

**Perspectives & Speculations On Knowing:  
And, Perhaps, A Basis For A Philosophy Of Research**

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## Abstract

# **Perspectives & Speculations On Knowing: And, Perhaps, A Basis For A Philosophy Of Research**

This paper provides perspectives and speculations on research intended to stimulate cognitive conflict and debate of issues important to the conduct and dissemination of research. The paper offers a lighthearted but thought provoking background, identifies issues of import in research, and addresses current perceived maladies in research and in the dissemination of research. The authors suggest important criteria for broadening the research value chain and offer a prescription useful in developing a research philosophy. They take aim at the role of “gate keepers” who inhibit the dissemination of research. In balance, however, they recognize the critical role of leaders in each field who seek to nurture and foster the research efforts of others and who make research efforts available and transparent – even if such efforts are at variance with their own notion of significance.

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# **Perspectives & Speculations On Knowing: And, Perhaps, A Basis For A Philosophy Of Research**

## **I. Introduction**

This paper offers perspectives and speculations on research. We depart from the normal prescription for writing which calls for clearly identifying the purpose at the beginning of a paper. Instead, we first seek to stimulate thoughts and foster debate. The notion that individual readers have insights, perspectives, and ideas in excess and of potential greater merits than our own prompts this tact. With this approach, we ask the reader to proceed without the necessity of a detailed road map connecting the dots. Along the way we seek to stimulate, elicit thoughts, and offer catalyst to the process of exploring the important topic of research and a philosophy for research – and eventually harvest from the minds of others.

We achieve our objectives to the extent we provoke thoughts and debate, and unearth ideas and perspectives about the philosophy of research. Other dimensions of interest include the attitude, motivation, nature, choice, preparation, design, method, execution, harvest, interpretation, scrutiny and implications that affect choices, strategy and execution of research, and it's "results."

We suggest that in many fields research suffers from maladies that may have profound implications in terms of the time-success of the efforts of people to extend the library of knowledge. Delaying knowledge accretion has potential great costs. Those costs may include esoteric but important costs such as impairment of progress towards understanding. As well, delays in knowledge accumulation and derivative application may adversely affect the availability and access of benefits to individuals. Examples: a more effective treatment of a disease, lower capital costs to a country, a more abundant food supply available to more people, a scheme of understanding that replaces bloodshed with peace and opportunity, or a teaching approach resulting in a more inclusive, effective classroom.

The new millennium is a perfect time to reflect on certain "aspects of research." We do not seek consensus. Our efforts will be successful to the extent we prompt cognitive conflict and unearth new ideas for intercourse and debate, scrutiny, and possible nurturing.

When we think we have approached the limit of your patience, we will move the discussion to several loci of import. Likewise, we will expose you to precipitates of our

thoughts, giving attention to issues of potential import in a perspective on the philosophy of research. Do not view our sharing as an attempt at closure on these important issues. Instead, we seek to lure from you fresh ideas and factors that contribute to our exploration of an important topic: a philosophy to guide research in the millennium.

## II. Adding To Perspectives, Possibly

### Issues In Research

A brief review of some factors related to research and philosophy will support our effort.

**Base.** The support or base for a research thrust varies. Some efforts rely on “current islands,” bedrock, or benchmarks of “understanding.” These efforts are born of existing and potential data sets given recognized and heretofore unrecognized variables, current “knowledge sets,” “accepted concepts and relationships,” existing theories, and on-hand models. At the other extreme, other research germinates in arenas extant to such paradigms, e.g., relativistic and quantum physics vs. Newtonian physics, or super luminal vs. luminal limit physics. From a practical and philosophical point, the differences are such as to make the extremes not comparable. One approach involves somehow walking from an existing point of references. The other requires and benefits from a different point of perspective and alternative relationships that might characterize the nature and functioning of existence and non-existence.<sup>1</sup>

**Forms.** At the rudimentary level, research may be deliberate and focused, casual, or even accidental. Characterizations of research customarily include basic, exploratory, theoretical or applied. Research methods, design, execution, analysis and interpretation take various forms.

**Approaches.** Approaches range from casual observation to ardent empirical. Efforts are passive or active. With a passive approach, we harvest observations of some sort. With an active approach, the researcher attempts to elicit data or seeks to stimulate and force response from the subject or “environment.” Approaches also range from targeted or rifle (I know what I am hunting and will fire for effect when I think I see it), to broad array or “shotgun” (I think I heard a noise somewhere in the bushes. Blast, and let’s see what I can find.)

**Techniques and paths.** A range of avenues for the research effort exists. Some approaches benefit from an inductive process, others deductive. Other “paths” seek to allow environment and circumstance for discovery, without forcing a scheme on an unknown underlying process. Others allow themselves to play or tinker, moving about and through things they do not understand in hopes of finding something of interest.

**Mind magic.** Some other paths include “looking inward;” the approach for doing so varies by person and circumstance – and perhaps seeming strange or unorthodox. Combinations of paths such as looking inward and freeform exploration seem to have worked

well for some people – illustrated well (both in unusual approach and output) by Thomas A. Edison or his contemporary and competitor, Nikola Tesla. To be sure, in many instances mind magic benefits from previous and current paths.

Still other research prospers from the magic of insight, the recognition of importance of the overlooked obvious, or the assemblage of parts to perspective, to insight, to understanding, and sometimes to implication and application.

**Imagination.** Imagination seems to play an important role in “defeating habit by originality.” In rough paraphrase, Einstein reported that fantasy and imagination played a far more important role in his explorations and eventual discoveries than great mathematical skill.

**Vision.** Suggesting the role of vision in research has risk, especially for those who believe in efficient markets or some form of random motion. Fortunately, “vision” has proven a powerful source of ideas and motivation. The “fantasy” of Jules Verne may well be the ultimate propellant of endeavors of the mind. Should you have the chance to witness the glow of a reactor core, you will then find it difficult to read Jules Verne and ignore such “powers.”

**Motivations.** Motivations for research span the gamut. Curiosity about why the apple falls and the characterization of attendant relationships, why sugar crystals under friction emit light, what causes polio, ulcers, ... how one can make money, and an array of other forces contribute to motivations.

Great need and desire, criticality of situation, and incredible “costs of absence” provide forces that provoke emotions and human talent and spawn research. A young man successfully wanted to court a woman having a passion for ice cream. By the time he rowed across the lake the ice cream melted. Some combination of love, desire, and perhaps even lust prompted Ole Evinrude to proceed from need, to alternatives, to possibility, to bridging the gap between theory and application. Talk about an effective “search engine!”

The “cost of absence” or non-current existence of a solution, and the failure to accept things as they are, can drive desire and action. A young scientist and his wife experience the joy of a new baby. A few days later, the parents discovered their daughter has five weeks to live – unless the father can figure out a way to drain accumulating fluids away from the skull and prevent the crushing of her brain. The resulting shunt valve saved the couple’s baby and the lives of countless others.

**Discovery.** Discovery results from positive actions and explorations and, as well, “accidents.” Such accidents sometimes derive from incredible preparation and work to provide the opportunity for accident. In contrast, other accidents arise from chance of circumstance. Research leading to discovery takes on many different forms and approaches. Research “efforts” also benefit or suffer from the motivations of researchers, the direction taken, the nature of the process, and the attitudes brought to bear on the task and, as well, the influence of attitudes in the interpretations of the “results.”

**Productivity.** The productivity of research varies for an array of reasons we will address on another occasion.

**Benefits.** The benefits of a research effort vary in terms of current relevance in time and importance in the content of circumstance. Some research adds to the base of knowledge. Other seeks to increase confidence of prior notions and theories.<sup>2</sup> Some well executed and arduous research provides no “answers” in the classical sense, but provides other advantages – perhaps letting us realize “what is not, where no fruit currently exists.” The output of some research often does not enjoy relevance on the time line of interest and importance.

**Yield, of product.** You know our children are protected from polio. You should also know, Evinrude solved the problem with a motor so he could get his boat with the ice cream across the lake before it melted. He married that young woman – and provided for a new industry(s). And, for interest, we have alternating current in the U.S. because Edison lost and Tesla won that debate. The young couple’s baby survived and enjoyed a normal life, as have so many others who benefited from the shunt valve that drains excess fluid from the cranial cavity.

**Yield, of seed.** Successful as well as “failed” research efforts can provide promise for future bounty. Other efforts yield questions that prompt future inquiry. Rather than bring immediate fruit, the output provides seeds to stoke curiosity, suggest paths, and provoke new efforts.

**Time of harvest.** Heike Kamerlingh Onnes, whose “laboratory motto” was “Through measurement to knowledge,” discovered superconductivity as a “by-product” of his pursuit of explorations of the behavior of things at very low temperatures – published in 1911.<sup>3</sup> Charles Goodyear discovered vulcanization, but died in poverty. Few had an interest in toys and jewelry made of vulcanized rubber. The time for nurturing and harvest is not always in season with the fall of seed.

**Dissemination.** Factors and forces affecting the disclosure and sharing of research results are many. These forces or propellants of dissemination fall into broad categories of:

- ⟨ Originator – The origin of the research efforts and results.
- ⟨ Non-human Environmental – Everything influencing the originator.
- ⟨ Non-originator – A participant(s) in the environment not part of the research effort.

Dissemination is a function of the behavior and relative forces within these categories of participants and non-human environmental forces. Active, passive, and neutral attitudes, barriers and behavior play an important part in the extent and speed of dissemination – as well as acceptance and potential application. Barbers continued to bleed people for many years before acceptance and change in behavior based on new models for treating the sick and injured. In modern days, the decision of a leading medical journal to release or not release – prior to official publication of a paper – the results of a study that may have immediate effects on the health or life of people represents an example of the interplay of dissemination forces at work.

### III. Background

## **Literature**

The standard recipe for journals calls for a literature review, usually with a sentence or two summarizing months or years of work of others. We offer none. In respect of many contributions as well as personal beliefs, we will not in a few paragraphs “summarize the research of others.” Instead, we recognize and thank all those who have provided the “knowledge base of today.”

We also confess that having a knowledge base is – at least for us – different than understanding. We must admit that many other experts have provided more erudite offerings on philosophy and on research philosophy than we currently provide. Yet, it is the nature of inquiring minds to allow exploration of topics and subjects – even absent great knowledge. We are somewhat motivated by the sense that we have the most to learn by exploring areas and topics of which we know relatively little. Second, an abundance of knowledge on our part will not prove a barrier to our thoughts. For us, our prior knowledge of the topic offers the chance for a quantum leap – rather than incremental nudging of the borders of mind.

We do offer a light-hearted background, reverting to a long time ago, someplace on this planet, perhaps not far away, when equity capital had a different meaning than it usually enjoys today.

## **Early Times**

Research of various forms has provided hallmarks on the time line of recorded history for people. The impact of basic and empirical research has both instigated and contributed to changes that have altered the “ways of life” as well as the span of life itself.

Either by accident, or as a result of the development of a new level of the brain that allowed the magic of “I wonder if,” a distant ancestor found that a stick allowed her to fight beast with some degree of remoteness. The beast had a negative alpha day. The mother and her child gorged themselves and then fed for several days on the residuals.

At that moment, the next day, or hundreds – or thousands – of years later, research and empirical testing confirmed that a sharp stick was more effective in the battle. Perhaps more empiricists – and their children – survived since they made and used the sharp stick. As important as empiricism is, we find it difficult to discount the role of the original idea. The “explanatory power,” “goodness of fit,” “robustness,” and other key indicators of success in research does not stem from the empirical effort – we hope – but rather trace their roots to underlying relationship.

These early times might explain the near absence of economists. We rather suspect that simply assuming a sharp stick in a non-tax world simply resulted in a shortage of economists – the risk tilted further since few beasts of the time understood or perhaps did not appreciate the notion of equilibrium.

Ideas and *successful* empirical tests had incredible value. On the other hand, if one did not like the outcome, one could not call on a different distortion for the test or somehow explain away the absence of significance.

### **Early Research Methods & Tools**

Recognition of the concept of alternatives, the “somehow” integration of other elements of possibility, eventually lead to heavy stones affixed to the end of stick, offering increased leverage of battle through transfer of momentum. Measures of success recorded by the powers of observation prompted larger sticks and bigger stones. Of course, building momentum in the now massive clubs took great energy and size.

Some lacked physical size, power, or endurance. Some – perhaps a woman of smaller stature who sought food for her child – could not call upon brute force to achieve purpose. Her need was great. It was not practical (nor risk prudent) to accept current “wisdom” of the methods and instruments of battle. She had hunted and fed on all the smaller creatures within her reach. Only the big ones remained. She did not have to run the test to know the outcome. If she died, her child would die.

Her circumstance and some very special characteristics prompted her to consider the alternative. She sought a new idea – thus recognizing and exploring the idea that mind capital had more to offer than her physical capital. She experimented with a sharp stick, or a stick point hardened in the fire, or found a slender sharp rock to affix to the end of the stick. She sought the beast that was both a threat as well as a source of expected return.

Test complete, and significant, in spite of having a sample size of only one. She could not have successfully fought two at once. She fed her child with the flesh – and more importantly, showed her child the power of cause and effect. Correlations in the battle were of little use. Correlations from observations did offer the chance to think about possible causes and effects.

Her work represented approach from force to the application of idea. Unknown to her, many thousands of years later those who existed because of her cleverness in time would offer things called IPOs – in organizations whose value found its roots in mind capital.

We might imagine that correlations may have helped support another hunter. Having survived many battles because of size, the large man one-day noticed that point of application and depth of penetration seemed to alter the force needed and success of battle. Since he had aged and at twenty-one was past his physical prime, leverage of application took on special import. He extended his life expectancy as a result of added knowledge and understanding that stemmed from empirical work with relatively large sample size. Even though the beast had but one tail – and was not normally or even Chi-square distributed – the outcome of the test was important.

Basic research as well as empirical tests altered outcomes and the risk-return relationships with attendant changes in the price of personal equity capital. Those who managed

mind and physical equity capital well enjoyed a greater chance of survival (we guess they had favorable *ex post* risk-return outcomes, or perhaps positive alphas.)

## **A Long Time Later**

Oppression and extinction of efforts to extend the range of knowledge and potential understanding were not without risk. Research could and often did put personal equity at risk. The visions of some extended beyond accepted “wisdom.” Ideas that found no references in the library or support by the gatekeepers “on and of wisdom” offered alternatives. To pursue the idea in concept and empirical efforts caused spikes in the cost of personal equity. Originality and entertaining possibility could and did prompt the powers of church, and state, and individuals of power to reject such basic and applied research.

Still, the motivations were so strong that people persisted – even in the face of great obstacles.

Philosophers, scientists, commentators -- and possibly barflies -- have alluded to various motivations for research. We will avoid paraphrasing and providing a cursory outline of the many significant and often erudite offerings that have addressed the importance, characteristics, and other factors related to research. Starting many thousands of years ago, people far more clever than us began their contributions, those contributions taking various forms including the spoken and later printed word. Much of what they discovered brought about changes in fact, in circumstance, and in the “knowledge base” to support the future.

We recognize and admit that the paths and contributions of so many provide what we enjoy today – and both stimulate and support the theme of this paper. Many dedicated their entire life to particular suits. Others, literally, gave their life.

Madame Curie earned her Nobel Prizes for her incredible efforts and discoveries that brought quantum change in the base of knowledge. Too, the very experiments that brought her knowledge took her life.

Still others dared to challenge the wisdom of the day with the promise of incremental or even quantum knowledge and the prospect of understanding more, rather than preserving the shadows of ignorance. On too frequent an occasion, the gatekeepers of “wisdom of the time” rewarded them by removing their eyes, or taking out their tongue, or burning them at the stake.

It would seem – on a relative scale – debates about how to estimate the cost of equity capital to the second decimal point somehow falls short of the goals and spirit of other efforts. Yet, the cost of capital is critically important, playing an important role in economies and in the potential fulfillment of human need.

The issue of import in research is not one of topic, but of substance. Understanding the social factors that result in crime and take the life of a six-year-old child is at least as important as knowing how to extend the biological basis for life. Understanding – in addition to knowing – how to provide for peaceful co-existence across wide cultural gaps and a history of violence against each other seems quite important.

Similarly, improving the economic efficiency of an economy so more can survive the trials of life, feed their child, and afford housing have importance. Avoiding the massacre of

conflict or the bullet of war provides little solace if your child dies that night from hunger – or survives that and many nights – but remains crippled from polio because she missed the few drops of liquid representing the precipitate of incredible research. Basic and applied research both have place.

### **Other Moments In Time**

Observation and empiricism had roles long ago and more recently – but not one at the expense of the other. Thousands of years ago in an all equity venture, a woman rose from the perimeter of the night fire. She could not reach her stone for defense. Instead, she grasped a stick with fire to fend off the beast. The vision and range of observation now extended far beyond the circle of firelight. Importantly, she recognized that trying things differently provided opportunity. Her empirical work affirmed her conclusions. Observation provided for light of mind – and the discovery that portable light of fire protected from the assault of the beast. The retained earnings of that instant added to and provided for her continuance of human equity. On the grand balance sheet, the retained earnings account reflects the accumulation of knowledge and experience.

A few centuries ago “wisdom” dictated certain prescriptions of how things had to be rather than how they were. A young man studied the expanse of night above him – seeing better by moving to darkness and escaping the firelight. He dared to entertain visions counter to the “learned” of the time. His case study was quite remarkable as well as viewed as necessary by others who already had “knowledge.” He observed. He cast aside preconceived notion. He studied individual elements. He sought to detect relationships and possible influence. He brought fresh light of mind to possibility. Based on his harvest from observations, Johannes Kepler [1571 – 1630] went on to develop the testable model that would confirm macro relationships.

History is replete with examples of how civilizations have suffered and the development and awareness of new knowledge slowed due to a lack of disclosure and discourse. To a greater or (most often) lesser extent, academia’s gatekeepers limit discourse to “safe” and “incremental” discussion and discovery. Perhaps the willingness to risk equity capital has declined.

## **IV. Current Maladies**

### **Confession**

To some degree, at one time of the other we have been part of, if not contributed to, some of these maladies.

### **Love In The Wrong Places?**

We found it amazing that a well respected editor of a “top journal” in a particular field – one in which those in that field should publish to gain tenure and promotion – bragged at the achievements of this journal over the years. We could not agree more: the journal has achieved much.

However, we found one of the dimensions of improved quality being the average number of references cited by those papers appearing in the journal. We found this assessment of success somewhat unusual and amazing. We looked at several volumes of the journal. In a number of instances, the references in smaller font than the paper covered more pages than the paper. The authors had endured an arduous and apparently required effort to reference everything even remotely related to the subject.

Citations that ascribe credit for original work, that actually support the current research, and others that offer perspectives at variance with the paper, or citations that improve the efficiency of presentation seem in order.

In contrast, alternative motivations for sometimes hundreds of references hold our attention. We suspect that the fear of not referencing every single person, something, somewhere stems from one of more of the following:

- < A sincere effort to offer a complete bibliography. Given writing bibliographies is so exciting compared to other alternative uses of time – including some of basic instinct – we would guess this motivation is at the very extreme tail of the distribution.
- < The fear a selected reviewer will not find his/her work cited, experience an assault of the ego, and move to the fault finding rather than merit assessment of the manuscript.
- < An effort to impress the editor with our knowledge of the literature.
- < One or more other equally good reasons we do not understand.

With little difficulty, we can imagine professors eager to gain an acceptance adding more references in hopes of enhancing the chances of an acceptance. Without difficulty, we can imagine the researcher plagued with an incredible challenge given many of the current systems and practices of review and evaluation. A researcher can't find any references on the topic of her research. Good grief Ms. Brown, you have stumbled upon – or even as a result of hard deliberate effort – an original idea.

## **A Different Library**

*The library of existing knowledge does not contain any original ideas on today's time line – just some ideas that were original in their time.* The library of the mind offers great promise – unless the future is different from history. Regrettably, researchers too often face the task of challenging current “wisdom” and “current interests” of the keepers of the gate – rather than encounter a terrain eager for new seeds of possible great crop.

## **The “Origin” Of The Offering**

We – and we know others – have been amazed by the contributions of many individuals who do not hold a degree, or a degree in a different area, or who do not hold “position.” We also find measures of wonder when we witness the originality of ideas and work emitted by people “from different fields,” from the young and inexperienced, and from the old and wise. Indeed, it is hard to imagine that a certain “class,” or categorization or subset of individuals has some monopoly on ideas of worth.

Some years ago certain individuals at IBM treated a young person with fresh ideas and new work product with measures of disrespect, allegedly because of his age and lack of experience, and the failure of this youngster to understand how things are done. Several years later, IBM would witness Bill Gates’ company surpass theirs in market value.

In the academic arena and world of “top tier journals,” perhaps we also suffer from blind review in the truest sense. In the area of economics and finance, we find perfect example. Eventually, the fresh ideas and models of what would become one of the greatest contributions of the century in financial economics were allowed to surface – but perhaps only because of the blessing of a “top gun” in the field. Apparently, in academics we sometimes go to the same school of charm and opportunity as those of past at IBM. Authors did not even enjoy the courtesy of a review from the first journals to which they submitted their work.<sup>4</sup> Fischer Black commented, “I suspected that one reason these journals didn’t take the paper seriously was my non-academic return address.”

Even within “academic circles,” we suspect that origin continues to play an important role, that role acerbated by recent years of specialization. For example, if a paper arrives at a finance journal submitted by a professor of marketing, we suspect the likelihood of equal treatment in processing and possible review differs from one arriving from a recognized “big gun” in finance.

### **Importance vs. Infatuation With Empiricism**

Empirical efforts are of great import for a host of reasons. The chance of rejecting (for the time being) false premises, notions, relationships, helps to protect us from mistakes. Empiricism also runs the risk of people avoiding truth. Statisticians have properly addressed such prospects in discussion of Type I and Type II errors and other important perspectives related to the proper use of quantitative techniques and methods.

At the same time, the inability to reject with some level of confidence contributes to our willingness to take the risk to try it: we administer the experimental drug to a human in hope of prolonging life. The prospects of potential great gain prompts us to sometimes take risks outside assumed distributions. Hope, faith, gut feel, and other human factors sometimes allow us to proceed beyond statistical chance.

The failure to reject – and to currently accept as reasonable – the hypothesis does not confirm truth. Statistical tests with even high levels of “significance” merely permit reasonable acceptance of confidence. Tests that “confirmed” notions of Kepler, of Newton, or the young doctor in Australia that figured out that two thirds of stomach ulcers were from bacteria provide

a new platform for search “proved” nothing in the scientific sense – but added to current understanding of “how things might be.”

Empiricism does not take the place of original ideas. We have too often heard Ph.D. candidates told: “You are not being evaluated on research results. Your goal is to demonstrate methodological skill.” Knowing how to dig is very different from knowing where to dig. Efforts to stimulate original ideas and avenues for research are critical, independent of the current ability to find a data set to torture until you get “significance.”

### **Future Void, If Empiricism Alone**

Time Magazine’s person of the century would have found it difficult to run empirical tests, even if that had been his interest and intent. In most “top tier journals” of today, gate keepers of recent decades would have found it somehow impossible to entertain an interest in a paper without empirical tests. Unless those gatekeepers are running simulations, we trust their love life sometimes works without the numbers.

For Einstein, the time for empiricism differed from the time of his development of original ideas that were much at variance with accepted wisdom. Many decades would pass before circumstances would allow empirical work that lends support (not proof)<sup>5</sup> to his theories.

For example, in the area of finance, in our lifetime we see similar evidence of obstacles to the free dissemination of ideas. The Black-Scholes option model and related issues represent a monumental contribution to not only the literature, but also in application, to practices of the world. In fact, based on certain underlying notions and relationships, a few people at a later date would bet a trillion dollars – and loose (mostly other people’s money). Revealing the Black-Scholes important work to the world was not without barriers, obstacles, and time delays.

Ideas stimulate thoughts, yet sometimes new ideas are at variance with current areas of interest and current “wisdom” enjoyed by keepers of the gates. Accepted wisdom faces the prospect of perpetuation, that prospect enhanced by the too often practice: current tenders pass the keys to those enjoying similar wisdom – perhaps in reward for loyal support stemming from their insights of finding value in the work and topic areas of the gate keepers.

The ability to subject ideas to scrutiny and let them stimulate and possibly support new paths to discovery is essential in research. Disclosure prior to acceptance of merit by gatekeepers does not imply “truth,” yet disclosure helps us to probe what might represent truth. The absence of a true test – to date – of the capital asset pricing model attests to the nature of the challenge, and to the cleverness of those who constructed the model as an expectations model. Nevertheless, the model has provoked many to test and explore – and that is a contribution that – perhaps – is more important than the particular model.

### **Gate Keepers - Danger**

Gate keepers that feel compelled to control the flow of ideas different from accepted wisdom and their current perspectives are, gate keepers. The danger is that incest is a family matter. However, we accept and recognize the importance of an editorial process. We take

issue with the limited number, scope, and “imagination” of outlets touted as “important.” We suspect that efforts to limit the number of “appropriate,” “good,” or “top tier gates,” in part serves to perpetuate the self value of the relatively few gate keepers.

## **Gardeners**

Some stewards of the dissemination process have taken their responsibilities differently. Leaders in various fields perform their important role in an exemplary manner. These men and woman have and continue to nurture the emergence of new crop and harvest. They do not seek to control plantings or nourish according to personal beliefs. Instead, they provide fertile ground and encouragement for a garden of ideas and crop. They believe in transparency of potential crop and allow those who will consume to ultimately determine value. We think this is something like, free markets.

We know that each of you can add to our diagnosis of the current sate of affairs – and call on you to do so. We will await your thoughts as we move on to more positive issues: possible criteria for consideration in a philosophy for research.

## **V. Criteria**

We suggest that an effective research philosophy should somehow embrace many of the following.

- ⟨ **Allow “defeat of habit.”** In his contributions on creativity, A. Koestler moves to a core issue important in research. Koestler characterizes creativity as “... the defeat of habit by originality.” Basic research intent on unearthing something new should insist on allowing originality. Although pre-conceived notions and “established” relationships have an important role in research efforts, a healthy attitude and research environment eliminates barriers and penalty functions based on existing norms.
- ⟨ **Entertain possibility.** J. Groth in his explorations of creativity stresses the importance of “always a willingness to entertain possibility.” This attitude allows for the chance of “defeat of habit.”
- ⟨ **Stimulate new questions.** The power of the right question in terms of provoking interest and motivating effort often is critical in research efforts. Reportedly, after her father took her picture at the beach the little girl wanted to see it now. His response that it was necessary to send the film to Rochester for processing provoked the “why?” from his daughter. The simple – but focused question – prompted Mr. Land

to break habit, entertain possibility, and defeat process of past with the originality of instant photography.

- < **Allow, if not seek, unusual sources of inquiry.** Renatus Cartesius (Descartes) provided much in terms of “work product.” At the same time, Descartes discovered – reportedly in several nights of dreams – questions of science and philosophy to provoke thoughts and explorations in the three and a half centuries since his death by those who seek to understand more. In more recent times, both Edison and Tesla took different approaches to the inner mind – and both harvested much. For a period of his life, Edison converted ideas to patents, averaging at a rate of about one patent every five days.
- < **Insist on mental honesty.** Mental honesty recognizes that current views may differ markedly from reality. Mental honesty insists on scrutinizing from vantage points that are quite different from one’s own, or those of others. Temporary credibility somehow surfaces from the outcome of careful scrutiny of the plausible – and sometimes what does not appear as plausible given historical perspective.

## VI. Prescription

We tender thoughts on some issues that may add to perspective and support new visions of a research philosophy and resultant effort. We are compelled to again grant that our offerings are incomplete and/or not always relevant to circumstance. The challenge of driving us away from the norms of conscious thoughts on the topic triggers us to suggest common criteria that seem relevant.

- < **In all fields of endeavor accepted wisdom has never enjoyed permanence.** Adopt and perpetuate an attitude of willingness to entertain possibility –even to the point of insisting on it – and bring great power to explorations.
- < **Research calls for the unfailing tolerance and indeed encouragement of ideas, concepts, beliefs that differ from our own and those of others.** Truth is independent of how we think. We must insist that our beliefs and how we think not inhibit movement towards discovery, potential understanding, and a movement closer to truth.
- < **Research should not require nor discount the role of observation.** The powers of observation lead to grasping of important elements and relationships. Empiricism has an important role at some stage within most research. Empiricism should not displace, discredit, or even disallow other forms of inquiry.

- < **Research should engage in honest inquiry of knowledge.** The too frequent apparent approach of trying to find (massage the data) empirical support for a pre-conceived notion or model seems inconsistent with the prescription of independent inquiry that allows the possibility of discovery of any and all alternatives.
- < **Choice (topic, approach, design, method, ... test,) in research should both seek and allow potential leverage of mind to relevant circumstance.** The intent is that researchers insist on examination and scrutiny and allow human capacity to offer the greatest contribution to outcome.
- < **Different objectives and stages of research and circumstance call for different degrees of “integration.”** In some instances, the pure pursuit of the “nature of things” calls for (and we should respectfully allow) an openness that does not require or even tolerate the necessity of purpose or application. In still other instances and stages in the pursuit of knowledge, the integration of elements to develop function, empirical tests, modeling, and then to achieve possible application to purpose are appropriate.
- < **Beyond reasonable replication and verification of previous research, repeated efforts to skin the cat seem out of order: you keep ending up with a skinned cat.** Hence, incremental research efforts should not as a matter of objective seek agreement or consensus or confirmation. Instead, the research in concept, design and execution should ensure the chance of detecting variance from pre-conceived notions and work that may have arrived at incomplete or even false conclusions.
- < **The process and outcome of research should add to the chances for variance of thoughts** and the unearthing of possibilities and alternatives outside the current subset.
- < **Research should distinguish between the increment and the “quantum.”** Development and refinement move along a line towards improvements. Research intent on adding great value should proceed beyond increments, and entertain the notion of alternatives, paths, lines, and even jumps to success.
- < **Hunches, feelings, faith, wonder, and other human dimensions are great strengths the human can bring to effort.** Great leaps of mind with attendant discovery have been a hallmark on the timeline. Faith and other human characteristics have proven critical in making leaps to new areas of discovery and potential undertaking.

- < **Tolerance and even an eagerness to consider new ideas totally at variance with your own offers great promise.** In all fields of endeavor accepted wisdom has never enjoyed permanence.
- < **Observation has great purpose and promise.** We can learn by watching (providing opportunity for visual and mental input), observing (seeking to detect elements and relationships relative to the whole), detecting (recognizing cause and effect), testing (allowing the honesty to uncover error of the process), allowing (for tentative conclusions), moving to the edge of judgment (to decide on what is relevant and significant). Observation is an important element of the holistic nature of good research. When we allow and even insist on looking – we permit the wonderful chance for discovery. And discovery is not properly characterized in terms of sigma. Discovery finds rebalance in significance.
- < **Different vantage points, the obtuse, the obverse and entirely new frames of references provide opportunity for new insights.** Observing with an attitude that allows fresh input rather than additions to images previously recorded allows for new picture rather than multiple exposures that cloud and hide potential truth. Observation does allow significant chance of discovery. In fact, measured precisely from the new vantage point and perspective of space lets us recognize our home is not flat, but also not exactly round.
- < **The disaggregate and aggregate both hold important roles in understanding.** As Bette Midler more than nicely reports, from a distance there is harmony. The harmony is not accidental. It stems from the holistic assimilation of individual parts all integral – without exception – to the whole. The dots are not connected accidentally. Research should recognize the macro and micro aspects are relevant.
- < **Convergence and divergence of thoughts and research efforts have important roles in exploration.** Insistence on one or the other is always at the expense of one or the other. Convergence before divergence may cause us to focus on the wrong element. Divergence without eventual convergence keeps us from bringing relationships into focus and detracts from efforts to understand.
- < **Exploration is often quite different from efforts at confirmation.** Both are important. Output of exploration offers output for attempts at confirmation.
- < **We must carefully respect the potential power of tests and statistics, but not allow substitution of statistics for judgement.** In a similar vein, models are models. Ignoring potential reality based on potential expectations is somewhat counter to the true nature of chance – and void of common sense. Example: Some suggest the chance of events going against the strategy and position of Long Term Capital was on the order of one chance in billions. Another suggested the event

plagued Long Term Capital (LTC) was a “fourteen sigma event,” likened to the chance of one event since the hypothesized beginning of the cosmos. The very nature of chance insists that the most unlikely outcome can transpire.

We should not plan on living in or outside of a fourteen-sigma event. We will live some particular event. We do not live concepts such as expected values, variances, or any moments of some generating function. Time seems intent on bringing the reality of actual events. Some events are virtually impossible, unimaginable, even rude. The recent financial debacle of LTC serves as a reminder of the difference between expectations and the prospect of reality and the nature of chance – and the danger of ignoring basic elements of fact.

- ⟨ **Common sense is not without merit – and offers some measure of protection.** Characterizing chance, state of nature, probabilities, a host of other relationships, and models are with great purpose and possible benefit. Having a weather model does not preclude looking outside to see if it is raining.

## VII. Filling The Prescription

If we are to understand the origin of the bloom and appreciate the aroma – the prescription is straightforward but not so easy to fill. We must not only allow, but also provide opportunity for the garden of knowledge. Our efforts should seek to provide opportunity, elicit seed, and allow ourselves and others to cultivate potential elements of knowledge. We must allow and even nurture work that differs from our own sense of values and perspectives on significance.

Stimulation and generation of original ideas is at least as important as empirical testing of ideas. Providing transparency of effort and outcome must take the place of filtering and premature “valuing” or judgement of outcome. We must guard that we allow exposure and transparency to the broad test of the many whom may have better understanding of importance and relevance.

Exploration and the prospects of continued discovery ask that we not accept and stand as gatekeepers of the current. Instead we must be good stewards of an environment that allows possibility. Gardeners need to replace gatekeepers if we are to continue to provide bloom and scent – rather than just passage and view. Bringing crop to the forces and test of scrutiny is important.

## VIII. Summary

This paper presents some ideas and thoughts with the intent of stimulating thoughts on research and the philosophy of research.

Research is vital in our efforts to extend the knowledge base and to support the pursuit of understanding the nature of how “things” are – and how things might or could be. The attitudes and practices we bring to the research effort and to the dissemination and interpretation of those efforts will influence the nature, variety, extent, and value of the harvest.

As we seek to explore, our efforts must entertain and even encourage possibility. In terms of attitude, the wise recognize that accepted wisdom is neither wisdom, nor permanent. Knowledge at a moment in time is a subset. The prospects of adding to that subset or developing an entirely new subset of knowledge offer the lure. Understanding is a possibility.

Assuming you know about all the seeds and how they should grow in a perfect garden. In fact, although the seeds share the same genetic origin and the fruit sometimes even the same vine, not all the pumpkins enjoy equivalence.

History keeps reminding us of both the power of ideas as well as the importance of empiricism. We should not insist on one at the expense of the other. Relatively recently in the history of humans, a man sought to explore “the whole” at the backdoor – so he set about to circumvent the globe. Despite predictions based on previous models, he did not fall off the earth.

Rather than gather resources for a great physical voyage, others took a different tact and worked closer at hand. A young man of modest physical stature enjoyed fantasy of how things might be rather than how things currently appeared to be. From the expanse of mind he took great crop about the relative nature of time. Einstein’s characterization with a few symbols of the relationship between energy and mass would change life and open new vistas for possible understanding.

Great crop of past allows for mutations as well as for fresh seed. In fact, Einstein’s great success and contributions suggest the possible need to attempt to extend or perhaps even replace the framework he offered with new. Then, and now, those who continue to explore never accept current benchmarks of the mind as permanent. We rather suspect that Einstein would find comfort knowing that others now have and will explore super luminal physics.

The positive side of future research offers much. Let’s work together. Let’s encourage others. Let’s go on with additions to human equity. An abundance of seeds and the incredibly fertile fields of the mind provide just the chance we need. Our efforts to grown great crop will benefit from balance. Not micro or macro – both have their place. Not empiricism at the expense of observation. Not observation and pure theoretical research in place of – but in support of the whole effort. Observation allows us to detect elements, association, relationships, correlation, cause-effect, and what not understood leads to theory. Models not necessarily accepted, but useful to explain or to predict at the individual level, and aggregate to the grand scale.

It would seem that in the grand scheme of research, we seek to distill from the macro the elixir of knowledge. From the elixir, to precipitate and extract understanding. In some cases of application, refinement offers value – that different from cutting and re-cutting and the same

cloth. If we've substantially depleted the vein, let's go on to prospect for new ore and the chance of fresh gems

As we proceed, let's foster and nurture an exploratory attitude. The earth may not be round is very different from saying the earth is round. One approach is an attitude of acceptance: the other allows for possibility. One approach leads to the acceptance of "wisdom:" the other allows for the chance of wisdom.

It would seem we are charge with the responsibility of engaging in honest inquiry of knowledge. We seek to witness and seek to understand the interplay of behavior and perceptions in a world that is somehow real but perhaps quite different from perceptions. On the other hand, an obverse view argues that perceptions result in reality of circumstance. As we explore, we need not abandon one perspective for the other, but somehow benefit from the appreciation of different perspectives.

We must allow ideas to surface for test, and results for scrutiny. We find it beyond interesting that seeds of nature have photoreceptors. Hidden from light, seed remains dormant and potential crops a mere possibility. Amazingly, brief exposure of seed to light triggers the process of germination. The right circumstances and environment offer promise for ideas of mind. Research should include preparation and search. Seeds of mind provide for origin of ideas. Mind light provides the stimulus. Vision allows us to detect possibility. Attitude causes us to prepare the fields of mind for the chance of crop.

In our quest and in our attitudes towards others that differ in view, our efforts should allow us to unearth unexpected seeds. Our attitudes should prepare the files of mind to offer fertile ground for new discovery. We should prepare gardens to allow fertile area for exposure of minds light, for germination, for possible crop.

We have incredible opportunity. True diversity of exploration will move us from the same crop of precious efforts and along the ranges of potential endeavor. There we will find virgin soils and plenty of seeds waiting for the light we bring.

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<sup>1</sup> Factors and details related to alternative perspectives on research base, process, exploration techniques, and related issues await another paper.

<sup>2</sup> To recall a conceptual point, research can never “prove,” only reject a hypothesis.

<sup>3</sup> See de Bruyn Ouboter (1997)

<sup>4</sup> See “How we came up with the option formula”

<sup>5</sup> Note 2 applies.