I

INTRODUCTION

> If we now look at established procedures in the physical sciences we find that the scientist begins to believe that (s)he is winning when (s)he gets reproducible results from several experiments done under various conditions, perhaps with different instruments at different sites, etc. Looking for reproducible results is a search for significant *sameness*, in contrast to the emphasis on the significant *difference* from a single experiment. (Nelder, 1986, p. 113)

> I propose that the future vitality and success of our profession depends on making sure our research-based knowledge is relevant and useful. This will require the Academy of Management . . . to be far more engaged with the real world than has traditionally been the case. (Cummings, 2007, p. 355)

Complaints about the usefulness of academic management and social science research in dealing with real-world issues are common. In addition to Cummings's (2007) observations in the introductory quotation, Van de Ven and Johnson (2006) cite a number of recent studies challenging the relevance of scholarly management research for solving practical problems. This view is supported by Ghoshal (2005, p. 75), T. G. Gill (2010, p. 1), and Pfeffer (2007, p. 1334). Indeed, Bennis and O'Toole (2005, p. 99) claim that academic publishing is seen as a "vast wasteland" as far as business people are concerned. Given Micklethwait and Wooldridge's (1996, p. 12) premise that much management theory is bedeviled by obfuscation, jargon, and faddishness, such impressions are understandable.[1]

In marketing, November (2004) advises practitioners to ignore academic research. Reibstein, Day, and Wind (2009, p. 1), meanwhile, in a *Journal of Marketing* guest editorial called "Is Marketing Academia Losing Its Way?", are worried about an alarming and growing gap between the interests and priorities of marketing academicians and the needs of marketing executives. Or take the area of management accounting, where Otley (2003, p. 319) comments that "we have done very little sound work in this field, and we have certainly failed to influence practice in a significant way."

Even economics, the discipline which the other business and social science fields often seek to equal, "positively extols esoteric irrelevance" (Ormerod, 1997, p. 20), "create[s] more confusion than clarification" (Hossein-zadeh, 2014, p. 44), and is "useless" as a means of understanding a capitalist economy (Keen, 2001, p. 10). Evidence of this discontent was seen when Hayek (1989, p. 3) took the opportunity in his Nobel Memorial Lecture, provocatively titled "The Pretence of Knowledge," to berate the economics discipline for making "a mess of things." It continues to be witnessed in Akerlof and Shiller's (2009, p. xi) charge, two more Nobel laureates, that "ignorance of how the economy really works has led to the current state of the world economy, with the breakdown of credit markets and threat of collapse of the real economy in train." Still another Nobel laureate, Stiglitz (2010, p. xx), blames the economics profession for helping to precipitate this crisis. As does Madrick (2014). Indeed, Shiller (cited in Fox, 2009, p. 232) calls the efficient market hypothesis, the backbone of academic and policy thinking no less, the most remarkable error in the history of economic theory.[2]

The contributions to useful knowledge made by the social sciences as a whole were roundly lampooned by Andreski (1972) in his *Social Sciences as Sorcery.* Three years later Elms (1975, p. 967) talked of a "crisis of confidence" in social psychology brought on by, among other things, a demand for relevant research. The following decade Shweder and Fiske (1986, p. 1) weighed in that

> there has been in the social sciences, at least in recent years, a vague sense of unease about the overall rate of progress of the disciplines. A . . . literature has emerged . . . either challenging the scientific status of social research or expressing concern about the accomplishments of the social sciences. Some have even talked of a "crisis" in social inquiry.

Yet again in the next decade, Loftus (1996, p. 161) disclosed:

> I have developed a certain angst over the intervening 30-something years [since entering the psychology discipline in 1964]—a constant, nagging feeling that our field spends a lot of time spinning its wheels without really making much progress. This problem shows up in obvious ways—for instance, in the regularity with which findings seem not to replicate.

And based on what they see as an unprecedented level of anxiety concerning the reliability of research findings in psychology, Pashler and Wagenmakers (2012, p. 528) ask once more whether there is a crisis of confidence in the field. They answer in the affirmative. Finally, while sympathetic, Flyvbjerg (2001, pp. 1–2) acknowledges the assaults on the credibility of the social sciences as sciences. In this respect, Bauer (1994, p. 128) goes so far as to say that "in the social sciences, little is known or predictable that is deeper than triviality or different from commonsense knowledge." Bauer's position is seconded by Taagepera (2008, p. 236): "The ruling emperor of social sciences has no clothes. His quantitative garb is largely make-believe."

Especially pertinent is that some investigators are troubled by the news that despite the enormous amount of data-based research taking place, there is nevertheless a paucity of empirical generalizations—which is to say empirical regularities, "stubborn facts," or phenomena—in the management and social sciences. In Barwise's (1995, p. G30) view, a very weak definition of an empirical generalization is "*any empirical observation which has been found to generalize*." Bass (1993, p. 2) comments that an empirical generalization "is a pattern or regularity that repeats over different circumstances and that can be described simply by mathematical, graphic, or symbolic methods." Subsequently, Bass and Wind (1995, p. G1) explained that an empirical generalization is a "pattern that repeats but need not be universal over all circumstances," while Ehrenberg (1995, p. G20) considers them to be "merely data-based regularities." Lastly, Shelby Hunt (1991, p. 113) explains that "an empirical regularity is a statement summarizing observed uniformities or relationships between two or more concepts or variables." The common thread running through these definitions is the idea that results are essentially *repeatable* over a wide range of conditions (e.g., organizations, geographic areas, time periods, measurement instruments, methods of data collection, researchers).[3]

Barwise (1995, pp. G30–G31) takes these definitions further when elaborating on five characteristics that "good" empirical generalizations or facts should possess. The first is *scope;* they are not universal, but nonetheless hold under a variety of different conditions. The second is *precision;* they describe a phenomenon that has been witnessed several or many times, and the more specific that description the better. The third is *parsimony;* they are uncluttered by erasing a number of variables that might have mattered. The fourth is *usefulness;* they are of considerable benefit to practitioners. And the fifth is a *link with theory;* they stimulate theory construction because their relative persistence deserves an explanation.

Unfortunately, the dearth of facts in the business and social sciences prompted Leone and Schultz (1980, p. 11) to assert that marketing's knowledge base is "more marsh than bedrock," and Armstrong and Schultz (1993) to conclude that marketing does not possess a body of "principles." In economics, Hicks (1979, pp. 1–2) admits: "There are very few economic facts which we know with precision. . . . There are few economic 'laws' which can be regarded as at all firmly based." Similarly, Keuzenkamp (2000, p. 1) concedes that if *Econometrica* were to publish a single issue containing well-established facts, "it might be very thin indeed." And while defending the field, Randall Collins (1989, p. 125) is sensitive to the accusation that after 100 years of research, sociology is dismissed as a science in many quarters because it has no findings (facts) or valid generalizations.

Unsparing criticisms, these. But accurate nonetheless. For, as told above, it is inescapable that there is growing concern over the lack of advancement in the management and social sciences. Moreover, the oft-invoked excuse that the disappointing progress is attributable to the comparative youth of the disciplines involved is wearing thin. This lack of forward momentum in the social and business sciences is by now a decades-long all-too-familiar refrain, one which will echo well into the future unless fundamental changes are wrought in these disciplines. To this end I am of the belief that progress in the behavioral and business disciplines will not come about short of a total reconceptualization of how we think about science. This book lays out what this reconceptualization must involve—a relinquishing of the emphasis on the idea of *significant difference* and a commitment to the notion of *significant sameness.*

The thesis of this book, expanding on Hubbard and Lindsay (2013a, 2013b), is that a crucial reason for the scarcity of useful knowledge is because members of the social and management sciences subscribe overwhelmingly to a single methodological paradigm, one that revolves around the idea of significant *difference.* With few exceptions (such as those doing qualitative research) the significant difference paradigm monopolizes graduate business and social science education. Consequently, those in these research communities have been taught that this approach describes *the* scientific method.

This is unfortunate because the significant difference paradigm militates against the procurement of facts and the theories which could be built around them. By and large, this model offers a poor description of how science works. For instance, it rests on a simplistic conception of knowledge, in which theories are articulated over an extremely short period of time. This paradigm sees the research process as one of testing these hastily assembled theories following the hypothetico-deductive model of explanation. The goal is to produce statistically significant ($p \leq .05$) outcomes, in the main on isolated data sets, in what must be presented as "original" or "novel" contributions if the manuscript is to be published. Explicit attention to external validity (generalizability) considerations for the most part is cursory, ignored, and/or taken to be handled satisfactorily within the statistical model of generalization—from sample *to* population—courtesy of random sampling. This latter belief, coupled with the widespread opinion that good theories should produce universal laws, feeds the propensity to overgeneralize the results of these unique studies to other contexts and time periods. The legacy of this monopolistic paradigm, when seen together with the well-known editorial-reviewer biases against publishing "negative" (i.e., $p > .05$) results and replication research, is an empirical literature consisting almost entirely of unverified, fragile results whose role in the development of cumulative knowledge is of the shakiest kind.

It is important to add that this monopoly enjoyed by the significant difference paradigm in the management and social sciences—and the counterproductive research attitudes and behaviors it sanctions—is no straw man (Hubbard & Lindsay, 2013b, pp. 1394–1395). Rather, it is a faithful description of the research culture in these areas, and it codifies bad science (Hubbard & Lindsay, 2013b).

Monopolies seldom are desirable, particularly when it comes to scientific inquiry. There are alternative pathways to knowledge acquisition, not just that touted by the significant difference tradition. A major one of these, labeled significant *sameness* by the eminent statistician John Nelder in an introductory quotation, is developed in greater depth throughout this book.[4] In doing so it is shown how the significant sameness paradigm views the conception of knowledge as messy: Data rarely speak for themselves; universal generalizations are impossible because of the intrinsically contingent nature of relationships; and model uncertainty is a fact of life and cannot be addressed by even the most sophisticated statistical manipulations in a single data set, but which can be resolved by gathering additional data via well-designed studies. This paradigm is concerned with developing theory in research programs whose first aim is the discovery of empirical regularities, followed by increasingly deeper and higher level generalizations, involving many sets of data over an extensive period of time. The role of statistical significance testing is marginalized. Instead the goal is to determine whether significant sameness is found between initial and subsequent studies as defined by *overlapping confidence intervals* around the parameter(s) of interest. The significant sameness paradigm emphasizes the importance of well-designed replications because replication is the only way to establish empirical regularities. These replications systematically probe, often using purposive sampling, the scope and limits of quantitative findings *across* relevant (sub)populations. It should not be thought, however, that significant sameness means "brute empiricism." On the contrary, in keeping with a *critical realist* philosophy, theory construction is invigorated by seeking explanations for these regularities (and exceptions), and this process depends heavily on the use of abductive inference as well as deductive and inductive reasoning.

The logical positivists' quest for certain knowledge is chimerical. Rather, consistent with the significant sameness viewpoint, the most that can be done is for the scientific community to continue eradicating mistakes in our knowledge. This is achieved by ruling out competing explanations for a phenomenon via the accumulation of evidence derived from critically testing our theories.

This book shows that the significant sameness model, viewed as a whole, represents a new and superior way for designing research and analyzing results in the management and social sciences than is offered by the significant difference approach. Yet an important caveat is necessary at this juncture. Calling the significant sameness paradigm—the need to uncover first empirical regularities and then the theories to account for them—a new approach to the establishment of knowledge is true enough when applied to modern-day social and management science.[5] But it is commonplace in the physical sciences, where it has been responsible for much of the knowledge development in these areas.

In addition it must be stressed that reservations concerning the viability of the significant difference paradigm documented in this book go far beyond the endemic problems of researchers misinterpreting the results and capabilities of statistical significance tests. This topic does, however, receive in-depth coverage where appropriate.

It must further be emphasized that too many academicians in the social and management sciences seem not to be concerned with attempting to provide useful knowledge for those making social and business policy decisions. For the most part, scholarly priorities are attuned to securing career advancement within the "publish or perish" academic world. This means an unbending fealty to the significant difference paradigm, and with it the lack of relevant, applicable knowledge produced by the management and social sciences now and in the future.

The book is organized around the head-to-head contrasting of the two conceptions of science over a number of broad dimensions—philosophical, methodological, and statistical. These are summarized in Table 1-1.

Table 1-1 Contrasting the Significant Difference and Significant Sameness Paradigms

Categories	*Significant Difference*	*Significant Sameness*
Philosophical		
Conception of knowledge	*Unproblematic.* Centers on rejecting the null hypothesis at the $p \leq .05$ level to establish facts.	*Problematic.* Data rarely speak for themselves. Proof in science is impossible. The focus, instead, needs to be on the scholarly community gradually weeding out errors by eliminating rival explanations for a phenomenon on the basis of the accumulation of evidence obtained from critically testing theories.
Model of science	Almost exclusive attention on *testing,* rather than developing, theory via the hypothetico-deductive method. Logical positivist/empiricist orientation.	*Developing* theory using inductive enumeration to identify and generalize empirical regularities over many data sets. These regularities, in turn, are accounted for using abductive inference. Consistent with a critical realist philosophy.
	A single study can produce rational knowledge.	Many studies within research programs are necessary to develop and establish theory. And this takes a great deal of time.

(Continued)

Table 1-1 (Continued)

Categories	*Significant Difference*	*Significant Sameness*
	Good theory produces (or should produce) *universal* generalizations.	Science deals with *restricted*, not universal, generalizations that possess extensive empirical backing and known boundary conditions.
Role of "negative" results	Rarely published; considered to reflect poorly on the researcher rather than on nature.	Crucial in establishing boundaries on findings. Negative results also are a heuristic for developing better (deeper) theory.
Methodological		
Importance of replication	Incidental and rarely done. Considered to be an inferior kind of research. "Novel" or "original" research is all-important.	Defines this paradigm. Protects literature from specious results. More important, replication is the only vehicle available for discovering empirical generalizations and placing bounds on their application. In addition, the entire validity generalization process is based on replication research.
Definition of replication success	Statistical significance in the same direction as the earlier study.	Significant sameness as revealed by overlapping confidence intervals around point estimates.
Conception of generalization/ external validity	*Statistical* generalization following the representative model (i.e., from sample to population). Emphasis on random sampling.	*Empirical* generalization across data sets (subpopulations), often using purposive sampling.
Statistical		
Model uncertainty	Often ignored, further contributing to the primacy placed on statistics in conferring knowledge status.	Explicitly acknowledged. Statistics is subordinate to research design. Model uncertainty can be overcome only by examining many sets of data.

Categories	*Significant Difference*	*Significant Sameness*
Nature of predictions	*Qualitative*. This is all that managerial and social science theories are capable of offering.	*Quantitative*. Notion of predictive precision and severe tests is crucial in dealing with model uncertainty.
Role of *p*-values	Lies at the heart of the entire research process. Statistical significance is considered to be the essential criterion for establishing knowledge claims.	Used as a heuristic, not as an objective measure of knowledge.
Role of effect sizes	Beginning to be reported in some business and social science disciplines.	Routinely reported and interpreted.
Role of confidence intervals	Rarely used in business and social science disciplines.	Routinely reported and interpreted.
Individual Researcher Philosophy		
	Centered on personal academic career advancement in a publish-or-perish environment. Publishing papers and accumulating citations are of the utmost importance. Contributions to knowledge are a secondary concern.	Centered on knowledge development in research programs. Career prospects stem from knowledge discoveries, not publication and citation counts.

Source: Adapted from Hubbard, Raymond and R. Murray Lindsay (2013a), "From Significant Difference to Significant Sameness: Proposing a Paradigm Shift in Business Research," *Journal of Business Research*, 66 (September), p.1379 with permission from Elsevier.

In a nutshell, this book demonstrates that the significant difference paradigm is philosophically suspect, methodologically impaired, and statistically broken. As such, even on its own terms it is a model of corrupt research to be discarded. Aggravating matters, the significant difference paradigm is embedded in an academic social structure whose publication biases complete the institutionalizing of this corruption. While no route to knowledge generation is perfect, the significant sameness approach avoids the above problems by offering an alternative, and better, perspective on the conduct of management and social science.

Contrasts between the two paradigms begin on philosophical grounds. Accordingly, Chapter 2 outlines the intellectual cornerstones of the significant difference model.

Notes

1. See also J. Gill and Whittle (1993, p. 281) and Pfeffer and Sutton (2006b, p. 13) in this regard.
2. Ariely (2008, 2009) and Kahneman (2011) are worth reading in this context.
3. See also Bass (1995) and Uncles and Wright (2004, p. 5).
4. Ehrenberg's (e.g., 1993a, 1993b; 1995) contributions on this topic also are foundational.
5. As will be shown in Section 8.4, however, what is not generally known is that the idea of significant sameness was instrumental in the 19th century evolution of both the statistics and social science disciplines. Thereafter, it lost favor.