	1.000	1.000	0.989	0.675	0.990	0.985	0.989	0.406	1.000

Sample Size	H4b	H4c	H5	H5aa	H5ab	H5ba	H5bb	H5ca	H5cb	H6	H6a	H6b	H6c
400	0.977	0.955	0.759	0.981	0.533	0.982	0.502	0.732	0.503	0.519	1.000	1.000	0.998
600	0.999	0.993	0.925	0.999	0.827	0.999	0.814	0.929	0.821	0.694	1.000	1.000	1.000
800	1.000	0.999	0.984	1.000	0.950	1.000	0.941	0.985	0.944	0.800	1.000	1.000	1.000

FIGURE 1

Frequency of Power Values

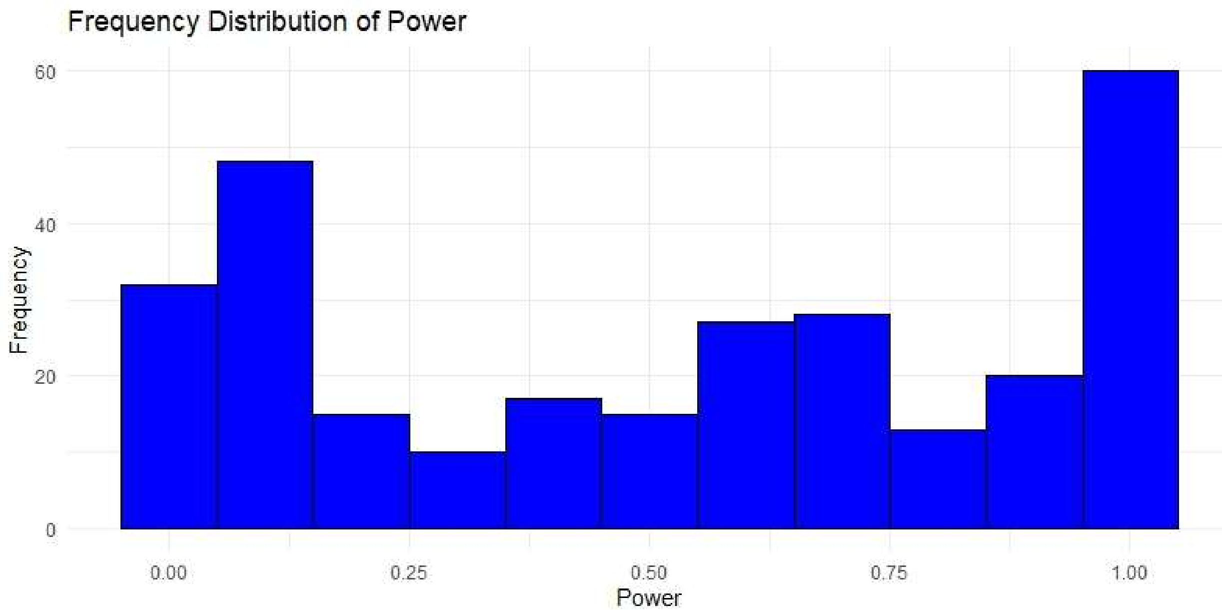

FIGURE 2

Simple Mediation Model

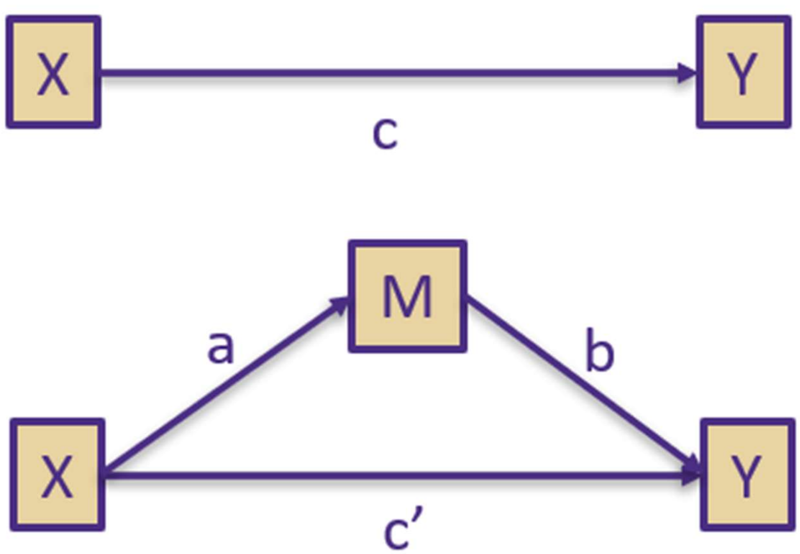

FIGURE 3

A Testing Model

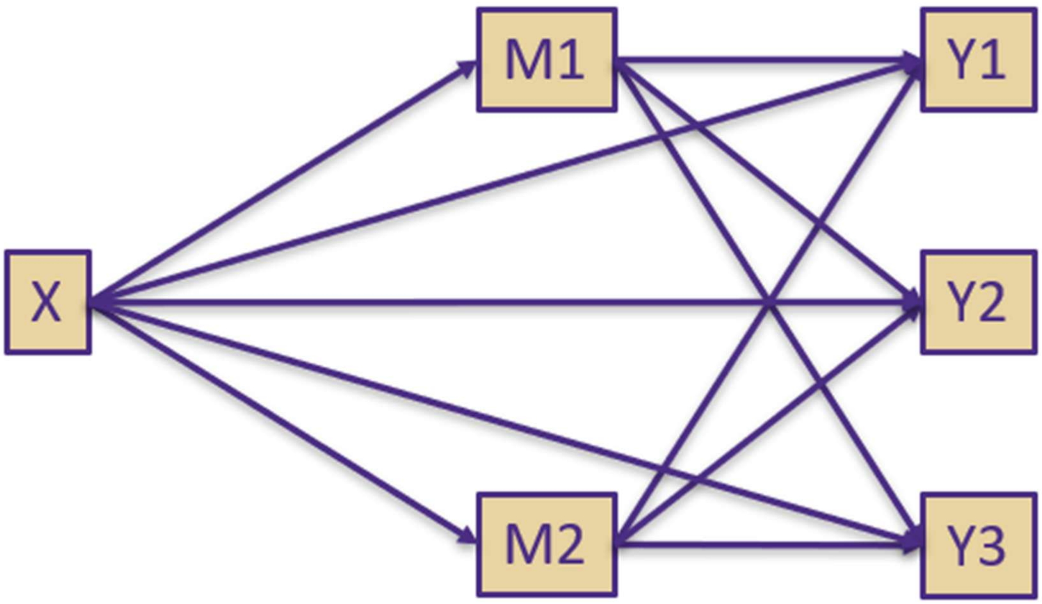

FIGURE 4a

Simple Mediation Model

```
# Define the model dynamically based on current coefficients
model <-'
#direct effect
Y ~ coeff_XtoY * X   #<-change the value
#mediaotr
M ~ coeff_MtoX * X   #<-change the value
Y ~ coeff_MtoY * M   #<-change the value
'
```

FIGURE 4b

Parallel Simple Mediation Model with 3DVs

```
# Define the model dynamically based on current coefficients
model <-'
#direct effect
Y1 ~ coeff_XtoY * X   #<-change the value
Y2 ~ coeff_XtoY * X   #<-change the value
Y3 ~ coeff_XtoY * X   #<-change the value
#mediator
M1 ~ coeff_MtoX * X   #<-change the value
M2 ~ coeff_MtoX * X   #<-change the value

Y1 ~ coeff_MtoY * M1   #<-change the value
Y2 ~ coeff_MtoY * M1   #<-change the value
Y3 ~ coeff_MtoY * M1   #<-change the value

Y1 ~ coeff_MtoY * M2   #<-change the value
Y2 ~ coeff_MtoY * M2   #<-change the value
Y3 ~ coeff_MtoY * M2   #<-change the value
'
```

FIGURE 4b

Parallel Simple Mediation Model with 3DVs and 2Covaraites

```
# Define the model dynamically based on current coefficients
model <-'
#direct effect
Y1 ~ coeff_XtoY * X + coeff_CV1toY *CV1 + coeff_CV2toY * CV2
Y2 ~ coeff_XtoY * X + coeff_CV1toY *CV1 + coeff_CV2toY * CV2
Y3 ~ coeff_XtoY * X + coeff_CV1toY *CV1 + coeff_CV2toY * CV2
#mediator
M1 ~ coeff_MtoX * X + coeff_CV1toY *CV1 + coeff_CV2toY * CV2
M2 ~ coeff_MtoX * X + coeff_CV1toY *CV1 + coeff_CV2toY * CV2

Y1 ~ coeff_MtoY * M1 + coeff_CV1toY *CV1 + coeff_CV2toY * CV2
Y2 ~ coeff_MtoY * M1 + coeff_CV1toY *CV1 + coeff_CV2toY * CV2
Y3 ~ coeff_MtoY * M1 + coeff_CV1toY *CV1 + coeff_CV2toY * CV2

Y1 ~ coeff_MtoY * M2 + coeff_CV1toY *CV1 + coeff_CV2toY * CV2
Y2 ~ coeff_MtoY * M2 + coeff_CV1toY *CV1 + coeff_CV2toY * CV2
Y3 ~ coeff_MtoY * M2 + coeff_CV1toY *CV1 + coeff_CV2toY * CV2
'
```

FIGURE 5

Process Model 7 with a Covariate

```r
# Generate predictors X, W, and CV (control variable)
X <- rnorm(n)
W <- rnorm(n)
CV <- rnorm(n)  # Control variable

# Simulate the mediator M (including CV)
M1 <- coeff_X_M1 * X + coeff_W_M1 * W
    + coeff_XW_M1 * X * W + coeff_CV_M1 * CV + rnorm(n, sd = 1)

# Simulate the outcome Y (including CV and varying direct path from X to Y)
Y <- coeff_M1_Y * M1 + direct_XY * X + coeff_CV_Y * CV + rnorm(n, sd = 1)

# Combine data into a data frame
sim_data <- data.frame(X = X, W = W, CV = CV, M1 = M1, Y = Y)
```

FIGURE 6

Path Model for Simple Mediation Model

```r
# Define the mediation model for analysis
analysis_model <- '
      # Mediation paths
      M ~ a*X
      Y ~ b*M + c*X

      # Indirect effect (a*b)
      indirect := a*b
      # Direct effect (c)
      direct := c
      # Total effect (direct + indirect)
      total := c + (a*b)
    '
```

FIGURE 7

Power Computation

```
  # Extract p-values
  indpval[i] <- pe$pvalue[pe$label == "indirect"]
  dpval[i] <- pe$pvalue[pe$label == "direct"]
  totalpval[i] <- pe$pvalue[pe$label == "total"]

  # Update progress bar after each repetition
  pb$tick()
}

# Store results for this combination of sample size, and direct
results[[paste(n, coef, direct_coef, sep = "_")]] <- list(
  "Sample_Size" = n,
  "Power_Indirect" = 1 - sum(indpval > 0.05) / reps,
  "Power_Direct" = 1 - sum(dpval > 0.05) / reps,
  "Power_Total" = 1 - sum(totalpval > 0.05) / reps
)
```

FIGURE 8

Example of a Simulation Result

```
> results
$`600`
$`600`$Sample_Size
[1] 600

$`600`$Power_Indirect
[1] 0.1956667

$`600`$Power_Direct
[1] 0.6636667

$`600`$Power_Total
[1] 0.802
```

CHAPTER THREE
The Impact of Covariates on Mediation Models

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Chapter 3

Mediation is a central analytic tool in management research, enabling scholars to uncover the mechanisms linking predictors and outcomes (Aguinis, Edwards, et al., 2017; Wood et al., 2008). By incorporating mediators, researchers can move beyond documenting associations to clarifying how and why effects occur, thereby refining, extending, and testing theory.

Covariates are frequently included in mediation models to address potential confounds and reduce unexplained variance (Becker, 2005; Bernerth & Aguinis, 2016; Carlson, 2023; Li, 2021; Mändli & Rönkkö, 2023). When carefully selected, covariates can increase the precision of estimates, reduce bias, and enhance statistical power. Yet, inappropriate or weakly correlated covariates can undermine these benefits, lowering power and increasing the risk of Type II errors (Becker, 2005). Despite their ubiquity, few studies have systematically examined how covariates influence statistical power in mediation analyses.

Researchers have long assumed that including covariates in statistical models invariably improves statistical power by accounting for additional variance. This assumption is deeply embedded in conventional practice and is often taken for granted. However, recent work suggests that covariates can reduce power, particularly when they are weakly related to focal constructs under certain conditions (Shen et al., 2024). Importantly, the consequences of covariate inclusion differ by design. In surveys, where predictors are often continuous and nonrandomized, covariates correlated with predictors can meaningfully alter power. In experiments with randomized dichotomous treatments, the covariate's link to the mediator is more consequential, given the treatment's independence from other variables.

This study challenges the assumption that covariates are always beneficial by using Monte Carlo simulations to systematically test when and how they alter power in mediation models. The simulations vary correlations between covariates and predictors (survey designs) or mediators (experimental designs) across simple, serial, and moderated mediation. Both continuous (e.g., age, income) and dichotomous (e.g., gender, race) covariates are modeled, given potential differences by measurement scale. By comparing designs and covariate types, this study demonstrates when covariates strengthen or weaken power, offering researchers practical guidance on their inclusion.

Overall, the findings call into question the conventional belief that covariates invariably enhance mediation analyses. Instead, the findings highlight when covariates undermine power, underscoring the need for power analyses that account for covariate effects. These insights aim to help scholars design adequately powered mediation studies, improving both methodological rigor and theoretical contribution in organizational research.

Across simulations that varied design context (survey vs. experiment) and covariate type (continuous vs. binary), results demonstrate that covariates do not uniformly enhance statistical power. In some settings, their inclusion has negligible impact, whereas in others it significantly reduces power. These findings contribute to the methodological literature by specifying the conditions under which covariates aid or hinder effect detection, thereby equipping researchers with practical guidance for building adequately powered mediation studies.

The Role of Covariates in Management Research

Including control variables is often considered essential for ruling out alternative explanations, reducing error variance, and increasing statistical power (e.g., Atinc et al., 2012; Becker, 2005; Bernerth & Aguinis, 2016). Management scholars have provided reviews and best-practice recommendations for the use of control variables (see Atinc et al., 2012; Becker, 2005;

Bernerth & Aguinis, 2016; K. D. Carlson & Wu, 2012; Hünermund & Louw, 2025; Mändli & Rönkkö, 2023; Spector & Brannick, 2011; Sturman et al., 2022). These best practices generally emphasize eliminating irrelevant controls and clearly documenting the criteria for covariate inclusion (Mändli & Rönkkö, 2023).

The use of covariates has important implications for organizational research. Statistical controls are widely applied in management studies to reduce potential confounds and strengthen the validity of findings. For example, Atinc et al. (2012) reported that 812 out of 1199 articles published in the *Academy of Management Journal*, *Journal of Applied Psychology*, *Journal of Management*, and *Strategic Management Journal* from 2005 to 2009 included control variables. Despite growing awareness of best practices, however, researchers often provide minimal justification for including controls or explaining how they relate to focal constructs. Notably, little improvement in these practices has been observed over the past decade (Bernerth & Aguinis, 2016, p. 229; Simmons et al., 2011).

The Role of Power Analyses in Theory Testing

Power analysis is a crucial tool for estimating the likelihood of detecting true effects in hypothesis testing, requiring consideration of effect size, sample size, significance level, and desired statistical power (Ferguson & Ketchen, 1999; Scherbaum & Ferreter, 2009). Without adequate power analysis, studies risk being underpowered, which can lead to unreliable results, inflated effect sizes, and increased Type I (e.g., Schmidt, 1996) or Type II errors (e.g., Maxwell, 2004). Conversely, studies may also be overpowered, resulting in unnecessary use of resources. Errors in power estimation, such as mismatches between the analytical model and the assumed power model (e.g., applying a simple model to estimate power for a mediation analysis), can further undermine study credibility and hinder theoretical development (J. Cohen, 1992;

Maxwell, 2004). Well-conducted power analyses enhance transparency, improve replicability, and support more rigorous theory testing in organizational research.

The Impact of Covariates on Power

Adding covariates can increase statistical power under the right conditions by reducing unexplained variance in the dependent variable (Deaton & Cartwright, 2018; Hernández et al., 2004). Control variables account for variability unrelated to the independent variables, thereby reducing error variance. This reduction tightens the confidence intervals around effect estimates, increasing precision and, consequently, power (Becker, 2005; K. D. Carlson & Wu, 2012). However, scholars have also noted that covariates can reduce power when they are unrelated to the outcome or highly correlated with the independent variable (Becker, 2005).

However, there are limitations to applying best-practice guidelines from regression models directly to mediation analysis. First, mediation models estimate both indirect and direct effects simultaneously (i.e., across two or more equations), unlike simple or multiple regression models, which typically estimate direct effects in a single equation. As a result, simulation-based findings regarding covariate effects from the broader management literature may not generalize to mediation models (Mändli & Rönkkö, 2023; Sturman et al., 2022).

Second, differences between survey and experimental designs may yield distinct implications. In survey studies, independent variables are typically continuous and not randomized, allowing correlations with covariates to vary. Under random assignment, the treatment is independent of baseline covariates in expectation. A recent study showed that in experiments, power substantially decreased (from .80 to .10) when the correlation between the mediator and a covariate ranged from 0 to .40; the authors recommended reverse coding the covariate in such cases (Shen et al., 2024, p. 10). Whether this finding generalizes to survey studies remains unclear, and management research often employs more complex models, such as serial or moderated mediation (e.g., Lau et al., under review), where power may behave differently. Thus, recommendations based on simpler models may not translate directly to more complex mediation designs.

Third, the measurement scale of a covariate may also influence power in mediation analysis, yet this issue has received little attention. While prior work has considered when to include covariates (e.g., Atinc et al., 2012; Mändli & Rönkkö, 2023), few studies have examined how covariate type (binary, categorical with three or more categories, or continuous) affects model performance. Because mediation analysis relies on the covariance matrix, both the variance of each variable and its correlations with other variables directly influence regression coefficients and, by extension, statistical power. For instance, the variance of a continuous variable can be unbounded, whereas the variance of a binary variable is capped at .25 (when p = 0.5).

In summary, the inclusion of covariates in mediation analysis is far from a straightforward decision. Although covariates can enhance statistical power under certain conditions, their effects depend on the model structure, study design, and measurement characteristics. The complexity of mediation models, particularly when extended to include multiple mediators or moderators, demands more tailored guidelines that move beyond general regression principles. In the following sections, I use Monte Carlo simulations to systematically examine how covariate properties influence statistical power across various mediation contexts.

Power Analysis for Mediation Models with Covariate

To examine statistical power in mediation models that include covariates, I employed a 2 × 2 × 2 × 2 factorial design. The design varied: (1) the type of mediation model (simple mediation vs. serial mediation with two mediators), (2) the study design (survey vs. experiment), (3) the type of covariate (continuous vs. binary), and (4) the covariate placement (controlling for the mediator only vs. controlling for both the mediator and the dependent variable).

The inclusion of covariates at different points in the model reflects common theoretical and methodological decisions in management research. In some cases, scholars may theorize that a covariate influences the mediator but not the dependent variable, leading them to include the covariate only on the mediator path. In other cases, researchers may control for the covariate's effect on both the mediator and the dependent variable to more fully partial out its influence. By simulating both scenarios, this study evaluates how alternative covariate placement strategies affect the statistical power of mediation models.

Survey Setting, Continuous Covariate

The Type of Mediation Model

Generally, the simple mediation model (see Figure 1) can be expressed in two regression equations (Baron & Kenny, 1986; MacKinnon, 2007). In this mediation model, **X** is the independent variable, **Z** is a control variable, **M** is the mediator, and **Y** is the dependent variable:

$$M = i_1 + a_1X + d_1Z + e_1 \ldots (3)$$

$$Y = i_2 + a_2X + b_1M + d_2Z + e_2 \ldots (4)$$

Scholars can assess three key effects in a mediation model: the **indirect effect** (i.e., $a1 \cdot b1$), the **direct effect** (i.e., $a2$), and the **total effect** (i.e., $a1 \cdot b1 + a2$) (e.g., Preacher & Hayes, 2004, 2008).

In this serial multiple mediation model, **X** is the independent variable, **Z** is a control variable, $\mathbf{M_1}$ and $\mathbf{M_2}$ are sequential mediators, and **Y** is the dependent variable. The model is specified as follows (Hayes, 2012, Process Model 6):

$$M_1 = i_1 + a_1X + d_1Z + e_1 \ldots (5)$$

$$M_2 = i_2 + a_2X + b_1M_1 + d_2Z + e_2 \ldots (6)$$

$$Y = i_3 + a_3X + b_2M_1 + c_1M_2 + d_3Z + e_3 \ldots (7)$$

In a serial mediation model, scholars can examine multiple indirect pathways by which **X** affects **Y** through two mediators, $\mathbf{M_1}$ and $\mathbf{M_2}$. Specifically, three indirect effects can be tested:

(1) $\mathbf{X} \rightarrow \mathbf{M_1} \rightarrow \mathbf{Y}$ ($a_1 \cdot b_2$),

(2) $\mathbf{X} \rightarrow \mathbf{M_1} \rightarrow \mathbf{M_2} \rightarrow \mathbf{Y}$ ($a_1 \cdot b_1 \cdot c_1$), and

(3) $\mathbf{X} \rightarrow \mathbf{M_2} \rightarrow \mathbf{Y}$ ($a_2 \cdot c_1$).

In addition, the **direct effect** of **X** on **Y** (c) and the **total effect** can also be estimated (e.g., Preacher & Hayes, 2004, 2008).

Study Design

Management researchers frequently rely on surveys and experiments to statistically examine and test theory-driven hypotheses about causal relationships in the workplace (Rogelberg, 2015; Schmiedel et al., 2019). Although both approaches contribute to organizational science, they serve distinct purposes and rest on different methodological assumptions. Survey studies typically involve observational designs in which independent variables are measured rather than manipulated (e.g., assessments of leadership style or job satisfaction using Likert-type scales; Schwab, 2013). Such studies are often conducted in field settings (e.g., organizations) to enhance ecological validity and generalizability. In contrast, experimental designs manipulate independent variables or moderators via random assignment (e.g., 2 × 2 between-subjects design), allowing for stronger causal inferences. Experiments are traditionally conducted in controlled laboratory settings, though organizational researchers increasingly use online platforms such as Amazon Mechanical Turk (MTurk), Prolific, or university subject pools to recruit participants efficiently and access more diverse populations. Together, these approaches provide complementary strengths for advancing organizational research. In this section, I focus exclusively on survey designs.

The mathematical implications of mediation also differ depending on research design. Consider a simple mediation model (i.e., PROCESS Model 4) estimated via structural equation modeling with both the mediator and dependent variable specified as continuous. In survey designs, the independent variable is typically correlated with both the mediator and the outcome due to natural covariation. In contrast, in experimental designs, the independent variable is manipulated and randomly assigned, ensuring that it is statistically uncorrelated with other variables at baseline because of randomization.

Here is a formal equation that links the covariance matrix (Σ) of observed variables to the structural equation model (SEM) parameters. This relationship is expressed as:

$$\Sigma = (I - B)^{-1} * \Gamma * \Psi * \Gamma^{T} * (I - B)^{-T}(I - B) + \Theta$$

Where:

- Σ: **Model-implied covariance matrix** of observed variables
- B: **Matrix of regression coefficients** among endogenous variables (e.g., effects from X → M, M → Y)
- *Γ*: **Matrix of regression coefficients** representing the effects of exogenous variables on endogenous variables (e.g., X → M, Z → Y)
- Ψ: **Covariance matrix of exogenous variables** (e.g., X, Z)
- Θ: **Covariance matrix of residuals** (errors in M and Y)
- I: Identity matrix
- $(I - B)^{-1}$, capturing the recursive influence of variables
- $(I - B)^{-T}$: Transpose of the inverse (used to preserve symmetry)

Furthermore, because mediation models estimate effects across two or more equations, researchers may choose to include covariates on the mediator, the dependent variable, or both, depending on theoretical considerations[2].

Survey

For demonstration purposes, I further assume that the mediator(s) and the dependent variable (as well as the independent variable in the survey design) are standardized. In other words, the variance of each variable is fixed at 1.

Simple Mediation, PROCESS Model 4

$$\Sigma = \begin{bmatrix} Var(X) & Cov(X,M) & COV(X,Y) & Cov(X,Z) \\ Cov(M,X) & Var(M) & Cov(M,Y) & Cov(M,Z) \\ Cov(Y,X) & Cov(Y,M) & Var(Y) & Cov(Y,Z) \\ Cov(Z,X) & Cov(Z,M) & Cov(Z,Y) & Var(Z) \end{bmatrix}$$

[2] Appendix 1 presents the covariance algebra for both simple and serial mediation models, under the condition that the covariate's effect is controlled only at the mediator level.

There are 6 Covariances in this simple mediation model (control for both mediator and DV):

1. $Cov(X,Z) = Corr(X,Z), which\ varies\ from\ -1\ to\ 1$
2. $Cov(X,M) = a1 + d1 * Corr(X,Z)$
3. $Cov(X,Y) = c' + ab + d_2 Corr(X,Z)$
4. $Cov(Y,M) = (a2 * a1 + d1 * d2 + b1) + (d2 * d1 + a2 * d1) * Corr(X,Z)$
5. $Cov(M,Z) = a1 * Corr(X,Z) + d1$
6. $Cov(Y,Z) = (d2 + b1 * d1) + (a2 + b1 * a1) * Corr(X,Z)$

Survey (Serial Mediation, PROCESS Model 6)

$$\Sigma = \begin{bmatrix} Var(X) & Cov(X,M1) & COV(X,M2) & Cov(X,Y) & Cov(X,Z) \\ Cov(M1,X) & Var(M1) & Cov(M1,M2) & Cov(M1,Y) & Cov(M1,Z) \\ Cov(M2,X) & Cov(M2,M1) & Var(M2) & Cov(M2,Y) & Cov(M2,Z) \\ Cov(Y,X) & Cov(Y,M1) & Cov(Y,M2) & Var(Y) & Cov(Y,Z) \\ Cov(Z,X) & Cov(Z,M1) & Cov(Z,M2) & Cov(Z,Y) & Var(Z) \end{bmatrix}$$

There are 10 Covariances in this serial mediation model (control for mediators and DV):

1. $Cov(X,Z) = Corr(X,Z), which\ varies\ from\ -1\ to\ 1$
2. $Cov(X,M1) = a1 + d1 * Corr(X,Z)$
3. $Cov(X,M2) = (a2 + b1 * a1) + (b1 * d1 + d2) * Corr(X,Z)$
4. $Cov(X,Y) = (a3 + b2 * a1 + c1 * a2 + a1 * b1 * c1) + (b2 * d1 + c1 * b1 * d1 + c1 * d2 + d3) * Corr(X,Z)$
5. $Cov(M1,M2) = (a2 * a1 + b1 + d2 * d1) + (a2 * d1 + d2 * a1) * Corr(X,Z)$
6. $Cov(M1,Y) = (a3 * a1 + b2 + c1 * a1 * a2 + b1 * c1 + c1 * d1 * d2 + d3 * d1) + (a3 * d1 + a2 * c1 * d1 + a1 * c1 * d2 + d3 * a1) * Corr(X,Z)$

7. $Cov(M2, Y) = (a3 * a2 + a3 * a1 * b1 + b2 * a2 * a1 + b2 * b1 + b2 * d1 * d2 + c1 + d3 * b1 * d1 + d3 * d2) + (a3 * b1 * d1 + a3 * d2 + b2 * a2 * d1 + b2 * d2 * a1 + d3 * a2 + d3 * b1 * a1) * Corr(X, Z)$
8. $Cov(M1, Z) = d1 + a1 * Corr(X, Z)$
9. $Cov(Y, Z) = (d3 + b2 * d1 + c1 * d2 + c1 * b1 * d1) + (a3 + b2 * a1 + c1 * a2 + c1 * a1 * b1) * Corr(X, Z)$
10. $Cov(M2, Z) = (d2 + b1 * d1) + (a2 + b1 * a1) * Corr(X, Z)$

Monte Carlo Simulation

I conducted Monte Carlo simulation studies to examine the impact of continuous covariates on the statistical power of mediation models. Four models were evaluated: (1) a simple mediation model with a covariate on the mediator, (2) a simple mediation model with a covariate on both the mediator and the dependent variable, (3) a serial mediation model with a covariate on M1M_1M1 only, and (4) a serial mediation model with covariates on both mediators and the dependent variable.

Data were generated using the *mvrnorm* function from the MASS package in R, under the assumption of multivariate normality and based on a specified covariance matrix. All simulated data were assumed to follow a multivariate normal distribution.

Each power analysis was based on 1,000 replications. Power was calculated for the indirect, direct, and total effects estimated via structural equation modeling. Sample sizes were set at 600 for the simple mediation models and 800 for the serial mediation models, as these values represent conservative thresholds for achieving adequate power (≥ .80; Lau et al., under review). A total of 1,000 replications was selected to balance computational efficiency with precision in estimating power, and the chosen sample sizes reflect practical guidelines for achieving stable and generalizable results in Monte Carlo studies.

Results

To ensure reproducibility and methodological transparency, the simulation code, including specific parameter settings and data generation procedures, is provided in Appendix 2. Readers may consult this appendix for comprehensive details and to replicate the findings. Data were simulated and analyzed using R (version 4.4.1), the MASS package (version 7.3-65), and the lavaan package (version 0.6-19). This study's design and analyses were not preregistered.

In the simple mediation model with a covariate on the mediator, statistical power varied substantially as a function of the correlation between the independent variable (X) and the covariate (W). As shown in Figure 1, the power to detect the indirect effect followed an inverted-U pattern, with very low power when the correlation between X and W approached –1 or +1, but reaching a maximum of approximately .90 when the correlation was near zero. This suggests that multicollinearity between X and W substantially reduces the ability to detect the indirect effect, whereas independence between the two variables maximizes power. In contrast, the power to detect the direct and total effects remained consistently low (.05–.10) across all levels of correlation, indicating that these pathways were relatively unaffected by the relationship between X and W.

Figure 1a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with a covariate on the mediator, varying the correlation between the independent variable (X) and the covariate (W).

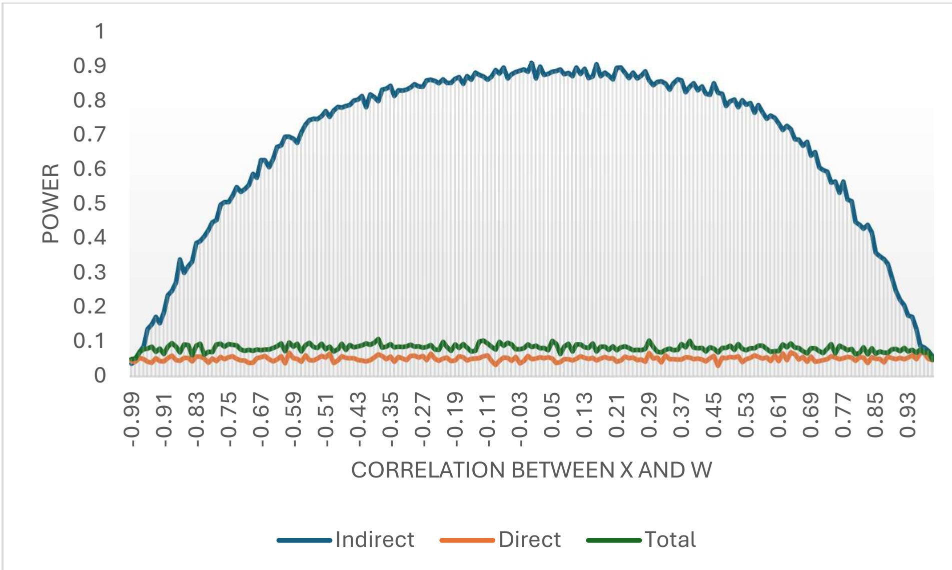
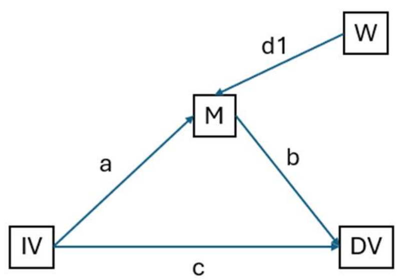


Figure 1b. Path Diagram of the Simulation.



In the simple mediation model with a covariate on both the mediator and the dependent variable, statistical power again varied systematically with the correlation between the independent variable (X) and the covariate (W). As illustrated in Figure 2, all three effects—indirect, direct, and total—displayed inverted-U shaped patterns. Power for the indirect and total effects was highest, peaking around .85 to .90 when X and W were uncorrelated, and declining sharply as the correlation approached –1 or +1. The direct effect followed the same trend, but with lower peak power (approximately .65 to .70). These results suggest that when covariates are included on both the mediator and the dependent variable, multicollinearity between X and W substantially reduces statistical power across all pathways, with the greatest detectability occurring when X and W are independent.

Figure 2a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with a covariate on both the mediator and the dependent variable, varying the correlation between the independent variable (X) and the covariate (W).

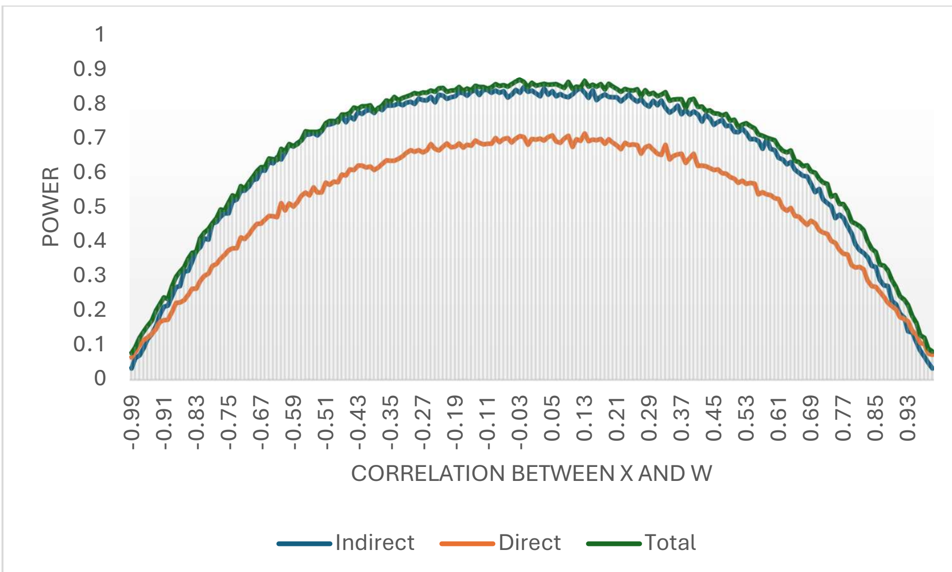
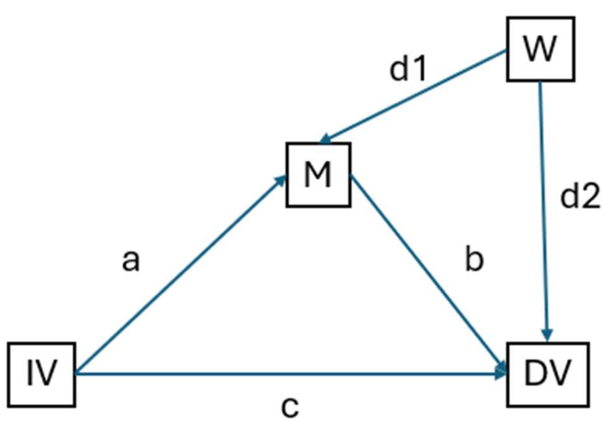


Figure 2b. Path Diagram of the Simulation.



In the serial mediation model with a covariate on the mediator, statistical power varied as a function of the correlation between the independent variable (X) and the covariate (W). As shown in Figure 3, the total effect consistently exhibited the highest power, remaining above .90 across most values of correlation. The sequential indirect effect (X→M1→M2→Y) and the direct effect both demonstrated strong power, generally ranging from .70 to .85, though each declined when X and W approached high positive or negative correlations. The indirect effects through M1 (X→M1→Y) and through M2 (X→M2→Y) were weaker overall, ranging from .50 to .70 depending on the level of correlation. These results indicate that multicollinearity between X and W reduces statistical power across pathways, with the total and sequential indirect effects being the most robust.

Figure 3a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with a covariate on the mediator, varying the correlation between the independent variable (X) and the covariate (W)

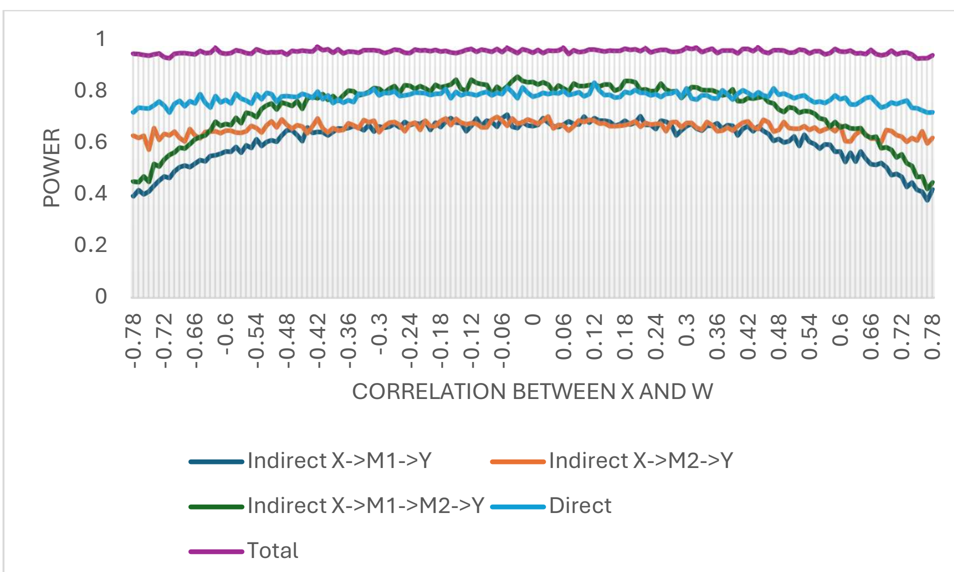
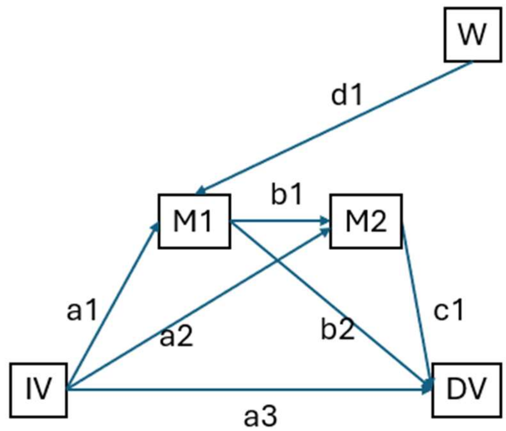


Figure 3b. Path Diagram of the Simulation.



In the serial mediation model with covariates on both the mediators and the dependent variable, statistical power displayed strong inverted-U shaped patterns across all pathways as a function of the correlation between the independent variable (X) and the covariate (W). As shown in Figure 4, the total effect consistently exhibited the highest power, approaching or exceeding .95 when X and W were uncorrelated, and declining substantially when the correlation approached –1 or +1. The indirect effect through M2 (X→M2→Y) demonstrated the strongest indirect power, peaking above .90 under low correlation conditions. The sequential indirect effect (X→M1→M2→Y) and the direct effect also reached relatively high levels of power (.75–.85) near zero correlation but weakened at the extremes. The indirect effect through M1 (X→M1→Y) was moderate, peaking around .80. These findings suggest that while all effects are maximally detectable when X and W are independent (i.e., 0), multicollinearity between them substantially reduces statistical power across all pathways.

Figure 4a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with covariates on both the mediators and the dependent variable, varying the correlation between the independent variable (X) and the covariate (W).

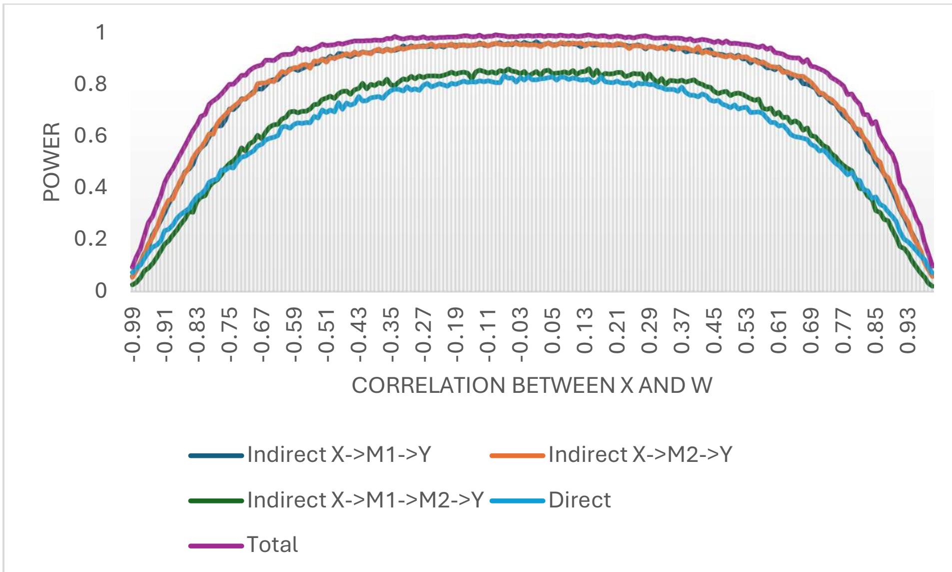
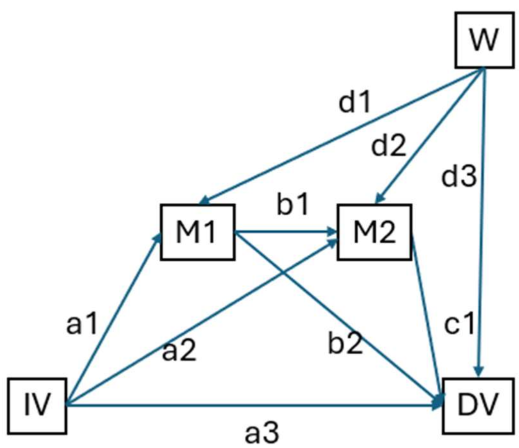


Figure 4b. Path Diagram of the Simulation.



Experimental Setting, Continuous Covariate

In management experimental research, scholars often randomly assign participants to two to four conditions, depending on the research context. In this exploratory study, I limit the design to two conditions for illustrative purposes, as two-condition experiments are the most common approach in management research (see Appendix X).

Mathematically, experimental studies differ from survey studies. Because independent variables and/or moderators are randomized in experiments, the covariance between the independent variable, T, and any covariates (Z) is expected to equal zero. To illustrate this distinction, I use a simple mediation model with a covariate on both the mediator and the dependent variable. In this model, **T** represents the independent variable, **Z** is a covariate, **M** is the mediator, and **Y** is the dependent variable. Note, however, that Cov(T, M) and Cov(M, Y) are generally nonzero when the corresponding path coefficients (e.g., a_1, b_1) differ from zero.

$$M = i_1 + a_1 T \ + d_1 Z + e_1 \ldots (8)$$

$$Y = i_2 + a_2 T + b_1 M + d_2 Z + e_2 \ldots (9)$$

There are 6 Covariances in this simple mediation model (control for both mediator and DV):

1. $Cov(T, Z) = \ 0$
2. $Cov(T, M) = a_1 * Var(T)$
3. $Cov(T, Y) = (a_1 * b_1 + a_2) * Var(T)$
4. $Cov(Y, M) = b1 + d1 * d2$
5. $Cov(M, Z) = d1, which\ varies\ from\ -1\ to\ 1$
6. $Cov(Y, Z) = b1 * d1 + d2$

To examine the impact of a continuous covariate on mediation models in an experimental context, I vary the value of d_1 from –1 to +1. From the covariance algebra result, I also showed that the regression coefficient d_1 equals to the correlation between the mediator and the covariate in the simple mediation model, mathematically.

Monte Carlo Simulation

I present results from four models: (1) a simple mediation model with a covariate on the mediator; (2) a simple mediation model with covariates on both the mediator and dependent variable (DV); (3) a serial mediation model with a covariate on M_1 only; and (4) a serial mediation model with covariates on both mediators and the DV.

However, I do not use the mvrnorm function from the **MASS** package to simulate data under the assumption of multivariate normality, as the independent variable in an experimental design is randomly assigned and therefore not continuous. Instead, I specify the model equations directly (see Appendix X for the syntax).

For the simple mediation model with a covariate on mediator only, the parameters are: $a = 0.40$, $b = 0.15$, $c = 0.125$, and d_1 varies from –1 to +1. For the simple mediation model with a covariate on mediator and DV, the parameters are: $a = 0.40$, $b = 0.15$, $c = 0.125$, d_1 varies from –1 to +1, and $d_2 = 0.10$.

For the serial mediation model with a covariate on mediator only, the parameters are: $a_1 = 0.40$, $b_1 = 0.15$, $a_2 = 0.125$, $b_2 = 0.15$, $a_3 = 0.125$, $c_1 = 0.15$, and d_1 varies from –1 to +1. For the serial mediation model with a covariate on mediators and DV, the parameters are: $a_1 = 0.40$, $b_1 = 0.15$, $a_2 = 0.125$, $b_2 = 0.15$, $a_3 = 0.125$, $c_1 = 0.15$, d_1 varies from –1 to +1, $d_2 = 0.10$, and $d_3 = 0.10$[3].

[3] Readers may wonder why a_1 (in the serial mediation model) and a (in the simple mediation model) are set to 0.40 rather than 0.15. First, this study aims to examine the impact of covariates on mediation models. Readers are encouraged to adjust these values based on prior literature or conservative assumptions (Lau et al., under review). Second, the latter part of this study simulates models in which the independent variable is dichotomously and randomly assigned. In such cases, the first path in the model resembles a *t*-test, and the effect size should be expressed using Cohen's *d*. Third, the indirect effect was set to ensure statistical power of at least 0.80 when the correlation between the covariate (Z) and the mediator (M) is zero.

Readers interested in understanding the influence of covariates on power are encouraged to consult the literature on power analysis, as relationships between variables may vary across contexts (Lau et al., under review).

Each power analysis involved 1,000 replications. Power was estimated for the indirect effect, direct effect, and total effect using structural equation modeling. Sample sizes were set at 600 for the simple mediation model and 800 for the serial mediation model, as these are conservative estimates sufficient to achieve adequate power (≥ .80; Lau et al., under review).

In the simple mediation model with a covariate on the mediator, statistical power remained relatively stable as a function of the regression coefficient d_1. As shown in Figure 5, the power to detect the indirect effect was consistently low to moderate, ranging from .25 to .30 across the full correlation range. The total effect demonstrated slightly higher but stable power, approximately .50 to .55. By contrast, the direct effect was highly sensitive to the regression coefficient d_1, following a U-shaped pattern: power was maximized when the correlation was strongly negative or strongly positive (approaching .95 to 1.00), and decreased when the correlation was near zero (.80). These findings suggest that while indirect and total effects are largely unaffected by variation in the regression coefficient d_1, the detectability of the direct effect depends strongly on the degree of association between these variables.

Figure 5a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with a covariate on the mediator, varying the regression coefficient d_1.

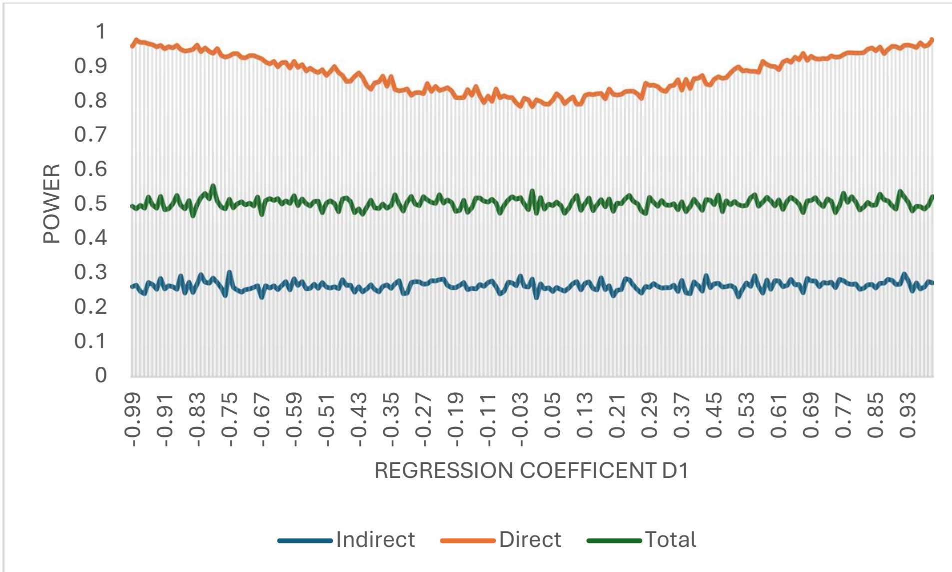
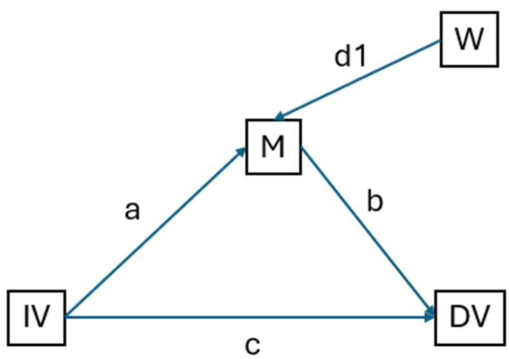


Figure 5b. Path Diagram of the Simulation.



In the simple mediation model with covariates on both the mediator and the dependent variable, statistical power was largely unaffected by variation in the egression coefficient d_1. As illustrated in Figure 6, the power to detect the indirect effect remained consistently low to moderate (.25–.30), while the total effect demonstrated moderate but stable power (.50–.55). The direct effect showed the highest power overall, remaining relatively constant at approximately .78 to .82 across all values of correlation. These findings suggest that, unlike the model with a covariate on the mediator alone (Figure 5), the detectability of the direct effect is robust to changes in the regression coefficient d_1.

Figure 6a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with covariates on both the mediator and the dependent variable, varying the regression coefficient d_1.

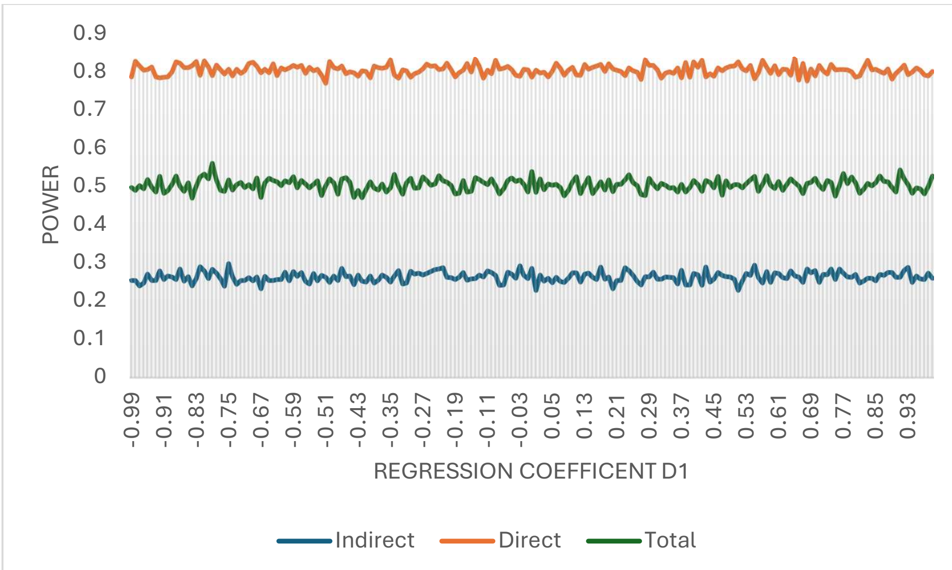
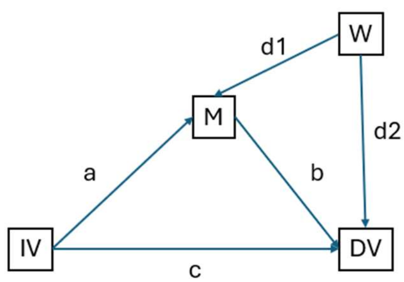


Figure 6b. Path Diagram of the Simulation.



In the serial mediation model with a covariate on the mediator, statistical power patterns remained relatively stable across values the regression coefficient d_1. As displayed in Figure 7, the power of indirect effect through M1 (X→M1→Y) exhibited the highest power, consistently exceeding .90 across the correlation range. The power of sequential indirect effect (X→M1→M2→Y) also showed strong detectability, ranging from .80 to .90. The power of indirect effect through M2 (X→M2→Y) was moderate, approximately .65 to .75, while the power of total effect remained stable around .70 to .75. In contrast, the power of direct effect was consistently the weakest, ranging from .15 to .20. These results indicate that in this serial mediation model, the pathways involving M1 are the most detectable, and statistical power is minimally influenced by variation in the regression coefficient d_1.

Figure 7a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with a covariate on the mediator, varying the regression coefficient d_1.

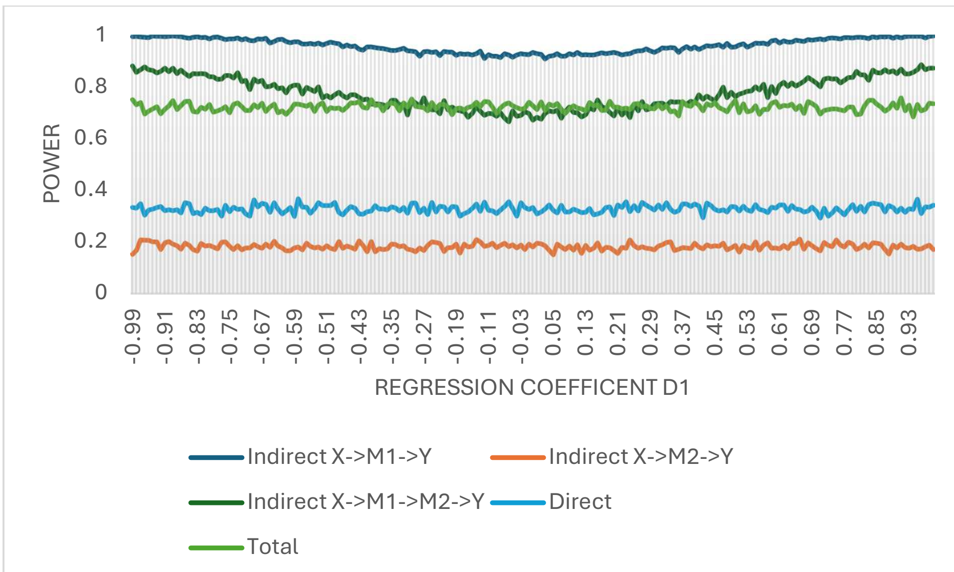
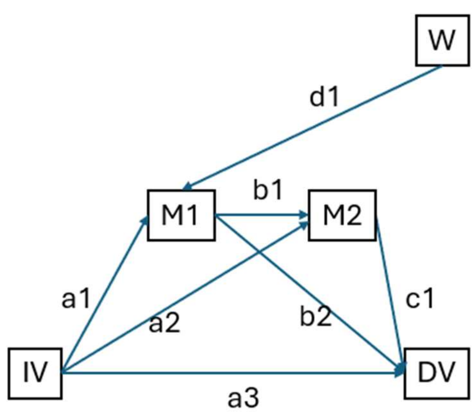


Figure 7b. Path Diagram of the Simulation.



In the serial mediation model with covariates on both the mediators and the dependent variable, statistical power remained highly stable across the full range of regression coefficient d_1. As shown in Figure 8, the power of indirect effect through M1 (X→M1→Y) demonstrated the highest detectability, consistently around .90 to .92. The power of sequential indirect effect (X→M1→M2→Y) and the power of total effect were also strong, ranging from .70 to .75 across the correlation values. The power of indirect effect through M2 (X→M2→Y) and the direct effect were weaker overall, both consistently ranging between .25 and .35. These findings indicate that, when covariates are included on both the mediators and the dependent variable, statistical power for most effects is largely unaffected.

Figure 8a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with covariates on both the mediators and the dependent variable, varying the regression coefficient d_1.

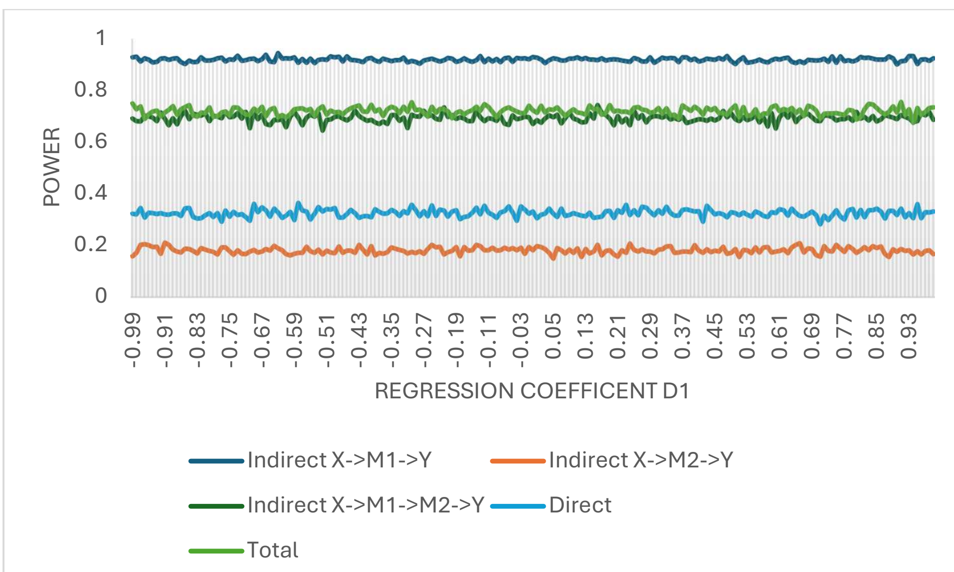
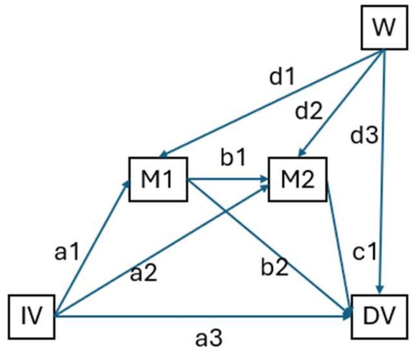


Figure 8b. Path Diagram of the Simulation.



Survey Setting, Binary Covariate

The research method literature in management has shed little light on the nature of the type of variable (i.e., binary, >2 and <5 categories, 5 or more categories), and its impact on power[4]. In management research, scholars often include binary variables as covariates to control for potential confounding effects. Common examples include gender (female vs. male), employment type (part-time vs. full-time), and race (non-White vs. White).

The key difference between a continuous variable and a binary variable is variance. The maximum variance of a binary variable must be 0.25 while that of a continuous variable is not limited. For a binary variable $Z \in \{0,1\}$, with probability $p = P(Z = 1)$, the variance is: $Var(Z) = p(1 - p)$. The variance is maximum when $p = 0.5$, because: $Var(Z) = 0.5(1 - 0.5) = 0.25$. Thus, the variance of a binary covariate is always less than that of a standardized continuous covariate (i.e., 1). Moreover, in management research practice, scholars typically do not control the proportion p, meaning the variance may be considerably smaller than 0.25.

Second, it is important to note that when the random variables being correlated are not normally distributed, the full theoretical range of the Pearson correlation coefficient, [–1, 1], is

[4] Scholars often treat 5 or more categorical variables as continuous.

no longer guaranteed. This mathematical constraint is known as the Fréchet-Hoeffding bounds and becomes particularly relevant when selecting population correlation values for Monte Carlo simulations. Depending on the shape of the marginal distributions, certain correlation values may be theoretically impossible to achieve. For a detailed overview, see Olvera Astivia, Kroc, and Zumbo (2020). Appendix A provides the Fréchet-Hoeffding bounds applicable to the current simulation study.

Compared to the simulation of Survey Setting, Binary Covariate scenario, that of Survey Setting, Continuous Covariate scenario is not same. Even though in a survey Setting, including binary covariate, compared to a continuous covariate, will violate the use of mvrnorm as it can only simulate the data for multivariate normal, and continuous variables. I can still deduce the covariance matrix, but there is no simple way to simulate the impact of a binary covariate on a mediation model like what I did previously. Instead, there are three ways to explore the impact of a binary covariate on mediation model.

In **Condition 1**, I investigated the impact of the correlation between the covariate and the independent variable on statistical power. Specifically, I varied the correlation between the binary covariate and the independent variable while fixing the regression coefficients and the binary covariate proportion. The regression coefficients were set to .10 for the direct effects and the covariate's regression coefficient, and .15 for the indirect effect's coefficient. The proportion was fixed at p =.40. To generate the data, I used the NORTA (Normal-To-Anything) approach, which combines a Gaussian copula with an intermediate correlation matrix to simulate the independent and covariate variables, followed by simulation of the remaining variables (see Appendix X for details).

In **Condition 2**, I examined the impact of the proportion of a binary covariate on statistical power. In this condition, the regression coefficients were fixed at .10 for the direct effects and the covariate's regression coefficient, and .15 for the indirect effect's coefficient. I then varied both the correlation between the independent variable and the covariate, and the proportion of the binary covariate, with p ranging from .10 to .50. For each value of p, the feasible correlation was constrained by the Fréchet–Hoeffding bounds: at $p = .10$, the allowable range was [–.58, .58]; at $p = .20$, [–.69, .69]; at $p = .30$, [–.75, .75]; at $p = .40$, [–.78, .78]; and at $p = .50$, [–.79, .79].

In **Condition 3**, I investigated the magnitude of the covariate's regression coefficient on the mediation model. In this condition, I varied the regression coefficient of d_1 from -1 to +1, while fixing the correlation between the independent variable and the covariate at .30, and the proportion of the binary covariate at $p = 0.4$.

In the simple mediation model with a covariate on the mediator (Condition 1), statistical power varied as a function of the correlation between the independent variable (X) and the binary covariate (W). As shown in Figure 9, the indirect effect exhibited the strongest power overall, peaking near .85 when the correlation between X and W was close to zero, but declining to approximately .45 when the correlation approached –.78 or +.78. The direct effect demonstrated lower and more stable power, ranging from .60 to .70, while the total effect maintained the highest and most consistent power, fluctuating between .80 and .85. These findings suggest that the correlation between X and W most strongly undermines the ability to detect the indirect effect, whereas the detectability of the total effect remains comparatively robust.

Figure 9a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with a covariate on the mediator (Condition 1). Power estimates are shown across varying correlations between the independent variable (X) and the binary covariate (W), with fixed regression coefficients and a fixed proportion of p.

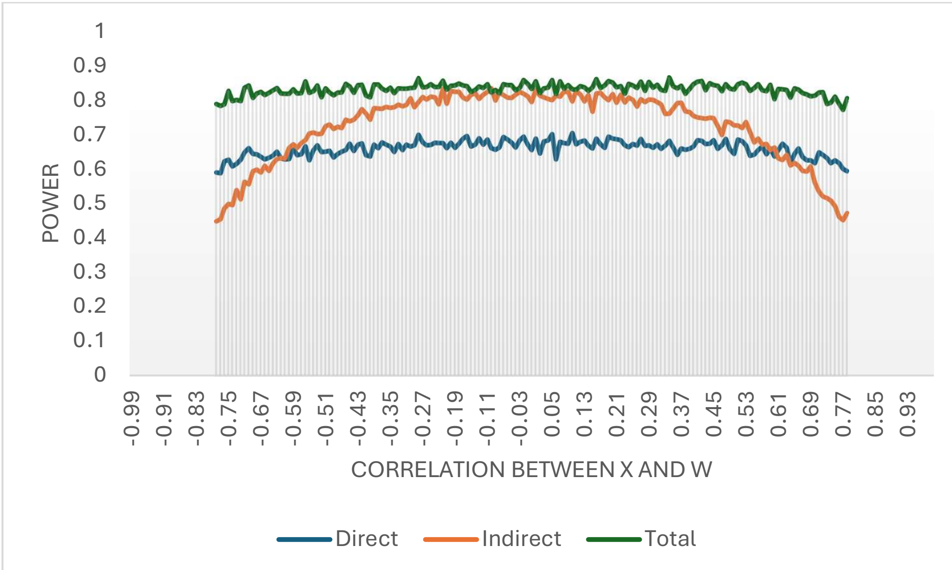
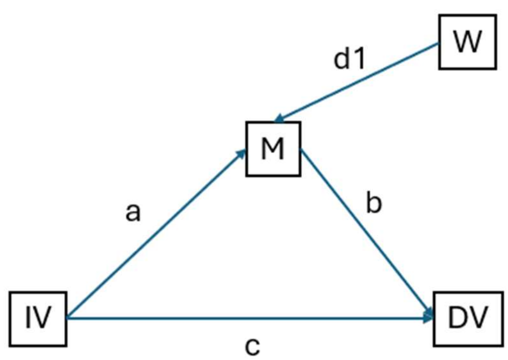


Figure 9b. Path Diagram of the Simulation.



In the simple mediation model with covariates on both the mediator and the dependent variable (Condition 1), statistical power exhibited inverted-U shaped patterns across the correlation between the independent variable (X) and the binary covariate (W). As illustrated in Figure 10, both the indirect and total effects displayed the strongest power, peaking at approximately .80 to .85 when the correlation was near zero, and declining to around .50 as the correlation approached the extremes of –.78 or +.78. The direct effect followed a similar pattern but with consistently lower power, ranging from .45 at the extremes to .70 at peak. These findings suggest that the correlation between X and W most strongly undermines the ability to detect the indirect effect, direct effect, and total effect.

Figure 10a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with covariates on both the mediator and the dependent variable (Condition 1). Power estimates are shown across varying correlations between the independent variable (X) and the binary covariate (W), with fixed regression coefficients and a fixed proportion of p.

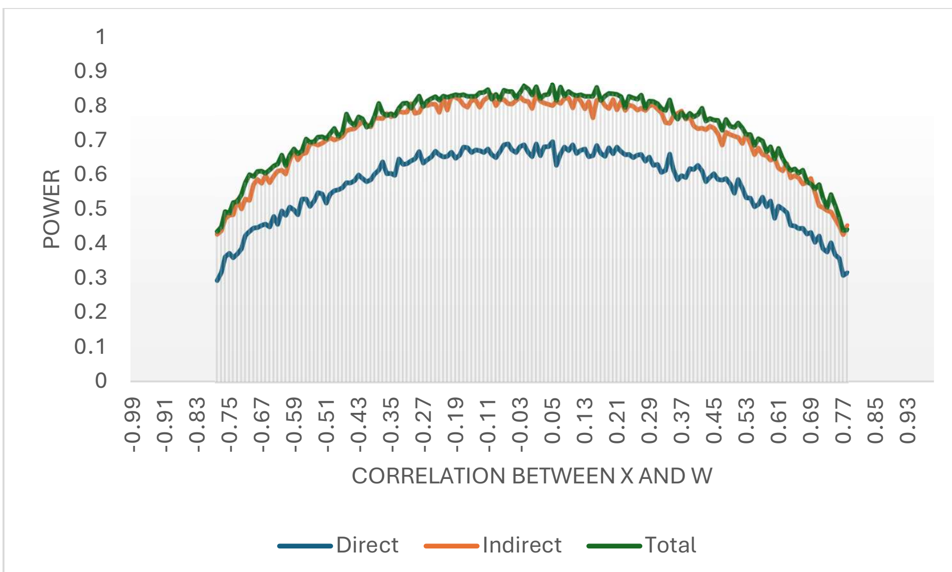
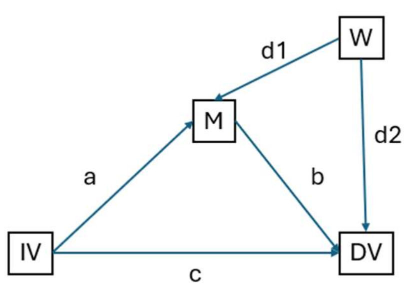


Figure 10b. Path Diagram of the Simulation.



In the serial mediation model with a covariate on the mediator (Condition 1), statistical power varied depending on whether the pathway involved M1, the mediator directly influenced by the covariate. As shown in Figure 11, the indirect effect through M1 (X→M1→Y) and the total effect were most sensitive to the correlation between the independent variable (X) and the covariate (W), with power maximized near zero correlation and declining as the correlation approached the extremes of –0.78 or +0.78. In contrast, the indirect effect through M2M_2M2 (X→M2→Y), the sequential indirect pathway (X→M1→M2→Y), and the direct effect were comparatively unaffected, as the covariate was not included on those paths. These findings suggest that the influence of multicollinearity between X and W is localized to pathways involving the mediator directly associated with the covariate.

Figure 11a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with a covariate on the mediator (Condition 1). Power estimates are shown across varying correlations between the independent variable (X) and the binary covariate (W), with fixed regression coefficients and a fixed proportion of p.

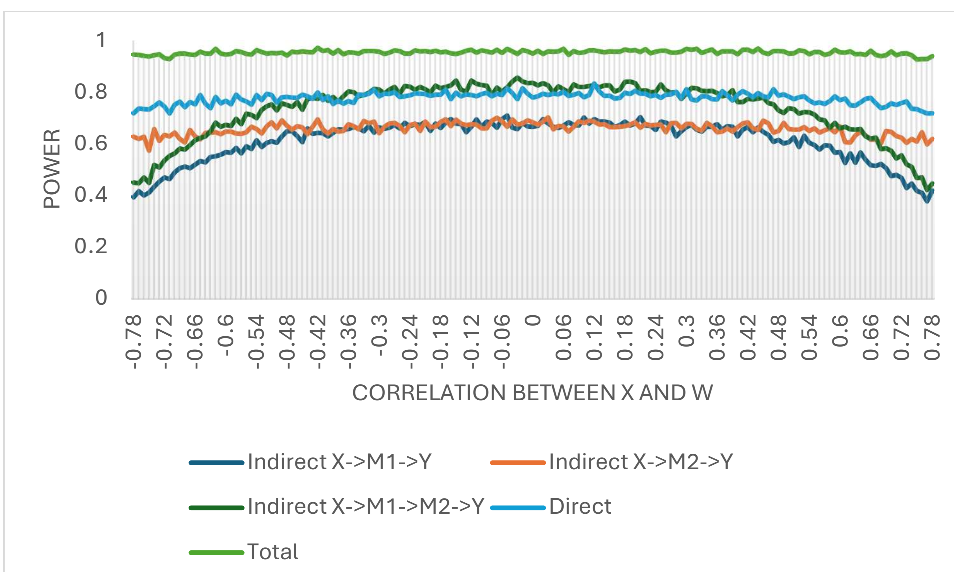
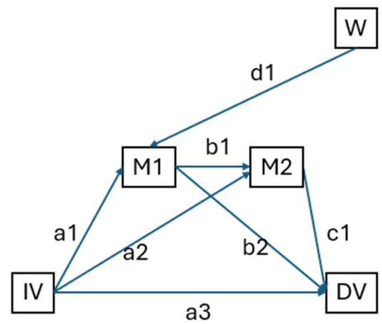


Figure 11b. Path Diagram of the Simulation.



In the serial mediation model with covariates on both mediators and the dependent variable (Condition 1), statistical power for all effects was influenced by the correlation between the independent variable (X) and the covariate (W). As illustrated in Figure 12, the total effect consistently demonstrated the highest power, peaking above .90 when the correlation was near zero and declining toward .70 as the correlation approached either extreme. The direct effect and each indirect pathway—including X→M1→Y, X→M2→Y, and the sequential pathway X→M1→M2→YX \to M_1 \to M_2 \to YX→M1→M2→Y—also followed inverted-U patterns, with maximum power occurring near zero correlation and reduced detectability as the correlation strengthened in either direction. Because the covariate was included on both mediators and the dependent variable, the influence of multicollinearity between X and W extended across all pathways, reducing statistical power when the two variables were highly correlated.

Figure 12a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with covariates on both mediators and the dependent variable (Condition 1). Power estimates are shown across varying correlations between the independent variable (X) and the binary covariate (W), with fixed regression coefficients and a fixed proportion of p.

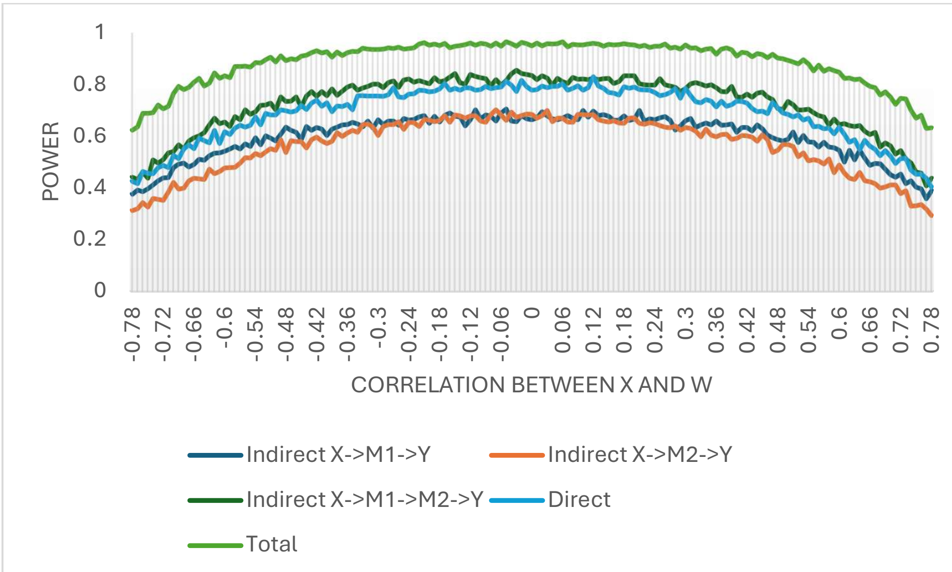
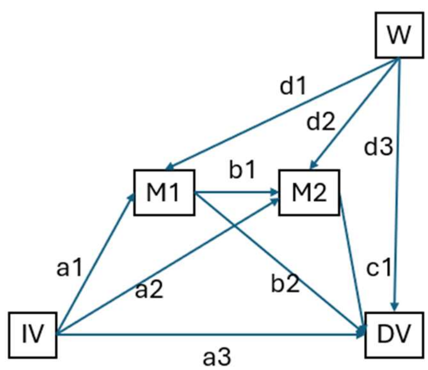


Figure 12b. Path Diagram of the Simulation.



In the simple mediation model with a covariate on the mediator (Condition 2), statistical power for the indirect effect followed an inverted-U shaped pattern across the correlation between the independent variable (X) and the binary covariate (W). As shown in Figure 13, power peaked near .80–.85 when the correlation between X and W was close to zero and declined toward .40–.50 as the correlation approached the extremes of –1 or +1. This overall pattern was consistent across all values of the covariate's proportion *p* (.10 to .50), with minimal differences observed between levels of *p*. These findings suggest that the detectability of the indirect effect is primarily determined by the correlation between X and W, whereas variation in the covariate's proportion has little influence on statistical power.

Figure 13a. Statistical power for detecting the indirect effect in the simple mediation model with a covariate on the mediator (Condition 2). Power estimates are shown across varying correlations between the independent variable (X) and the binary covariate (W), with fixed regression coefficients and the binary covariate proportion varying from *p* =.10 to *p* =.50.

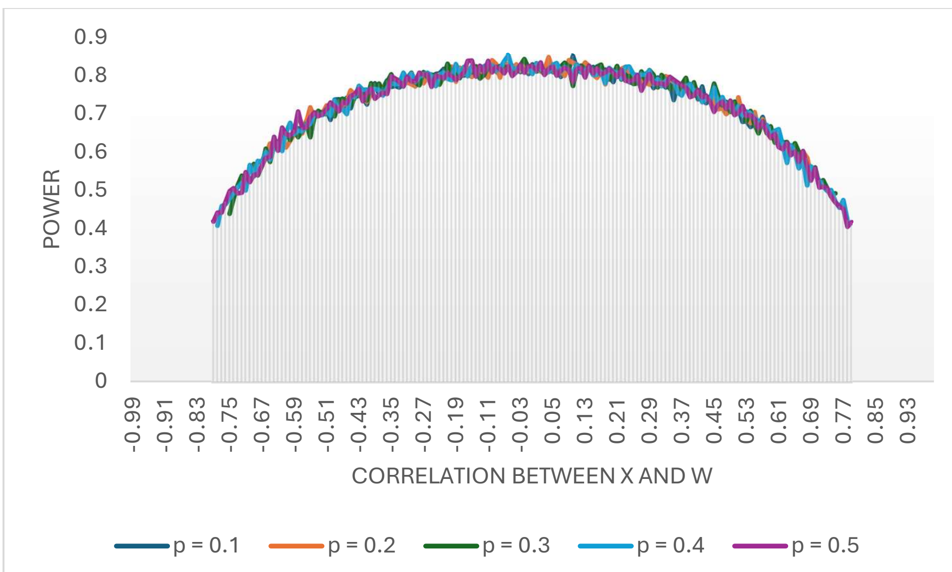
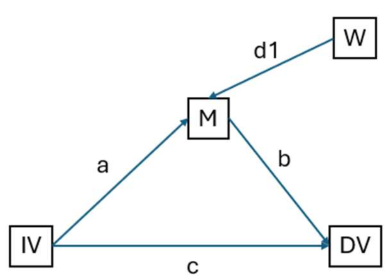
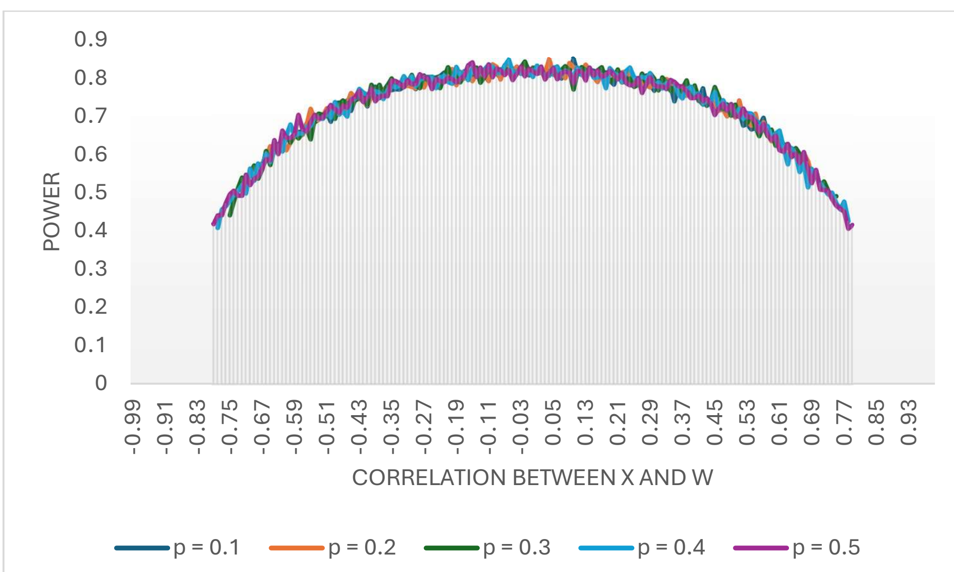


Figure 13b. Path Diagram of the Simulation.



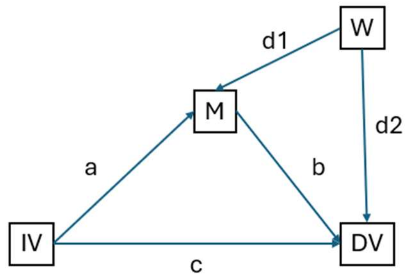
In the simple mediation model with covariates on both the mediator and the dependent variable (Condition 2), statistical power for the indirect effect followed an inverted-U shaped pattern across the correlation between the independent variable (X) and the binary covariate (W). As shown in Figure 14, power peaked near .80–.85 when X and W were uncorrelated and declined toward .40–.50 as the correlation approached either boundary[5]. This trend was consistent across all levels of the covariate's proportion *p* (.10 to .50), with only minimal differences in power observed between them. These results suggest that, similar to Figure 13, statistical power for the indirect effect is primarily driven by the correlation between X and W, whereas variation in the covariate's proportion has negligible influence.

Figure 14a. Statistical power for detecting the indirect effect in the simple mediation model with covariates on both the mediator and the dependent variable (Condition 2). Power estimates are shown across varying correlations between the independent variable (X) and the binary covariate (W), with fixed regression coefficients, and the binary covariate proportion *p* set at .10, .20, .30, .40, and .50.



[5] The boundaries depend on the proportion *p* value.

Figure 14b. Path Diagram of the Simulation.



In the serial mediation model with a covariate on the mediator (Condition 2), statistical power for detecting the indirect effect followed a clear inverted-U shaped pattern across the correlation between the independent variable (X) and the covariate (W). As shown in Figure 15, power peaked near .80–.85 when X and W were uncorrelated and declined substantially toward .40–.50 as the correlation approached boundaries. This pattern was consistent across all levels of the covariate's proportion *p* (.10 to .50), with only negligible differences between conditions. These findings suggest that the correlation between X and W, rather than the proportion of the binary covariate, is the primary determinant of statistical power for the indirect effect in this model.

Figure 15a. Statistical power for detecting the indirect effect in the serial mediation model with a covariate on the mediator (Condition 2). Power estimates are shown across varying correlations between the independent variable (X) and the binary covariate (W), with fixed regression coefficients, and the binary covariate proportion p set at .10, .20, .30, .40, and .50.

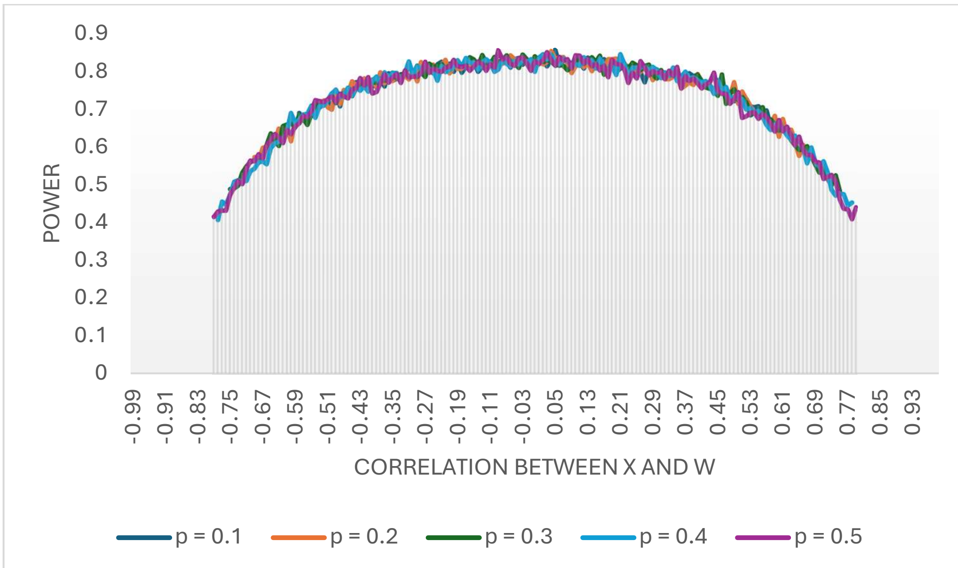
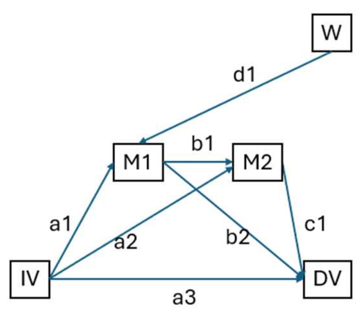


Figure 15b. Path Diagram of the Simulation.



In the serial mediation model with covariates on both mediators and the dependent variable (Condition 2), statistical power for the indirect effect followed the same inverted-U shaped trajectory observed in prior models. As displayed in Figure 16, power peaked near .80–.85 when the correlation between the independent variable (X) and the covariate (W) was close to zero, and declined toward .40–.50 as the correlation approached the boundaries.

This trend was consistent across all values of the covariate's proportion *p* (.10 to .50), with only trivial differences in statistical power between proportions. These results indicate that the detectability of indirect effects in this model is driven primarily by the correlation between X and W, while the proportion of the binary covariate exerts negligible influence.

Figure 16a. Statistical power for detecting the indirect effect in the serial mediation model with covariates on both mediators and the dependent variable (Condition 2). Power estimates are shown across varying correlations between the independent variable (X) and the binary covariate (W), with fixed regression coefficients, and the binary covariate proportion *p* set at .10, .20, .30, .40, and .50.

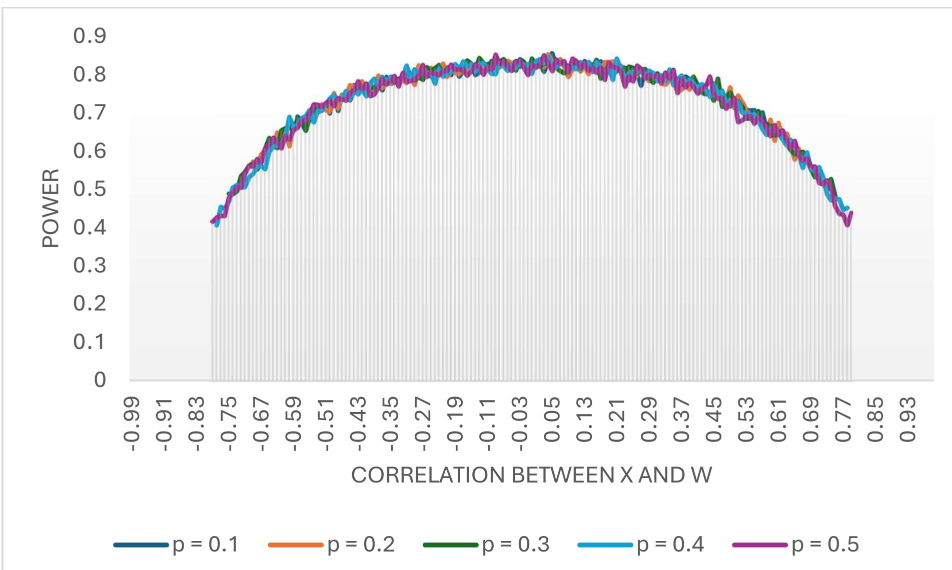
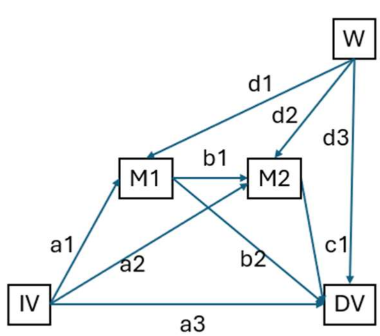


Figure 16b. Path Diagram of the Simulation.



In the simple mediation model with a covariate on the mediator (Condition 3), statistical power remained relatively stable across the full range of the covariate's regression coefficient (d_1). As shown in Figure 17, the indirect effect consistently demonstrated moderate power, approximately .78 to .82, while the total effect showed slightly higher power, around .80 to .85. The direct effect was somewhat weaker, ranging from .65 to .70, but also remained stable across values of d_1. These results indicate that, under this condition, the magnitude of the covariate's regression coefficient had little influence on the detectability of direct, indirect, or total effects, with all effects showing robust and consistent power.

Figure 17a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with a covariate on the mediator (Condition 3). Power estimates are shown across varying values of the covariate's regression coefficient (d_1, –1 to +1), with the correlation between the independent variable (X) and the covariate (W) fixed at .30 and the covariate proportion fixed at p = .40.

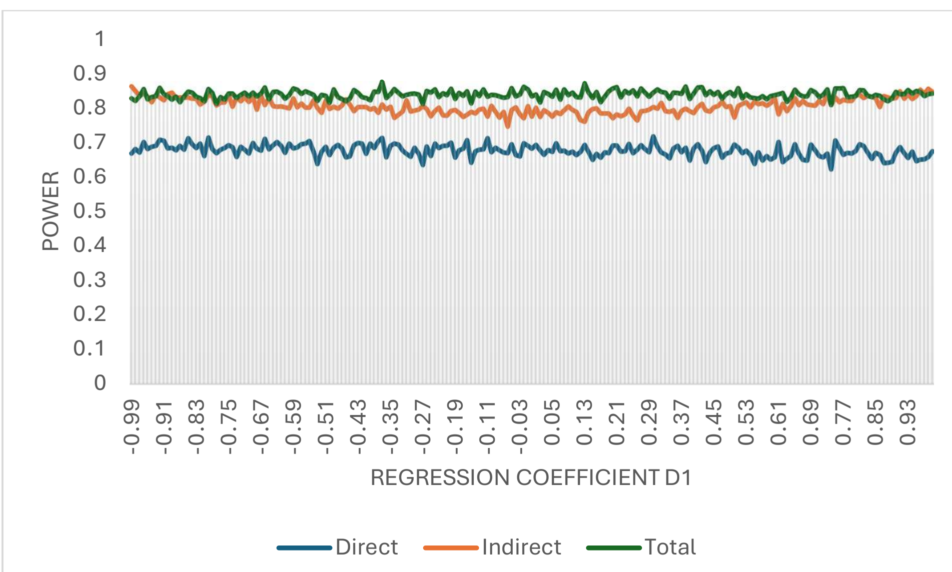
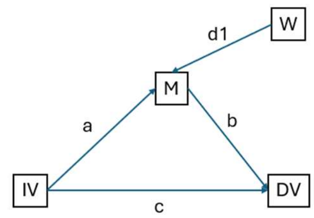


Figure 17b. Path Diagram of the Simulation.



In the simple mediation model with covariates on both the mediator and the dependent variable (Condition 3), statistical power remained stable across values of the covariate's regression coefficient (d_1). As illustrated in Figure 18, the total effect and indirect effect consistently exhibited strong power, approximately .78 to .82, whereas the direct effect showed lower but steady power, ranging from .62 to .66. These findings suggest that the magnitude of the covariate's regression coefficient had little influence on statistical power under this condition, with all effects maintaining stable detectability across the full range of d_1.

Figure 18a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with covariates on both the mediator and the dependent variable (Condition 3). Power estimates are shown across varying values of the covariate's regression coefficient (d_1, –1 to +1), with the correlation between the independent variable (X) and the covariate (W) fixed at .30 and the covariate proportion fixed at p = .40.

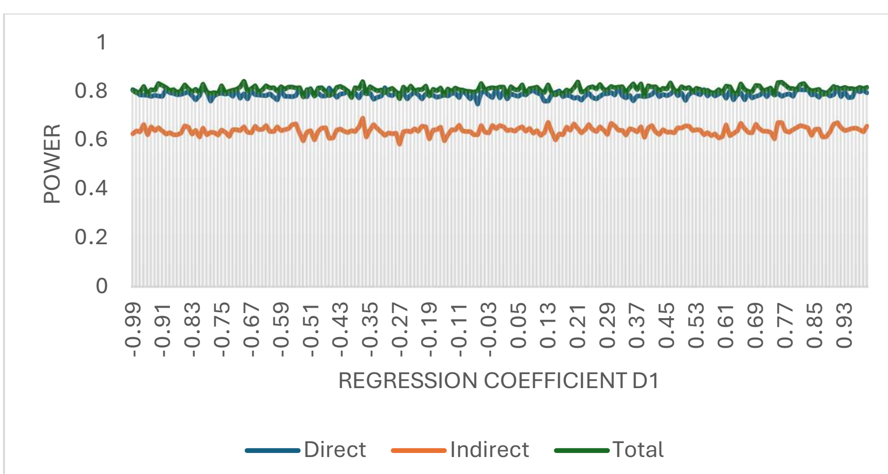
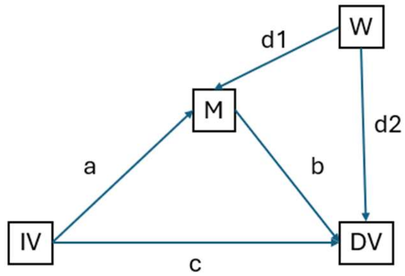


Figure 18b. Path Diagram of the Simulation.



In the serial mediation model with a covariate on the mediator (Condition 3), statistical power remained highly stable across the full range of the covariate's regression coefficient (*d_1*). As shown in Figure 19, the total effect exhibited the strongest detectability (≈ .94–.98), followed by the sequential indirect effect X→M1→M2→Y (≈ .84–.90) and the indirect effect through M1 (≈ .77–.83). The direct effect was comparable to the X→M1→Y pathway (≈ .76–.82), while the indirect effect through M2 was consistently lower (≈ .62–.68). Across all paths, variation in *d_1* produced only minimal fluctuations in power, indicating that—when the X–W correlation (.30) and the binary proportion (p = .40) are held constant—the magnitude of the covariate's effect on M1 has little impact on effect detectability.

Figure 19a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with a covariate on the mediator (Condition 3). Power estimates are shown across varying values of the covariate's regression coefficient (d_1, –1 to +1), with the correlation between the independent variable (X) and the covariate (W) fixed at .30 and the covariate proportion fixed at p = .40.

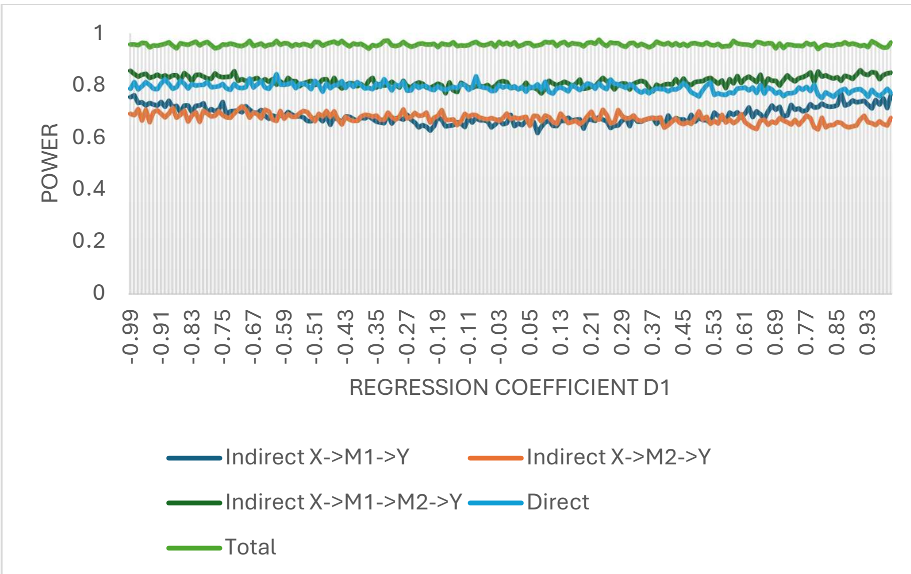
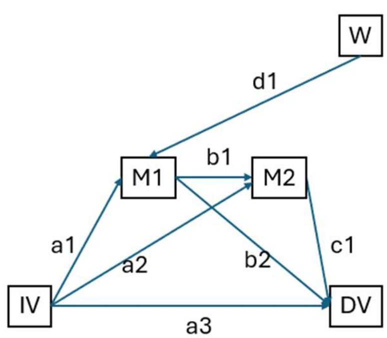


Figure 19b. Path Diagram of the Simulation.



In the serial mediation model with covariates on both mediators and the dependent variable (Condition 3), statistical power remained highly stable across the full range of the covariate's regression coefficient (d_1). As shown in Figure 20, the total effect consistently exhibited the strongest power (≈ .92–.96). Among the indirect effects, the sequential pathway

(X→M1→M2→Y) demonstrated strong detectability (≈ .80–.85), followed by the pathway through M1 (X→M1→Y), which was similar in magnitude (≈ .76–.82). The direct effect also maintained stable power (≈ .75–.80). By contrast, the indirect pathway through M2 (X→M2→Y) was consistently weaker (≈ .61–.66). These results indicate that the magnitude of the covariate's regression coefficient exerted little influence on power when covariates were included on both mediators and the dependent variable, with overall power levels remaining robust across all effects.

Figure 20a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with covariates on both mediators and the dependent variable (Condition 3). Power estimates are shown across varying values of the covariate's regression coefficient (d_1, –1 to +1), with the correlation between the independent variable (X) and the covariate (W) fixed at .30 and the covariate proportion fixed at p = .40.

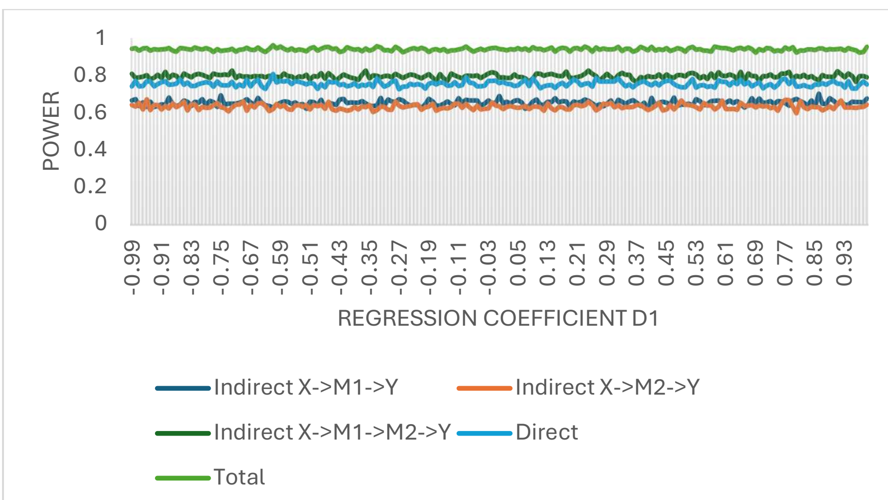
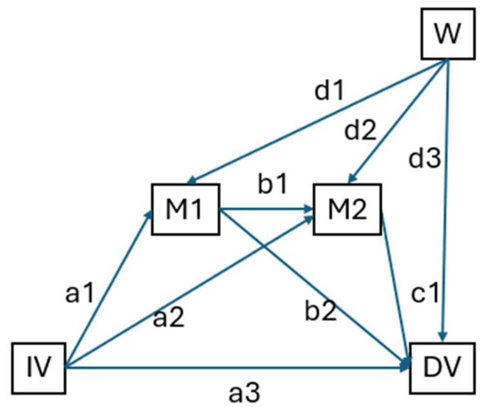


Figure 20b. Path Diagram of the Simulation.



Experimental Setting, Binary Covariate

Organizational scientists also include binary covariate in experimental designs, like those covariates in survey study (e.g., gender, employment type, and race). However, scholars cannot extend the results from survey design with binary covariate to conclude the impact of covariate on the power of mediation model as the independent variable is randomly assigned[6].

To simulate the experimental design with binary covariate, I did simulation in two ways to understand the impact. In condition 1, I investigated the magnitude of the covariate's regression coefficient on the mediation model. I vary the regression coefficient of d_1, ranging from -1 to +1, fix other regression coefficients, and fix the proportion p = 0.4.

In condition 2, I explore the impact of the proportion of a binary variable on the power of indirect effect[7]. I fix the regression coefficients (i.e., 0.1 for the direct effects and covariate's regression coefficient, and 0.15 for the indirect effect's coefficient), and vary the proportion p from 0.1 to 0.5.

[6] Explained details can be found at the early section (Experimental Setting, Continuous Covariate)
[7] Power of other effects are also simulated. Results can be found at Appendix X.

Monte Carlo Simulation (experimental setting, binary covariate)

In the simple mediation model with a covariate on the mediator (Condition 1), statistical power estimates remained stable across the full range of the regression coefficient d_1 (–0.99 to +0.99). As shown in Figure 21, the power to detect the indirect effect ranged from .28 to .32, whereas the power to detect the direct effect was consistently lower, approximately .22 to .24. The total effect demonstrated slightly higher power, ranging from .30 to .35. These results suggest that variation in d_1 exerted negligible influence on the statistical power of the model, with the total effect achieving the highest power overall.

Figure 21a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with a covariate on the mediator (Condition 1).

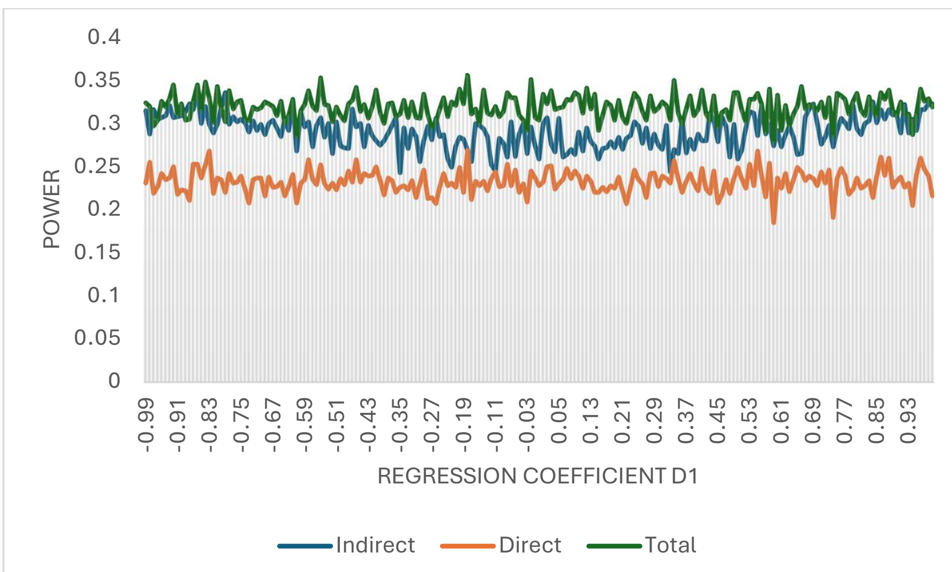
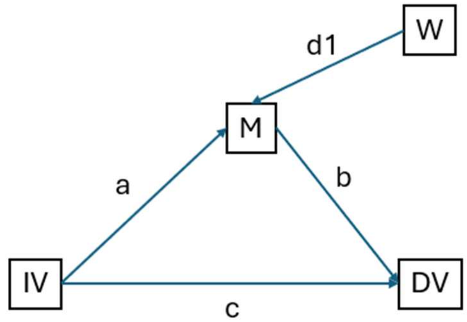


Figure 21b. Path Diagram of the Simulation.



In the simple mediation model with a covariate on both the mediator and the dependent variable (Condition 1), statistical power remained stable across values of the regression coefficient d_1 (–0.99 to +0.99). As shown in Figure 22, the power to detect the indirect effect ranged between .28 and .32, whereas the power to detect the direct effect remained lower, approximately .22 to .24. The power to detect the total effect was the highest, consistently around .30 to .35. Again, these results suggest that variation in d_1 exerted negligible influence on power, and the overall pattern of power remained consistent across conditions.

Figure 22a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with a covariate on the mediator and DV (Condition 1). Power remained stable across values of the regression coefficient d_1.

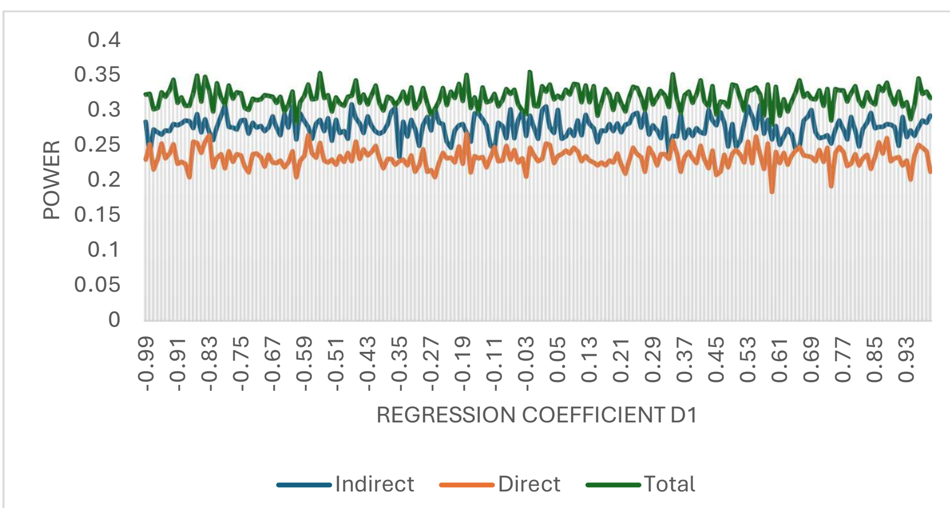
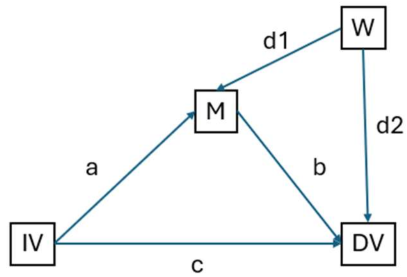


Figure 22b. Path Diagram of the Simulation.



In the serial mediation model with a covariate on the mediator (Condition 1), the power to detect the indirect effect through M1 (X→M1→Y) was approximately .25–.30, while the indirect effect through M2 (X→M2→Y) remained lower, at about .18–.20. The sequential indirect effect through both mediators (X→M1→M2→Y) demonstrated the highest power, approximately .42–.48. The direct effect exhibited consistently low power (.22–.26), whereas the total effect achieved the highest power levels overall (.44–.48). Across all effects, varying d_1 had negligible influence on power.

Figure 23a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with a covariate on the mediator (Condition 1). Power remained stable across values of the regression coefficient d_1.

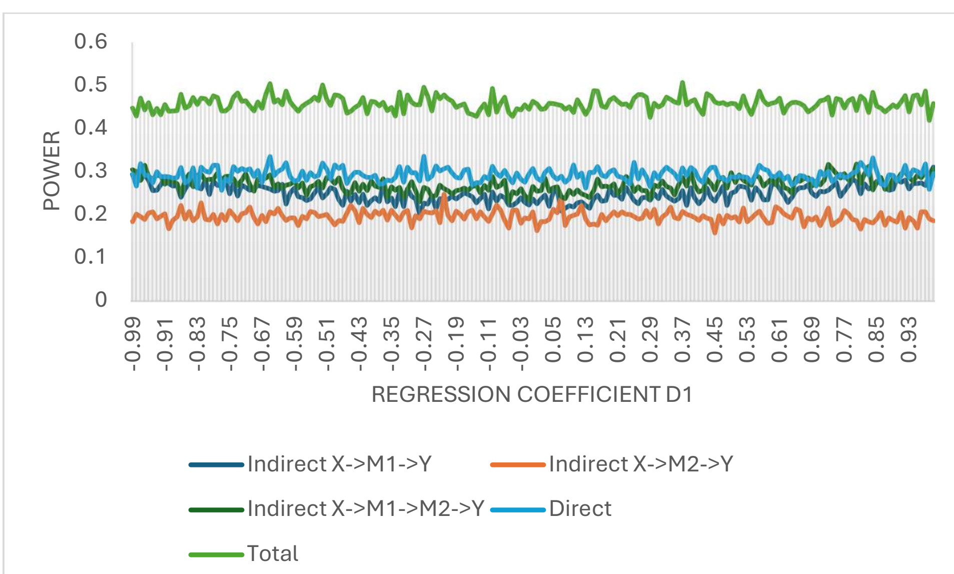
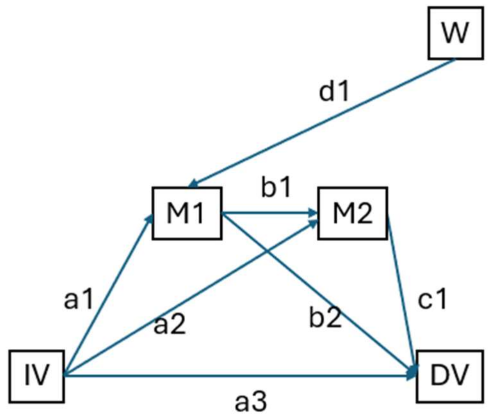


Figure 23b. Path Diagram of the Simulation.



In the serial mediation model with a covariate on both the mediator and the dependent variable (Condition 1), statistical power estimates remained largely stable across the full range of the regression coefficient d_1 (–0.99 to +0.99). As displayed in Figure 24, the power to detect the indirect effect through M1 (X→M1→Y) was moderate, ranging between .25 and .30, whereas the indirect effect through M2 (X→M2→Y) was consistently lower, approximately .18 to .20. The sequential indirect effect spanning both mediators (X→M1→M2→Y) demonstrated the highest indirect power, approximately .42 to .48. These results suggest that variation in d_1 did not meaningfully influence the power of any pathway.

Figure 24a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with a covariate on the mediator and DV (Condition 1). Power remained stable across values of the regression coefficient d_1.

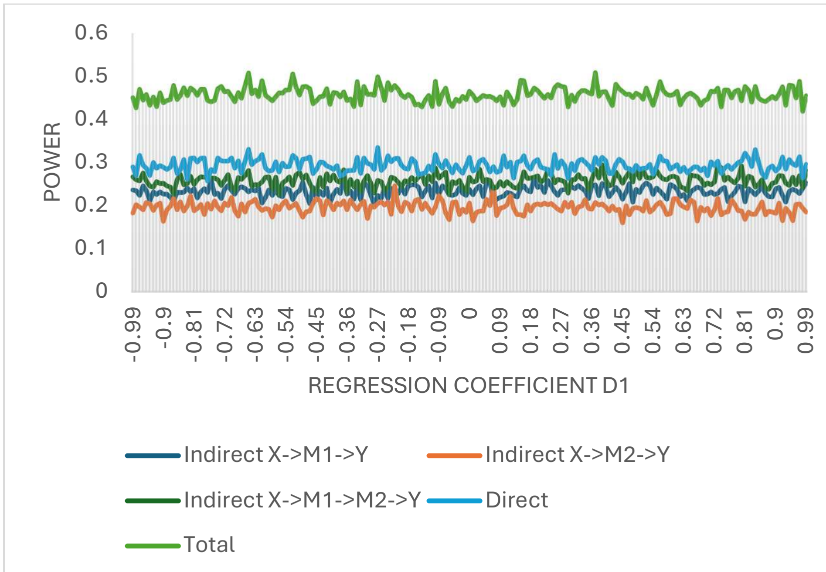
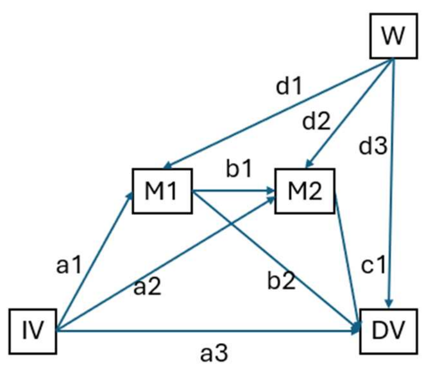


Figure 24b. Path Diagram of the Simulation.



In the simple mediation model with a covariate on the mediator (Condition 2), power estimates were consistent across values of the parameter *p* (0.10 to 0.50). As shown in Figure 25, the power to detect the indirect effect was moderate, ranging from .27 to .31, while the power to detect the direct effect was consistently lower, approximately .22 to .25. The total effect demonstrated the highest power, ranging from .31 to .36. These results indicate that variation in *p* did not meaningfully influence statistical power, and the total effect remained the most detectable pathway.

Figure 25a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with a covariate on the mediator (Condition 2). Power estimates remained stable across values of the parameter *p*.

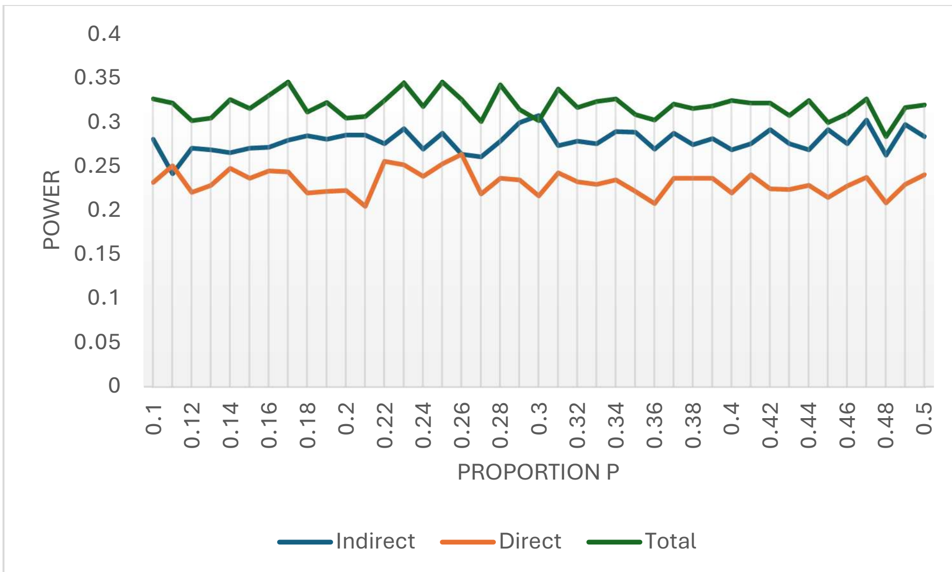
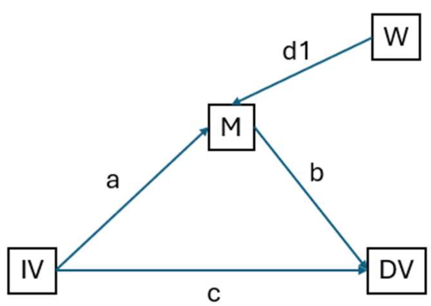


Figure 25b. Path Diagram of the Simulation.



In the simple mediation model with a covariate on both the mediator and the dependent variable (Condition 2), power remained largely stable across values of the parameter *p* (0.10 to 0.50). As shown in Figure 26, the power to detect the indirect effect was moderate, ranging between .27 and .31, whereas the power to detect the direct effect was consistently lower, approximately .22 to .25. The total effect again demonstrated the highest power, ranging from .31 to .36. These findings indicate that the inclusion of a covariate on both the mediator and outcome variable did not meaningfully affect the relative detectability of the three effects, with the total effect remaining the most robust across values of *p*.

Figure 26a. Statistical power for detecting indirect, direct, and total effects in the simple mediation model with a covariate on both the mediator and the dependent variable (Condition 2). Power estimates remained stable across values of the parameter *p*.

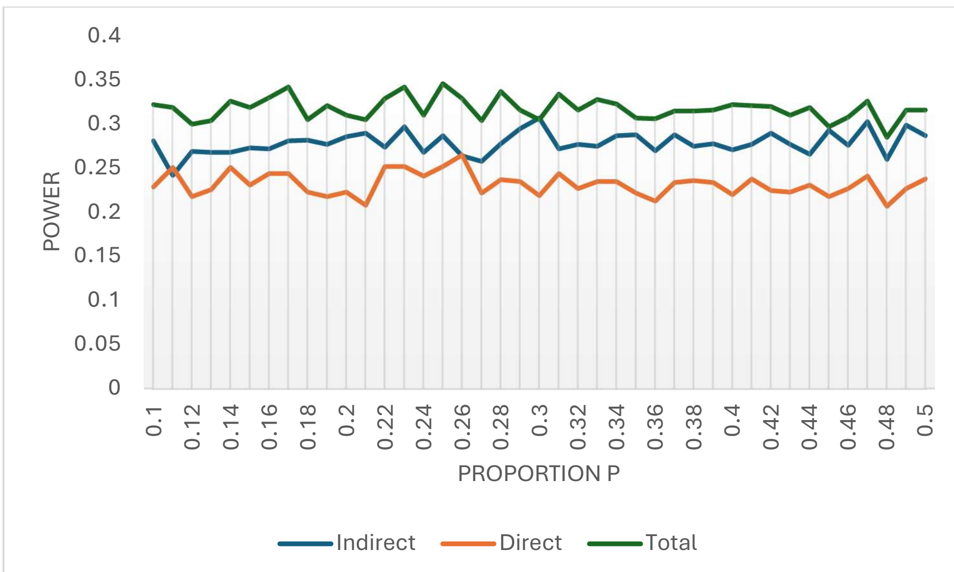
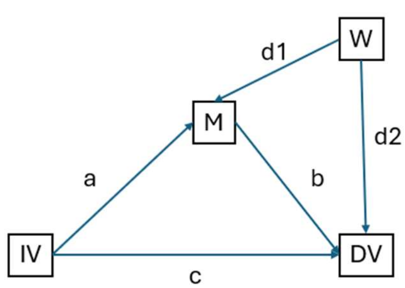


Figure 26b. Path Diagram of the Simulation.



In the serial mediation model with a covariate on the mediator (Condition 2), power estimates were consistent across values of the parameter *p* (0.10 to 0.50). As displayed in Figure 27, the indirect effect through M1 (X→M1→Y) showed moderate power, ranging between .25

and .30, while the indirect effect through M2 (X→M2→Y) was weaker, approximately .18 to .20. The sequential indirect pathway (X→M1→M2→Y) demonstrated the highest indirect effect power, ranging from .42 to .48. These results indicate that variation in *p* did not substantially alter statistical power, with the sequential indirect and total effects remaining the most detectable.

Figure 27a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with a covariate on the mediator (Condition 2). Power estimates remained stable across values of the parameter *p*.

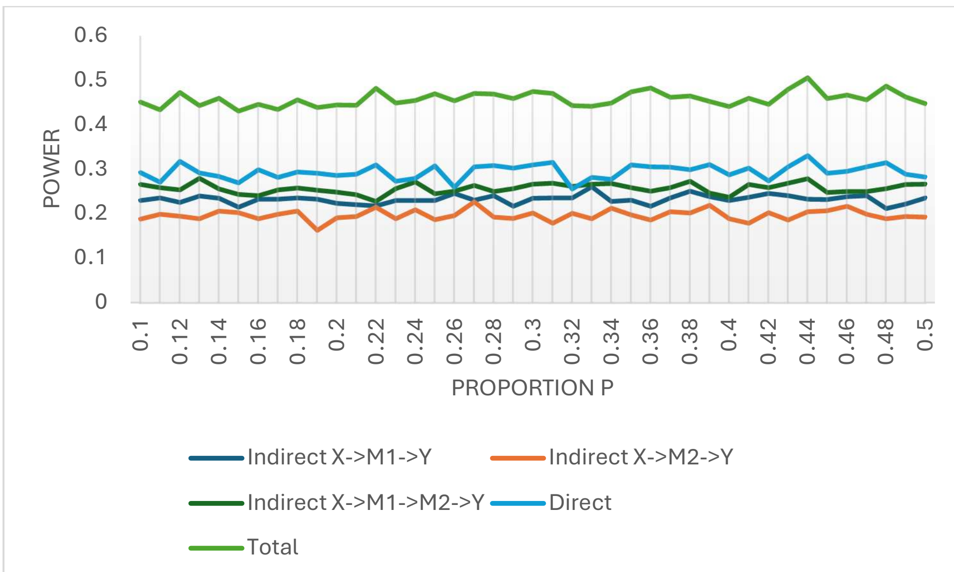
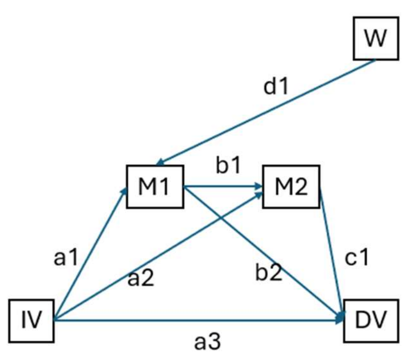


Figure 27b. Path Diagram of the Simulation.



In the serial mediation model with covariates on both the mediators and the dependent variable (Condition 2), power estimates remained consistent across values of parameter *p* (0.10 to 0.50). As shown in Figure 28, the indirect effect through M1 (X→M1→Y) demonstrated moderate power (.25–.30), while the indirect effect through M2 (X→M2→Y) was weaker, approximately .18 to .20. The sequential indirect effect through both mediators (X→M1→M2→Y) exhibited the strongest indirect power (.42–.48). The direct effect was consistently low (.22–.26), whereas the total effect demonstrated the highest power overall (.44–.50). These results indicate that the inclusion of covariates on both mediators and the dependent variable did not substantially affect the detectability of effects, with the sequential indirect and total effects remaining most robust.

Figure 28a. Statistical power for detecting indirect, direct, and total effects in the serial mediation model with covariates on both the mediators and the dependent variable (Condition 2). Power estimates remained stable across values of the parameter *p*.

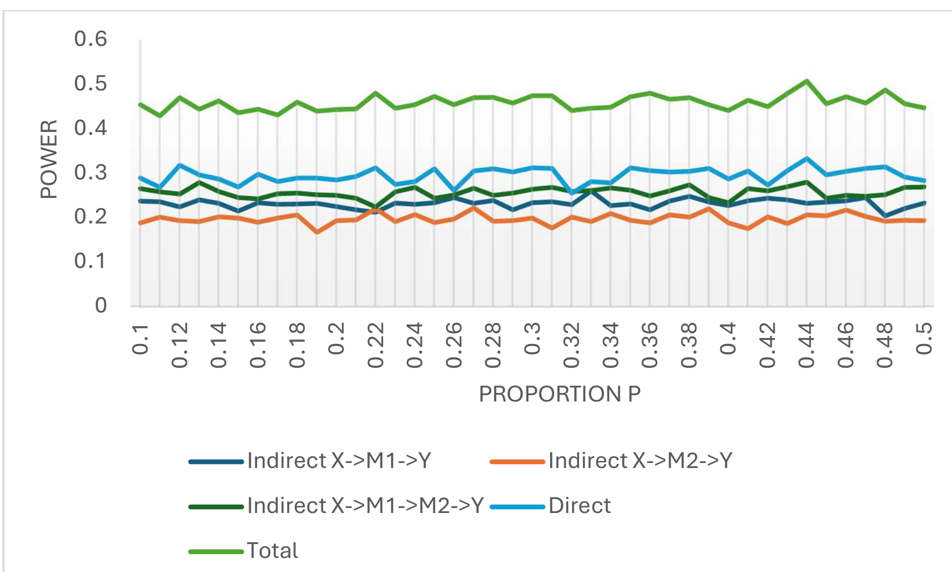
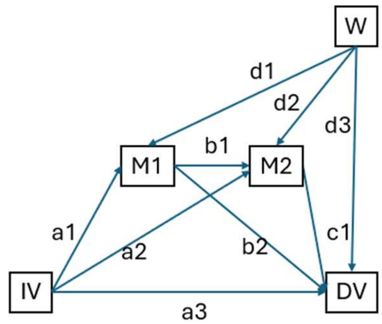


Figure 28b. Path Diagram of the Simulation.



Implication

This study demonstrates that the inclusion of covariates in mediation models does not uniformly enhance statistical power. In survey designs, whether covariates were continuous or binary, power was maximized when covariates were theoretically relevant and independent of the predictor, but multicollinearity substantially reduced detectability of effects, sometimes by half or more. In contrast, variation in the distribution of binary covariates (e.g., proportion or regression strength) had little impact on power. In experimental designs, covariates influenced power primarily through their association with the mediator. For continuous covariates, stronger associations reduced the detectability of indirect and total effects, particularly when the covariate was entered only on the mediator path. By comparison, binary covariates exerted little influence on power in experimental settings. Collectively, these findings show that covariates can either bolster or erode statistical power, depending on design context and measurement scale, and highlight the importance of carefully evaluating their inclusion in mediation studies.

Table 1

Summary of Simulation Findings on Covariate Inclusion and Statistical Power in Mediation Models

Design Type	Covariate Type	Key Findings	Implications for Power and Study Design
Survey	Continuous	Power maximized when covariates were theoretically relevant and independent of the predictor. Multicollinearity with predictors substantially reduced power (up to 50% loss).	Covariates should always be included if justified by theory or prior literature. However, when a covariate is correlated with the IV, power decreases; researchers should plan for larger sample sizes to maintain adequate power.
Survey	Binary	Power was maximized when covariates were theoretically relevant and independent of the predictor. In contrast, multicollinearity with predictors substantially reduced power, while the proportion p had little influence.	The proportion p of a binary covariate exerted minimal influence on power when independent of the predictor. However, similar to continuous covariates, when a binary covariate was correlated with the independent variable, statistical power decreased. Researchers should therefore plan for larger sample sizes to maintain adequate power in such cases.
Experiment	Continuous	Stronger covariate–mediator associations increased the size of both the indirect and total effects, particularly when the covariate was included only on the mediator path. In contrast, the effect size of the covariate itself (i.e., its regression coefficient) did not influence statistical power.	Covariates can increase statistical power; smaller samples may be sufficient when continuous covariates are included for mediators.
Experiment	Binary	Binary covariates exerted little influence on power.	Binary covariates exert minimal influence; inclusion unlikely to meaningfully alter power.

These findings challenge the conventional assumption that statistical controls always strengthen analyses and highlight the importance of carefully evaluating covariate inclusion during study design and power analysis.

Methodologically, this work provides simulation-based evidence clarifying when covariates aid versus hinder effect detection in mediation. It extends existing guidance by moving beyond regression-based principles and offering recommendations specific to mediation contexts. Practically, the findings suggest that researchers should consider increasing sample size when including covariates to preserve adequate power. Theoretically, the study underscores the importance of aligning covariate use with causal assumptions, thereby improving the validity of mediation-based theory testing in management research.

Limitation

Several limitations should be acknowledged. The simulations assumed normally distributed data, linear relations, and fixed effect size ranges. Real-world data may involve measurement error, nonlinearities, or multilevel dependencies that alter these patterns. Moreover, while this work examined continuous and binary covariates, categorical covariates with more than two levels remain unexplored. Addressing these issues presents an important direction for future research.

Conclusion

Taken together, this study advances both methodological rigor and theoretical precision in organizational science. By showing that covariates can either bolster or erode statistical power, it calls for more deliberate justification of control variables and for power analyses that explicitly incorporate their effects. Ultimately, these insights equip researchers with practical guidance for designing adequately powered mediation studies, thereby fostering more credible and impactful contributions to management scholarship.

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CHAPTER FOUR
GENERAL DISCUSSION AND CONCLUSION

Overview of the Dissertation

The overarching purpose of this dissertation was to strengthen the production and translation of management knowledge. Management scholars frequently seek to explain not only whether organizational phenomena are related, but also how and why those relationships occur. Mediation models have consequently become one of the most common tools for developing and testing theories about organizational processes. However, mediation findings can contribute meaningfully to theory and practice only when the studies producing them are appropriately designed, sufficiently powered, and interpreted.

This dissertation began with the broader problem of the science-practice gap and then examined two methodological issues that influence whether management research can produce sufficiently trustworthy evidence to cross that gap. Chapter 1 developed a conceptual model of translational management research organized around the reduction of uncertainty. Chapter 2 examined whether management studies using mediation models possess sufficient statistical power and introduced a simulation-based toolkit for designing more adequately powered studies. Chapter 3 investigated how covariate decisions affect statistical power under different mediation models, study designs, covariate types, and covariate placements.

Taken together, the chapters advance a central argument: methodological rigor is not separate from the translational value of management research. Rather, rigor is one of the conditions that makes translation possible. Researchers cannot responsibly recommend that practitioners implement an intervention when there remains substantial uncertainty about the effect. Statistical power, model specification, and covariate selection therefore have consequences that extend beyond technical accuracy. They influence credibility, interpretability, and eventual usability of management knowledge.

Integrating the Chapters

The three chapters operate at different levels of analysis, but they address the same underlying problem. Chapter 1 asked how management knowledge can move responsibly from scientific discovery to organizational application. Chapters 2 and 3 examined two upstream conditions that determine whether that movement is warranted.

The translational model developed in Chapter 1 emphasizes that uncertainty must be reduced at multiple stages. Chapter 2 showed that inadequate statistical power preserves uncertainty because researchers may be unable to distinguish the absence of an effect from the absence of sufficient sensitivity to detect it. An underpowered mediation study cannot provide strong evidence that a proposed mechanism does not operate. At the same time, statistically significant findings from low-powered research may be imprecise, further complicating decisions about replication and implementation.

Chapter 3 showed that analytical decisions can create or intensify uncertainty even when the nominal sample size remains unchanged. Covariates can alter the precision of individual paths, the estimated indirect effect, and the probability that an effect will be detected. The same covariate can have different consequences in a survey than in an experiment and in a simple model than in a serial model. Thus, researchers cannot assess the credibility of mediation evidence solely by looking at sample size or statistical significance. They must evaluate the full system of design assumptions and model decisions that produced the result.

Together, the chapters suggest that translation begins before data collection. It begins when researchers define the effect they need to detect, select a design capable of identifying that effect, determine a defensible sample size, decide which covariates belong in the model, and assess how those decisions influence the study's sensitivity. Translational management is therefore not only a later-stage activity in which completed findings are communicated to practitioners. It is a principle that should guide the design.

Conclusion

This dissertation sought to strengthen mediation-based management research by connecting translational value, statistical power, and covariate decisions. Chapter 1 proposed that management knowledge becomes more actionable as researchers systematically identify and reduce the uncertainties that stand between discovery and implementation. Chapter 2 demonstrated that many mediation studies are not designed with sufficient attention to the power of their focal effects and provided a customizable simulation-based alternative. Chapter 3 showed that covariates do not uniformly increase power and that their consequences depend on the study design, model structure, measurement characteristics, and relationships among variables.

The common lesson across the three chapters is that responsible translation requires more than producing a statistically significant result and communicating it clearly. It requires evidence generated by designs that are capable of detecting the effects being claimed, models that reflect the theoretical process under investigation, and analytical decisions that have been justified before conclusions are drawn.

Management research can influence organizations only when its findings are sufficiently trustworthy to support action. By clarifying how power, model complexity, and covariate decisions shape that trustworthiness, this dissertation offers a methodological foundation for reducing uncertainty and moving management knowledge more responsibly from research into practice.