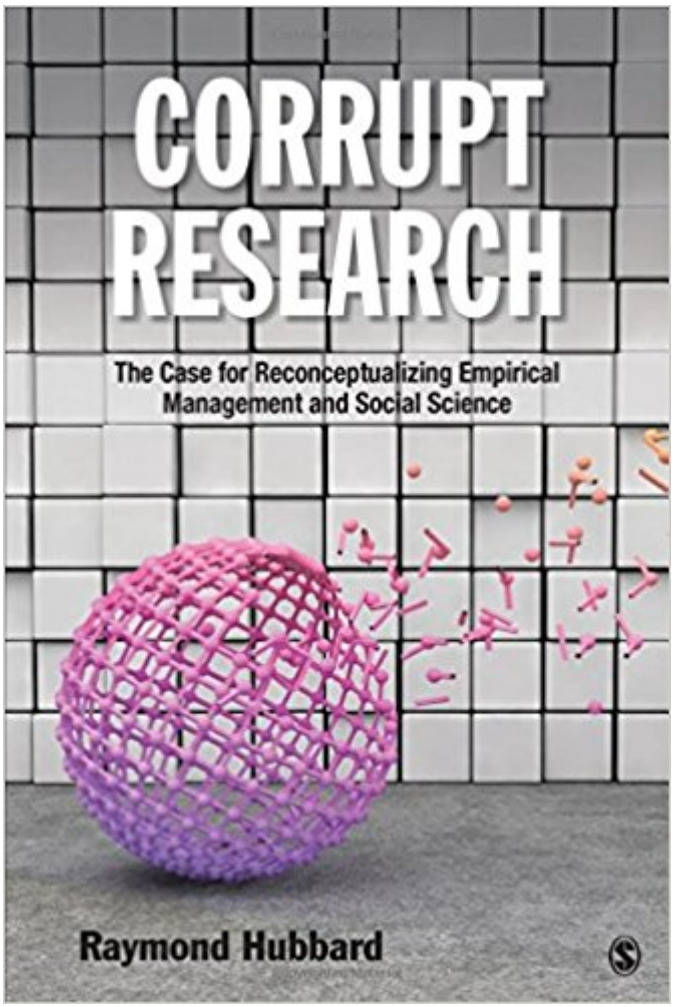
# Corrupt Research: The Case For Reconceptualizing Empirical Management And Social Science



## Synopsis

Addressing the immensely important topic of research credibility, Raymond HubbardÃ¢â ¬â„¢s groundbreaking Corrupt Research proposes that we must treat such information with a healthy dose of skepticism. This book argues that the dominant model of knowledge procurement subscribed to in these areasÃ¢â ¬â ¢the significant difference paradigmÃ¢â ¬â ¢is philosophically suspect, methodologically impaired, and statistically broken. Hubbard introduces a more accurate, alternative frameworkÃ¢â ¬â ¢the significant sameness paradigmÃ¢â ¬â ¢for developing scientific knowledge. The majority of the book comprises a head-to-head comparison of the "significant difference" versus "significant sameness" conceptions of science across philosophical, methodological, and statistical perspectives. Ã Â

## Book Information

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## Customer Reviews

Raymond Hubbard is Professor Emeritus of Marketing at Drake University, Des Moines, Iowa, USA. He holds a B.Sc. (Econ) Hons degree from the University of London, England; an M.Sc. in Geography from the University of the West Indies, Kingston, Jamaica; and an M.A. in Economics and a Ph.D. in Geography from the University of Nebraska, Lincoln. He taught previously at SUNY Fredonia, New York; and held visiting positions at the University of Washington, Seattle, and at the University of Auckland, New Zealand. His research interests include applied methodology, and the sociology and history of knowledge development in the management and social sciences. He has published numerous articles on these topics in journals in these fields. He is a lifelong supporter of

Sunderland A.F.C. and a Cornhusker fan since the early 1970s.

As someone who has taught research methods on the doctoral level for the past 30 years I think that this is one of the most important books on research that I have come across during that time. It should be read by anyone in the social and managerial sciences who (a) teaches research methods, (b) conducts research, and (c) serves on the editorial boards of journals and foundations. The work convincingly shows that rather than producing unbiased findings, current scientific conventions actually encourage widespread cheating and the publication of research whose findings are either trivial or invalid. It has evolved into a system that serves the professional needs of academicians and editors but that places the credibility of scientific knowledge at risk. The author writes in a compelling stye that is accessible and interesting even to those without a technical background. He also provides reasonable recommendations for reforming the production and dissemination of scientific research. Chief among them are to use more robust scientific criteria and place increased emphasis on publishing replication studies to see if the results actually hold up before they are considered to be valid guides for practice and policy. I highly recommend this book.

This is an impressive and important book! It is timely, scholarly, broad-ranging, and so very right.The basic argument is simple: Dominant research practice based on statistical significance testing is damaging, even corrupt, and there is an urgent need to change.Hubbard centres his discussion in the management sciences, but he draws on many other disciplines. His argument applies to any discipline that relies on statistical significance testing, meaning an enormously wide range from social sciences to biomedicine and beyond.Numerous critiques of statistical significance testing have been published, but HubbardÃƒÂ¢Ã â ¬Ã â„¢s excoriation of what he terms the ÃƒÂ¢Ã â ¬Ã Ëœsignificant differenceÃƒÂ¢Ã â ¬Ã â„¢ paradigm is especially detailed and well-informed. Researchers go to extraordinary lengths to achieve ÃƒÂ¢Ã â ¬Ã Ëœstatistical significance, p < .05ÃƒÂ¢Ã â ¬Ã â„¢, as seems necessary for publication and career advancement. Many erroneous findings are publishedÃƒÂ¢Ã â ¬Ã â •this is the replicability crisis. Few replications are carried out, so error often persists.Hubbard advocates what he terms the ÃƒÂ¢Ã â ¬Ã Ëœsignificant samenessÃƒÂ¢Ã â ¬Ã â„¢ strategy, which focusses on replication and sustained problem solving. He gives us a terrific discussion of various types of replication studies, used to build cumulative knowledge. Estimation and meta-analysis play a central role. These are what I refer to as ÃƒÂ¢Ã â ¬Ã Ëœthe new statisticsÃƒÂ¢Ã â ¬Ã â„¢: the techniques themselves are not new, but using them as the primary way to draw conclusions from data would be

new for many researchers, as well as a great step forward. The new statistics fit perfectly with what Hubbard is advocating.(I declare an interest. My 2012 book ÃƒÂ¢Ã â ¬Ã ËœUnderstanding the new statistics: Effect sizes, confidence intervals, and meta-analysisÃƒÂ¢Ã â ¬Ã â„¢ and 2017 book just released, co-authored with Robert Calin-Jageman, ÃƒÂ¢Ã â ¬Ã ËœIntroduction to the new statistics: Estimation, Open Science, and BeyondÃƒÂ¢Ã â ¬Ã â„¢, both published by Routledge, align closely with HubbardÃƒÂ¢Ã â ¬Ã â„¢s recommended research strategy.)A particular strength of HubbardÃƒÂ¢Ã â ¬Ã â„¢s book is its strong conceptual and philosophical framework. Hubbard argues that the significant difference approach uses a superficial and flawed version of hypothetico-deductivism; he advocates instead a critical realist framework as providing a richer basis for research.I have one quibble: The word ÃƒÂ¢Ã â ¬Ã ËœsignificantÃƒÂ¢Ã â ¬Ã â„¢ is so deeply ambiguous and so often misleading that I hesitate to use it in the title (significant sameness) of an advocated strategy. I should also mention that Open Science, which is probably the most positive and valuable development in research methodology of recent years, receives only a few brief mentionsÃƒÂ¢Ã â ¬Ã â •although these are enthusiastic. There is great scope for HubbardÃƒÂ¢Ã â ¬Ã â„¢s analysis to help guide the development of good Open Science practices. Perhaps a second edition will build such valuable bridges?Any researcher or graduate student who still relies on statistical significance testing should read Hubbard and consider carefully why they persist. Any who are moving on from such testing will find invaluable support and guidance as they participate in what should be a great leap forward in research methodology. Yes, itÃƒÂ¢Ã â ¬Ã â„¢s that important a book.

Summarizes lots of evidence that the pervasive use of NHTS (null hypothesis statistical testing) is bad practice. Hubbard calls attention to editorials by Trafimow (2014) and Trafimow and Marks (2015). Trafimow is the Editor in Chief of BASIC AND APPLIED PSYCHOLOGY; Trafimow states that NHST findings as well as confidence intervals in articles will need to be removed if an article is accepted for publication and contains such findings. Experts (e.g., J. Scott Armstrong, Gerd Gigerenzer) take similar/same perspectives on the bad practices nearly always observable with the use of multiple regression analysis and other symmetric tests. Hubbard's book is no quite sufficient in explaining how to actually do what he recommends: statistical sameness testing (SST). Constructing algorithm based theories that consistently and accurately predictive high scores or the negation of high scores in an outcome is what to do and such SST analytic methods is appearing now in the marketing and management literature (e.g., "Moving beyond multiple regression analysis to algorithms," Woodside, 2013).Also describes how almost all researchers ignore the issue of

achieving reproducible findings--many editors reject replication studies. Hubbard provides names in his discussions of rather bad thinking. Refreshing to read such blunt assessments. Hubbard describes "precise outcome modeling" in the behavioral sciences--an exciting prospect whose time has arrived. The focus on relative size of standardized partial regression coefficient still dominants research in most journals and the sub-disciplines of management, marketing, finance, accounting--these studies are shallow in theory and findings; a rather shockingly bad state of affairs in research in fields of business studies. The main point: reading Hubbard (2016) is a great learning experience--not to be missed!