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The Halo Effect and the Challenge of Management Inquiry: A Dialog Between Phil Rosenzweig and Paul Olk

Paul Olk[1] and Phil Rosenzweig[2]

Abstract

The ideas found in Phil Rosenzweig's book *"The Halo Effect … and the Eight Other Business Delusions That Deceive Managers"* provide the basis for a conversation between Phil and Paul Olk. Besides discussing Phil's critique of the research methods found in some popular business books, they address implications of the halo effect and the other delusions for teaching and conducting academic research, including the difficulty in conducting research that is both methodologically rigorous and practically relevant, teaching executives, and bridging the relationship between theory and practice.

Keywords

halo effect, management education, performance, research methods, teaching

Introduction

In *The Halo Effect … and the Eight Other Business Delusions That Deceive Managers* (2007, The Free Press), Phil Rosenzweig addresses an issue that has bothered him—and many academics—for years: the methodological flaws in some of the most popular recent business books. Specifically, he notes that in books like *Built to Last, Good to Great,* and *In Search of Excellence,* the researchers start by identifying successful companies and then work backward to discover what distinguished these companies from less successful ones. In so doing, Phil argues, their data are often contaminated by the halo effect. Identified by Edward Thorndike in the early 20th century, the halo effect occurs when a general perception shapes specific judgments. As it pertains to business performance, knowledge about a company's performance—which is seemingly concrete and precise—often shapes perceptions of other variables that are less precise and often more subjective, such as leadership, culture, execution, and so forth. As a result, studies that draw on data that are shaped by the halo effect may misattribute the causes of company performance. Phil argues that since the methodology and data in these studies are suspect, managers should be skeptical of the recommendations. Drawing on questionable data, in turn, leads to a number of mistakes which pervade the business world (Table 1).

The Halo Effect has received many favorable reviews, including an award for Best Business Book of the Year at the Frankfurt Book Fair, mention as one of the Best Business Books of 2007 by *The Financial Times*, and mention in the *Wall Street Journal* as one of the most noteworthy business books of the year. An article based upon the book received the 2008 Accenture Award for the article in the preceding volume of *California Management Review* that made the most important contribution in improving the practice of management. Whereas the book is written for the "reflective manager" and is critical of the research found in many popular books, its message has implications for academics regarding research methods and the relationship between theory and practice. Paul Olk engaged Phil in a dialog to explore some of the general ideas of the book, as well as implications for teaching and academic research.

Specific Arguments Made in *the Halo Effect*

Paul: You identify the halo effect as contaminating the validity of the conclusions in popular business books and mention several other delusions as well. Besides what I assume was your annoyance in seeing bad methods—which irritates most of us—what motivated you to write the book? Were managers parroting these ideas without thinking about them? Were you having students present this material to you as factual?

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Table 1. The Nine Delusions

1. The Halo effect
   The tendency to look at a company's overall performance and make attributions about its culture, leadership, values, and more. In fact, many things we commonly claim drive company performance are simply attributions based on prior performance.
2. The delusions of correlation and causality
   Two things may be correlated, but we may not know which one causes which. Does employee satisfaction lead to high performance? The evidence suggests it's mainly the other way around—company success has a strong impact on employee satisfaction.
3. The delusions of single explanations
   Many studies show that a particular factor—strong company culture or customer focus or leadership—leads to improved performance. But since many of these factors are highly correlated, the effect of each on is usually less than suggested.
4. The delusions of connecting the winning dot
   If we pick a bunch of successful companies and search for what they have in common, we'll never isolate the reasons for their success, because we have no way of comparing them with less successful companies.
5. The delusion of rigorous research
   If the data aren't of good quality, it doesn't matter how much we've gathered or how sophisticated the research methods appear to be.
6. The delusion of lasting success
   Almost all high performing companies regress over time. The promise of a blueprint for lasting success is attractive but not realistic.
7. The delusion of absolute performance
   Company performance is relative, not absolute. A company can improve and fall further behind its rivals at the same time.
8. The delusion of the wrong end of the stick
   It may be true that successful companies often pursued a highly focused strategy, but that doesn't mean highly focused strategies often lead to success.
9. The delusion of organizational physics
   Company performance doesn't obey immutable laws of nature and can't be predicted with the accuracy of science—despite our desire for certainty and order.

Note: Adapted from *The Halo Effect*. Used with Permission.

Phil: I've worked mostly in executive education for the past 12 years, and have found that most managers are smart and eager to learn, but lacking in their ability to think critically. They seem willing to accept the findings of questionable studies based on face validity. Consultants are no better—and often worse. And many of our colleagues, who should know better, seem to set aside basic rules of data validity when writing for managers.

My book is an effort to raise the level of critical thinking in the business world. If managers aren't very good at critical thinking, the problem isn't with them—it's that we haven't helped educate them to be sufficiently discerning. I'm trying to help managers become better consumers of business research.

Paul: While reading your book, the reviewer in me had several questions about some of your claims that I would like to discuss. You have noted that newspaper and magazine accounts of company performance are often shaped by the halo effect. Some reporters don't have much of a memory for the explanations put forth only a few years ago for a company's success. They attribute success at one time to "being close to the customer" and poor performance only a few years later to "being arrogant to the customers." You criticize books that rely upon newspaper accounts and company sources for information as based on biased data. But don't popular accounts provide some useful information? Some very good academic research relies upon newspaper accounts and company documents to corroborate other sources. Barry Staw, whose research with Jerry Ross on decision making and organizational commitment, relied upon newspaper accounts—along with retrospective interviews and unpublished documents—in a paper that was awarded the best article of the year award in the *Academy of Management Journal* (Ross & Staw, 1993).

Phil: There's nothing wrong with newspaper accounts per se. We just have to be careful what we use them for. When we take data that are very likely *shaped by* performance, we have to make sure that we do not use those data as independent variables to test the *drivers of* performance. If we do that, we'll have real problems. And that's exactly what some well-known books do.

Paul: You also criticize many popular books for relying upon post hoc interviews. Retrospective bias is well-known as an issue for most interview efforts, but good researchers will be on the lookout for it, and attempt to triangulate the information with archival, factual data. Often there is no other way to collect this information than a retrospective interview, since researchers typically do not have access to the decision makers at the time the decisions are made, and this information does not appear in financial statements or public filings. You seem to be setting the bar for acceptable research higher than what many journals would accept.

Phil: I don't say that all the data gathered from interview sources are invalid. Much of it is very good, at least for some purposes. But much is of it cannot be used as independent variables to explain performance outcomes, since the recollections are themselves shaped by performance. That's why it's important to triangulate interview data with other sources.

Paul: Though you argue that some well-known books are not based on valid data, you seem to agree with some of their findings. In fact, some of your

recommendations—For example, (a) taking risks and (b) persistence and tenacity overlap some with those found in *Good to Great*.

Phil: Many ideas in popular management books are probably helpful much of the time. I'm not saying they're of no value at all. A book like *Good to Great* offers some good, common sense ideas about managing a business: It's important to have good people, to know what you are good at, to be persistent, to have strong values, and so forth. Who can be against those sorts of things? My complaint about books like *Good to Great* is twofold. First, they claim to be examples of rigorous research but are nothing of the sort. Second, and perhaps more important, they give a false sense that companies can predictably achieve success by following a formula. This may be a comforting story, but misses the fundamental notion that in a competitive market, performance is more relative than absolute. You can't become Great by following a formula for an obvious reason: if every company in an industry followed that formula, they would not all become Great! Success requires doing some things differently than rivals. Strategy is about choice—it's about choice under conditions of uncertainty and this means risk. Many of these popular books are dangerous in that they divert our attention from the fundamentally relative nature of performance and the need for managers to take risks.

Paul: As you bring the dimension of competition into the discussion, I was struck by how closely your description of risk and decision making resembles new venture investments; where the focus is on angel investors or venture capitalists who take informed risks in an often very competitive context, and recognize that all of these will not be successful. While this approach is clearly understood in the entrepreneurship literature, it has not been successfully adopted in books which focus on larger, more established companies.

Phil: Exactly right—high performance is about taking informed risks in a competitive setting. And yes, it is also true that strategic decisions for companies are very different from making many small investments in a portfolio. Companies can't diversify their risks in the same way—strategic choices are hugely consequential and hard to reverse.

Paul: Your critique focuses on the "right-side" of the regression equation—the independent variables that help explain performance. However, interesting questions can also be posed about the dependent variable—performance. How we should measure organizational performance is an issue you don't

explore. But as Denrell (2005), Meyer (2005), and Starbuck (2005) noted in a special forum of the *Journal of Management Inquiry*, the measurement of performance is problematic and a "noisy signal of capabilities." Furthermore, Hirsch and Levin (1999) noted not only is there no consensus on how to evaluate performance but that the issue is a dialectic—debates go on for decades between umbrella advocates and validity policy over such issues as relevance and rigor, until both sides give up, nothing is solved and the scholars go on to research other issues. You criticize these popular books for their sampling methods, a compelling argument can be made that these studies suffer because of the challenging task of assessing performance. My experience with trying to assess strategic alliance performance and in reviewing hundred of studies that use alliance performance is that we don't have a good understanding of how to evaluate performance.

Phil: True, I don't spend lot of time on the definition of performance. Trying to define a single measure of performance is probably futile, and I don't think it's necessary—there are many measures we might use. But I would make a very important distinction between measures of performance that are *absolute* (such things as quality defect rates, inventory turnover, customer satisfaction, etc.), and those that are in part *relative* (revenues and profit). I would also distinguish between those that are filtered through the expectations of investors (share price) and those that are not. Each of these ways of thinking about the dependent variable, performance, calls for different thinking regarding hypothesis formulation, data gathering, and testing—and also has different managerial implications. The point isn't to have a consensus on a single measure of performance, but to select appropriate research designs to study our measure of choice.

Implications for Research

Paul: Let's expand our discussion to consider management inquiry more broadly. Central to many discussions in our field is the question: "How or should we enhance the relevance of academy research?" One view is held by those who have argued that the social systems of academia and practice are separate and the trade-off between rigor and relevance will never be solved (e.g., Astley, 1985; Kieser & Nicolai, 2005). Another is held by those who argue that we need to be more careful about the type of research we conduct. Starbuck (2007) noted that

collaboration with managers can lead to better questions and better interpretation of findings. More emphatically, Tushman (2007) proposed that business research should focus on "Pasteur's Quandrant," which is Use-inspired Basic Research. This separates us from Pure Basic Research found in the primary disciplines and Pure Applied Research which is generally conducted by consulting firms. But as you mentioned, our findings are not often sexy for managers—they have low explanatory power, only apply to narrow situations, and often don't lead to prescriptive advice. Furthermore, in looking at most research articles appearing in our top tier research journals, the management implications brought up in the discussion section are—to be kind—usually not very insightful. My own work included, I doubt if many managers would find these "takeaways" very helpful in their jobs. It seems that there is a tricky balance in combining rigor and relevance.

Phil: I think there's room for both rigor and relevance, but we should try not to confuse the two. There's nothing wrong with pursuing some questions with the rigor of good social science research, but we should understand that the findings might not always be presented in ways that are directly applicable for practitioners. Similarly, what is written for managers need not always have the rigor of science—it should be actionable, practical, and useful. My book tries to expose managerially relevant work that pretends to be rigorous science, but that does not admit its own shortcomings.

Paul: Some of the authors you criticize in your book are academics at top schools. This seems to be another example of the challenges of trying to write material appropriate for managers. Do you think these academics get caught up in trying to dress up the research to make it appealing to a wide audience?

Phil: I think so. And it's okay for academics to use one set of language for academic work, then use a somewhat different language when addressing managers. But one would hope they would not relax their standards of integrity. Sometimes these writers completely let go of the standards of quality that guide them in their academic work—but do not admit they have done so. In fact, they want the best of everything—to make the claim of scientific validity, while at the same time discarding the very tenets of scientific research.

Paul: It seems like it is very rare to write a good selling book that is not a "feel good book" for managers. To have an effect on practice, researchers

not only have to tell a good story but make sure the take-aways are along the lines that "management can make anything happen." And to do this, researchers often have to go beyond the data and make recommendations that sound good but for which there is very little support.

Phil: There are several good books that meet the test of validity and that also appeal to managers. One example is Clayton Christensen's *The Innovator's Dilemma*, which is based on solid research and offers a hugely insightful story with enormous practical relevance. Another is Diane Vaughn's study of the Challenger launch decision, which is an example of outstanding retrospective intraorganizational research. Of course, she was fortunate because her subject, a governmental agency and its contractors, kept meticulous records and transcripts. There are other books that are very good and don't claim to be science—they don't make claims they cannot support.

Implications for Teaching

Paul: How do you avoid these biases in the classroom? In particular I am wondering about class discussions about best practices. In looking for examples to illustrate a concept, I—and I am sure others as well—sometimes pick a company that is doing well and highlight what appear to be its unique features. Many students complain that by the end of their program, they have studied several companies—Wal-Mart, Dell, Toyota and so on—in almost every course. While part of this can be addressed by more careful consideration of our examples, because we often have to rely upon mass media for these examples, how can we avoid these biases?

Phil: That's a great question. I try to bring critical thinking into the classroom—both in the frameworks we use and in the data we draw upon. I also try to resist easy attributions based on outcomes. Show me *any* successful company, and it's easy to say that it must have a brilliant strategy, a visionary leader, terrific people, great execution, and so on. Show me *any* struggling company, and we can make the opposite attributions. I try to insist that we not make such easy attributions, but rely as much as we can on objective data. Just yesterday, a manager in one of my courses said that a certain company failed because it had been "arrogant." I asked:

What's the difference between "arrogant" and "confident?" In our everyday conversations, as long as the company is successful we tend to say it is confident,

but when it's unsuccessful we say it is arrogant. But if the choice of terms is based on outcomes, and not on an objective assessment, then it's likely we're making an attribution based on performance, and not offering an explanation of performance.

Paul: Your book is written for "thoughtful managers" which would seem to be an important market. I like the phrase you use: "wise managers know that business is about finding ways to improve the odds of success—but never imagining that success is certain."

Phil: Introducing an appreciation for risk and probability is important, but it seems that promises of certainty are what sell. Unfortunately, many authors suspend what they know is correct in order to cater to the desires of the market.

Paul: When I have students in executive education who are thoughtful and are not seeking just simple formulas, it is fun. But unfortunately I find too many executives are overworked and cannot give the attention often needed to develop thoughtful answers. While talented, they seem to be balancing many different activities and if they can cut some corners by having someone listing some basic steps to take, they are more than appreciative.

Phil: Perhaps, but in my experience the number of managers who are receptive to critical thinking is larger than we often think. My sense is that many business best-sellers talk down to managers instead of trying to raise their thinking. Authors sell them short by spoon feeding formulas that they think readers will like to hear, instead of speaking to their more thoughtful and more critical sides. I've received compliments from many people who respect the integrity of data, and want to draw the appropriate conclusions from those data. There are a lot of people in these fields and managers in general who we can speak to. There's a lot we can do to nudge them in the right direction.

Paul: One of the recent suggestions for addressing managers has been to revise what we present to our students. Part of the discussion about the relevance of business education has been some calls (e.g., Pearce, 2007) to think about writing text books for experienced managers. The goal is not to introduce a topic that has been historically included in a textbook, but rather one that draws from research to address the problems faced by more practiced managers. Similarly, Rousseau and McCarthy (2007) argued for evidence-based management when teaching MBAs. In this approach, teachers should introduce ideas that have solid scientific support.

Phil: I'm all for evidence-based thinking, but we have to be clear what that really means. I very much like the recent book, *Hard Facts, Dangerous Half-Truths, and Total Nonsense* by Robert Sutton and Jeffrey Pfeffer, which talks about evidence-based management. But it's not enough simply to say "We need evidence." We also have to define what constitutes valid "evidence." The key is to make sure that our evidence is valid—miss that, and your "evidence" may be faulty. We also have to be careful when we apply a concept from medicine—"evidence-based management" comes from "evidence-based medicine"—because of the notion of relative versus absolute performance. If I have a ward of patients suffering from a disease, and I find a way to cure them, the improvement of each one is absolute—the recovery of one patient does not depend on the lack of improvement of another. But in business, company performance is largely relative—all the firms in an industry won't achieve better revenues and profits and market share at the same time. So the basic notion of evidence-based thinking has to be applied with care in business—the nature of the dependent variable is fundamentally different in business than it is in medicine.

Paul: I would imagine that most academics and managers would agree with the emphasis on valid research. If someone is going to give me advice, the more scientific support for the relationship the better. But is this an unrealistic expectation for both academics and managers? I know very few academics who turn to the teaching journals to inform their classroom efforts, or who turn to research when they move into administration, or, for that matter, who look at journals for the latest advice when they deal with other aspects of their lives (e.g., parenting questions). Rather, we reflect on our practices, talk to peers, observe others, and we may read some popular book on the issue, in hopes of finding something—often a trick of the trade—that will help. But we usually don't look at the evidence and ensure that it has been scientifically validated before trying it. In this way, I see most academics acting in practice more like managers. But I feel with the proposed approach, we are asking managers to do something—turn to academic research for guidance—which we ourselves don't do.

Phil: You're touching on a very important issue—the difference between the logic of science and the logic of narratives. In fact, the appeal of many of these books is that they tell good stories. Stories are hugely important in our lives: they help us make sense of the world, they help inspire, and they give

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us hope. There's nothing wrong with stories. But here's the paradox: many popular business books are effective as stories precisely because they claim not to be stories at all, but are dressed up to look like science! And most readers can't tell the difference.

Paul: Are you suggesting that if research hasn't provided strong evidence for what a manager should do, then managers should conduct experiments or other research on their own?

Phil: I am not expecting managers to become social scientists. I wouldn't urge managers to take the time to test hypotheses and gather data. What I'm suggesting is that managers learn to be appropriately skeptical when thinking about research. Take reports from consultants. Just last year I saw a study by a leading consulting firm that claimed to explain how some companies achieve rapid organic growth. Unfortunately, the research had all the common problems: They selected their sample based on their dependent variable, and then gathered data by relying on surveys that asked managers to retrospectively self-report. They claimed to have located the drivers of high performance, but in fact what they found were more likely *reflections of* past performance. Now, I don't expect managers to conduct a study like this. But if given such a study by a consultant, I would like them to be able to ask: "If this study is based on retrospective, self-reported data, maybe its conclusions should be scrutinized closely." I would hope that managers develop the capacity to hold us and others to higher standards.

如果这项研究是基于回顾性的、自我报告的数据
也许它的结论应该仔细审查

Paul: This discussion reminds me of Fleck (1979) observation on the role of practice in the development of a research. He noted that:

> Every discipline, in fact almost every problem, has its own *vanguard*, the group of research scientists working practically on a given problem. This is followed by the *main body*, the official community. … The vanguard does not occupy a fixed position. It changes its quarters from day to day and even from hour to hour. The main body advances more slowly, changing its stand – often spasmodically – only after years or even decades. (p. 124)

先锋队做高质量的研究是多么具有挑战性
但也是多么重要
否则主体可能会驳回调查结果而不遵循

Your book and our discussion reinforces how challenging but important it is to do high quality research in the vanguard; otherwise the main body may dismiss the findings and not follow. It has also encouraged me to be more demanding with my students—not only executives but also MBAs and undergraduates—and challenging them to not get caught up in a nice story, to keep questioning after deriving a preliminary explanation, and to avoid settling for someone's simple recipe that claims to solve all of their problems. Thank you for your time.

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Bios

Paul Olk is professor of Management at the Daniels College of Business of the University of Denver. His primary research interests focus on strategic alliance formation and performance evaluation and his publications have appeared in several books and in a number of management journals. He currently serves as

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Phil Rosenzweig is professor at IMD in Lausanne, Switzerland, where he works with leading companies on questions of strategy and organization. He has published extensively on topics of global strategy and international management. More recently, his interests center on critical thinking in business. His 2007 book, *The Halo Effect … and the Eight Other Busines Delusions that Deceive Managers* (New York: Free Press) was named Best Business Book of 2007 by getAbstacts, and has been translated into 14 languages.