Anne S. Tsui

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From Traditional Research to Responsible Research: The Necessity of Scientific Freedom and Scientific Responsibility for Better Societies

Anne S. Tsui[1,2,3,4]

[1]W.P. Carey School of Business, Arizona State University, Tempe, Arizona, USA; email: anne.tsui@asu.edu

[2]Mendoza College of Business, University of Notre Dame, Notre Dame, Indiana, USA

[3]Guanghua School of Management, Peking University, Beijing, China

[4]School of Management, Fudan University, Shanghai, China

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Abstract

My research journey spans a period of exciting new theories informing innovative practices in businesses and other organizations and a period littered with concerns about the research-practice gap, questionable research practices, and a strong emphasis on the number of publications in top journals for hiring and promotions. These recent developments led to the dilution of both scientific freedom and scientific responsibility in our scientific work. I offer my research journey to illustrate the importance of both, with the most recent endeavor in a global responsible research movement to produce useful and credible knowledge that will enable business and management practices to serve all stakeholders and to solve the world's most challenging problems. Emerging institutional changes in business schools, journals, associations, and accreditation agencies provide hope that scholars will soon find the conditions favorable for both freedom and responsibility to support their aspiration to pursue research that will contribute to better societies and meaningful careers.

> Concern for making life better for ordinary humans must be the chief objective of science. Never forget this when you are pondering over your diagrams and equations!
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> —Albert Einstein (from Isaacson 2008, p. 374)

INTRODUCTION

When I started my doctoral studies in 1978, I discovered that one of the best things about an academic life is the idea of the autonomy of inquiry (Kaplan 1964). This idea refers to the scientific freedom we have to study any problem that is important and interesting. At the same time, I was inspired by the above Albert Einstein quote, which reminds us that the purpose of science is to improve the lives of ordinary humans, not only those with power or money. Business and management research seeks to discover knowledge and offer ideas that managers or policy makers can use to improve the lives of employees, customers, and suppliers and to meet the needs of society through the high-quality goods and service they produce or offer. I believe this is the motivation that most students have when they enter the PhD program, myself included.

During my doctoral student days, I felt I was in an intellectual feast every day. The literature I read included many classic and contemporary books as well as journal articles, and they were full of insightful theories along with robust evidence from repeated testing in field and lab settings. Most of them provided actionable ideas for managers at all levels of the organizations.

My tenure evaluation was based on what I had written, the problems I was working on, and the quality of the published work. When I asked if I had to produce a certain number of papers, the answer was "No, we don't count." There was no journal list we had to follow. We were encouraged to publish in the best journals (defined by reputation, since the journal impact factor was not yet popular then); yet publishing in the next tier as part of the portfolio was acceptable and valued. Candidates were judged (by both internal and external reviewers) on the quality of their research studies and the importance of the contributions. This scientific freedom—I realized—is the fertile ground for innovative studies and theories. The scientific community also tried to live up to its responsibility by offering solutions to challenging problems (although the major focus of most published work was on improving the economic performance of the firm, which became a problem, as I explain below). Assured of this freedom and the accompanying responsibility, most of my research projects reflect contemporary management issues in the contexts of where I worked and lived, which included the United States, Hong Kong, and the United States again.

Then, approximately 20 years into my career, something changed. The research culture was directing faculty to publish in a set of journals defined by the school's tenure and promotion committee. Top journals would publish only papers with novel ideas that contributed to theory. Replication or research without theory was not valued by editors and reviewers. In hiring, promotion, and tenure decisions, what problems a scholar studied or whether the findings offered actionable solutions to practice were not as important as the number of articles published in the defined set of journals. Doctoral students were told papers in the top journals are necessary for getting hired. Young scholars were reluctant to work on problems that were difficult or new in the journals. I too found research to be less fun and less meaningful and the pressure of having to produce papers so that my students could get jobs and I could keep up with my peers during the annual performance evaluations to be stressful. I felt constrained—unfree to pursue problems I care about because of the need to meet production goals. Research became a career issue, a matter of life and death (in academic career), rather than a calling to solve important problems and improve human conditions.

My 40 years as an academic have convinced me that most if not all members of our research community sincerely desire improving management practices and making life better for the millions of people affected by managers' decisions as leaders, marketers, producers, or designers. However, in recent years, this noble wish has become harder to realize due to the changes in the research culture in business schools. When asked to write this perspectives article, for which I am deeply grateful to *Annual Review of Organizational Psychology and Organizational Behavior* Editor Frederick Morgeson, I decided to use the opportunity to discuss the ideas of scientific freedom and scientific responsibility as both the privilege and obligation of social scientists, using my research career as an illustrative case. My modest aim is to remind our community of scholars that science is to serve society, which requires contributing valid and useful knowledge that can be used to improve the lives of all citizens. We must protect our scientific freedom as a necessary condition to live up to our scientific responsibilities.

I first briefly describe the evolution of social science research in business schools as the context for discussing my research career. I provide more details on the gradual reduction of freedom and responsibility associated with the research evaluation practices by journals and schools in recent years. Then I invite readers to join me on a brief tour of my research journey to reflect on the role of scientific freedom and responsibility in guiding (my) scientific endeavors and career decisions. I conclude with a call to reclaim our scientific freedom as a necessary condition to live up to our responsibility as public servants of societies.

A BRIEF HISTORY OF SOCIAL SCIENCE RESEARCH IN BUSINESS SCHOOLS[1]

Science is an important institution in modern societies. There is common agreement that the essential goal of science has two parts: The first is the pursuit of truth about nature, and the second is the use of knowledge discovered to advance the human condition. As much scientific work occurs in universities, science has evolved into a public institution explicitly dedicated to the greater good. Scientists are supposed to be objective and disinterested actors (without self-interest or personal agendas) and public servants who devote their professional lives to advancing the common good (Merton [1942] 1973).

The book *From Higher Aims to Hired Hands: The Social Transformation of American Business Schools and the Unfulfilled Promise of Management as a Profession* (Khurana 2007) provides a historical account of the rise, development, and "fall" (unfulfilled promise) of American business education and management as a profession. The important milestone in this history is the Gordon & Howell (1959) study funded by the US Ford Foundation. The authors called for "improving the academic quality of business schools by raising the intellectual caliber of the faculty" (Khurana 2007, p. 273). Their suggestions included treating management as a science, training faculty in social sciences, and encouraging researchers to build a verified body of knowledge, including new theories and concepts, that would be taught to students and used to help solve business problems (McLaren 2019). The business schools responded to this call by hiring new faculty with doctoral degrees in economics, psychology, sociology, and even math and statistics.

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[1]In this article, I use the terms business and management interchangeably when referring to schools, but when I refer to research, management is one of the five or six core disciplines within the business or management school, which includes accounting, finance, management, marketing, and operations/supply chain management.

The 30-plus years following the Gordon & Howell report is sometimes considered the "Golden Age" of business and management research in building the legitimacy and reputation of quantitative social science research in business schools (McKiernan & Tsui 2020). There were many important contributions to practice through the theoretical and empirical discoveries by research faculty in business schools during those years. Below are a few illustrative examples of the major contributions, suggested by my distinguished colleagues in each of the disciplines.

In finance, the Black & Scholes (1973) asset-pricing model and Malkiel & Fama's (1970) efficient market hypothesis transformed investment management. Jensen & Meckling's (1976) agency theory led to the wide-scale adoption of stock options in executive compensation in America and beyond. In the Marketing area, there is consensus that a most influential stream of research contributing to practice began with Parasuraman, Zeithaml, and Berry's (PZB's) (1985) article on service quality. This paper stimulated many academic and practitioner papers, books, and applications across industries. The PZB model is the organizing framework for the leading textbook *Services Marketing* (Zeithaml et al. 2017). There are also significant contributions in production and operations management (POM), which grew out of both economics and engineering. Important discoveries in POM were usually based on studying systems that have been shown to be effective, e.g., the Toyota Production System (Ohno 1988) or phenomena that were surprising, e.g., the bullwhip effect observed by Lee et al. (1997). POM is an interesting example of how practice contributes to research and research in turn contributes to the diffusion of the best practices.

In the management discipline, there are several theories that have influenced practice. The matrix organizational structure (Galbraith 1971) came from the research on contingency theory (Lawrence & Lorsch 1967). A common organizational practice known as management by objectives has its foundation on a strong tradition of goal-setting research (Locke & Latham 2002). Research on performance appraisals and feedback has greatly improved the performance management process in organizations (DeNisi & Murphy 2017). Another interesting example is the research on the escalation of commitment (Staw 1981), the tendency to invest more into failing projects. This idea has applications in financial, political, and psychological commitments. Applying this theory, individuals, groups, and companies can learn to accept the sunk cost and stop further investment in a failing cause of action.

An exhaustive review of how management or industrial and organizational (I/O) psychology research has contributed to good management practices or even bad practice (Ghoshal 2005) is beyond the scope of this article. An interesting revelation from this brief review of the contribution of research to practice is that most of the high-impact ideas were developed in the 30 years after the Gordon & Howell (1959) report. The book *Great Minds in Management* (Smith & Hitt 2005) features 24 scholars whose groundbreaking works endured the test of time. Most of these works were developed in the 1960s to 1980s, during the so-called Golden Age of business and management research.

However, over time, "individual faculty members were more concerned with building a reputation in their respective disciplines than with making contributions to multidisciplinary team projects" (Khurana 2007, p. 283). Solving complex problems in the real world was not the interest of most theory-oriented social scientists. The gulf between the disciplinary orientation of business school faculty and the practical and multidisciplinary interests of students and business practitioners widened over time. The year 1993 marked the second milestone of this history with Don Hambrick's Presidential address at the national meeting of the Academy of Management (AoM), titled "What if the Academy Actually Mattered?" (Hambrick 1994). In this speech, Hambrick challenged the academy to pay more attention to the world outside of our ivory tower. Many subsequent presidents joined the chorus. They include, to name just a few, Michael Hitt (1998), Andy Van de Ven (2002), Tom Cummings (2007), Angelo DeNisi (2010), and Anne Tsui

(2013b). They all challenged our Academy members to "do a better job of connecting our research to the world around us" (DeNisi 2010, p. 196). Anita McGahan's concluding words in her 2017 Presidential address in Atlanta were powerful and inspiring. She challenged us to work for real and sustainable prosperity (for all). This speech was given almost four years ago, a sober reminder that we are still far away from our collective aspiration to be caring social scientists:

> We must come to terms with the privileges that we enjoy and the responsibilities we hold in this profession. As social scientists, we can lead the world in taking on inequities and discrimination and injustice and bad leadership and bad organizational practice; it is we who can stand on our field's diversity and multidisciplinary breadth to offer solutions to the problems around us; it is we who can reject false prosperity in favor of a secure future on this planet. (McGahan 2018, p. 176)

The problem of an increasing gap between research and practice is not unique to the management discipline; similar concern was expressed in marketing (e.g., Reibstein et al. 2009), operations management (e.g., Tang 2016), accounting (e.g., Rajgopal 2020), and finance (e.g., Zingales 2015). There is now a public discourse encouraging business researchers to do better in providing science-informed ideas to decision makers in business, government, nonprofit, and other forms of organizations (Stephen & Pauwels 2018, Jack 2020).

In addition to the research-practice gap, beginning in the early 2010s, researchers also began to report on the prevalence of questionable (unethical) research practices (Bedeian et al. 2010). They use a variety of practices to maximize, at all costs, the chance of successful publications in the A-journals (Aguinis et al. 2020). Although few researchers engage in blatant cheating, many commit small "sins" (Schwab & Starbuck 2017). Scholars in management (e.g., Tourish 2019) and in psychology (John et al. 2012) have exposed the problems of data manipulation to find the best results, selecting and reporting only the significant findings (P-hacking) and formulating or revising hypotheses after the results are known (HARKing). These questionable practices render the results of most published work not trustworthy and not replicable (Ritchie 2020). This is known as the replication crisis, and it sent alarm bells through the research communities in medicine (Harris 2017), psychology (Nosek et al. 2015), management (Tourish 2019), and sciences in general (Ritchie 2020).

Some scholars (e.g., Glick et al. 2007) have observed that young scholars are under a constant "publish or perish" pressure, and life is miserable in the field of organization science. Walsh (2011, p. 215) believes that given the centrality of business in our lives, this should be "Our Golden Age," but "something is keeping us from all that we can be" and that something is the "audit culture that has emerged around us." In his perspectives article for the 2018 volume of this journal, Terry Mitchell wrote the following:

> The anxiety about getting promoted, getting tenure, and making it to full professor is palpable and makes many people, I'm sure, behave in ways that hurt them and the field. They discount their teaching. They do "safe" research. They take shortcuts. In terms of the field itself, it appears that many people burn out and move onto other endeavors. And, of course, there is a lot of good work that is never seen in published form. Substantial effort and insight by very smart people get lost. (Mitchell 2018, p. 17)

McKiernan & Tsui (2020) view these conditions as, in contrast to the Golden Age, the "Dark Age" of management research.

Although the above may seem like a bleak picture, the cause is not all lost. Self-criticism is a healthy sign that the community is engaging in self-reflection, along with self-correction. The journals are beginning to address the credibility issue by introducing editorial policies on data accessibility, replication, and transparency, led by the *Strategic Management Journal* (Bettis et al. 2016), the *Journal of International Business Studies* (Meyer et al. 2017), and *Management and Organization Review* (Lewin et al. 2016). More journals have adopted such policies, although compliance is not mandatory in most journals.

Another encouraging sign is that the AoM journals have published editorials (e.g., Tihanyi 2020) encouraging and inviting authors to join the conversations in society, to address important problems, and to recognize that being "interesting" is not enough. There also have been special issues in *Academy of Management Review* on compassion (Rynes et al. 2012), in *Academy of Management Journal* (*AMJ*) on society's grand challenges (George et al. 2016), and in *Academy of Management Discoveries* on the United Nations' Sustainable Development Goals (Howard-Grenville et al. 2019). The *Journal of Applied Psychology* has a special issue on understanding racism at the workplace. The submissions are in process at the time of this writing. Other disciplinary journals have introduced similar special issues, e.g., *Journal of Marketing* (**https://www.ama.org/journal-of-marketing-special-issue-better-marketing-for-a-better-world/**) and *Manufacturing and Service Operations Management* (Netessine 2021).

The American Association of Collegiate Schools of Business (AACSB), which accredits business schools for their quality, put societal impact of management education and research front and center in its revised accreditation standards published in August 2020 (**https://www.aacsb.edu/accreditation/standards/business**). The global coronavirus pandemic since 2019 (COVID-19) has strengthened the resolve of many that management research should serve as a transformative force to enable better business and more humanistic societies.

A RESEARCH JOURNEY IN CONTEXT

Through retrospective reconstruction, I divide my research career roughly into three phases, and each phase underscores an intention to have impact on practice—to fulfill my responsibility of producing valid and practically useful knowledge. During the first phase (1981 to 1995), I was a traditional researcher in the United States, solving management problems in the American society. An invitation to work in Hong Kong began the second phase (1995 to 2010), but it continued even after I left Hong Kong in 2003. It led me to the study of Chinese and international management, especially on the nature of social networks and employment relationships in China, and executive leadership in Chinese firms. My colleagues and I in Hong Kong organized workshops for Chinese scholars to learn empirical research methods as practiced in the United States. I began to pay attention to the development of research in emerging research communities. I observed a strong tendency to adopt the research questions, theories, and methods in the American paradigm among Chinese scholars. This homogenization tendency was partly fueled by Chinese business schools' emphasis on publications in internationally highly ranked journals. I began to write on the importance of contextualization and indigenous research. This began the third phase (2011 to the present), focusing on the responsible research movement for the purpose of transforming our research toward producing both credible and useful knowledge to inform policies and practices for better business and a better world, both locally and globally.

My research journey is a mix of serendipitous opportunities (e.g., invitation to be a department head in Hong Kong) and deliberate choices (e.g., agreeing to do so). I took scientific freedom and scientific responsibilities both to heart and for granted. Most if not all of my decisions and choices in research and my professional engagements were due to the freedom I had and the associated responsibility I felt. My problem-focused research orientation sensitized me to the challenging management issues in the context where I work and live. Readers should be able to see the signs of the time and the landscape based on the kind of research projects I worked on.

Although there is a chronology to the three phases, projects in one phase often flowed into the next. The phases demarcate the primary intellectual foci in each. Also, some projects were wonderful opportunities to work with a colleague or a student,[2] providing valuable learning and lasting friendships. According to the principle of equifinality, there are many roads to Rome. I offer my research journey as one illustration of the key idea that both freedom and responsibility are sacred to our scientific work. Only when we have both are we able to fulfill our professional responsibility as responsible and contributing social scientists.

In this article I begin with a brief introduction of my background. Then I offer a brief overview of the major research programs in each phase, concluding with an emphasis on the responsibilities embedded in our scientific roles: (*a*) to contribute credible knowledge with robust evidence; (*b*) to produce knowledge useful for solving real-life challenges; and (*c*) to engage in education, internalization of scientific norms, self-monitoring, and self-correction. The third responsibility ensures that we live up to our first two responsibilities consistently and continuously, and that we have the freedom to do so.

A HUMBLE BEGINNING: FROM A VILLAGE TO A DOCTORATE

I was born in a village on the outskirts of Shanghai. From birth to nine years old, I lived in that small village without plumbing and electricity and slept on the same bed with my mother, the bed on which I was born. Father left Shanghai as a political refugee in the year after China became the Republic of China ruled by the Chinese Communist Party. The border was closed, and emigration was almost impossible. With good luck, Mother and I were granted a visa to leave China. One day in early May 1958, my mother took me on a train from Shanghai to Guangzhou and then Macau. In Macau, she went on a fishing boat to be smuggled into Hong Kong. She put me in the hands of a middle-aged woman who brought me to Hong Kong the next morning. At the border, she claimed that I was her daughter (an immigration loophole, as children under 12 years old did not need documents to go between Macau and Hong Kong with their parents). I finished primary and secondary schools in Hong Kong and entered a community (teaching) college because my mother had passed away and my father was too poor to send me to the university. After I graduated from the teaching college, I left Hong Kong and entered the University of Minnesota Duluth (UMD) with a scholarship supplemented by a part-time job as a waitress in a Chinese restaurant. I majored in psychology and minored in business administration. My honor's thesis, an experiment studying how infants reacted to men with or without a beard, introduced me to the mystery and fun of psychological research.

After graduating from UMD, I enrolled in the master's program in industrial relations at the University of Minnesota, Twin Cities campus. I chose the thesis option (instead of more courses and a term paper). Using the data from the research projects being conducted at the Industrial Relations Center, my thesis focused on the factor structure of employee job satisfaction.[3] The data were contained on IBM cards, and there were 15 boxes of them (if I recall correctly, each box had 300 cards). I pushed the cart with the 15 boxes to the Computer Center in the basement of the building to run the data. Each time I ran a factor analysis, I had to feed the 15 boxes of IBM cards

[2] Some of these projects are doctoral dissertations, and most have been published in the top journals, e.g., Nifadkar et al. (2012), Wu et al. (2010), Zhang & Tsui (2013).

[3] The Industrial Relations Center conducted attitude surveys for multiple companies in the Minneapolis-St. Paul area. Companies contributed the data for research by faculty and students affiliated with the research center.

into the card feeder. When the cards got stuck or were destroyed, I had to use another machine to reconstruct the data on a new card. The computer did not store the information over night, so I had to feed the cards every morning again into the card reader if I wanted to do more analysis. The Computer Center staff became my best friends. This project taught me the joy of finding interesting patterns in the data contained in the 4,500 cards (equivalent to 4,500 surveys). This experience was formative in both strengthening my interest in research and providing a realistic preview of some of the tedious but important aspects of conducting scientific research.

During the second year of the master's program, I began full-time work in the Personnel Department of the University of Minnesota Hospitals. My job title was Research Analyst. I was "free" to propose new projects that might be useful to the hospital administration. I decided to study the reasons for and the cost of nursing turnover. After completing the research, I wrote up the study and sent it to the American Medical Association journal *Hospitals*. After a month, I received an acceptance letter from the editor. It greatly boosted my confidence as a researcher.

I spent three years at the University of Minnesota Hospitals and then joined the Human Resource (HR) Department at the Control Data Corporation. Control Data was known for its very advanced HR research function, headed by an I/O psychologist, Dr. Walter Tornow, and a team of I/O psychologists, most of them doctoral graduates from the I/O Psychology and Industrial Relations programs at the University of Minnesota. I worked as a staff HR specialist in the mainframe manufacturing plant supporting the line managers in the factory. Dr. Tornow invited me to join his group to design a performance evaluation system for managers. I was very impressed by these PhDs, and the idea of pursuing a doctoral degree began to ferment in my mind. After two years, I started the doctoral program in Behavioral Sciences in the School of Management at the University of California, Los Angeles (UCLA). It was my intention to return to Control Data after I finished the degree. After one semester at UCLA, I fell deeply in love with research. I learned all I could from those around me, including professors at the University of Southern California. Among many, I was indebted in particular to six professors who trained me and advised me on my dissertation. They are Charles O'Reilly, Chet Schreisheim, Mary Ann Von Glinow, Barbara Gutek, Bill McKelvey, and my chair Tony Raia. Each taught me something unique and each helped me grow and develop. Charles and I wrote a paper during my first semester at UCLA, which we submitted to the *Journal of Applied Psychology*. It was rejected. Charles said it was due to the fatal flaw of common method variance, which intrigued me. I was not sad about the rejection. Instead, I found research to be mysterious and challenging. At that time, students did not need to have publications to get a job. Writing papers and submitting them to conferences or journals were for the purpose of learning the research process and sharpening research skills.

During the third year, as I was completing the dissertation, I had to decide if I wanted to go back to Control Data or to pursue an academic career. I thought that if I worked for the company, I would be helping one company, whereas if I became a professor, I could potentially help many more people and many more companies through my teaching and research. I did not know if that kind of thinking was simple naiveté or unrealistic idealism, and I don't know where it came from. Wishing to serve, if possible, more rather than fewer people influenced most of my career decisions throughout my life. In the summer of 1981, I began my academic career at the Fuqua School of Business, Duke University. Like many, I could not believe that I would be paid (well) and had the freedom to do what I loved.

PHASE I: TRADITIONAL PRACTICE-INSPIRED RESEARCH

In this first phase, my research fell into four major topics, each spanning five to ten years. Armed with the desire to improve practice, I chose topics that were important in the managerial world;

e.g., my dissertation on managers and the subsequent project on HR units directly grew out of my work experience. During those days, our professors did not tell us to "fill gaps in the literature"; instead, we were encouraged to study what interested us or mattered to practice. I use the word traditional to refer to the research paradigm in North America, specifically the United States, where I was trained. Some refer to it as mainstream research, understanding and explaining management in business firms in the North American context.

Reputational Effectiveness

My dissertation focused on how to define and improve the effectiveness of middle managers. When and how do they become effective in their complex relationships in all three directions—with supervisor (up), with peers (lateral), and with subordinates (down)? The puzzle came from my work at Control Data where I had frequent interactions with many middle-level line managers. They often talked about the difficulty of pleasing everyone (referring to bosses, peers, and subordinates). Role theory (Biddle 1979, Katz & Kahn 1978) seemed ideal to understand the middle manager's complex web of relationships. Because there is no common objective metric to measure the performance of middle managers who work in a variety of functions, we often rely on the subjective perception of role senders based on task-related interactions with the middle manager who is the focal role holder. I introduced the term reputational effectiveness to refer to the idiosyncratic nature of role expectations, experiences, and evaluations of the middle managers by the role senders (Tsui 1984a). My research showed that managers with this reputation for effectiveness from all three directions of role senders enjoyed a higher level of career success (promotions, salary increases) than middle managers who earned this assessment from fewer role sender groups (Tsui 1984b), and the effect was similar for both male and female managers (Tsui & Gutek 1984). The divergence in effectiveness evaluations was explained in part by the different role sender expectations and the nature of the relationship between them, such as how much the supervisor "liked" the subordinates (Tsui & Barry 1986, Tsui & Ohlott 1988).

To address the question of how managers gain and improve their reputation for effectiveness in their role set, Ashford and I proposed a model of adaptive self-regulation (Tsui & Ashford 1994) and tested the model in two empirical studies (Ashford & Tsui 1991, Tsui et al. 1995a). The data for both studies came from executive education programs, one while I was at Duke and the second after I joined the University of California, Irvine (UCI). I knew teaching in executive education programs was not recommended for junior faculty, but I was able to negotiate with the director to combine research and teaching. I collected 360-degree feedback for each executive and provided each a personal report along with a one-on-one conversation about their leadership profile. I was gratified in being able to both help these managers improve their leadership and use the data for my research. This research contributed to the literature on solving the puzzle of managerial effectiveness (e.g., Campbell et al. 1970) by offering a role-based theory to define managers' work and measure their effectiveness.

As I was wrapping up the papers from my dissertation dataset, I turned my attention to a new project on the effectiveness of HR departments at the operating level of the organization. These operating-level HR departments have many stakeholders, including the executives served by HR, peer managers in line departments,[4] employees, unions, and applicants or potential employees. They have divergent needs and expectations. Around that time, a new literature emerged

[4]Line departments are directly involved in the production of goods and service, whereas HR, finance, and administration are staff units.

on the multiple constituency theory of organizational effectiveness (e.g., Zammuto 1984). This organizational-level theory is conceptually parallel to the individual-level role theory. It is ideal for analyzing and understanding the activities and effectiveness of the HR units. Upon the suggestion of a colleague, I wrote a grant proposal to the Office of Naval Research. As luck would have it, the proposal was approved. I was in my fourth year as an assistant professor.

Drawing on the HR and organizational theory literature, I proposed a tripartite framework for defining the activities and identifying the criteria for effectiveness of HR units (Tsui 1984c). I also conducted several empirical studies which confirmed that different constituencies desired different activities of the HR unit and preferred different criteria for effectiveness (Tsui 1987, Tsui & Milkovich 1987). Also, different adaptive responses were associated with the reputational effectiveness assessments of the HR unit by different constituencies (Tsui 1990). This HR unit research was included in several HR textbooks, and a large technology company offered to pay for the survey instrument I used in my research. I also prepared a detailed report for each company that participated in the research. I was gratified that this research had direct and immediate usefulness to the HR practice community. Having some impact on practice was always a high priority and a source of satisfaction for this young scholar.

After completing these two reputational effectiveness projects, I decided to integrate and synthesize my learning about effectiveness of organizational members and units into a conceptual framework in a paper titled "Reputational Effectiveness: Toward a Mutual Responsiveness Framework" (Tsui 1994). The paper integrates 10 years of conceptual and empirical work spanning two levels of analysis. This is one of my favorite papers due to its integrative nature and a conceptual model that is practically useful for leaders at any level. I recall the famous quote of Kurt Lewin, "There is nothing more practical than a good theory" (Lewin 1952, p. 169).

I do not want to give the impression that all my projects were successful. One paper from the dissertation data was rejected outright by *Administrative Science Quarterly*. The major problem was inappropriate measurement of some of the independent variables and a degree of common method variance that threatened the causal claim. I agreed with the reviewers and never resubmitted it. My professors taught me not to waste time on papers that had major flaws or that I would not be proud of. I also failed two studies, a simulation study of group processes using MBA students and a field experiment of feedback given to superiors by subordinates in a commercial bank. The reviewers found serious flaws in both projects. Since I could not fix the faults, both papers went to the circular bin also. I was influenced by Barry Staw's (1981) escalation of commitment idea and decided that I should not throw good money (investing more time) after bad (failing projects). The time was not all wasted, however, because I also learned that I should never work on a project requiring skills that I had not mastered. I did not know enough about designing lab studies or field experiments to pull off those projects. I was overconfident after some small successes.

The next research project was the result of a serendipitous opportunity. The dissertation research on middle managers and the HR unit effectiveness study offered the ideal data for exploring a diversity issue that was an active research topic at the time and a matter of practical concern in society then and still now: How does being different from others, in certain personal and background characteristics, in a work relationship or a work unit influence work behaviors and attitudes? I stumbled into the next project on the idea of "relational demography."

Relational Demography

In 1983, Jeff Pfeffer published an interesting paper on organizational demography, focusing on the length of service distribution within an organization and its effects on organizational performance, innovation, succession, power distribution, cohort identity, intercohort conflict, and more (Pfeffer 1983). He suggested that the social dynamic of a social unit with divergence in tenure, or other demographic variables, would be quite different from that of a social unit with relatively more homogeneity in these demographic attributes. I realized that my dissertation data on middle managers and their role senders would be suitable to explore the implications of difference in demographic attributes between the focal managers and their role senders, including superiors, peers, and subordinates. Focusing on the difference between the focal managers and their superiors, Charles O'Reilly and I examined the multivariate effects of superior-subordinate differences on six variables—age, gender, race, education, and company and job tenure—on superior ratings of subordinate performance, superiors' affect toward the subordinates, and role ambiguity experienced by subordinates. We used the term relational demography to refer to these differences and were fortunate that *AMJ* reviewers liked this study (Tsui & O'Reilly 1989).

What about being different in a large unit with several hundred employees? This was a risky research idea because the effect might be difficult to detect in large units. The HR unit dataset has demographic profiles of 1,705 employees in 151 work units (in three large organizations in different industries) with an average work unit size of approximately 840 employees. Using self-categorization theory, we proposed that people use characteristics such as age, tenure, education, race, or gender to define psychological groups and to derive a positive self-identity (Tsui et al. 1992). Those most different from others in a social group would be least attached to this group, measured by level of psychological and behavioral commitment, absences, and turnover intention. Results confirmed our expectations but also offered a surprising finding. The impact of being different on commitment was greater for men and for white employees than for women and non-white workers. These results question the fundamental assumption underlying gender and race research at that time: that the effect of diversity is always felt by females or ethnic minorities. This paper received much media attention due to the "politically incorrect" nature of the finding, that diversity affected the majority more than the minority. It won the Outstanding Publication in Organizational Behavior Award from the Organizational Behavior Division of the Academy of Management in 1993 and the *ASQ* Award for Scholarly Contribution in 1998 (five years post publication).

After a few additional papers (Tsui et al. 2002; 1995b,d), I decided to integrate the learning from the published work by diversity and demography researchers, mine included, into a book titled *Demographic Differences in Organizations: Current Research and Future Directions* (Tsui & Gutek 1999).

Diversity and inclusion have reemerged as critical issues in (American) society in the past few years. Gender, racial and economic injustice confront us almost every day in the news. The "Me Too" movement, "Black Lives Matter," the COVID-induced unequal economic and health sufferings among Hispanic and Black people, and now the Anti-Asian sentiment are wake-up calls to the ills of our society. The aggrieved middle class with their stagnant income and continuing decline of jobs in traditional industries reminded us of their discontent with their votes in 2016. Economic inequality cannot be solved just by raising the minimum wage but will require a solution that ensures every citizen, regardless of gender, race, ethnicity, and economic background, to share in the prosperity that businesses are generating, and not only the top one percent. Two colleagues and I expressed our thoughts on the income inequality issue even before the pandemic in an invited essay (Tsui et al. 2018). Diversity and inclusion deserve research attention by all business disciplines, not only management. Special issues topics such as grand challenges, sustainable development goals, racism, or COVID-19 are examples of how editorial policies can influence research agendas and improve the relevance of our research. Editors have the leadership opportunity to emphasize both the freedom and the responsibility of our scientific community.

Employment Relationships in a Competitive Field

The late 1980s witnessed major corporate restructuring in the United States with significant changes in internal labor market structures (Osterman 1988), due to extreme competitive pressure and the need for cost containment. A massive reduction of managerial and professional jobs replaced traditional employment security (Buono & Bowditch 1989). Organizations sought flexibility in employment practices but expected extra commitment from their employees in return. I moved to UCI in 1988. My colleagues at UCI, Lyman Porter and Jone Pearce, and I decided to study the nature and consequences of this unbalanced employment relationship. We began with proposing a job-focused and an organization-focused employment relationship (Tsui et al. 1995c). The former is a pure economic exchange, and the latter is a mixture of economic and social exchange. A good example of the former is the exchange between the taxi driver and the passenger. The latter involves an open-ended relationship with a high level of investment in employees by the employer in exchange for flexibility in terms of work assignment or work location by the employees. This type of arrangement is typically reserved for a company's core workers. Both types of employment arrangements are balanced. Generous (or limited) pay and benefits offered by the employer are matched with broad (or narrow) expected contributions from employees.

What about the unbalanced employment arrangements that we observed during corporate restructuring? Building on the inducement-contribution framework (Barnard 1938, March & Simon 1958), we (Tsui et al. 1997) proposed four types of employee-organization relationships (EOR), renaming the two balanced types as quasi-spot-contract (job-focused) and mutual-investment (organization-focused), and introducing two unbalanced types, overinvestment (a high level of inducements offered by the employers coupled with a lower and narrower set of contributions expected of employees) and underinvestment (a low level of inducements coupled with a high level of expected contributions). Through a National Science Foundation grant, we tested our model using survey data from a random sample of employees in each of ten companies from five highly competitive industries. This paper received the 1998 *AMJ* Best Paper Award and the Academy of Management Human Resources Division's Scholarly Achievement Award. According to the editor Angelo DeNisi, the reviews were mixed. One of the three reviewers was strongly negative, but Angelo agreed with the two more positive reviews. This shows that when reviewers disagree, the editor can have influence on the fate of manuscripts. This experience reinforced my understanding of the freedom and responsibility that journal editors have. Their editorial policies and decisions can have a huge influence on how we conduct our research, what topics are welcome, and who benefits from the scientific work.

PHASE 2: CHINESE AND INTERNATIONAL MANAGEMENT RESEARCH

In 1992, I was invited to teach a summer course at the newly founded (in 1991) Hong Kong University of Science and Technology (HKUST). Having spent 12 years in Hong Kong before coming to study in the United States, this was like a homecoming opportunity. I taught again the following summer and stayed at HKUST for a sabbatical in fall 1993. The Dean of the business school asked if I would help to build the Management Department as its founding head. I didn't know how to respond to this opportunity. The administrative work would interfere with my research. There was the risk of "out of sight, out of mind." I weighed the benefits of staying at UCI where I had a tenured job, a comfortable home, and wonderful colleagues against the cost of being far away. Yet, I was excited by the opportunity to build a world-class research department along with a felt obligation (responsibility) to give back to this society. UCI allowed a two-year no-pay leave. I decided to do that. How naïve of me to think that I could build a world-class academic department in two years. At the end of two years, I had to decide if I would return to UCI or stay at HKUST (similar to the decision I had to make between Control Data and an academic life). There were risks with staying in Hong Kong, but I was reasonably confident that I could find a job in a satisfactory school in the United States if I should decide to leave Hong Kong.

I did not know, nor could I have known, that this move to HKUST in 1995 was the pivot in my career. All of a sudden, I plunged into an economic-social-cultural context vastly different from that of the United States. Hong Kong was preparing to return to China in two years' time. Universities in Hong Kong became both intrigued and obliged to engage in exchange and collaboration with universities in mainland China. The Head of the Economic Department and I prepared and received a large grant to study Chinese business management. This grant provided funding to organize annual training workshops to teach empirical research methods to young faculty in the leading business schools in China. After four years, we trained more than 150 young faculty colleagues, and many of them are now academic leaders in Chinese business schools. We funded research and organized conferences that led to several edited books.

With strong interest in Chinese management by overseas scholars and the mutual interest for collaboration by Chinese and international scholars, the International Association for Chinese Management Research (IACMR) came to life in 2002. I also founded the journal *Management and Organization Review* to publish Chinese management research and the first issue (March 2005) was published a year early in June 2004 to coincide with the inaugural conference of IACMR in Beijing. These institutional building activities occurred at the same time I was building a new department, recruiting new faculty, creating a new PhD program and a research center dedicated to Chinese management research, while serving as the editor for *AMJ*. I discovered that I quite enjoyed these institution-building activities. I also realized that these activities were more about the practice of research than the practice of management. Understanding and responding to the needs of my immediate context and creating or changing institutions to improve how we practice our profession came to define what I did for the years to come.

When you work outside the United States, you become instantly an international researcher. My work began to take on an international flavor, some of which fell into the realm of universal knowledge (generalizable to many contexts), along with an interest to discover culture-specific knowledge. With exposure to the changes in the Chinese economy while working in Hong Kong, I became intrigued by the power of CEOs in Chinese firms, especially those in the privately owned small and medium enterprises. I was curious about their leadership style and management approach and the simultaneous influence on them of Chinese culture and Western business norms. Chinese executive leadership became a focus of my research, along with extension of my work on relational demography and employment relationship to the Chinese context.

As the editor of a journal devoted to Chinese management research, I had first access to the type of research being conducted by Chinese scholars. I noticed that most of them were studying topics popular in Western journals, especially the top journals based in the United States. Although I had encouraged imitation as a way to learn, I also became concerned that these scholars were missing the opportunity to contribute knowledge to the local community by ignoring important problems in the local contexts. I began to write about the need for contextualization of research questions, theories, and methods, and the value of high-quality indigenous research. These writings became the foundation of my work in the third phase of my research career. Below I briefly describe the four main programs of research or writing during this second phase.

Relational Demography, Chinese *Guanxi*, and Social Networks

Chinese people place special affinity on people with similar backgrounds such as the same hometown origin or attending the same school or college (this phenomenon certainly exists in the

United States and in other cultures as well). *Guanxi* refers to the existence of these particularistic ties. In a conceptual paper (Tsui & Farh 1997), we compared the similarities and differences of relational demography and *guanxi*, and an empirical study (Farh et al. 1998) tested the influence of both relational demography and *guanxi* on subordinates' trust in superiors and the executives' trust in their business connections. In another conceptual paper (Fu et al. 2006), we explored the dynamic of *guanxi* in the context of Chinese entrepreneurial firms. Treating *guanxi* as a source of social capital (Xiao & Tsui 2007), we found structural holes to be negatively associated with career success. This is contrary to the positive role of structural holes found in the US (Burt 1992) or French (Burt et al. 2000) studies. This suggests a cultural boundary to the structural holes theory and offers implications for the cross-cultural understanding of network ties.

Applying *guanxi* research to the comparative context, my colleagues and I analyzed the role of social networks on revenue growth of entrepreneurial firms in settings with varying strength of formal institutions and informal relational norms (Batjargal et al. 2013). The settings were China and Russia—representing weak formal institutions and strong relational norms—and the United States and France—representing strong formal institutions and relatively weaker relational norms. We also investigated the role of family ties in entrepreneurial networks (Arregle et al. 2015) and how social networks played a different role for male and female entrepreneurs in different national cultures (Batjargal et al. 2019). These studies contributed to the contingent nature of social networks in different socio-economic contexts and for different types of workers (entrepreneurs in these studies versus employees in the Xiao & Tsui 2007 study). This research program also contributed both context-specific and universal knowledge, ideas that I discuss in the section on contextualization and indigenous research, below.

Employment Relationships in China

I was fortunate to have several wonderful doctoral students at HKUST who shared my interests on employment relationships, and we could test the EOR framework in Chinese firms. Kevin Wang's dissertation found the mutual-investment EOR approach with middle managers to be most conducive to high firm performance (Wang et al. 2003). However, the underinvestment EOR approach was associated with stronger performance when the firm pursued a prospector strategy or if the firm was domestic and privately owned. Lynda Song included executive leadership style and organizational culture in addition to EOR as competing factors influencing employee commitment and performance (Song et al. 2009). Studies with other colleagues confirmed the efficacy of the mutual-investment EOR for a variety of outcomes, including managers' trust in the organization (Zhang et al. 2008), employee commitment through the mediation of social exchange perception and job embeddedness (Hom et al. 2009), and employee creativity through work-related communication density (Jia et al. 2014). Zhang et al. (2014) tested the relationship of the two components of EOR, offered inducements and expected contributions, to performance and commitments of middle managers, moderated by the manager's traditionality, referring to a cultural orientation of submission to authority and endorsement of hierarchical role relationships.

These studies extended an employment relationship framework that originated in the United States to an emerging economy with different social-cultural norms about supervisor-subordinate relationships, thus contributing to both universal (context-free) and local (context-specific) knowledge. The Chinese economic reform offered opportunities to study many topics that are relevant in this context. Next, I provide a brief overview of two new research programs that were inspired by developments in the Chinese economic reform.

Chinese Firms and Chinese Executive Leadership

My editing work at the journal *Management and Organization Review* combined with working with students as they developed their dissertations educated me on China's economic development. Equally important learning came from coediting three books on the Chinese economic reform and Chinese firms, and a special issue in *Organization Science*. All three books and the special issue were preceded by a conference at which authors presented their papers and received feedback to refine the chapters. Li et al. (2000) focused on management in the Chinese context, joint venture management, Chinese organizational behavior, and managerial perspectives (written by three practicing executives). Tsui & Lau (2002) reported the results of fourteen empirical studies of management issues in Chinese firms and interviews of CEOs of three successful firms operating in China, one international joint venture, one whole-owned foreign firm, and a private firm founded by an Olympic gold medalist in gymnastics.

After the regime change in 1949, the Chinese government nationalized all firms and private property was outlawed, except small family-operated shops or restaurants. The economic reform that started in 1979 allowed new small entrepreneurial activities beginning in the rural communities. This private sector grew from less than 10% of the National Gross Domestic Product in the early 1980s to almost a third of GDP by 2003 and since then has remained strong at above 50% of GDP and contributes more than 80% of jobs (see **http://www.xinhuanet.com/english/2018-03/06/c_137020127.htm**). The third edited book (Tsui et al. 2006a) focuses exclusively on these private domestic firms, taking a multidisciplinary approach, with contributions from economic, sociology, and management scholars. The book provides a comprehensive analysis of the factors important to the successful operation and growth of these privately owned firms.

The *Organization Science* special issue (Tsui et al. 2004a) was published approximately 25 years into China's economic reform. In the introduction essay, the guest editors describe the empirical context of China at that time, review contemporary research on Chinese management and organizations, and describe the nine papers in the special issue. The papers provide a close examination of how massive corporate transformation in China in the state sector, a robust private sector, and the steady presence of foreign enterprises have influenced interfirm relationships, affected opportunity structures and social processes, and modified individual behaviors within firms. The paradoxes in this intellectual terrain challenge organizational scholars to think deeply about China as an empirical setting where the boundaries of existing knowledge and theories on organizations and management can be extended or tested.

On Chinese executive leadership, I focused on the leadership behaviors and personal attributes of the CEOs. Tsui et al. (2004b) focused on the variation of leadership styles among Chinese CEOs, using the metaphor of "let a thousand flowers bloom" during the economic transition with concurrent influences of traditional Chinese culture and Western or modern leadership practices. Wang et al. (2011) related relationship-oriented and task-oriented leadership behavior of CEOs to middle managers performance and firm performance. We analyzed the influence of CEOs' personal values (self-enhancement and self-transcendence) on the efficacy of transformation leadership in obtaining employee commitment (Fu et al. 2010), CEO humility on empowering culture and middle manager response (Ou et al. 2014), and CEOs' reflective capacity (Jia et al. 2021) on firms' sustainability performance (economic, social, and environmental). In Ma & Tsui (2015), we analyzed how Chinese leadership practices reflected the Chinese philosophy of Confucianism, Daoism, and Legalism.

Another study (Tsui et al. 2006b) explored when and why decoupling of CEO leadership behavior and organizational culture may occur. This question is meaningful in China, due to large variance on leader discretion in the state-owned (least discretion), foreign-invested (moderate discretion), and privately owned (most discretion) firms. For a further understanding of Chinese leadership, Tsui et al.'s (2017) book provides in-depth interviews of thirteen successful Chinese private entrepreneurs in four industries—financial and insurance, IT and e-commerce, construction and real estate, and consumer goods. Content analysis of the interview data identifies eight managerial processes and four leadership attributes that may account for the entrepreneurial success in the highly competitive and inhospitable environments.

Guanxi, social networks, employment relationship, and executive leadership focused on substantive management issues. Most projects focused on problems that I had worked on in the previous phase (in the United States) and utilized existing (Western) theories to analyze local (Chinese) phenomena. These projects involve some contextualization of existing theories and measures. Only two projects (Tsui et al. 2017, Jia et al. 2021) may qualify as indigenous research, investigating executive behaviors and mental processes, unbounded by an existing theory of leadership. Reflecting on the development of research in China and other emerging contexts gave me a great appreciation for the importance of the phrase "knowledge is contextual" (Polanyi 1966). I began to write about the importance of developing locally relevant knowledge, as social scientists should (a responsibility) and could (with their freedom of inquiry). I ventured into a new program of research, which continued into the third phase of my research career and is still active to date.

Contextualization and Indigenous Research

To encourage local scholarship and to encourage scholars in new research contexts to engage in high-quality indigenous research as a way to contribute to global management knowledge, I identified three types of knowledge: context-free, context-bound, and context-specific (Tsui 2004). The first is general universal knowledge. The second is knowledge contingent on the context (what is true in one context may not be true, or may be the opposite, in another). The third is indigenous knowledge unique to the context. The extant literature, based on research in the Western context, until recently did not consider the generalization of knowledge to other socio-economic-political-cultural contexts. However, all research conducted within a socio-cultural context (e.g., the United States) produces context-specific knowledge, unless its generalization to other contexts is affirmed through contextual replication, cross-cultural analysis, or comparative analysis. Context-free knowledge comes from replicating research in different contexts with invariant results. Organizational researchers have understood the importance of context in studying phenomena within or about organizations (Johns 2006, Rousseau & Fried 2001).

Contextualization is important in choosing the phenomena or question to study, in measurement, and in research methods (Tsui 2006, 2012). In Tsui et al. (2007), we offered seven recommendations including a polycontextual approach to capture the multiple contexts for a holistic and valid understanding of phenomena in any context. It is also important to ensure construct validity of a local phenomenon by going beyond back translation, as well as to "go native" when conducting country-specific research. We agreed with Cavusgil & Das (1997) that valid cross-cultural studies must begin with substantive knowledge of relevant phenomena in all the contexts before making meaningful comparison between them. I also advocate pluralistic scholarship, deep contextualization, or indigenous research to build novel and context-relevant theories (Tsui 2007a). Along with novel questions in new contexts, such efforts regenerate intellectual excitement and fill the gaps in both local and global management knowledge (Tsui 2007b).

The major benefit of deductive testing of theories developed in the West is the validation of these existing theories or extension of the theories' contextual boundaries. I refer to this as contextual replication. Such studies are a good illustration of the contribution of extent theory in explaining phenomena in new contexts (Whetten 2009). A limitation of this approach is the discovery and understanding of phenomena unique in the new contexts. This approach also does not assure that the findings in the context are valid, given that the observing was based on the theoretical lens from the West, which serves as a filter coloring the observations of the researcher.

The desire to join the international intellectual conversation by publishing in leading journals has another interesting consequence. The researchers surrendered their autonomy of inquiry by voluntarily adopting the dominant paradigm. As I wrote in Tsui (2009, p. 6), "scientific research is not a random pursuit. There is a purpose to scientific inquiry. It is to understand reality or to pursue 'truth,'" even if only approximate truth. To achieve this purpose, we must take context seriously. I further argued that to enable contextualized research on problems meaningful to a local context, we must revise the institutional conditions, such as promotion criteria and evaluation standards. Most importantly, the change should come from within the scientific community. Scholars in any context are encouraged to remember the words of Kaplan:

> The various sciences, taken together, are not colonies subject to the governance of logic, methodology, philosophy of science, or any other discipline whatever, but are, and of right ought to be, free and independent. [He referred] to this declaration of scientific independence as the principle of *autonomy of inquiry*. It is the principle that the pursuit of truth is accountable to nothing and to no one not a part of that pursuit itself. (Kaplan 1964, p. 3, original italics)

As I reflected on how the work of young scholars was constrained by a bureaucratic requirement, i.e., publishing a specific number of papers in a defined list of journals, which brought unintended negative consequences, I became extremely concerned. I considered what I could do to change this status quo. In 2008, I was invited to run for a leadership position on the AoM Board of Governors. I thought that if I got elected, I could mobilize some changes through AoM, the largest association of management scholars worldwide. I won the election and became the program chair for the 2010 annual meeting. I chose the theme "Dare to Care: Passion and Compassion in Management Practice and Research" for this meeting. In 2012, my presidential address was titled "On compassion in scholarship: Why should we care?" (Tsui 2013b). This theme aimed to encourage the AoM members to care about stakeholders beyond shareholders.

A sense of responsibility to change the nature of our research culture and to improve the opportunity for meaningful contributions by young scholars influenced my decision to run for the officer position at AoM. Although a bit naïve, this decision marked the beginning of the third phase, which is mainly about serving my beloved management research community.

PHASE 3: RESPONSIBLE RESEARCH—CREDIBLE AND USEFUL KNOWLEDGE FOR A BETTER WORLD

The First Problem: The Usefulness of Our Research

I focused my five years of service on the AoM Board on the relevance issue, to encourage our scholars to engage in, and to encourage our journals to publish, research that benefits all stakeholders (employees, customers, suppliers, community) not only the owners or the shareholders. Walsh et al.'s (2003) paper was eye-opening. It reminded us that the AoM was created to serve the public interest; as the founding editor of the *Journal of the Academy of Management* (the original name of the *AMJ*) wrote, "(the) public would be served by scholarship designed to help accomplish both the economic and social objectives of an industrial society" (p. 859). In analyzing empirical research published between 1958 and 2000 in *AMJ*, they found our field has pursued society's economic objectives much more than it has its social ones. In fact, approximately

80% of empirical research focused on firm performance and only approximately 20% focused on human welfare such as health, satisfaction, justice, social responsibility, and environmental stewardship. My colleague and I (Tsui & Jia 2013) replicated this study in China, assuming that this context would focus more on social outcomes given its socialist ideology. We included all the articles using Chinese samples in the top six English language management journals and the three top Chinese language journals that publish management and organization studies. We discovered almost 90% of the studies focused on economic performance in both sets of journals, suggesting the primacy of serving the economic goal of shareholders. This could be due to either the homogenization tendency or responding to the needs of the Chinese economic reform, or both. My presidential address (Tsui 2013b) discussed this imbalanced focus and called for more compassion in our research to alleviate the suffering of people and the planet and to expand the usefulness of our research to more than just shareholders, owners, or executives of the firm.

Another development in the journals, especially the top ones, is the focus on theory. Hambrick (2007, p. 1346) remarked that this focus might be "too much of a good thing," indirectly questioning the usefulness of our research with such a strong devotion to theory. He reported that management journals emphasize theory much more than journals in marketing, finance, and accounting. He wondered if "we have gone overboard in our obsession with theory" and this devotion to theory "unnecessarily limits research" (Hambrick 2007, p. 1346). He proposed that journals, including the AoM journals, should value reporting of interesting facts without theory, which has the potential of spawning important and influential research. I could not agree with him more.

An analysis of the top five economics journals (T5) shows that journals that emphasize novelty and theory, ironically, may discourage innovative research (Heckman & Moktan 2020). The authors found papers in the non-T5 journals were cited as often as the T5 papers. Furthermore, innovative papers were more often found in non-T5 journals where many leading scholars chose to publish their work. This may be the consequence of the peer review process, which tends to perpetuate the status quo by approving research that contributes to established ideas or paradigms and rejects those that deviate from them, or interesting facts that do not have a theory to explain them. Akerlof (2020) observed that economics as a discipline gives rewards that are biased in favor of the "hard" and against the "soft" (p. 406). This leads to "sins of omission" whereby important problems (e.g., financial crisis, global warming) or problems difficult to approach in a hard way (e.g., mathematical modeling) are avoided.

Persuaded by the arguments of AoM's Journals Committee, chaired by John Hollenbeck, the Board of Governors approved the creation of the *Academy of Management Discoveries* during the year of my AoM presidency. It is a journal devoted to publishing phenomenon-inspired research on new and complex problems that defy theoretical explanations.

In addition to special issues on topics important in society today, some editors are requiring authors to add societal impact of the research in their manuscripts. Some journals are adding relevance for practice or for society as an additional criterion in their evaluation of manuscripts. These are good steps in the right direction, encouraging research that departs from traditional paradigms. Certainly, paradigmatic research, although incremental, has value. After all, normal science is necessary to refine theories and instruments for more precise predictions (Kuhn [1962] 1996). However, as Kuhn observed, new paradigms are necessary when the existing paradigm is not solving either old problems or new challenges.

The Second Problem: The Credibility of Our Research

Amid lively discussions on the research-practice problem and exciting actions on narrowing this gap, some scholars (e.g., Bedeian et al. 2010) observed the troubling trend of many researchers engaging in questionable research practices in order to get published. These practices threaten the credibility of the conclusions reported in the published research—including those in the A-journals. The credibility crisis caught the attention of many scholars (e.g., Honig et al. 2014, Schwab & Starbuck 2017, Tourish 2019).

The Bedeian et al. (2010) paper identifies eleven such practices. Their sample included 1,940 research faculty in 104 PhD-granting management departments in the United States. More than 70% reported having observed colleagues (or themselves) participating in withholding method details, selectively reporting results, using others' ideas without credit. More than 50% dropped observations to improve statistical results, withholding data or findings that contradicted previous research. More than a fourth of the respondents reported fabrication of results—blatant cheating—by colleagues. The top practice of developing hypotheses after the results were known (HARKing) is reported by almost 92% of the respondents. A similar trend was reported in psychological sciences. John et al. (2012) surveyed 2,155 US psychologists on how often they engaged in ten research practices. The top three are, (*a*) failing to report all of a study's dependent variables (66%), (*b*) deciding whether to collect more data based on the results (58%), and (*c*) selectively reporting studies that "worked" (50%).

Given publication pressure and that social phenomena can be highly ambiguous and may involve many alternative explanations or "multiple realizability" (Risjord 2014, pp. 125–27), data massaging may be both encouraged and tolerated. These practices may lead to flawed conclusions and unreliable or nonreplicable results. Such flaws are hard to detect in published research because most of the papers do not contain enough details on the method to allow for careful scrutiny or for replication. For example, more than two-thirds (70%) of the 88 studies analyzed by Bergh et al. (2017a) provided incomplete information on the data or the procedure for replication testing. Hubbard & Vetter (1996) analyzed 4,270 empirical studies in 18 top journals in accounting, economics, finance, management, and marketing, using data from 1970 to 1991. They reported that less than 10% are replication studies in accounting, economics, and finance, and 5% or less in management and marketing.

Part of the reason for few replication studies in the journals could be that replication studies are simply difficult to do, especially on social phenomena. But the foundation of credible knowledge is replications of observed effects. Prescription for practice could not be based on the findings of one study or on effects that cannot be replicated. In social science research, how many replications and how many successful replications are acceptable is still an open question. However, within reason, the principle should be the more the better. The empirical evidence on replication in management or psychological research, however, does not give us comfort. Goldfarb & King (2016) analyzed 300 articles published between 2003 and 2012 in five top journals (60 each journal) as well as 60 conference submissions (pre journal review). Using a simulation to estimate the repeatability of results in the next single study, they found 38% to 40% of the significant results in these papers fail to replicate. Those that were replicated have an inflation of effect size at 13%. There was no significant difference between published articles and conference papers.

Bergh et al. (2017b) examined the credibility crisis in strategic management research. They conducted reproducibility tests, i.e., retesting the hypotheses using the same data, analyzing 88 linear regression or structural equation modeling articles in the *Strategic Management Journal*. They reported that nearly one in three hypotheses could not be reproduced. Focusing on laboratory experiments, which are usually more robust than other kinds of empirical studies, Camerer et al. (2016) analyzed 18 experiments in *American Economic Review* and *Quarterly Journal of Economics*, published between 2011 and 2014. They were able to find 61% successful replications, with effect size at 66% of the original estimates (mean effect size was 0.47 in the original studies and 0.28 in the replication studies). The strength of this study was the affirmation by the 18 original authors on the replication designs. This paper further reports that nonexperimental economic research had 13% to 23% replication success with more than 75% not replicable.

Many researchers, concerned with the replication crisis, have called for prioritizing "truth over publication" and "replication over novelty" (Nosek et al. 2012, 2015; van Witteloostuijn 2016). The replication crisis is related to the incentive system that rewards number of publications rather than truth and usefulness of scientific discoveries. I was party to some of these problematic practices, and I know now that they are wrong. I decided to join this call by emphasizing the scientific spirit (Tsui 2013a) and socially responsible research (Tsui 2015). This problem is global; it is present in countries beyond China and the United States.

Freedom Compromised or Lost

In Tsui (2016), I discussed my observation that instrumental rationality (career concerns by individual scholars or ranking priority by school leadership) dominates the decisions of what and how to study, where to publish, and what to measure and reward in the faculty evaluation system. In essence, researchers in business schools have become paper production workers and have lost their scientific freedom. I explained "This counting method of evaluation essentially eliminates the intellectual autonomy of scientists. Researchers focus on writing for these journals, perusing them for ideas to study, following the dominant theories and research methods, avoiding new topics that might be risky and focusing on filling the theoretical gaps in the literature" (Tsui 2016, p. 18).

In a more recent paper (Tsui 2021), I further elaborated on the importance of this freedom, quoting Albert Einstein: "The development of science and of the creative activities of the spirit requires a freedom that consists in the independence of thought from the restrictions of authoritarian and social prejudice" (Isaacson 2008, p. 550). Kuhn reminded us that this independence of thought "permits the individual scientist to concentrate his attention upon problems that he has good reason to believe that he can solve" (Kuhn [1962] 1996, p. 164). Kuhn claimed that autonomy of inquiry is largely responsible for success in the natural sciences.

I observed that this "reduced freedom appears to have found company in a reduced sense of responsibility" (Tsui 2021, p. 181). Researchers have become insensitive to the risks of wrongful conclusions, given that no one (not school leaders, not journal editors) is holding them responsible for errors. Furthermore, there does not seem to be any punishment for faking data or engaging in highly questionable practices. I know of authors who engaged in self-plagiarism and received no reprimand from their universities. Authors whose papers were retracted from multiple journals were able to move to new universities with higher salaries.[5] Without public sanctioning of such explicit and public misconduct, how will other scholars be motivated to change their behavior? The bureaucratic rationality (using impersonal criteria for evaluation, such as the number of papers or citations) and instrumental rationality (for promotion or tenure) have turned researchers into what Mills ([1959] 2000) called "cheerful robots," working behind the façade of neutrality, impartiality, or objectivity. Given what Glick et al. (2007), Mitchell (2018), and many others have observed, are these robots really cheerful? We should be concerned about the well-being of both the scientists and the society for which they are expected to serve as public servants.

The formal statement by the American Association for the Advancement of Science (AAAS) emphasizes the importance of both scientific freedom and scientific responsibility:

> Scientific freedom and scientific responsibility are essential to the advancement of human knowledge for the benefit of all. Scientific freedom is the freedom to engage in scientific inquiry, pursue and apply

[5] I do not reveal the identity of these authors out of respect for their confidentiality.

knowledge, and communicate openly. This freedom is inextricably linked to and must be exercised in accordance with scientific responsibility. Scientific responsibility is the duty to conduct and apply science with integrity, in the interest of humanity, in a spirit of stewardship for the environment, and with respect for human rights. (Jarvis 2017, p. 462)

In July 2014, I gave a talk at a conference about these problems and called for socially responsible research. I discussed the questionable research practices that threaten the credibility of our findings and how the focus on economic outcomes of the firm with many negative externalities has made the world a worse place. I encouraged scholars to direct our research to addressing the grand challenges of our world (poverty, resource depletion, global warming, inequity, inequality) and to turn management research into a force for good. I was invited to publish the talk in *Global Focus* (Tsui 2015), a magazine of the European Foundation for Management Development (EFMD). The officer of EFMD who heard my talk persuaded me to form a team of leading scholars to explore possible solutions to this conundrum. This led to the creation of the Responsible Research in Business and Management (RRBM) network.[6]

Responsible Research: Focus on Both Rigor and Relevance

The RRBM movement was cofounded in 2015 by 24 leading scholars in five core business disciplines in ten countries (plus leaders of four associations tightly connected with business education). It focuses on both rigor and relevance and defines responsible research as scientific work that produces credible knowledge that is useful for practice toward a better world (RRBM [2017] 2020). Achieving either alone does not qualify as responsible research. Unreliable or nonreplicable research findings may misinform practice. Reliable findings on trivial problems are not useful for society.

RRBM's mission is to promote, facilitate, and catalyze the transformation of business research away from the current ecosystem that focuses on a numerical publication goal to an ecosystem that values the content of the work and its ability to address society's grand challenges or wicked problems, and produce knowledge that enables businesses to be positive change agents. Responsible researchers are directing their attention to improving human lives, socially, economically, and sustainably and engaging in rigorous research design to ensure the findings are reliable and replicable, along with careful estimates on the consequences of wrongful conclusions. The cofounders further formulated seven principles to guide research design.[7]

The first principle, "*Service to Society*," serves as a foundational principle: Science should bring benefits to all citizens and should avoid negative effects on humanity. As Einstein reminded us, improving the life of the ordinary human must be the chief objective of science. Principles 2 to 4 seek to enhance the credibility of research findings, valuing interdisciplinary analysis, diversity in methods, ontology, and epistemology in different parts of the world, and encouraging plurality, context sensitivity, and indigenous research. They focus on the first responsibility of science for producing reliable and replicable knowledge. Principles 5 to 7 encourage improvement in the usefulness of discoveries for the end-users by involving stakeholders in the research process and ensuring research will bring benefits and not harms to those being studied and those who apply the

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[6] RRBM cofounders represent the five core disciplines in a business school—accounting, finance, management, marketing, and operations management—given that the problems, to varying degrees, pervade all disciplines.
[7] The RRBM position paper (see here: **http://www.rrbm.network**) provides detailed discussions on these seven principles.

knowledge. These principles reference the second responsibility of science, which is to develop useful knowledge for society.

The responsible research movement is in its sixth year, achieving some small wins along the way. There are more journal special issues calling for papers on important societal problems, as well as responsible research awards in management, marketing, and operations management disciplines to recognize recently published articles and books for their potential in societal impact. Some schools are asking faculty to include a statement of the societal impact of their research in annual reviews. RRBM has organized global summits to discuss actions to advance responsible research by different stakeholders. There are webinars on how to engage in responsible research.[8]

Such efforts are under way globally. The top ten business schools in China are leading the way to transform management research in service of society. They organized a summit in December 2019 and signed a joint declaration to lead this change in China[9]. A second summit in 2020 attracted more than 100 deans. The Guanghua School of Management provided millions of funds for faculty to study economic, business, and social problems in the Chinese society. The School of Economics and Management at Tsinghua University has changed its hiring and promotion criteria to emphasize research on problems in Chinese society. The Antai School at the Shanghai Jiao Tong University encourages faculty to develop management expertise for specific industries. This is encouraging momentum to avert the homogenization tendency and to increase the relevance of management research for Chinese firms and Chinese society.

Doctoral Education and Future Generations

Senior scholars are calling on each other to serve as good examples for young scholars (Harley 2019, McKiernan & Tsui 2019). Harley & Cornelissen (2019) even argue the situation is so desperate that senior academics must intervene and help to create and support a new, corrective path forward. McKiernan & Tsui (2019) specifically encourage professors emeriti to engage with doctoral education actively and to prepare a new generation of responsible scholars. The training needs to include not only technical skills about how to do sound science but also an introduction to topics on philosophy of science such as objectivity, science and policy, science and society, the role of values in science, and an accurate understanding of scientific freedom and scientific responsibility. Although conversion is difficult for those entrenched in the existing paradigms, hope lies in "young and rising naturalists, who will be able to view both sides of the question with impartiality" (Kuhn [1962] 1996, p. 151). The history of scientific progress encourages us to place our trust and hope in the new generation of scholars.

It is understandable that most young scholars may not wish to take the risk of jeopardizing their career by engaging in research studies or topics they perceive to be low priorities in journals or tackle problems that cannot be easily explained by existing theories. However, there are now many examples of young scholars who have taken the risk, despite the advice of their advisors or mentors, to study problems for which they have great passion. One good example is Ivona Hideg who decided to study maternity leave policies despite advice against it. Her paper from her dissertation (Hideg et al. 2018) won the 2019 Responsible Research in Management Award. She is now the associate editor of *AMJ* and holds a chaired associate professor position at York University.

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[8] See the Actions page of the RRBM website (**http://www.rrbm.network**) for information on awards, summits, journal special issues, webinars, and other initiatives.

[9] See the RRBM website (specifically here: **https://www.rrbm.network/taking-action/events/responsible-research-summit-2019/chinese-business-deans-summit-on-responsible-research/**) for a brief description of this summit.

Another example is Wen Montgomery who was interested in the water problem in Detroit and wanted to study it using the qualitative method. She was advised to choose a safer topic, but she followed her passion, holding on to her freedom and her sense of responsibility. Her research paper based on her dissertation (Montgomery & Dacin 2020) also won the Responsible Research Award. Tima Bansal had a similar experience about taking up sustainability research more than 20 years ago. In fact, most of the winners of this award are junior scholars.[10] As business schools will have to meet the new AACSB standards on demonstrating the societal impact of their research outputs, young scholars who work on responsible topics will have a competitive advantage in hiring and promotions.

Motivation is good, but some substantive support will also be important. RRBM is in the process of introducing a Dare to Care Doctoral Dissertation Scholarship encouraging students to study social justice issues in organizations, including economic, gender, racial, and other forms of injustice.[11] In collaboration with Arizona State University and Hong Kong Baptist University, students will have the opportunity to participate in an online doctoral seminar "Philosophical Foundations of Responsible Research."[12] RRBM will commit during the next several years to preparing doctoral students worldwide to be the torch bearers of the Vision 2030 laid out in the RRBM position paper. I am proud to be a major contributor to these efforts.

The Third Responsibility, Scientific Norms and Metascience

The work of RRBM and the self-correction efforts by various stakeholders of the research ecosystem indicate a third responsibility by the research community (Tsui 2021). It is to protect the sacredness and integrity of the research enterprise, ensuring that we exercise our freedom responsibly and deliver our scientific responsibility with integrity. Scientists not only have the responsibility to "get it right" (technically right, à la credibility) but also the responsibility of working on the "right things" (morally right, à la usefulness). The third responsibility includes attention to the training of nascent scientists to undertake their scientific responsibilities, to define the norms and values to govern scientific behavior, to engage in self-correction, and to bring back the deviants.

Inspired by and building on the ideas of giants in science such as Einstein, Kuhn, and Merton, the AAAS statement, and the vision/mission of RRBM, I suggest in Tsui (2021, pp. 184–85) five scientific norms that we may consider for the business and management research community:

1. Independence: Having freedom to pursue problems in society deserving research attention, with institutional rules and performance metrics supporting rather than reducing this freedom.
2. Detachment: Accepting scientific work as service to society, as a calling, detached from a scientist's professional (e.g., career advancement) or personal (e.g., money) interests.

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[10]See here for a list of past award winners: **https://www.rrbm.network/taking-action/awards/responsible-research-in-management-award/**.

[11]Information about this scholarship program can be found on the RRBM website (see, specifically, here: **https://www.rrbm.network/daretocare/**).

[12]This online course is adapted from a classroom course titled "Philosophy of Management Research" that I introduced in 2015 to doctoral students in the three leading business schools in China: Guanghua School of Management, Peking University; School of Management, Fudan University; and Antai College of Economics and Management, Shanghai Jiao Tong University. Many more Chinese business schools now are teaching this course. There is also an annual four-day workshop to train faculty to teach this course. Information about this online course sponsored by RRBM can be found, here: **https://www.rrbm.network/daretocare/online-courses/**.

3. Impartiality: Using impersonal criteria and objective processes, unbiased by personal background or preferences, in conducting or evaluating scientific work.
4. Humility: Recognizing science as one of many ways to advance human societies, maintaining modesty about the truth of discoveries, welcoming criticism, and self-correcting.
5. Communality: Treating scientific discoveries as a public good, accessible to all, with open and timely sharing of discoveries, regardless of the source of funding or ability to pay.

These norms complement the seven Principles of Responsible Research proposed by RRBM. The seven principles are structural guidelines to enable responsible research. Norms are noncodified commonly accepted informal guides to scientific behavior. I invite our research community to discuss and debate these scientific norms as complementary efforts to guide our scientific work in fulfilling our responsibility to deliver credible and useful knowledge.

There is one more tool available to the research community to implement the third responsibility, the idea of metascience (Enserink 2018). Metascience differs from the philosophy of science with its use of quantitative analysis to study how scientific practices influence the veracity of scientific conclusions. It differs from research methods by its broader focus on the factors contributing to successes or failures in scientific work (Schooler et al. 2016). Metascientists believe that better understanding how scientists do their work can contribute to better science.

Metascience aims to understand, identify, and potentially correct research practices that may compromise the credibility of research findings. It focuses solely on the credibility issues and is based on data or traces of research footprints. The Principles of Responsible Research and the scientific norms are designed to guide the choices researchers make when they undertake a scientific endeavor. The principles and the norms serve a complementary function; one does not replace the other. I encourage the management research community to consider forming a responsible research council made up of the editors of the major journals in the core business and management disciplines to be the guardian of this third responsibility of science.[13]

CONCLUSION: A BRIGHT FUTURE AND A PRIVILEGE TO SERVE

The mission of university-based business schools is to create knowledge to improve business practices that will bring benefits to multiple stakeholders, including shareholders. We emphasize multiple stakeholders because the business ecosystem consists of not only those who provide the funds (owners or shareholders) but also those who provide their expertise and labor (workers), those who purchase their products or services (customers), those who supply raw materials or parts (suppliers), and all those (citizens) whose life may be affected by the business indirectly, e.g., the quality of water, air, and soil, byproducts of the production activities of business firms. As social scientists, our goal is not only to solve the mystery of our social and natural world but also, more importantly, to produce knowledge or evidence-based solutions to business problems with the ultimate aim of improving the human condition. Yet, in the past three decades business school research seems to have been derailed by two choices. One is giving stronger preference to economic than to social outcomes. This preference is revealed in the predominance of scientific inquiries examining and promoting economic outcomes that benefit shareholders more than outcomes that are important to stakeholders like customers, employees, suppliers, and people in the society supporting the business. The second is the pursuit of publications by all means and at all costs, even if it means

[13] Space restrictions do not allow full development of this idea. I invite interested colleagues to contact me to jointly explore and possibly develop this idea.

compromising the objectivity and truth-seeking purpose of science. Fortunately, self-correction has started. I think we can be cautiously optimistic about a bright future in management research.

It is still too early to know if the RRBM movement will bring about the changes to liberate our scholars so they can have the freedom to pursue their passion and to work on problems of the world that they care about. Most young scholars choose to devote five or more years to study for a degree that will give them the opportunity to solve problems and that will provide them with a meaningful career and a fulfilling life. As senior scholars, we must do our best to make sure that the research culture and practices allow young and old scholars alike to live out their dreams of making the world a better place through their life's scientific work and teaching. We must stop suppressing the curiosity and ambition of young minds by advising them to publish as many papers in the A-journals as possible, rather than the best work. I truly hope this article has provided promising signs of positive changes, and more are on the horizon. We can look forward to another Golden Age of management scholarship in general and organizational behavior/organizational psychology scholarship more specifically.

This article is a continuation of the call for attention to the state of our research by previous *Annual Review of Organizational Psychology and Organizational Behavior* perspectives authors. Paul Sackett (2021) found "the prioritization of theory" by the flagship journals at the expense of other contributions (e.g., strong data, methods, or practice) to be troubling. Barry Staw (2016, p. 1) argues that "future research needs to become more contextual and phenomenon-driven." Instead of applying social psychology to organizations, he invites researchers to shift toward a social psychology of organizations. Both Gary Latham (2019) and Denise Rousseau (2020) call for a better balance between theory and practice. These are the voices of scholars who feel a sense of responsibility toward society.

Every academic can tell a wonderful and unique story of his or her intellectual life. I am grateful for the opportunity to share mine. As other scholars have experienced, my research journey encountered the ordinary challenges of an academic life, such as planning projects, collecting and analyzing data, writing papers, responding to reviewer comments, experiencing the disappointment of rejections and the ecstasy of acceptances. Along the way, this journey has been blessed also with wonderful companions, and encouraging experience many readers may have had as well. I started my career in science because I was confronted with and intrigued by real leadership problems in business and wanted to help managers solve these problems and be better leaders. Then, as my work and responsibility in academia advanced, I increasingly realized that we also face serious problems in management research. So, together with like-minded colleagues, I wanted to improve the lives of those in the community that we care deeply about. The context shapes my choice of problems to focus on. Scientific freedom and scientific responsibility go hand in hand.

More than 80 years ago, Merton described the norm of disinterestedness to refer to scientists' detachment from personal gains and needs as they serve society through their scientific work. It's not a cliché to say that a career in science is a calling, not just a job, and not a cliché for Albert Einstein to say, "only a life lived for others is worth living." In all humility and sincerity, I wish to end by expressing my deep feeling that it has been a privilege to serve. This privilege is available to all management and organization scholars. Let's use and safeguard the freedom and responsibility that are needed for us to pursue important and meaningful scientific work.

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