

**SELF-REINFORCEMENT AND NEGATIVE ENTROPY:
THE BLACK HOLE OF BUSINESS-SCHOOL RESEARCH**

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ABSTRACT

The astrophysical phenomenon of the black hole is an excellent analogy for the current status of business-school academic-journal research, which reviews consistently show to be ignored by managers and professionals, failing to ask questions of interest or value to the management community, and to be “ignored with impunity” by professionals, according to one major study. Other reviews of the development of key management ideas over time trace virtually none of them to research published in academic journals. This status is frequently decried among the scholars who produce this research, appearing as a frequent topic of discussion in the journals of the Academy of Management.

While a number of authors have speculated on reasons why this state of affairs persists as it does, none of them have apparently used the literature of research methodology to examine our practices as possible contributors to the problem. That literature clearly identifies a small number of core practices that anchor our interpretations of research outcomes incorrectly; a growing mythology of interpretational errors which reinforce and expand these erroneous interpretations; and a systemic, cyclical pattern of reinforcement of these mistakes over time. While in most scientific endeavors peer review would be expected to reveal and correct such errors, peer review itself has been compromised within this system.

This paper discusses a model of these internal factors and the reinforcing external factors that make it a stable system of negative entropy. By AACSB International estimates, US business schools spend roughly 1/3 of a billion dollars on research each year, and the reviews make it clear that virtually no new information, of either theoretical or practical value, escapes.

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INTRODUCTION

Since envy is the closest affiliation business-school research faculty will ever have to physics and real science, I thought it appropriate to use the astrophysical entity of the black hole as a model to explain the waste and futility of that research. The choice of terms like “waste” and “futility” may upset some readers, but unfortunately I have been unable to find adjectives that are more appropriate to the large majority of the research being conducted in business schools. This is especially and unfortunately true in U.S. business schools, which many international scholars admire and whose methods are often emulated.

There is, however, an important difference between the physical and the social-science black holes—where physical black holes are theorized to emit Hawking radiation and exhibit entropy as they gradually “evaporate,” the business-school-research black hole (hereinafter BSRBH) exhibits stability and longevity through its properties of self-reinforcement and negative entropy. The BSRBH thus persists and continues to be an impediment to scientific understanding of management and organization.

This paper consists of three sections. Part I will explain the characteristics of the BSRBH and my reasons for choosing that model. Part II describes self-reinforcing systems and negative entropy, properties of the black hole that maintain it over time. Part III will describe the structure of the black hole in detail, identifying key components and the linkages between them that account for the self-reinforcing nature of the black hole and its longevity. A brief conclusion will suggest the potential for allowing the black hole to evaporate as we believe happens to astrophysical black holes, although history suggests this is unlikely; thus, in view of current global economic realities, the conclusion also suggests that BSR faculty may need to take corrective measures on their own initiative or have them taken for them.

PART I: WHY A “BLACK HOLE”?

At the outset, I want to be clear that the research I am discussing here is that which follows methods I refer to as the Generally Accepted Soft Social Science Publishing Process, or GASSSPP (Kmetz, 2011). The GASSSPP embodies the general research procedures common to “soft” psychology (Meehl, 1978) and will be discussed below; the research community generally accepts GASSSPP in the same way as the US accounting profession accepts its Generally Accepted Accounting Principles. In contrast, there are a few non-GASSSPP studies in b-school research (BSR) which have some value, principally in operations (which was partially rescued from obscurity from fascination with pure mathematics by the Japanese emphasis on quality in the 1970s and 1980s) and in selected areas of finance (e.g., Malkiel, 2003, on investing in equities). Unfortunately, the majority of disciplines and journals in BSR follow GASSSPP practices, which are neither statistically nor scientifically sound (Kmetz, 2011; the case against the scientific validity of this research may be found on my weblog, <http://sites.udel.edu/mjs/>, which includes nearly 200 references not listed here to conserve space).

As is detailed in Part III, most of the interior structure of the black hole consists of the beliefs and practices that make up the GASSSPP. In this section, I want to explain why I characterize BSR in this manner. A large part of this is explained in my 2011 paper:

Instead, the literature on evidence of research impact is a long record of studies showing that academic research in management is neither used by practitioners nor perceived as relevant to their interests (Behrman & Levin 1984; Bennis & O’Toole 2005; Buckley *et al.* 1998; Chia & Holt 2008; Cohen 2007; Deadrick & Gibson 2007; Duncan 1974a, 1974b; Dunnette & Brown 1968; Gopinath & Hoffman 1995; Guest 2007; Hambrick 1993, 1994; Hoffman & Gopinath 1994; Kilmann *et al.*, 1983; Lyles 1990; Miller 1999; Pfeffer & Fong 2002; Rynes *et al.* 2007; Starbuck 2006; Van de Ven 2000; Van de Ven & Johnson 2006). In their extensive first-hand study of business education, Porter & McKibbin (1988) concluded that managers “ignore academic research with impunity” (p. 304). In reviewing “key

management ideas,” Crainer (1998) cites not a single top journal, and the only “key idea” based on empirical research is Herzberg’s Two-Factor Theory, which most motivation researchers consider to be a discredited idea. Similarly, Mol and Birkinshaw (2008) trace not a single one of their “giant steps in management” to empirical research, and they also cite not a single top journal; this is especially ironic because their focus is on management innovations that have had significant impact.* The business press has consistently noted the lack of relevance of academic research over the years (Panning for gold 2004; Practically irrelevant 2007; Signifying nothing? 2004; Byrne 1990, 1997; Oviatt & Miller 1989; Skapinker 2008, 2011).

I analyzed the references from 30 best-selling business books published between 1996 and 2005 (Kmetz, 2009). In this analysis, I deliberately selected authors with academic backgrounds in greater proportion than non-academics, such that 13 of the 30 authors (43 percent) had academic experience. Among the 3,162 references cited in these books, there were only 361 academic journal citations. Of these, 154 are from economics, by far the most frequently cited discipline, and 152 of the 154 are cited in only three books, by authors with academic affiliations. The GASSSPP literature, of primary interest here, accounted for only 131 citations, and 90 of these are found in four books alone. I examined the sources of these, and only 40 of the 131 came from “top” journals; half of these 40 came from one book with an academic author, who cited the *Strategic Management Journal* 20 times. The 131 GASSSPP articles are scattered across 57 journals and at least a score of disciplines; the mode is 1 with a secondary mode of 20. They are also scattered across four decades (1964 - 2004), and as such necessarily across a wide range of standards of rigor. For many of them, even the title makes it difficult to second-guess why the author(s) cited it in the book.

Deeper examination of the nature of these papers suggests they are highly idiosyncratic choices. For example, three books in the sample use the word “leadership” in their titles, but one cites not a single study from any GASSSPP journals which would

include the voluminous leadership literature; only 10 articles are cited in the other two books, six of which are from psychology journals. Eight of the 10 papers cited were used by the one author with an academic affiliation; however, four of the eight are about happiness research, and two more seem to be concerned with that subject indirectly.

In this paper, I estimated conservatively that 63,750 GASSSPP journal articles could be in the potential pool of references. As one indication of how conservative my estimate of research production is, AACSB reportedly estimates that all journals in the field publish 15,000 to 20,000 articles per year (AACSB, 2007: 6), which would suggest that as many as 300,000 articles could be in the potential pool, and this does not include economics. Whichever figure is used as a baseline, it is clear that no one, academics included, use BSR. No matter what analysis is examined, the depressingly similar conclusion is that once published, BSR disappears from every potential application except the promulgation of more BSR.

For reasons to be discussed in Part III, the GASSSPP destroys all work done following its processes. There are doubtless high-quality papers produced by BSR, but the nature of the GASSSPP is to render confirmation of that quality impossible. Unfortunately, the GASSSPP also makes the discovery of error, or even fraud, equally impossible. Thus, the black hole is real and the black hole destroys everything. Research efforts conforming to the GASSSPP are wasted, and efforts to advance scientific understanding are futile. Despite the continuing documentation of the fatal flaws of the GASSSPP (for examples 34 of the 140 items in my weblog references on statistical significance testing have been published since 2001), the BSRBH shows every promise of remaining the self-reinforcing system it has been since 1959.

PART II: SELF-REINFORCEMENT AND NEGATIVE ENTROPY

It is appropriate to define the concepts of “self-reinforcement” and “negative entropy” as used in the context of the BSRBH. I will begin with negative entropy, since I regard that as a property on which a self-reinforcing system depends.

Entropy is embodied in the Second Law of Thermodynamics, which states that energy is gradually lost in a physical system until it cools to a point where activity effectively stops. It is easy to assert that this law applies to organizations, and indeed any form of organized behavior, since we witness the rapid decline in numbers of new companies, the absorption of organizations into others, and over long periods the rise and decline of empires. Not surprisingly, organizations formed in environments characterized by competition for resources tend to have shorter lives than those in non-competitive environments, and so we find that three organizations among the very few to have survived more than a millennium are the Catholic Church, the monarchy of England, and the University of al-Karaouine, founded in 859 in Fez, Morocco (and also a religious organization). Corporations like DuPont, which has survived for over two hundred years, are very rare; according to one recent study (Marmer *et al.*, 2011) the five-year survival rate for new companies is less than 10 percent.

But organizations are composed of sentient beings rather than solely physical entities, and can take actions to arrest, contain, and sometimes reverse the forces that would lead to their demise. Katz & Kahn (1966: 150ff) articulated this argument in terms of psychological open-systems theory, in which the adaptive capabilities of people enabled organizations to import more energy than they consumed for coordination and production, and thereby exhibit what they called “negentropy,” or negative entropy (which must not be confused with Weiner’s [1948] use of the same term for the degree of disorganization in a system). Being an organization by no means assures open-system negative entropy, but it does enable the possibility of it.

One of the characteristics of organizations that Katz & Kahn considered to enable negative entropy was that organizations are information processors (from Miller, 1963). It can be argued (although they did not) that this is a general property of systems as defined by General Systems Theory (von Bertalanffy, 1950), wherein the whole is greater than the sum of its parts. I agree with the theoretical view that organizations are information processors (Kmetz, 1984, 1998), and that information processing in human systems is fundamental to many system functions (Cyert & March, 1963; Galbraith, 1974; Kmetz,

2012; March, 1999). Indeed, it is precisely this property that enables systems to be self-reinforcing, since the abilities to acquire, store, transform, and transmit information underlie any form of adaptive behavior.

The basis for this may be seen in the Input-Transformation-Output (ITO) model of a system, as shown in Figure 1. Multiple interdependent processes are required for an organization to function, and all of these depend on information (Kmetz, 1998, 2012)—there are boundary-spanning processes to maintain relationships with the environment, and internal processes to transform inputs into outputs. One type of information dependency is that which is *inherent to the process*, meaning that some information is used specifically for that process. We apply technologies to transform inputs, and we apply tools and methods to monitor customers and stakeholders, etc. Organizations also depend on *regulatory* information in two generic forms: *feedback*, where some information or output is consumed to maintain control and evaluate performance against expectations—i.e., some of the work done by the system is consumed by the system for self-regulation; and *feedforward*, which establishes criteria, determines metrics, and sets targets to guide future actions and decisions, where again energy is consumed for internal purposes, not production. These are typically used interactively over time, so these are dynamically related to each other. All feedbacks and feedforwards are information, and the content of that information directly affects how it modifies organizational processes. That content in turn partially depends on the metrics we select, a process in itself; as Einstein observed, “Not everything that can be counted counts, and not everything that counts can be counted.”

Figure 1 here

System control is effected by these feedbacks and feedforwards. In the terminology of classic information theory and engineering, a positive (“hot”) feedback is destabilizing because it returns energy or information to points in a system in ways that

make one or more processes unsustainable. The classic physical example in electrical engineering is a circuit that overheats and self-destructs; Hall (1976) illustrated the effects “hot feedback” of information in the self-destruction of the old *Saturday Evening Post* magazine. Negative feedback is stabilizing, as it provides information that regulates the amplitude of processes, as with governors and speed limiters in the physical world and fact-finding processes in contentious disputes. Similar consequences apply to positive and negative feedforward. We might conceptualize these positive or negative attributes as the “charge” of the information, in terms of both its sign and its strength.

This implies that system stability requires balanced “charges” for both feedback and feedforward. At the minimum, there would have to be two equal feedbacks or feedforwards, and it is more likely there would be at least two of each. In my view, complex systems are complex because they comprise multiple, interacting feedbacks and feedforwards, not the simple individual ones shown in Figure 1. Figure 2 illustrates how the regulation of a simple process can become very complex through the interactions of different feedbacks and feedforwards, which I refer to as *multiplex regulatory information feeds*, or MRIFs:

Figure 2 here

A marketing plan (1) is produced to guide Company X in its quest to increase market share and performance of its distribution channels. Since market share and channel performance reflect the desired impact of the plan, they are the metrics (criteria) used to evaluate plan performance (2), acquired by standard measures of market share, time to market, etc. Additional feedback is obtained through our Customer Relationship Management data (3), and these unfortunately indicate that what is being done to increase share and channel performance is alienating some of our major customers. The feedback data indicate that the plan is awry in that it has targeted the wrong market segment (4), and needs to be materially changed if it is to be anything more than a short-term success. However, since the reward system metrics (5) are based on the plan performance metrics (2), there is strong pressure from those committed to the plan and doing well in terms of these metrics (and in the short term) not to change it.

While not a real company, stories like this are common and easy to imagine; similar conflicts between R&D and marketing, accounting and manufacturing, and so on, are literally textbook cases of how complexity challenges organizational functioning. (Kmetz, 2012, in press.)

Three key points are illustrated by this example of MRIFs. First, information modifies information. Information is not neutral, either in terms of what it directly conveys or with respect to its impact on processes. Second, the charges of the interacting feedback and feedforward information flows are likely to be of opposite sign but variable strength; it is therefore not assured that charged pairs automatically balance. Third, it is the net effect of these complex interactions, i.e., the MRIFs, that determine the effect on regulated processes, where that net effect is the product of the individual charges, not simply a summation.

Self-reinforcement requires both self-regulation and negative entropy. Self-regulation alone may not require high degrees of complexity (a thermostatically controlled furnace is not a very complex system), but self-reinforcement does, and negative entropy is necessary to the long-term sustainability of the system. I agree with Thompson (1967) that systems (or organizations) do not pursue goals in the same sense as sentient individuals; however those sentient individuals in a dominant coalition may very well aspire to see the system sustain specific processes and outcomes, including those that bring in new members, new money, and new legitimacy for system survival. These processes greatly reduce the probability that the system will succumb to entropy, and strongly suggest that a system with redundant MRIFs is more likely to survive.

Further, it is axiomatic that the reliability of physical systems (where “reliability” is defined as one minus the probability of failure) increases with subsystem redundancy. Redundancy means the existence of multiple subsystems with mutually supportive goals and processes, which both require and support information flows. These subsystems may be thought of as “nodes” in both the anatomical sense of specific physical structures and in the engineering sense of a connection, and they fulfill both roles. Thus, we would predict that a system comprising multiple nodes and multiplex regulatory information feeds, by

definition, has a high degree of system reliability; this, coupled with the ability to import energy, creates a highly stable, self-reinforcing system.

Self-reinforcing systems exhibit a high degree of reflexivity, where the “self-fulfilling prophecy” (Merton, 1957) is an ordinary outcome. Depending on the goals of the dominant coalition, this can be a powerful force for stability or change. This paper argues that the BSRBH is such a structure.

PART III: THE STRUCTURE OF THE BUSINESS-SCHOOL RESEARCH BLACK HOLE

Figure 3 is a graphical model of the interior structure of the BSRBH, all of which exists within the confines of the event horizon, which in this case consists of approval through peer review. The model shows all nodes and those linkages I judge to be the most important informational relationships between them; in fact, none of them are unrelated to any others, but I have attempted to identify MRIF elements I consider to be the most important in the self-reinforcement of the BSRBH. Although the model in Figure 3 is shown in two dimensions, it is a three-dimensional structure, with different substructures orbiting the interior in very much the same relationship as the nucleus and electrons in the structure of the atom.

Figure 3 here

The nodes and linkages in the interior of the BSRBH are the elements that make up the GASSSPP introduced in Part I. As noted above, the nodes are both physical bodies, in this case a body of information, and a connection between nodes. Nodes are found in four clusters bound internally by linkages of “forces,” and there are additional force linkages between the clusters, which also have properties of nodes themselves. The three inner clusters comprise the major GASSSPP elements; the outermost cluster is institutional, also

linked internally, and while not part of the GASSSPP directly, it forms a fundamental body of binding attraction itself.

The nucleus of the BSRBH is the conflation of Fisherian and Neyman-Pearson methods of statistical testing in GASSSPP research, which have anonymously become confused through many statistical textbooks (Carver, 1978; Hubbard & Armstrong, 2006; Huberty, 1993) and researcher usage; what has occurred is that the inferential p of Fisher's method has become perceived as interchangeable with the Neyman-Pearson measure of evidence against the null hypothesis, the α probability level of Type I error. A crucial distinction that has been lost in this confusion is that p is a random variable dependent on sample data; α is a pre-selected probability value, where the chosen value is determined by the level of evidence needed to reject the null hypothesis (Cohen, 1994; Hubbard & Bayardi, 2003; Hubbard & Armstrong, 2006; Hubbard & Lindsay, 2008). This conflation became the dominant Null Hypothesis Significance Testing (NHST) technique which grew explosively in psychology between 1940 and 1955 (Hubbard *et al.*, 1997; Hubbard & Ryan, 2000), and has essentially become "synonymous with empirical analysis" (Hubbard *et al.*, 1997) in b-school research, while also attaining a status in which p values have become "an end in themselves" more broadly in the social sciences (Cicchetti, 1998). NHST was imported into b-schools by the large influx of social scientists recruited to improve the quality of research following the Ford and Carnegie Foundation reports of 1959 (Gordon & Howell, 1959; Pierson, 1959), as part of the larger GASSSPP syndrome.

BSRBH Nodes and Node Clusters

The **NHST** nucleus of the BSRBH is tightly bound to four derivative myths, all of which interact with **NHST** and each other, and both shield and reinforce **NHST**. All four of these myths gain plausibility from the mistaken belief that p and α are the same and interchangeable, so much so that through practice and repetition it has become difficult to even argue against their status as myths (Nickerson, 2000). The first is **P(Rare)**, implying that p is a form of "discovered α " and reveals the rarity of an outcome. The second myth is **P(H|D)**, the belief that p reveals the likelihood that the null hypothesis has been

correctly rejected given the data, when in fact it actually expresses the likelihood of getting the observed data under conditions of the null hypothesis or $P(D|H)$; these are two fundamentally different outcomes (Abelson, 1995; Carver, 1978, 1993; Cohen, 1994) with entirely different base rates, a point not well understood by many researchers. The third myth, **Effect**, is that p is the primary source of information about the importance or materiality of a finding, equivalent to, and a substitute for statistical effect, when in fact it provides no information about effect size whatsoever. The combination of these myths support a fourth, **P(Rep)**, which is that p is the likelihood of nonreplication of an outcome, when in fact it provides no information about replication and is unrelated in any way.

This most fundamental level of errors and myths surrounding the **NHST** core is primarily based on convention and mimicry; these errors are so common and repeated so often in top-tier journals that they are no longer questioned by the large majority of researchers. There is a large methodology literature that spans many decades and encompasses scores of papers, books, conferences, websites, and other source materials that confirm the scientific dysfunctionality of these beliefs (Kmetz, 1992, 2009, 2011), but they persist. Given the large number of references on these myths, I have cited but a few of the authors who point out the errors and deficiencies associated with NHST, but over 140 citations on this subject published from 1932 to 2011 (many including abstracts) may be found at <http://sites.udel.edu/mjs/statistical-significance-references>, and readers should be aware that this list is not exhaustive of this literature. While general agreement on the application of standards, methods, and procedures is considered a foundation for a “community of practice,” what has happened in the context of GASSSPP research, however unintentional it may be, is a “community of malpractice.” Given its origins in confusion, the label I have assigned to this node cluster in the model is a kinder and gentler “*Community of Misinterpretation*.”

At the next level of the BSRBH, we find four nodes that share two common properties, one a “community of practice” quality in that it rests on shared and accepted behaviors, and the other a form of self-censorship on the part of contributors to the GASSSPP literature. The four nodes at this level are **Lack of Replication**, the near-total

absence of replication studies; **Editorial Practices** that both support the community of misinterpretation and generally reject “mere replications” despite the need for them and their fundamental role in sound scientific practice; **Dysfunctional Uniqueness**, where the GASSSPP literature seems to regard journal studies as theses and dissertations, requiring every work to be original and thereby further obstructing needed replication studies (Kmetz, 2002, 2007); and the **File Drawer**, where unknown volumes of papers with unseen findings may (and probably do) reside, but will never be made available to the research community (Rosenthal, 1979).

These interact in a manner that produces one of the best examples of several “groupthink” properties I can find anywhere (Janis, 1972). I must confess that I harbor considerable skepticism about the soundness of the groupthink concept, and I am not alone in this (Baron, 2005; Park, 1990; Whyte, 1989). However, the willingness of authors, reviewers, and editors to behave in ways that clearly contradict our own methodology literature requires at least an unconscious conspiracy if not outright collusion. The specific groupthink properties embodied in this cluster are those Janis (1972) referred to as “mindguarding” and “self-censorship,” and to some extent two others are also evident—“rationalization,” or the discounting of negative or contradictory information, and the “suppression of dissent,” where both active and passive measures are taken to inform skeptics and dissidents of their status and the inadmissibility of their evidence. Thus, despite my reservations about construct validity, I label this cluster “*Groupthink*.”

The third cluster is labeled the “*Quality Delusion*,” in which four nodes interact to support the impression that GASSSPP research is not only scientifically sound but technically superior to other forms of published information, so much so that conclusions derived from it should be given preference to other information. First, the **Illusion of Scientific Validity**, is in part inherent to any publication in the black hole, since it is largely created by being successfully peer reviewed and published. Peer review will be discussed in more detail below, but it is the “event horizon” over which a manuscript must pass, and is widely accepted as a determinant of quality. The profession has placed increasing emphasis on the two other nodes in this cluster: **SSCI Rankings**, the rankings

of papers from the Social Science Citation Index; and **Journal “Impact” Rankings** for journals themselves. The fourth node, **Accreditation**, may be attained under the auspices of a variety of accrediting organizations. Once established, accreditation implies that all work performed by the university has been approved by some external authority, lending still further support to the illusion of scientific validity, both internally and externally. It is especially useful for deflecting external criticism, since it implies that the other three nodes are also valid and rigorous.

Despite significant long-term evidence that BSR produces little useful output, either by way of strong theories or useful professional guidance, internally-focused GASSSPP MRIFs have grown into implicit standards, whereby the “impact” of BSR is closely watched and fed back into the system. Disconfirming evidence is simply ignored or disregarded, and is seldom seen. This contributes to the emission of one form of Hawking radiation from the **Illusion of Scientific Validity**, in the form of delusional particles known as Evidence-Based Management (EBM), wherein some researchers contend that BSR has actually produced enough knowledge to allow scholars to prescribe management behavior.

One Hawking EBM particle emitted by the Quality Delusion cluster is the Pfefferon, which contends that EBM is the application of scientific methods of inquiry into what managers do, and use of the results to improve performance (Pfeffer & Sutton, 2006). I completely agree with that form, and this is a recommendation that is as valid as when it was first made by Frederick W. Taylor (1911) a century ago, and later by Walter Shewhart (1931) and William Deming (1986). The other EBM particle is the Rynes-Rousseau which is actually a closely-bound pair differentiated by the focus of the Rynes particle on human resource management issues (Rynes, 2007), and the Rousseau particle on more general management (Rousseau & McCarthy, 2007). Neither has sufficient charge to attract interest beyond academia, and neither can demonstrate more than one or two component subparticles; Locke’s goal-setting (Latham & Locke, 1979) seems to be the single common underlying subparticle, with little or no agreement on how many others may exist or what they are.

These two intermediate clusters, *Groupthink* and the *Quality Delusion*, orbit as groups above the *Community of Misinterpretation* cluster. Above these two is the third and final energy “shell” inside the BSRBH, the *Reward System*, consisting of three nodes: **Personal Identification, Career Advancement, and Professional Recognition**, such as election to professional office in an academic organization. These are self-explanatory, and all share the common properties of being part of the formal academic reward system while at the same time binding those who participate in the GASSSPP literature to the system. Research has become dysfunctional in many U.S. b-schools (Bennis & O’Toole, 2005), and it has long been noted that promotion in the social sciences depends on the accumulation of published “hits” in the academic journals (Campbell, 1967). There is an abundance of psychological literature to support the relationship between one’s ego, or sense of self-esteem, and accomplishment in one’s chosen work; that applies in the realm of GASSSPP research as in any other line of employment. Given the entrenched flaws in the GASSSPP, advancement without conformity to its practices is effectively impossible, despite their unscientific properties; promotion based on that work becomes identified with the individual, who may even be elected to office in a professional organization on that basis. If so, that enables the individual to recommend against GASSSPP practices on the basis of “idiosyncrasy credits” that accrue to leaders (Hollander, 2006), but at the cost of a career of having engaged in practices one now condemns.

Thus, this shell is a significant source of Hawking radiation, in the form of official addresses from those elected to professional office eschewing the very practices they often followed to attain that level of recognition. I refer to these as “hairshirt” particles in recognition of the tendency for those who make these speeches to don the hair shirt of outward repentance. This is very common in the Academy of Management, where I have personally witnessed and read such presidential addresses for nearly the past 40 years, and unfortunately has lately become more common in the Academy of International Business. Much the same applies to the American Psychological Association’s Task Force on Statistical Inference (Wilkinson, 1999), which has had little discernible impact on publishing practices in psychology. The ultimate message to junior faculty researchers is to disregard these “calls to action” as disingenuous, and continue to publish following the

GASSSPP practices found in the interior of the BSRBH. Obviously, none of these pronouncements have had any effect on the core of the BSRBH, which goes on with seemingly more energy each year, and which has resulted in a half-century of misdirected and wasteful research effort (Kmetz, 2011).

BSHBR Linkages

The four linkages between **NHST** within the *Community of Misinterpretation* cluster are self-evident—the confusion over p and α directly reinforces **P(Rare)** and **P(H|D)**, and decreases **P(Rep)** and **Effect**. **NHST** and these four myths thus far provide five nodes in the BSRBH model, but the number of feedbacks and feedforwards is greater than that. Again, I have shown only those relationships that I judge to be the most important of all the potential linkages (what might be thought of as the “strong forces”) to avoid unnecessary clutter in Figure 3.

For example, the one linkage shown between **P(Rare)** and **P(Rep)** is the suppression of replication studies, based on the erroneous belief that nonreplication of a result is unlikely given a low p value. However, it is quite straightforward to envision the reinforcing feedforward of the lack of replication studies on **P(Rare)**, which allows the appearance that published research is correct and has established a “fact baseline” which other research must both acknowledge as truth and account for in any attempt to advance a field of study. Replications are in fact virtually non-existent—I found only 68 nominal replications in a sample of 13,161 articles from 20 management subdisciplines, or 0.51 percent; economics fared even worse, with only 51 replications found in 28,571 papers, or 0.18 percent (Kmetz, 1998: 75). Replication studies in marketing are very rare—Hubbard & Armstrong (1994) found none in a sample of 1,120 papers from three marketing journals, and only 20 extensions. Of those 20 extensions, 12 conflicted with the original studies and only 3 fully supported the original work, but non-supporting papers appear to be largely ignored and are rarely cited. They also found replications in marketing to be in decline since the 1970s, a major and continuing departure from valid science.

In the case of **Effect**, a common error among researchers is to disregard effect sizes and base their decisions entirely on statistical significance (many researchers report only p levels for their hypotheses; in several recent presentations I have attended, researchers had not even prepared slides to show effect sizes when they were asked for them). In one recent presentation and in two recent A-level journal articles, all using regression modeling with large samples, statistically significant b values of .03 and .04 were claimed to support their associated hypotheses—literally, evidence that there is no relationship between the X variable and the dependent variable was ignored because p was .05 or lower in each case (I have not given references to avoid embarrassing the authors)!

The *Groupthink* cluster contains several mutually-reinforcing linkages, particularly between **Editorial Practice**, **Lack of Replication** and **P(Rep)**, which is unsurprising and suppresses replication, as we have seen above; and between **Dysfunctional Uniqueness**, **Lack of Replication**, and **Editorial Practice**, which maintains the myth that the published literature is not only accurate but the best research available. The **File Drawer** binds to **Editorial Practice** through self-censorship to further prevent contrarian research from being published, and **Dysfunctional Uniqueness** similarly prevents replication studies indirectly. The linkage between **Dysfunctional Uniqueness** and **P(H|D)** also prevents the comparison of results. Thus, the combined effects of these forces shields GASSSPP publications from exposure to disconfirming information from nearly every quarter. Both the **File Drawer** and **Dysfunctional Uniqueness** bind *Groupthink* to the *Quality Delusion* via their linkage to the **Illusion of Scientific Validity**, and the latter, as we have seen, is maintained by **SSCI Rankings** and **Journal “Impact” Rankings**. The **Illusion of Scientific Validity** has strong mutually reinforcing feedbacks from both **SSCI** and **Journal “Impact” Rankings**. Given the apparent rigor of statistical methods and the interpretation of technical and sometimes arcane results from them, the NHST cluster provides powerful support for the **Illusion of Scientific Validity**, which is given further support by the strong force between it and peer review.

The nodes in topmost shell of the BSHBR, **Personal Identification**, **Career Advancement**, and **Professional Recognition**, are all strongly related to each other and to

the cumulative force created by the two intermediate force shells and the NHST core. These forces are both specific to the individual nodes and exert a general binding force analogous to gravity. Unlike physical gravity, these forces bind with the inner shells and core of the BSHBR to prevent explosion of the inner structure through unintended scientific validity; at the same time, they bind the event horizon, **Peer Review**, to the inner BSRBH. It is the latter phenomenon which remains to be discussed.

Peer Review is considered the *sine qua non* of academic research quality, but that status is difficult to justify on the basis of evidence. The biomedical sciences have been particularly concerned with the effects and effectiveness of peer review, given the potential for grave consequences that may obtain to error in their work (Bachetti, 2002; Ioannidis, 2005; Jefferson *et al.*, 2002a, 2002b, 2006; Wager & Jefferson, 2001). In a special issue of the *Behavioral and Brain Sciences*, Peters & Ceci (1982) found that of 12 psychology articles submitted to the same journals that had already published them 18 to 32 months earlier, only three of the 12 were detected, and of the remaining nine papers, eight were rejected for “serious errors.” This finding generated considerable commentary and many questions about the presumed benefits of peer review in that issue of the journal, but in the ways typical of GASSSPP research, no discernible calls for change have resulted in the past 30 years (except perhaps that this author was denied human-subjects clearance to do a replication study owing to the requirement that previously published works would need to be fraudulently resubmitted). Indeed, the very “blindness” of peer review was called into question (Ceci & Peter, 1984). Most investigations of peer review find that it has unclear objectives, is not measured or evaluated, and is at best inconsistent (Fiske & Fogg, 1990).

Even on the basis of citation criteria and journal rankings purely internal to the GASSSPP, SSCI article rankings and journal rankings are loosely correlated at best (Rothwell & Martin, 2000; Seglen, 1997; Singh *et al.*, 2007; Starbuck, 2005). Starbuck recounts the story of a colleague who had an article rejected by the *Academy of Management Journal* who then rewrote it to specifically address reviewer concerns and in the end won a “best paper” award for an article he acknowledged he did not believe himself. That and my own experience lead me to believe that peer review may play as

important a role in the distortion of research papers as an assurance of quality (Tsang & Frey, 2007), and that review should not be serial editing and reconstruction.

The GASSSPP research itself makes the strongest case against the assumption that peer review assures quality. In my own non-exhaustive database on NHST, I presently have over 140 references published between 1931 and 2011, the sum of which constitutes a clear, powerful case against the misuse and misinterpretation of p values in the GASSSPP literature and the social and biomedical sciences in general. Yet peer review has neither been effective in detecting this obvious problem and preventing it from becoming pervasive in our literature, nor spearheading measures to correct the severe damage to scientific validity resulting from the GASSSPP. Bachetti (2002) raises similar concerns about the medical journals, and believes that half of the reviewers in them do not know statistics sufficiently to review empirical research. Bedeians's (2004) assertion that peer review is really a mechanism for the "social construction" of knowledge is a much better description of its role than claims for its assurance of research quality, with the exception that the GASSSPP collectively produces disinformation, not knowledge.

Of greater concern is Bedeian *et al.*'s (2010) finding of widespread evidence of research misconduct among 104 PhD-granting US universities (all AACSB accredited). What I find genuinely alarming is the extent to which questionable research practices reflected in items 1 – 4 and item 7 have been "observed"—these raise questions about the very basis of the objectivity underlying the derivation of hypotheses and the collection and reporting of relevant data. Over one of every four respondents to this survey report knowing of someone who simply fabricated results. Well over half of respondents engaged in other Category II practices that may assure publication but do so through means not related to the strength of their findings. It is one thing to have widespread errors in the interpretation of outcomes, as is now the case, but these findings raise fundamental questions about not just the validity of GASSSPP research but the integrity of those who do it. The reward-system pressures on researchers to generate publications in the face of GASSSPP practices that inhibit science makes the existence of these practices both shocking and greatly worrisome. While proven instances of outright fraud are rare in

the social sciences, the extent of the malpractice which Bedeian *et al.* report raises the specter of the profession being widely discredited. I have reproduced Table 1 in its entirety in the attachments.

Table 1 here

What is more, if peer review is to be considered an assurance of quality, it is problematic to find that this extent of research misconduct can go on and apparently remain undetected. Since nearly the entire population of authors, reviewers, and editors of the GASSSPP literature come from the same educational background, to some extent we may need to make allowances for the failure of peer review to assure quality. As Hubbard and Bayari (2003) observed with respect to the core NHST problem, “So complete is this misunderstanding over measures of evidence [p] versus error [α] that is not viewed as even being a problem among the vast majority of researchers.” What I personally find most troubling is that this state of affairs coexists with efforts on the part of many researchers cited throughout this paper to educate the profession, while the profession goes about its business as if these issues do not exist. The documentation of statistical and scientific deficiencies in the GASSSPP literature has grown over recent decades, not declined, and yet the GASSSPP goes on unperturbed.

Peer review is at best an inexact social process, and in the era of the Internet and a greatly expanded global b-school professorate, a far better approach to evaluate research is to subject it to open review (Armstrong, 2002) and especially to replication studies. The absence of replication noted above is not only one of the greater violations of sound scientific practice, being fundamental to the procedures of “normal science” (Kuhn, 1962), but can play a fundamental role in the review of research, as it does in the real sciences.

One final comment on the structure of the BSRBH is in order—given that several forms of Hawking radiation have been identified, why does the BSRBH not “evaporate” over time? Part of the answer to this is the extremely strong force of the NHST core,

which has now had over half a century to become accepted practice in a community which has many more members than before; however, a rarely seen form of academic “dark energy” is the attraction of antiKuhn particles into the core. AntiKuhn particles are the unjustified but sometimes articulated belief that Kuhn’s (1962) seminal discussion of scientific revolutions, which compared “normal science” to work leading to paradigm shifts, implies that science comes in multiple varieties and that GASSSPP is merely a different form of science, equally as valid as any other. Having read Kuhn multiple times and indexed it for purposes of specifically investigating this claim, what becomes evident is that those who make these statements have never read Kuhn, a problem not unseen elsewhere in the GASSSPP literature (Wright & Armstrong, 2008). Kuhn does not draw on the social sciences to illustrate the structure of scientific revolutions, and his only discussion of the social sciences comes in his introduction, where he points out that social scientists were still debating issues long resolved elsewhere in science. In the context of the GASSSPP, antiKuhn particles are seldom seen but always pose a barrier to questioning our methods—if such an esteemed and respected scholar can be cited as having approved of them, why do our methods even need justification, let alone change?

CONCLUDING REMARKS

Obviously, I hope, I have written this paper with one eye toward finding humor in a matter of some seriousness. I wish I could say this were completely a parody, but it is not. As a young scholar starting out years ago, I would have argued passionately that by 2012, the problems in the basic social-science model adopted by U.S. business schools in the 1960s would have been identified and long since resolved. Not only has this not happened, but the fundamental problems have become even more entrenched and pervasive over time. I know that some readers will take offense at some of my statements here, but my objective is not to offend—it is to point out the need for change lest our profession lose all credibility.

I admit to focusing on the bad news about our research, but that news is so bad that the small forward progress we can claim can be completely undone by it in a few

heartbeats. On 27 January, 2012, President Barack Obama made a speech at the University of Michigan in which he pointed out the unsustainability of rising college costs in the U.S., a theme he has returned to several times since (Speech, 2012). Five years ago, AACSB International estimated that U.S. universities were spending nearly USD 330 million annually (AACSB, 2007), a figure that has continued to grow at a rate much greater than consumer inflation, and conservatively is now probably at least USD 500 million. Put simply, BSR is now a multibillion-dollar industry which has little or nothing to show for its work, either in the form of strong theory or guidance for practice. In the face of public deficits in most of the industrialized world, this cannot continue.

Universities around the world are under financial pressure, and the b-school professorate is likely to be called upon to justify the costs of its work and the contributions those costs produce. U.S. b-schools cannot. To the extent that the American model of charging tuition and fees spreads to the rest of the world, these costs will be under increasing scrutiny, and the inability to demonstrate valid and useful results calls the entire mission of the business school into question.

No study supports the value of GASSSPP research except as a means to build personal scholarly reputation (and thereby further increase the cost of its production). When I referred above to a matter of “some seriousness” I meant that in two ways, the other being that for the most part, I do not take our own research literature seriously. For the public at large, the professorate has probably been given a great deal of latitude in being perceived as doing esoteric work that most do not understand, and so they pay it little attention. As long as we could afford that luxury, no one asked what they were receiving in return for the tax and tuition money that supports the research enterprise. I strongly suggest that time is over.

But my final concern is the greatest. We must act to establish scientific credibility—if we do not we will either be declared irrelevant, if we are lucky, or frauds if we are not. The scientific and statistical training we have is exactly the basis for doing valid research and correcting our problems; our professional organizations (us, in other

words) can play a critical role to set standards that rectify and expunge misinterpretations of research outcomes, to use peer review to its best advantage, to not only allow but encourage replication and vigorous re-examination of results, to encourage the universities that employ us and to exploit the power of modern information technology and the internet to make all results of research available to the community. The Business School Research Black Hole should be made to disappear. The power to do that is ours, as is the choice.

Endnote

* This quotation comes from the context of a paper discussing BSR that followed the 1959 Ford and Carnegie Foundation reports discussed in Part III. A colleague objected to my characterization of the Mol and Birkinshaw book, which cites 26 research articles (6 from GASSSPP journals) in enumerating 50 “giant steps” in management. Many of these articles are business history (including two papers in the *Academy of Management Review*, which some contend is not a GASSSPP journal since it primarily publishes non-empirical papers), but the only one that really appears to be a “big idea” is a *Journal of Marketing* article on market segmentation (Smith, 1956). I excluded this because it is not only not a GASSSPP paper, but also because it predates the post-1959 era of “scientific” BSR. Without splitting hairs, it is clear the impact of academic-journal BSR is minimal, if it exists at all, even in marketing (Armstrong, 2002).

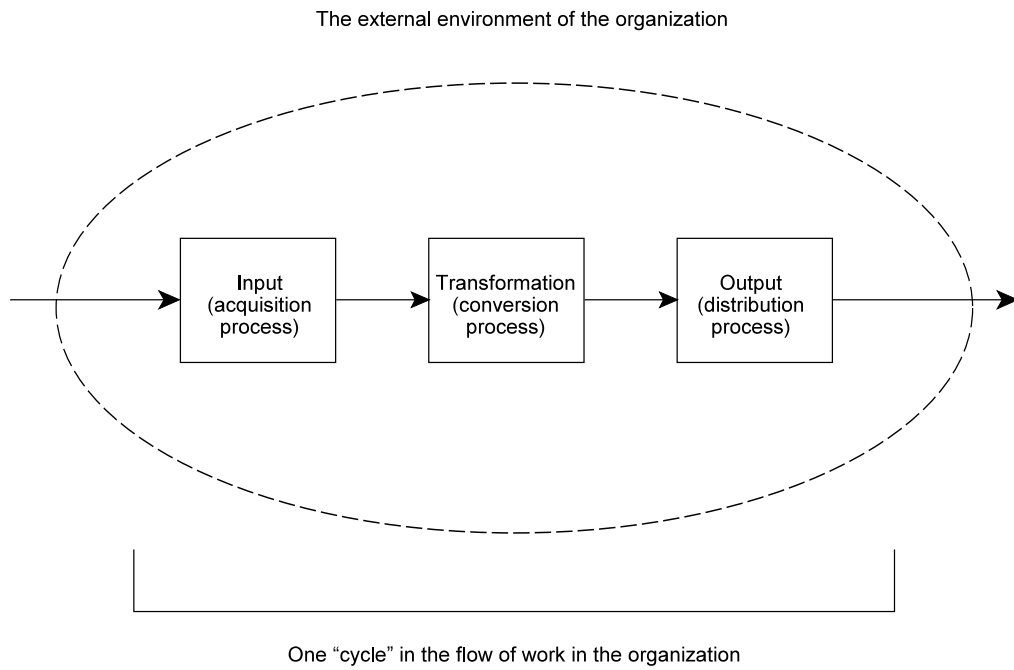


Figure 1. The basic Input-Transformation-Output model.

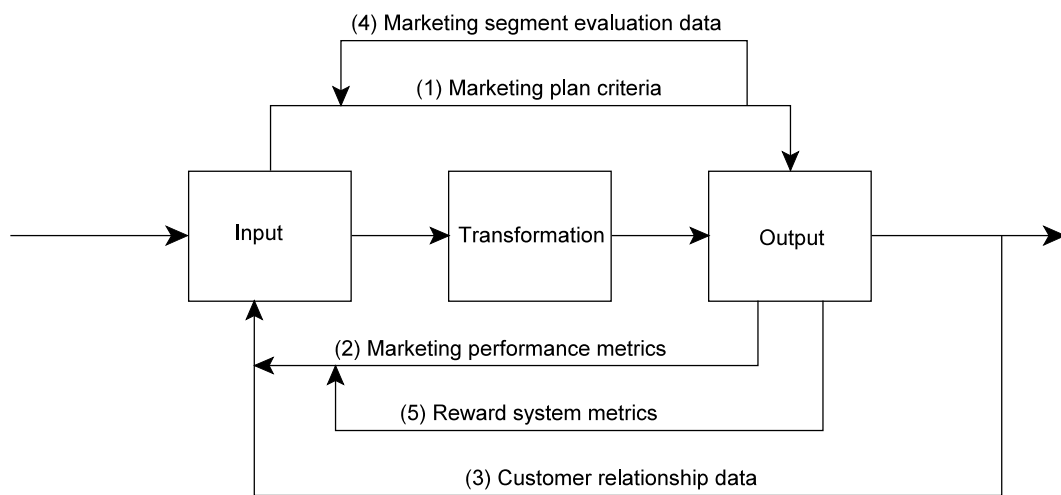


Figure 2. Multiplex regulatory information feeds (MRIFs)

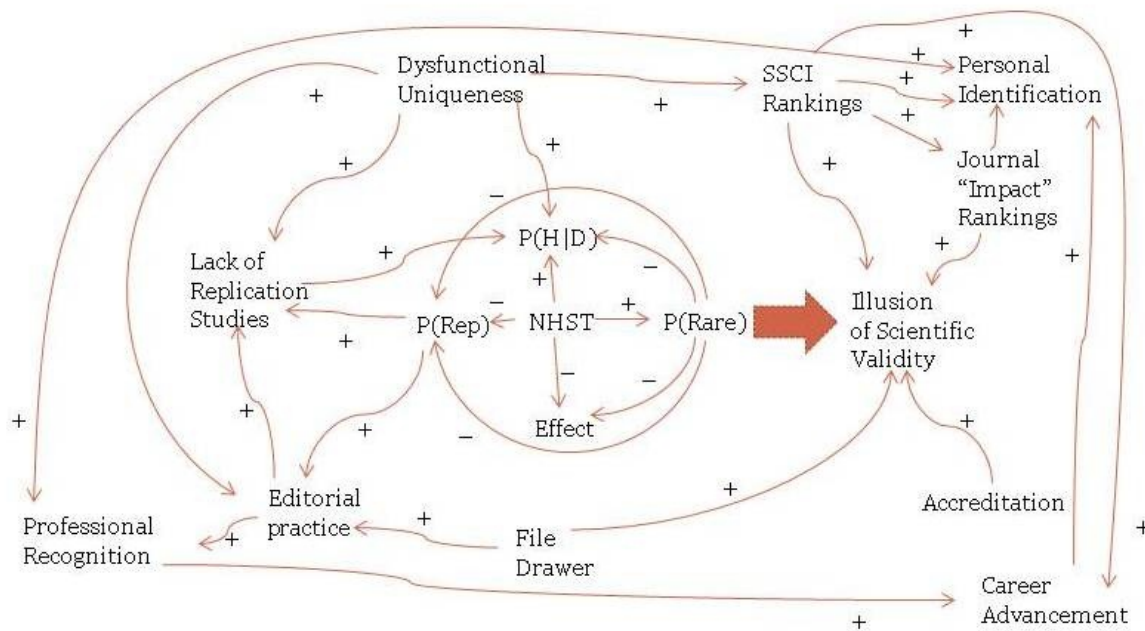


Figure 3. The interior structure of the Business-School Research Black Hole

TABLE 1
Percentage of Management Faculty Who Reported Knowledge of Faculty Engaging in the Listed Behavior Within the Previous Year

Behavior	All	Tenured	Nontenured
<i>Category I – Fabrication, Falsification, and Plagiarism</i>			
1. Withheld methodological details or results	79.2	79.7	78.4
2. Selected only those data that support a hypothesis and withheld the rest	77.6	77.9	77.1
3. Used another’s ideas without permission or giving due credit	72.1	75.3	67.3
4. Dropped observations or data points from analyses based on a gut feeling that they were inaccurate	59.6	62.3	55.6
5. Withheld data that contradicted their previous research	49.5	50.6	47.7
6. Fabricated results	26.8	26.4	27.5
<i>Category II – Questionable Research Practices</i>			
7. Developed hypotheses after results were known	91.9	92.2	91.5
8. Published the same data or results in two or more publications	86.2	88.7	82.4
9. Developed “ins” with journal editors	83.3	82.3	85.0
10. Inappropriately accepted or assigned authorship credit	78.9	82.3	73.9
11. Circumvented aspects of human-subjects requirements	58.1	61.9	52.3

Note. $n = 384$. Significance of X^2 tests of differences between tenured and nontenured faculty $p > .05$ in all cases. Items 3, 4, adapted from Martinson, Anderson, & de Vries (2005). Used with permission.

Source: Bedeian *et al.*, 2010.

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