October 24, 2004

# THE FUTURE OF *GREAT IDEAS*

Team Collaboration in Basic Science

Published in: Wagner, C.G., ed.,
*Foresight, Innovation and Strategy: Towards a Wiser Future*,
World Future Society, 2005, pp. 395-408

Dr. Alex Pavlak, Thales Research, 315 Dunham Ct., Severna Park, MD 21146, (410) 647-7334, *thales@comcast.net*

# INTRODUCTION

*Team Collaboration has the potential to ignite a neo Renaissance.*

The conceptual foundations of our society rest on the performance of a very few gifted individuals. With awesome insight and clarity of thought, each of these geniuses synthesized theory that explained and unified a broad range of disparate phenomena. *Great Ideas* profoundly changed our relationship with the natural world; they spawned fundamental new paradigms. While every field can point to *Great Ideas,* the natural sciences point to Newton’s Mechanics, Darwin’s Evolution, and Einstein’s Relativity as the three archetypes.

Why is it that all the *Great Ideas* seem to have been discovered a long time ago? By definition, *Great Ideas* are multi-disciplinary and deep. As the decades pass, disciplines proliferate and relentlessly become more narrow and deep. Today everybody is a specialist and no generalist has the depth to truly grasp the frontier. It becomes increasingly difficult for individuals to integrate deep disparate disciplines into a new concept. Sooner or later this difficulty in developing deep multi-disciplinary ideas will become noticeable, a clear anomaly, something we can measure.

When John Horgan publishes *The End of Science: Facing the Limits of Knowledge in the Twilight of the Scientific Age* (Addison Wesley, 1996) perhaps his sense of an anomaly is correct but his diagnosis is wrong. Perhaps we have not discovered all that there is to be discovered; rather, we have discovered all that individuals are capable of discovering. This paper looks to classical problem-solving theory to investigate the nature of our human limits. Once we understand out limits we may be able to overcome them.

During the past 100 years, man has learned how to overcome individual limits by learning how to function effectively in groups and teams. We have replaced hereditary monarchies with representative democracies. Laws are enforced by a system of police, prosecutors, judges and juries rather than by the lone ranger. Following this trend, it is time for us to learn how to think more effectively about complex problems using tightly integrated teams. While this paper uses basic science as a discussion framework, Team Collaboration can impact all fields of intellectual endeavor. The potential is an intellectual Renaissance.

## WHAT ARE *GREAT IDEAS?*

***Great Ideas** are multi-disciplinary, unifying principles, overarching concepts.*

In "Truth and Beauty" (*Bulletin of American Academy of Arts and Sciences* **43**:14-29, 1989) the theoretical physicist Chandrasekhar characterizes beauty as a simple elegance. That is, the fundamental components of the theory are so primitive, and combined in such a natural, simple manner that any other combination seems improbable. This aesthetic sense of beauty leads theorists to the subjective conviction that a theory is a correct description of nature independently of experimental verification.

Claude Shannon's Information Theory is a *Great Idea*. In a paper titled "A Mathematical Theory of Communications" (*Bell System Technical Journal*, 1948), he presents a set of equations that describe communication channel capacity - e.g. how much information can be transmitted over a specific radio link. His equations describe theoretical limits analogous to the second law of thermodynamics. Shannon's fundamental ideas get to the heart of what information is and have ramifications beyond electrical engineering.

In contrast, the Internet is not a *Great Idea*. True, the Internet certainly has a simple elegance and is changing the way we live. But the idea behind it, packet switching, is not very profound or fundamental and is not a unifying principle. Packet switching is an architectural concept fairly obvious to skilled electrical engineers even back in the 1950's. The Internet is harvesting the results of a variety of *Great Ideas* that made digital switching practical.

## MEASURING *GREAT IDEAS*

*The influence of **Great Ideas** seems to have declined dramatically over the past 50 years.*

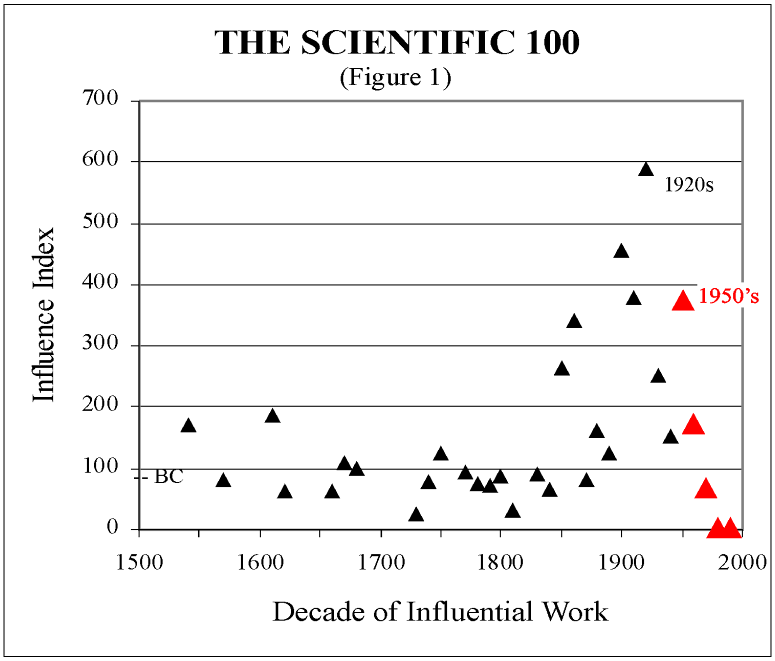
With the "... consultation of prominent scientists and historians of science..." the biographer John Simmons wrote a book titled *The Scientific 100: A Ranking of the most Influential Scientists, Past and Present* (Citadel Press, 1996). Simmons identifies and ranks the most influential scientists from Euclid to modern times. His effort is to assess what ideas today's scientists feel were most important throughout history. Scientists were ranked from 1 to 100 on the basis of their positive accomplishments and the significance of what they did. While this is a terribly subjective and controversial task, as an independent biographer Mr. Simmons provides a measure of objectivity. In the following analysis Mr. Simmons made all the value judgments. This paper simply crunches numbers.

With one or two exceptions, *Great Ideas* characterize every entry on Simmons' list. Each biography identifies the date of the scientist's most significant contribution and each scientist is assigned an influence index based on Simmons' rank. Newton, the top of Simmons' list is assigned an influence index of 100, Archimedes, bottom of the list, is assigned an influence index of 1. The *Great Ideas* are summed into decades. Figure 1 presents the sum of the influence index for scientists who made their contribution in the decade.

For example, the 1970's saw two *Great Ideas* according to Simmons. In 1974 Steven Hawking described black holes as having temperature and emitting radiation for an influence index of 46. In 1975 Edward O. Wilson published his book *Sociobiology: The New Synthesis* for an index of 17. The influence index for the 1970's is 63 (46+17).

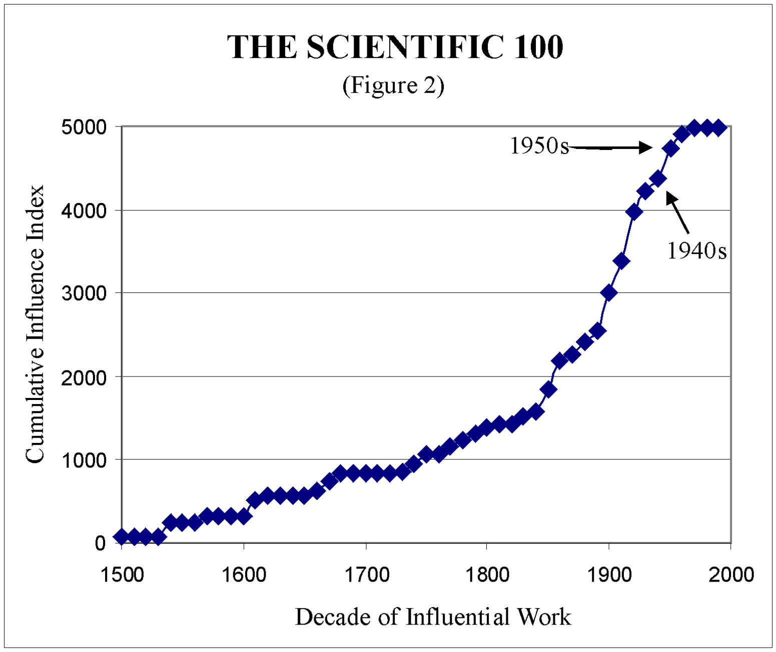
Simons lists no scientists for the 90's and 80's, two for the 70's, five for the 60's, and eleven, the most during any decade, for the 50's. The influence of the eleven scientists in the 1950's does not rival the significance of the nine scientists in the 1920's (372 vs. 587). In the 1950's scientists were discovering more *Great Ideas* than in the 1920's but today they are not judged to be as influential as those ideas discovered in the 1920's.

The decline in the influence index after WW II begs for an explanation. It appears that both the number and the influence of *Great Ideas* have been declining.




Dr. Alex Pavlak, Thales Research, 315 Dunham Ct., Severna Park, MD 21146, (410) 647-7334, *thales@comcast.net*

Figure 2 presents another view of the same data by integrating or summing the influence index from Euclid to present day. We get a classic “S” curve. We can see the renaissance, and rapid growth during the industrial revolution. After WW-II something seems to be inhibiting new *Great Ideas*.




At the risk of reading too much into the data, we see the beginning of a decline in the 1930's and 1940's followed by a spike in the 1950's. This spike can be attributed to the 3x increase in effort (as measured by membership in the American Physical Society) after the WW-II. And yet in spite of that effort the influence index flattens out. Why?

One explanation is that there is a time lag, the significance of *Great Ideas* is not immediately apparent. This is certainly true on occasion. Mendel’s laws of inheritance were ignored for 35 years. Plate tectonics was rejected until verified by data 30 years later. However, these instances appear to be exceptions rather than the rule. There was no delay in recognizing the importance of DNA structure or quantum mechanics. Also, 50 years is a lot of delay.

A second possibility is perhaps science has discovered all there is to be discovered. This is certainly true in a few fields, such as inorganic chemistry. But most working scientists are very impressed with what they do not know. The “Persistent Anomalies” section of this paper suggests ample opportunity.  The quantum mechanics section shows a painfully clear need for a *Great Idea* that has defied solution for 80 years. Vannevar Bush’s concept of an Endless Frontier still seems to be intact.

The third possibility is that perhaps science has discovered all that Homo sapiens is capable of discovering. Perhaps we picked the low hanging fruit. We solved the easy problems and in some way fundamental problems are now simply too hard. The following sections, both “Persistent Anomalies” and “Frustration Metaphor” support this explanation and launch our investigation into problem-solving psychology.

Dr. Alex Pavlak, Thales Research, 315 Dunham Ct., Severna Park, MD 21146, (410) 647-7334, *thales@comcast.net*

**FRUSTRATION METAPHORS**

*Practicing scientists and engineers sense that something is amiss.*

On occasion, perceptive practicing scientists express their frustrations, sometimes through allegory or metaphor. Their thoughts are noteworthy because they offer occasional clues to the cause of what may be happening.

?? David Mermin, a Cornell University quantum physicist, uses lighthearted allegory to express a very serious concern. In "What's Wrong with Those Epochs?" (*Physics Today*, November 1990), his fictional professor Mozart looks at all of the accomplishments since WW II and concludes: *"I have to admit that particle physics over the last 40 or 50 years has been a disappointment. Who would have expected that in a half century we wouldn't learn anything really profound."*

?? In *Dreams of a Final Theory* (Pantheon, 1992), Steven Weinberg, a Nobel Laureate in physics, is more serious but also more perceptive. He suggests a cause: *"In our hunt for the final theory, physicists are more like hounds than hawks; we have become good at sniffing around on the ground for traces of the beauty we expect in the laws of nature, but we do not seem to be able to see a path to the truth from the heights of philosophy*." Weinberg is suggesting that while physicists are very good with the details, they are having trouble seeing the big picture in depth.

?? Robert W. Lucky, an IEEE editor, was asked to edit material for a new edition of an encyclopedia in the field of communications theory. In "When Giants Walked the Earth" (*IEEE Spectrum*, April 2001), he noted that while the biographies contained the familiar pioneers, there was nobody after 1950. Who to add? He was shocked to realize that aside from Claude E. Shannon, there was no one. *"... now there is a cast of millions, doing such important but forgettable things as creating small variations on the details of protocols."*

?? John L. Casti, a mathematician at the Santa Fe Institute, wrote a review for John Horgan's *End of Science* (Addison Wesley, 1996) book for the magazine *Nature*. While disagreeing with Horgan's thesis that we somehow "Face the Limits of Knowledge in the Twilight of the Scientific Age," Casti writes (*Nature 382*, 769, 1996): "*There is one genuinely interesting point struggling to emerge from this whole debate. It is not whether science as we know it is coming to an end. Rather, it is whether the real world may be just too complex for the human mind to comprehend.*"

**PERSISTENT ANOMALIES**

*Persistent anomalies point to problems that are too hard.*

Persistent anomalies are open questions that have not been resolved for a period of time that seems unreasonably long because there is no clear barrier. In fact there are anomalies that seem to fit. Three of the oldest and most clearly defined are:

FOUNDATIONS OF QUANTUM MECHANICS – Modern physicists employ quantum mechanics as an ordinary tool. The formalism (the equations) agrees with experimental data to extraordinary precision. But, while the equations provide correct answers, the physical reality - a picture of the concepts represented by these equations - remains elusive. The debate began 80 years ago between Bohr and deBroglie and remains unresolved. Today we have the Copenhagen Interpretation, Bohm's Ontological interpretation and others, all of which are consistent with the equations yet inconsistent with each other (Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*, Chicago, 1994).

A picture of the underlying concept is important because it provides the basis for inferring new ideas. Nature exhibits a variety of mysteries that might be explained by a better grasp of the conceptual underpinnings of quantum mechanics. These include: the integration of general relativity and quantum mechanics, the relationship between mind and brain, mental telepathy, emergence from chaos to order, and the structure we observe in complex ecosystems.

FLUID DYNAMIC TURBULENCE – Turbulence has been called the last great-unsolved problem in classical physics. While scientists today know the physics, know the equations, and there are no approximations, practical solutions are elusive. Turbulence today remains "... unsolved in the sense that a clear physical understanding of the observed phenomena does not exist." The last *Great Idea* seems to be Kolmogorov's scaling in 1941.

ARTIFICIAL INTELLIGENCE – The birth of artificial intelligence as a branch of computer science is generally attributed to a conference at Dartmouth in 1956. Since then, we have seen beguiling progress in specific, narrowly defined applications such as artificial neural networks. But, every effort to generalize these results has failed. For many years inadequate computer hardware has been a very real constraint. Today that is no longer true. Something conceptual seems to be missing.

There are many other persistent anomalies. In *The Millennium Problems:* (Basic Books 2002) Keith Devlin identifies the seven greatest unsolved mathematical puzzles of our time. John Maddox takes a cut at *What Remains to be Discovered* (Free Press 1998).

## THE PROBLEM DEFINED

*How can we reignite our capacity for **Great Ideas**?*

We do not seem to be producing *Great Ideas* at a pace consistent with historical norms.

There appears to be ample opportunity. Anomalies persist for no apparent reason other than the problem is somehow too difficult. We have not run out of problems, though we may have solved the easy problems.

From time to time practicing scientists use metaphor to express their frustration. These frustrations suggest difficulty with integration, Weinberg's "The view of the hawk."

The frustration metaphors also warn that while we are witnessing enormous progress as a result of the huge effort since WWII, the quality of this progress more superficial than fundamental. We are advancing exciting new fields like nanotechnology and biotechnology by picking low hanging fruit, the easy and the obvious. In mature fields like physics, we solved the easy stuff and the difficult remains unsolved. Now decades pass with little fundamental progress in spite of opportunity and extraordinary time, effort and money.

These observations suggest that we need to explore the limits of the human mind. What is the nature of these limits and how can they be overcome?

## ON THE NATURE OF COGNITIVE CONSTRAINTS

*If our ability to think has limits, how would we know?*

Clearly, the biological capacity of the human brain is finite. Anthropologists tell us that in terms of size, weight and basic features, human brains have not changed for thousands of years. Morphologically, there was nothing particularly remarkable about Einstein's brain. Based on the biological constraints it is reasonable to expect that the humans have cognitive limits and to assume that, like athletes, there is not a huge variation of capabilities among gifted individuals.

In other words, Einstein was not a god, and sitting around waiting for another Einstein is not likely to be a productive strategy.

To better understand the nature of cognitive constraints we will look at the ability to comprehend and the ability to problem-solve.

Dr. Alex Pavlak, Thales Research, 315 Dunham Ct., Severna Park, MD 21146, (410) 647-7334, *thales@comcast.net*

## GENERALISTS VS. SPECIALISTS

*Today, everybody is a specialist.*

The scope of a scientific discipline can be delimited by the capacity of an individual. By devoting a career to a discipline, a scientist can become an expert. S/he can develop a thorough grasp of the fundamentals, the archetypical problems, and the current puzzles and challenges. As a field matures and the frontier becomes too broad for one individual, the discipline splits. Sub-disciplines emerge, each with their own journals, lingo, conferences, values and culture. Subsequent generations become true in-depth experts by narrowing their focus and specializing. By definition of the word discipline, multi-disciplinary problem-solving involves multiple people.

The intellectual founders of any field have wrestled with fundamental issues and tend to have a sound grasp of its foundations. Then, as the field matures, the knowledge base expands and sub-disciplines proliferate.

As generations pass and disciplines proliferate, the founders eventually lose touch with the frontier and latter day specialists lose touch with the foundations. New people can choose to take their finite knowledge capacity and become either a generalist (broad but shallow) or a specialist (deep but narrow). Nobody has the capacity to be both broad and deep.

We see here the basis for Weinberg's hawks and hounds metaphor. The specialists (hounds) scope of understanding is too narrow to develop new global concepts. Conversely, the generalists (hawks) may have a sound sense of overall balance but they lack the depth necessary to develop new global concepts. An examination of classical problem-solving theory reinforces this depth vs. breath conundrum.

## CLASSICAL PROBLEM-SOLVING THEORY

*A problem is an obstructed goal.*

In *Thinking, Problem-Solving, Cognition*, (Freeman, 1992) Richard E, Mayer notes that there are two main theories of problem-solving: the Associationist and the Gestalt psychology. These two theories present alternative views of the psychology of thinking. Neither one is right nor wrong, rather both perspectives offer useful guidance about how to solve problems under different circumstances.

While we touch on Associationism for completeness, our focus is on Gestalt. Gestalt psychology clearly illustrates the simultaneous need for both breadth and depth of understanding to synthesize *Great Ideas*.

**ASSOCIATIONISM**

*Thinking as Learning by Reinforcement.*

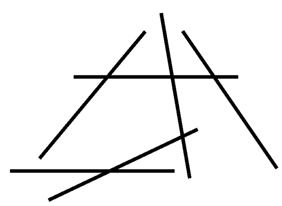
Associationism is usually traced back to rules originally expressed by Aristotle. The belief is that mental life can be expressed in terms of two basic components: ideas (or elements) and associations (or links) between them. In *The Mind's New Science: History of the Cognitive Revolution* (Basic Books, 1985) Howard Gardner views Associationism as explaining a primitive form of learning. Associationist ideas are behind many simple problem-solving heuristics such as brainstorming. Gardner suggests that Gestalt psychology better describes higher learning or intelligent processes.

**GESTALT**

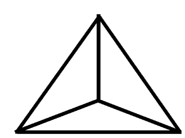
*Insight: Thinking as Restructuring Problems*

Gestalt psychology originated in 1912 with an effort to understand perceptual phenomena. According to Gestalt psychologists, the process of problem-solving is a search to relate all the aspects of a problem situation to each other. This results in structural understanding - the ability to comprehend how all the parts of the problem fit together to satisfy the requirements of the goal. "Insightful problem-solving involves productive thinking – that is, the person must go beyond past experience and overcome misleading situational influences to formulate a novel approach to the problem."

A simple classic example of Gestalt thinking is the so-called six-stick problem: Given six sticks, how can they be arranged to form four equilateral triangles? One could push those sticks around on the table forever without getting anywhere (a persistent anomaly). The solution is "a-ha" a tetrahedron! The insight required to solve the problem is to think in three dimensions rather than two.



GIVEN: 6 STICKS, EQUAL LENGTH
ARRANGE INTO 4 EQUILATERAL TRIANGLES



AH HA!
A TETRAHEDRON

Mandler and Mandler go to the heart of our problem in *Thinking: From Associationism to Gestalt* (Wiley 1964). Insight involves reorganizing the primitive structural elements of the problem in a new way. The whole problem must be solved at once. <u>The intellect must have the capacity to thoroughly understand all aspects and nuances of the primitive elements to envision how they could fit together in a new arrangement</u>. The resulting whole is greater than the sum of the parts. The subjective experience of insight is compelling. At a given point in time one suddenly "sees" the solution to the problem, the patterns fit. The experience has often been described as "Ah-Ha."

A prerequisite to the synthesis of *Great Ideas* is the capacity to thoroughly understand all aspects and nuances of the primitive elements - simultaneous depth and breadth of understanding.

Dr. Alex Pavlak, Thales Research, 315 Dunham Ct., Severna Park, MD 21146, (410) 647-7334, *thales@comcast.net*

**INDUCTIVE REASONING**

*Thinking as hypothesis testing.*

Deduction and Induction are not theoretical approaches like Associationism or Gestalt but rather tasks that are performed in both theories. Problem-solving involves both deductive and inductive reasoning in varying combinations. For example, in mathematics we have the inductive synthesis of theorems and conjectures followed by their deductive proof.

Induction is defined as inferring a general conclusion from specific instances. Through inductive reasoning, a set of rules enables the formation of general concepts from raw data. The critical step in inductive reasoning is the synthesis of a plausible hypothesis. The hypothesis is a speculation that is then tested to see if can be proven wrong. The hypothesis is a guess, but it cannot be a wild guess. There are simply too many possibilities for testing wild guesses to be productive. The hypothesis must be guided, inspired, plausible, insightful, and intuitive. A beautiful hypothesis is a *Great Idea*. When the multi-disciplinary roots of a problem are too deep, an individual does not have the capacity to synthesize a *Great Idea*. S/he is unable to present an appropriate hypothesis and inductive reasoning fails.

Note that this does not suggest a limit on man's ability to comprehend a concept once it has been generated. It does imply a limit on man's ability to <u>induce</u> a solution.

## THE POTENTIAL OF TEAM COLLABORATION

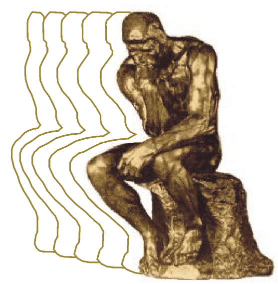
*The ability to draw inductive inferences when the multi-disciplinary roots are broad and deep.*

If the dearth of Great Ideas is the result of individual capacity limits, we should then see two person collaborations outperforming individuals. In fact, this is exactly what happens.

In the race to discover DNA structure, Francis Crick and James Watson beat Linus Pauling because Pauling made mistakes that the collaboration avoided. During the past 50 years, science has seen any number of remarkable two-person collaborations. At the appropriate time, two scientists with compatible personalities and complimentary backgrounds "accidentally" meet and make history. Indeed one of the most productive aspects of multi-disciplinary science today is providing opportunities for appropriate individuals to make connections.

Dyads (two-person collaborations) outperform individuals for two reasons: they bring more expertise to bear against the problem; and the intimate dialog raises the quality of the thinking process. The product is greater than the sum of the parts.

There are two aspects to the high quality of collaborative dialog. First, it is open and honest, collaborators say what they mean, no politics. Second, collaborators stimulate each other to think about things in different ways. In "Creative Tensions in the Research and Development Climate" (*Science* 3785, 1967) Donald Pelz notes that in isolation, an individual's thinking tends to get stale, bogged down in a rut. "...We need a certain amount of dither in our mental mechanisms. We need to have our ideas jostled about a bit so that we do not become intellectually sluggish." An isolated individual tends to develop tunnel vision. Once a series of connections has been made it is difficult to break the chain to establish a new connection. Breaking the chain comes from interaction with the collaborator.



But if two is better than one, why not three? Why don't we see high performance three-person collaborations? The answer to this question lies in the psychology of how people interact with each other. Social psychologists call a two- person unit a dyad, three or more a group. A dyad has the potential to be long term self-sustaining. The introduction of a third person changes the basic nature of the communication. Natural groups (no leader) are unstable and suffer from all sorts of dysfunctions. Effective groups and teams need some mechanism for coordinating their efforts, external support, skilled leadership.

There is a clear track record for Team Collaboration in applied technology. On occasion teams have synthesized complex novel ideas beyond the capacity of gifted individuals. Xerox Parc in the '80s, the Rand Corporation in the '50s are examples are examples of the environment. Also we have seen Tiger Teams, extraordinary problem-solving teams, outperform individuals in times of crisis. The potential for complex problem-solving by collaborative teams is clear. They can synthesize ideas beyond the simple sum of their experiences. So why don't we see fundamental progress by high performance teams in basic science?

## THE REALITIES OF TEAM COLLABORATION

*Science today is the last bastion of rugged individualism.*

Individuals and dyad (two person) collaborations are natural self-sustaining intellectual units. They require tools and motivation but little explicit external support. Unfortunately we seem to be stuck with dyads even though the success of technology teams suggests that we can expand our performance by learning how to think about fundamental questions using larger expert teams.

In contrast to individuals and dyads, teams are not natural units. Simply assembling a group of smart people rarely accomplishes much. Expanding beyond dyads to Team Collaboration would require us to develop something new. A mechanism is necessary to select the team, build it, and then to enable the team to coordinate its efforts. We need new forms of leadership. We need an explicit understanding of the process of synthesizing *Great Ideas*

Traditionally, extraordinary problem-solving teams come together during times of crisis. High motivation and a clear common goal bonds the group and stimulates traditional group leaders to emerge. One challenge is to learn how to accomplish this without the crisis and with fuzzy goals.

Once a team is established it requires leadership that is accepted by its members. The more sophisticated forms of team leadership split the role into content and process. The process leader is a "coach," someone who provides content neutral coordination. The role of coach is complex. The coach must be culturally astute, have respected content judgment, and have the skill to orchestrate the team problem-solving process. The coach is a sophisticated form of the more primitive team facilitator. An important question is how to provide trained coaching services?

Effective teams solve problems as a group in real time. This requires an explicit understanding of appropriate problem-solving heuristics. Heuristic are semi-empirical techniques that have proven effective for certain problems. Team problem-solving heuristics today (brainstorming, root cause analysis, nominal groups...) are directed at relatively shallow simple problems. More sophisticated heuristics need to be developed for deeper structural problems confronting scientists. In *Mental Leaps: Analogy in Creative Thought* (MIT press, 1999) Holyoak and Thagard provide a model of analogical thinking that could be developed into a creative problem-solving heuristic.

Team Collaboration will also require appropriate team-based rewards. Scientific institutions today encourage competition, reward individual achievement and implicitly discourage trust and cooperation. At their core today, scientists are islands. The consequence is that what passes for multi-disciplinary collaboration today is superficial cooperation as opposed to deep intellectual thought.

**THE FUTURE OF *GREAT IDEAS***

*Un-imaginable* ***Great Ideas*** *ignite a new Renaissance.*

Evidence suggests that we are no longer discovering *Great Ideas* on a pace consistent with historical norms. The reason for this is not a lack of opportunity but rather a lack of capability. In mature scientific fields we have already picked the low hanging fruit; we solved the easy problems and the remaining persistent anomalies are simply too hard. The world has become far too complex for extraordinary individual achievement to produce fundamental progress through *Great Ideas*.

Gestalt psychology offers an explanation. In a complex field like quantum mechanics, no one person can grasp all aspects and nuances in sufficient depth to provide a structural rearrangement – the *Great Idea*. The barrier is the finite capacity of the human brain. At some point even gifted individuals do not have sufficient breadth and depth of understanding.

One future scenario is more of the same. We continue to do what we know how to do. Disciplines continue to become deeper and narrower and more isolated. The effectiveness of traditional multi-disciplinary collaboration continues to decline. New paradigms become more rare and (like artificial intelligence) fail to deliver on their promise. For another 50 years we can continue to make progress by harvesting the products of past *Great Ideas*. Gradually the harvest completes, progress stagnates we slide into stasis, a neo-dark age.

A second future scenario is a neo Renaissance involving a commitment to pursue multi-disciplinary goals. Development begins by experimenting with novel forms of collaboration - modified workshops where the goal is no longer just the dissemination of information but also real time problem-solving, results and conclusions. Integration occurs by the group, not just in the minds of the participants. A support structure enables expert teams to set goals and self-select. Reward structures are constructed to cultivate the best of individual passion/commitment with modern high performance teamwork. Training and support infrastructures, problem-solving heuristics and leadership techniques are refined.

Unfortunately, scientific culture has little curiosity about proactively exploring, experimenting and developing novel intellectual processes. There seems to be little recognition of the possibility of improving the intellectual process. Within the traditional culture, it is risky for people to advocate development of something this radical. Perhaps this resistance can be overcome from within the system. Perhaps it will be necessary for advocacy and financial support to come from outside the system. The future is uncertain.

Dr. Alex Pavlak, Thales Research, 315 Dunham Ct., Severna Park, MD 21146, (410) 647-7334, *thales@comcast.net*