Managing for Revolutionary Gains
— Going Beyond Traditional Innovation

Marc G. Millis

Tau Zero Foundation, Cleveland OH, USA

marc@tauzero.aero

Abstract- **A typical challenge of managing research projects is to decide how best to disburse limited resources to competing options to achieve the greatest overall progress. When it comes to seeking 'game-changing' advances, the situation is even more challenging. First, research aimed at revolutionary advancements is different from just innovation. On both the individual and organization level, it is natural to doubt the viability of new, unfamiliar concepts. Foresight is required to extend beyond the known in combination with rigor to make genuine progress. Additionally, such research can span multiple disciplines and different levels of progress and applicability. And lastly, on topics that appear far from fruition, available resources are minimal. To address these challenges and provide insights for managing projects aimed at 'game-changing' advances, historic lessons are compiled and condensed to provide a set of recommendations that include: combining vision and rigor, separate revolutionary research from taking care of existing business, devise prioritization criteria, break long-range goals into shorter-term tasks, judge rigor instead of feasibility, define success as gaining reliable knowledge, and embrace failures.**

Keywords- Engineering Managment Education; R&D Strategies; Project Orgnaizaton; Leadership & Control; Planning & Forcasting; Innovation; Out-of-Box; Revolutionary; Breakthrough

I. INTRODUCTION

Terms like, game-changing, disruptive, out-of-box, leap-frog, revolutionary, and breakthrough, have all been used to evoke the image of technological advances that could usher in a new, more rewarding era. Confusingly, these terms can mean more than one thing. For example, game-changing can just as easily refer to changing the scale of investment, as it can refer to replacing incumbent technology with revolutionary technology.

To be clear, this report deals with the challenges of managing projects aimed at revolutionary technology – advances whose performance will exceed the limits of the incumbent technology. For example, this is like the situation during World War II regarding jet engines amidst the more dominant activities to improve piston-propeller aircraft.

Because research methods toward revolutionary advancements are different from just improving existing technology, the management challenges are also different. First, there is the unfamiliarity and reflexive doubt about new approaches. Next, research options can span multiple disciplines and different levels of progress and applicability. To use a colloquial expression, this presents the challenge of comparing apples to oranges. Also, researchers who attempt revolutionary research (pioneers) are different from the more numerous researchers who are adept at improving established methods (innovators and masters). And lastly, on topics that appear so far from fruition, available resources are minimal. This compounds the challenge when partitioning resources in portions sufficient to ensure progress.

In this context, innovation is not the same as seeking revolutionary gains. The distinction, here, is that innovation means improving existing technology and methods, while revolutionary research is about departing from that legacy to discover entirely new operating principles that will surpass the incumbent technology. For example, the notion of adding steam power to ships was a revolutionary goal at the time when other ship advancements consisted of innovations to sails and rigging.

The lessons in this paper are distilled from a variety of sources that identify recurring trends from prior technological and scientific revolutions. By first understanding the impediments, suggestions are then offered to overcome those impediments. One version of these suggestions was employed by the NASA Breakthrough Propulsion Physics Project [1] and that one example stands as a data point for the potential viability of these suggestions.

The Breakthrough Propulsion Physics Project was designed to manage research on gravity control and faster-than-light travel. Although no breakthroughs were achieved during its 7-year term (1996–2002), the project assessed 10 different approaches, introduced at least 2 new approaches, produced 16 peer-reviewed journal articles, and an award-winning website, all for a total investment of $1.6 million. An independent review scored this project as the most effective amongst a set of other advanced propulsion research during a 1999 assessment [2]. Additionally, discretionary efforts by several volunteers continued thereafter which eventually led to the compilation of a scholarly book about the status and directions of that field of study, Frontiers of Propulsion Science [3].

Although promising, this one test case is not sufficient to determine if the suggestions listed here are broadly applicable. It is hoped that managers who face similar challenges will consider applying these suggestions and then report back on their effectiveness.

II. OTHER ATTEMPTS

The Breakthrough Propulsion Physics (BPP) Project was not the only government funded effort to seek revolutionary advances. Others included the larger NASA Institute of Advanced Concepts (NIAC) [4, 5] and the still larger Defence Advanced Research Projects Agency (DARPA) [6].

Since both of these other efforts have different scopes and operating budgets, direct one-to-one comparisons are inappropriate, but some features merit mention.

First, regarding the differences in scope: the BPP project only addressed emerging science relevant to space propulsion and had an effective budget of approximately \$0.2M/yr (total budget divided by years of operation) [7]. NIAC's scope was broader, seeking revolutionary concepts on "architectures or systems" of space exploration in general, and had an effective budget of more than ten times the BPP effort, specifically of about \$3.4 M/yr [4]. NIAC began in 1998 and was terminated in 2007. Although NIAC was resurrected in 2011 under the slightly different name, "NASA Innovative Advanced Concepts," the comparisons here are limited to the data of its first incarnation. DARPA has been in existence since 1958 and has an even larger scope and budget; seeking revolutionary advances on any subjects related to defense. In 2007, DARPA's portion of the budget that was devoted to just space activities was almost \$500 M [5], which is roughly two orders of magnitude higher than NIAC funding. With these differences, the mechanisms of how work is selected and supported are quite different and would be difficult to compare objectively.

What can be examined are some of the operating principles followed by both DARPA and NIAC. For example, DARPA rotates its staff in positions no longer than 6 years to avoid the "we tried that, it didn't work" syndrome. This tactic is one way to avoid institutional paradigms or the hesitation to venture beyond current approaches. Another DARPA practice is that they have "the freedom to fail". This tactic allows researchers to take the risks necessary to extend beyond the known. For NIAC, one of its founding characteristics addressed the issue of incumbent limitations. NIAC was directed to seek innovations from outside NASA. NASA researchers were not allowed to compete for NIAC funding. Also, one of the criteria was to judge if the work was revolutionary instead of evolutionary. And finally, to reflect its operating attitude, the following quote is prominently displayed on their 2005 brochure [5]: "Don't let your preoccupation with reality stifle your imagination…" These practices are indicative of the same historical lessons found here.

III. RECURRING LESSONS FROM HISTORY

A. Sustaining Pre-Eminence - Departing From Tradition

History has shown that simply innovating is not sufficient to sustain competitive advantage [8]. As illustrated by the recurring S-curve pattern of technological advancement, Fig. 1, it is time to look for revolutionary approaches when the existing methods are approaching the point of diminishing returns. For example, jet aircraft did not result from mastering piston-propeller aircraft. Transistors were not invented by mastering vacuum tubes. Photocopiers did not result from further innovations with carbon paper. The recurring theme is that entirely different operating principles were pursued to surpass the limits of prior technology and thus sustain competitive advantage.

The S-curve evolution shown in Fig. 1 is typical of any successful technology. The pattern begins with only minor advancements until a breakthrough occurs. The breakthrough, at the lower knee of the curve, is where the technology has finally demonstrated its viability. After this point significant progress is made as several embodiments are produced and the technology becomes widely established. Eventually, however, the physical limits of the technology are reached, and continued innovation results in little additional advancement. This upper plateau is the "point of diminishing returns". To go beyond these limits, a new alternative (with its own S-curve) must be created.

Shifting to alternatives (new S-curves) is what is meant by pursuing "game-changing" and "revolutionary" advancements in this paper. Because such pioneering work faces different challenges than innovating with existing approaches, it requires different methods for both the researchers and managers [9].

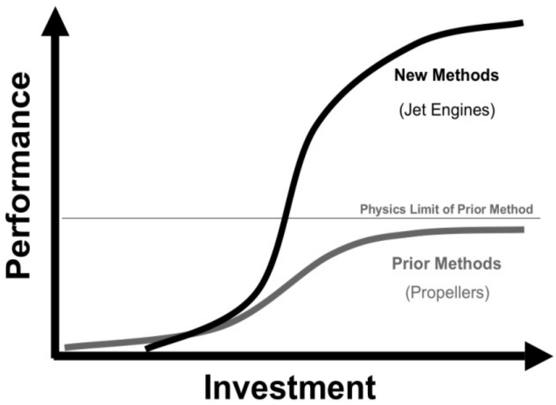

Fig 1. S-Curve Pattern Technological Advancement

B. Pioneers Instead of Masters

The main emphasis of day-to-day engineering is to be a master of your chosen technology. Mastery is achieved through continuous improvements; refining, augmenting and finding new applications while sustaining expertise throughout this process. The work style depends on established knowledge and tends to be systematic, relatively predictable, and has a relatively short-term return on investment.

Creating revolutionary technologies, however, is a wholly different type of work. Going beyond the limits of an existing technology requires imagination to envision future possibilities. It requires confronting the shortcomings of established knowledge to create new knowledge. It requires intuition and subjective judgements to navigate in the absence of an established knowledge base. And because progress is unpredictable and the returns on investment are long-term, it requires the ability to take risks and to persevere.

Historically, pioneering new ideas have been the jurisdiction of exceptional, often rogue, individuals who not only possessed the vision to realize their creations, but also the determination to weather the setbacks, the skills to translate their ideas into proofs-of-concepts, and the ability to make others comprehend their creations.

Individuals who possess all these skills are rare, but this skill mix can exist within organizations, spread amongst many individuals. Forging and managing such pioneering teams face issues different than typical innovation management.

C. Natural Organizational Impediments

It has been found that it is most difficult for incumbent organizations to consider new S-curves when their familiar approaches are at the point of diminishing returns [8, 10]. By then, the institutions have become too uniquely adept at their accrued technology to consider alternatives. They are also tied so closely with their existing customers that it is difficult to explore new opportunities. When at the point of diminishing returns, established institutions prefer to modify, add new features, or to repackage their technology rather than to search for ways to go beyond their technology.

Because new approaches emerge in a still-developing state and have unfamiliar principles, it is also difficult for the incumbent to properly assess their merit. This difficulty is compounded since the incumbents still use their prior values to judge the new approach, values that are rooted in the evaluation criteria for the obsolescing, prior technology.

Even the notion of finding new markets for prior technology is resisted in incumbent organizations. The term for reconfiguring existing technology to address a new opportunity is “architectural innovation” [11]. Even here, the incumbent organizations will typically dismiss such innovations because the new opportunity is seemingly irrelevant when viewed per their prior values. Additionally for architectural innovations, their value is even harder to appreciate because the technical aspects of the innovation do not appear to be noteworthy advancements.

In the case of spaceflight (career specialty of author), the space tourism entrepreneurs are examples of such architectural innovations. They are taking existing technology and applying it in new configurations to reach new markets [12, 13]. This example is relevant in the context of identifying obsolescing values. As evidenced by the emergence of such firms outside the incumbent aerospace organizations, it is clear that the original values that drove the emergence of spaceflight are no longer complete, and perhaps obsolete.

In other words, the criteria against which early spaceflight emerged are no longer the only drivers of future progress. Significant changes have occurred in societal values, technological options, and emerging science. Following historical patterns, it is likely that the incumbent spaceflight organizations will have difficulty recognizing and adapting to the contemporary opportunities and constraints [8, 10, 11, 14].

Considering these patterns, it is not surprising that the emergence of revolutionary advances often come from outside the established organizations [15]. A classic aerospace example is how the Wright Brothers (bicycle mechanics) succeeded in heavier-than-air manned flight well in advance of the government funded (Smithsonian Institution) aerospace research of Samuel P. Langley.

Such departures from legacy approaches have also been referred to as “paradigm shifts” [16]. The organizational challenge when dealing with paradigms is the implicit value system used to judge emerging possibilities. With paradigms there are implicit commitments within incumbent organizations for setting work priorities that are based on the prior technology. This results in a tendency to dismiss novel approaches that are inconsistent with the established paradigm.

The final noteworthy impediment is that revolutionary research is disruptive. While pursing revolutionary methods, numerous ideas are likely to fail. Failure is disturbing to organizations that have become accustomed to incremental successes from less risky innovations. What is especially disruptive is when one of those revolutionary ideas succeeds. In that case, practitioners of the incumbent technology will be threatened with obsolescence. This is understandably disturbing, but not as bad as it might seem. If the revolution comes from within the incumbent organization, that organization can plan for, and take advantage of, that transition. Also, many technologies that have been eclipsed with disruptive technologies still retain a market niche. Sailing ships still exist. Propeller-driven aircraft still exists. Even carbon paper still exists.

D. Natural Individual Impediments

Genuine pioneers have a rare blend of vision to create new ides and the rigor to advance those ideas. It is more common that individuals have only vision or rigor. Many individuals lack the vision to entertain new possibilities. Such individuals tend to reflexively dismiss novel ideas for the same reasons that organizations have difficulty with new ideas. On the other extreme, there are many individuals who can envision new possibilities, but lack the rigor to convert those ideas into reality.

1) Absence of Vision - Reflexive Dismissals:

As reflected by the investigations of Foster, Utterback, Kuhn, and Clarke [8, 11, 15, 17], it is common to have established experts summarily dismiss emerging possibilities. Statistically speaking, one is likely to be correct by dismissing all unfamiliar assertions. Viable revolutionary ideas are rare while errant ideas are easily generated.

Reliably determining the feasibility of new ideas requires openness to consider the possibility and scepticism to rigorously check for weak points and to judge how well these weaknesses are addressed. This is time consuming considering the unfamiliarity inherent in revolutionary ideas. It is analogous to assessing a topic outside of one’s normal discipline. With innovations to existing approaches, there are already precedents with which to compare. With unfamiliar revolutionary approaches, however, it takes time to check the citations and ensure that the assertions are logically constructed. This is compounded since emerging ideas are inherently less refined, which can make them appear non-rigorous compared to established knowledge. Conducting a full assessment of an unfamiliar approach is comparable to a full research task unto itself.

2) Vision Without Rigor:

On the other end of the spectrum, some individuals tend to dabble in vision without rigor. Regrettably, such non-rigorous enthusiasts can taint the serious pursuit of revolutionary research. The grand claims typical of non-rigorous work can attract undue media attention, exacerbating the difficulty of focusing serious attention on promising approaches [18, 19].

A lack of rigor is easier to detect than conducting a feasibility study. Classic symptoms of non-rigorous work are reflected in Langmuir's lecture on "Pathological Science" [20], Carl Sagan's "Baloney Detector" [21], John Baez's "Crackpot Index" [22], and lessons from the NASA Breakthrough Propulsion Physics Project [1]. Representative symptoms from those sources include:

- Fantastic theories that run contrary to observations;
- Selectively addressing supporting evidence while neglecting to address contrary evidence or the possibility of false-positives;
- The magnitude of effect remains close to the limit of detectability, along with claims of great accuracy;
- Confusing correlation with causation;
- Misunderstanding the nature of statistics (President Eisenhower expressing alarm upon learning that half of all Americans are below average intelligence);
- Drawing conclusions from inadequate sample sizes (Statistics of small numbers);
- Blaming their lack of success on historical tendencies for reflexive dismissals, instead of considering the possibility that their work has flaws;
- Lack of relevant reference citations.

3) Additional Psychological Considerations:

Studies have shown that those who are most incompetent also lack the competence to realize their incompetence [23]. This is not a glib comment. This is a real psychological characteristic that compounds the difficulty of addressing both proponents and critics of new ideas.

The cited study, by Kruger & Dunning [23], tested many different perceived skills, including humour, grammar, and logic; and the trends were similar throughout. The poorest performers are the least aware of their limitations. There is, however, a crossover point typically with the third quartile that tends to accurately judge their ranking. The most competent quartile has the opposite perspective. They tend to underestimate how well they fare compared to their peers. Although they accurately estimate their test scores, they tend to over-estimate the performance of others. In other words, they tend to think others are similarly competent to themselves. In short, there is a tendency for us to consider ourselves 'above average', regardless of our actual ranking.

Such self-awareness errors obviously apply to unskilled enthusiasts, but can also apply to established experts when it comes to their ability to recognize genuinely pioneering advancements [8, 10, 11, 15-17, 24]. Recall Clarke's First Law: "When a distinguished but elderly scientist states that something is possible, he is almost certainly right. When he states that something is impossible, he is very probably wrong" [17]. While not a 'natural law' in the strictest sense, it does echo the findings from the more rigorous cited studies.

Kruger-Dunning also found that raising the skill level of the less competent helped them better realize their limits. By teaching the less competent how to improve their skills, they become more aware of their deficiencies.

IV. PURSUING NEW S-CURVES

To begin the process of revolutionary work requires finding the right people, identifying new candidate S-curves, ranking those options for efficiently disbursing limited resources, and getting support from the organization.

A. Assembling a Skill Mix.

While it is easy to spot pioneers after they have become successful, there is no established method to identify those individuals before the fact. Consider the 1986 lecture, by Richard Hamming (A pioneer in error-correcting codes), who reflected on the distinctions between good and great researchers [25]. According to Hamming, good researchers are those who make competent, incremental advancements, while great researchers are those who achieve advancements beyond their peers. In the terminology of this paper, good researchers are innovators, while great researchers are pioneers.

Hamming notes that a major distinguishing characteristic is the problem that those researchers choose to tackle. Good researchers work on legacy S-curves, while the great researchers start new S-curves. The language used by Hamming was that the great researchers have the courage to tackle "Important Problems", defined as those "grand challenges" that will make a significant difference if solved, and where enough progress has been made to enable these problems to be pursued. These are the problems that their peers will not attempt. Instead, those peers (the good researchers) opt to pursue objectives that are already well established in their field and where there is little chance of failing with their innovation (mature S-curves).

The choice of problem is not the only distinction. Hamming discussed numerous characteristics that great researchers tend to possess. The recurring theme is that the great researchers have both the confidence to approach the problem, plus enough self-doubt and awareness of their shortcomings to sustain their objectivity.

In the context of this paper, when seeking those who are capable of revolutionary research, here is an abbreviated list of the major characteristics identified by Hamming of great researchers. When assembling a team to explore new S-curves, seek individuals who:

- Have the courage to tackle "Important Problems":

  Grand challenges that will make a significant difference, not just the "safe" research,

  Attackable: meaning that there is a way to begin solving the problems;

- Start with independent thoughts and then collaborate;
- Make steady progress, driven and focused;
- Tolerate ambiguity;
- Open to learn things beyond their own field; "Knowledge is like compound interest";
- Redirect what is difficult into something easier;
- Honest with personal flaws and work toward overcoming their flaws:

  Believes enough in self to proceed,

  Doubts self enough to see flaws honestly;

- Sells themselves well:

  Writes well,

  Presents well,

  Able to communicate at executive-level.

In addition, during the course of the BPP Project, it was found useful to pair up individuals with visionary tendencies with those more prone to rigor. Provided that professional respect exists within that pairing, that dichotomy of vision and rigor accelerates the process to identify new possibilities and then scrutinize them.

B. Identifying New Possibilities.

One technique for finding new S-curves is the "Horizon Mission Methodology" [26]. This method is a systematic approach for provoking revolutionary research within organizations. The method employs lessons from prior technological revolutions.

The process can be used by individuals or teams. Its first step is to impose a general goal that is impossible to achieve with projected technology. The use of an impossible goal is deliberately intended to counteract the habit of researchers to extrapolate their familiar technologies. To use a colloquial expression, it forces researchers to think "out of the box". Along with the seemingly impossible goal is the requirement that the team considers that the goal is achievable by some undefined, far-future technology, akin to science fiction speculations.

Next, through brainstorming, science-fiction-like ideas can be used as placeholders for the visions of the ultimate solutions. From those provisional "solutions" the team is then asked to "look back from this future" to identify the limiting assumptions. In other words, the team is asked to determine the specific make-or-break issues that would have to be solved to make such a future plausible. In short, this means defining the "grand challenges" around which to aim research objectives.

With those grand challenges defined, the next step is to identify the knowledge gaps. By comparing the grand challenges with the accumulated knowledge to date, the key unknowns and critical issues are identified. From there, the researchers are asked what steps could be taken to begin addressing those unknowns and issues. In terms of Hamming's lecture, this means articulating the "important problems".

The NASA Breakthrough Propulsion Physics Project employed this Horizon Method. It was used to devise the Project's grand challenges, and then in a subsequent exercise, to identify the foundational knowledge related to those challenges [27]. Research options to address those gaps were then generated in a workshop [28]. From there, the actual work began via a suite of competitively selected research [29].

V. PIONEERING WITHIN ORGANIZATIONS.

A. Advocating New Possibilities.

The Horizon Mission Methodology, by itself, is not sufficient to overcome organizational impediments. For those who seek revolutionary progress within existing organizations, the following programmatic tasks can help overcome those impediments, based on historic lessons [8, 10, 11, 15, 17, 23, 30].

Compare the goals of the organization to the ultimate performance limits of the organization's established technology. Make it clear which revolutionary advancements are required to fully satisfy the organization's goals, such as sustaining national preeminence or a competitive business advantage. For example, if a WWII piston-propeller aircraft company's goal is to create the fastest aircraft, the ultimate performance limits of propellers (e.g. can't break sound barrier) should be challenged while introducing the more promising prospects of jet engines.

Familiarize the organization with the emerging possibilities, building on scholarly publications and impartially identifying both their strengths and weaknesses. Offer foundational information that will help the organization comprehend the opportunities and build confidence that the topic has reached credible foundations. In the language of Hamming's lecture, this means showing how the revolutionary goals are now "attackable".

Using the foundational information in comparison to the revolutionary goals, identify the "important problems". The Horizon Mission Methodology is an excellent tool to accomplish this step.

To enable the organization to properly assess the emerging alternatives, develop new criteria against which the emerging alternatives can be compared to the organization's broader goals. Again, using the example of a WWII aircraft company, shift the goal from a 5%

improvement in airspeed to the goal of breaking the sound barrier. This avoids the pitfall of assessing fledgling alternatives in terms of criteria that are specific to prior, more developed methods. An example with steam ships is to shift away from metrics of sail area and rigging efficiency to now consider the broader performance of overall and maneuverability in all wind conditions.

Next, tailor the earliest research proposals to fit within the relatively minor resources available to the far-future options, and then demonstrate from those steps that progress can indeed be made toward the revolutionary goal.

One caveat however: when reporting on the task findings, focus more on the reliable details instead of on their ultimate revolutionary implications. Before a research project has reached a defensible conclusion, it is premature to tout its revolutionary implications. Such premature claims are more characteristic of amateurs, and will raise doubt about the fidelity of the research team. Once the research has reached a defensible conclusion, and if revolutionary progress has indeed been achieved, then the implications can be announced. Prior to that, it is best to establish momentum of producing reliable, incremental, and non-threatening progress.

And lastly, look to other organizations and disciplines for new ideas and pioneers. This echoes the historic pattern where most revolutions come from outside the incumbent organizations.

B. Organizational Processes

Applying the suggestions just given would improve the chances of getting support. This section deals with implementation.

Some organizations already have internal processes to support novel ideas. If the option exists to revise or create a new in-house process, then consider these further suggestions which are based on a variety of studies about high-gain research [30-33].

- Separate revolutionary research (budgets, review process, people) from the rest of the organization's responsibility to take care of, and improve, existing products.

- Prepare for, and take advantage of failed ideas.

- Seek the most forward-thinking individuals from your customers and your most visionary employees to work together to identify new opportunities.

- Improve the organization's ability to recognize and apply emerging advances (called "absorptive capacity" [33]) by:

Sponsoring training and conference travel, including topics beyond the organization's core competencies;

Sustaining in-house research activities;

Tackling the most difficult technical challenges in-house instead of contracting out.

- Characteristics of more successful research groups:

Small teams (5-15 people);

Skill mix dominated by researchers, but includes support staff (technicians, administrative, etc.);

Given discretionary budgets and autonomy to act as a independent entity – but only for a fixed duration (years);

"Allowed to 'play in the sand'", [31] but only for a fixed duration (years) whereupon they must compile their findings into conclusions;

Despite autonomy, groups still report progress to management regularly;

- Cycle through a process of idea exploration, then focus back on the organization's long-range goals, where each emphasis dominates (goals vs. ideas) only for a fixed duration.

VI. SELECTING THE BEST OPTIONS.

As stated previously, the prior values used in an organization for prioritizing options will be inappropriate for selecting new S-curve concepts. It is necessary to develop scoring criteria tailored to the new revolution-seeking project.

During his tenure at the NASA Glenn Research Center, Bruce Banks developed a procedure for developing such criteria and refined this process over several iterations. A documented example of this process is the selection of the replacement thermal control materials for the Hubble Space Telescope [34]. This decision-making process is designed for prioritizing options where there are many issues of varying influence that make such selections complex. Here is an abbreviated list of the key advantages of this method. Full details are in [1]:

- Every issue and every opinion is considered.

- The process employs multiplicative scoring that is significantly more sensitive to critical issues than additive scoring.

- The decisions are prioritized in a quantified manner.

- The process minimizes skewing from overly assertive people.

- The mathematical methods of the process can be automated in software.

- The decision-making process results in excellent "buy-in" by those using the process.

- The resulting decisions are highly defensible.

A. Participants

The process starts by assembling a team of representative experts and customers of the desired technology. Customers are the research sponsors, and the experts are representative practitioners who are capable of conducting the research. Through brainstorming and voting, the team defines the relevant evaluation criteria, and then narrows these criteria down to a minimal list with weighting factors for each. The group also must distinguish between those criteria that are mandatory (criteria that must be met), and those criteria that are just enhancing.

It is essential that the research customers and practitioners concur with the criteria before applying the criteria to actually evaluate the options.

B. Characteristics of Evaluation Criteria

To determine the criteria to be used in the selection, it is helpful to have a suite of illustrative options – a sample of items whose prioritization is sought. When the committee discusses the evaluation criteria, every proposed criterion should be listed, regardless of how many people feel, it is meritorious. Once those criteria are listed, individual or group voting will be done to determine the relative importance of each. Evaluation criteria should be:

- Phrased in positive terms (express desired characteristics or freedom from undesirable);
- Phrased such that there is majority acceptance of the wording;
- One independent issue per criteria;
- Able to be numerically scored (or graded);
- Include all relevant issues.

C. Relative Weighting of Criteria

Once the criteria have been selected, the committee decides, by consensus or majority vote, which criterion is the most important. The "relative importance" of this criterion is assigned a value of 1. The relative importance of the remaining criteria will have proportional importance values between 0 and 1. For example, a criterion half as important as the most important criteria should be rated 0.50.

D. Multiplicative Scoring Principles

To quickly filter out substandard submissions, it is desired to have a feature whereby any failure to meet a mandatory criterion will eliminate the entire submission from competition. To provide this feature as an integral part of an evaluation system, the total score is determined by multiplying together, rather than by adding, the individual criteria scores. In this manner, any zero score (failing grade) on any mandatory criterion will result in a total score of zero.

To implement such a system, there are three details to take into account: (1) how to handle non-mandatory criteria, (2) how to handle weighting functions, and (3) how to normalize scores. The sample equation below illustrates a multiplicative system for two mandatory criteria and one non-mandatory criterion.

$$TotalScore = \left(\frac{A}{N_A}\right)^a \left(\frac{B}{N_B}\right)^b \left(\frac{C + C_{\min}}{N_C}\right)^c \quad (1)$$

Where:

A, B, C = criteria scores.

a, b, c = weighting factors, where 1 is the maximum value, and lower priorities are fractions of 1.

N_A, N_B, N_C = normalizing values or functions.

C_{min} = a preset non-zero value to prevent the parenthetical term from equalling zero, in the event that C = 0, thereby making criterion C non-mandatory.

To allow non-mandatory criteria into a multiplicative system, two different approaches can be employed. The easiest is just to assign a score range for that criterion where the lowest possible score is not zero. The alternate approach, shown in the equation above, is to include a non-zero value in the criterion's equation. This second approach, however, complicates the normalizing functions.

In practice, the effect of the weighting functions also is tied to the maximum-point-value that each criterion can attain. Therefore it is necessary that each criterion be normalized to the same maximum-point-value (the terms within the parentheses). For normalization, which means equalizing each criterion prior to applying its weighting exponent, a simple fractional coefficient is applied, so that the maximum possible values of all the criteria are equal.

Although a generic set of equations can be derived for how to implement a multiplicative system that accommodates all possibilities of mandatory and non-mandatory criteria, and accommodates criteria with differing scoring ranges, it is far simpler to implement the system with constraints on the scoring ranges. If all criteria have the same maximum point value, no normalization is required. If all non-mandatory criteria have a non-zero value as their minimum possible score, then no additional constants or associated normalization functions are required.

E. Scholastic Grading Standard

Experience has shown that an evaluation depends not only on the perceived merit of the idea, but also on the evaluators' interpretations of how to score the idea. For example, if the scoring range is 0 to 25 on a given criterion, such as with the Small Business Innovative Research (SBIR) evaluations [35], two different evaluators may use significantly different point values to mean the same grade. To avoid this problem, it is recommended to use a familiar and limited grading system such as the scholastic 4-point scale:

A (4 pts) = Excellent, meeting the criteria to the maximum amount;

B (3 pts) = Good, or well above average;

C (2 pts) = Average, or the score to use if there is no reason to score high or low;

D (1 pt) = Poor or well below average;

F (0 pts) = Fails to meet the criteria.

In those cases where these discriminators do not fit, it is still recommended to have the scoring range limited to about 5 gradations where possible, and to have clear text explanations to accompany each gradation. Since the final scores combine several criteria, it is possible to get sufficient distinctions with the final composite scores even with such limited gradations.

F. Recommended Evaluation Criteria

1) Evaluate Rigor, Not Feasibility:

Considering the previous insights about reflexive dismissals and the difficulty of impartially determining feasibility during a proposal review, it is recommended to use criteria that address the rigor of the work rather than its feasibility.

In addition to the easy-to-spot symptoms of non-rigorous work, here is a list of attributes that indicate rigorous work:

- The submitter is aware of the focal make-break issues related to their approach;
- The submitter is cognizant of the reliable relevant literature. Note, however, that it is common that credible researchers are not aware of *all* of the relevant literature. Some omissions are reasonable to expect;
- Any alternative and unconventional interpretations of known phenomenon are accompanied by correct citations of those phenomenon;
- The submitter cites examples of their prior work to reflect their competence to conduct the proposed work.

2) Measured Progress:

To help identify a suitable research increment and to provide managers a means to measure progress, the Technology Readiness Levels can be used [36]. For more fundamental research that is still in the realm of science instead of technology, the NASA BPP project developed a set of “Applied Science Readiness Levels” [1: Table A.1].

Once the status of a given research objective has been ranked relative to these scales, the next logical increment of research would be to advance that topic to its next readiness level.

VII. FROM TASKS TO OVERALL PROGRESS

The ranking process just described is used to select which tasks are supported, but a research project must orchestrate these incremental tasks into overall, relevant, defensible progress. Accordingly, the following project operating principles are recommended.

A. Defining Success as Reliable Knowledge Gained

Although it is a common practice when advocating research to emphasize the ultimate technical benefits, this practice is not constructive when seeking revolutionary advances. Instead, it is more constructive to emphasize the reliability of the information to be gained. Although breakthroughs, by their very definition, happen sooner than expected, no breakthrough is genuine until it has been proven to be genuine. Hence, the reliability of the information is a paramount prerequisite to the validity of any conclusions.

To place the emphasis where it is needed, no research approach should be considered unless it is sufficiently rigorous, regardless of the magnitude of claimed benefit. Success is defined as acquiring reliable knowledge, rather than as achieving a breakthrough.

This success criterion even means that a failed concept (test, device, etc.) is still a success if the information gleaned from that failure provides a reliable foundation for future decisions.

This is a departure from the more common notion of judging success by how closely outcomes match expectations. In the more common approach, a task fails if the device does not work as desired, regardless of the lessons gained from the attempt. While such expectation-specific success criteria are appropriate for manufactured goods, they can bias fundamental research.

Placing the emphasis on the fidelity of the findings, encourages researchers to apply rigor to their work and to take the risks necessary to discover what others have overlooked. It also makes it easier to accept the results as they are, rather than to be tempted to skew the findings to match the expectations.

B. Immediate Research Steps

Another technique to shift the emphasis away from provocative situations and toward constructive practices is to focus the research on the immediate questions at hand. These immediate unknowns, issues, and curious effects can be identified by contrasting established knowledge to the desired performance. The Horizon Mission is suggested to complete this step [26].

The scope of any research task should ideally be set to the minimum level of effort needed to resolve an immediate “go/no-go” decision on a particular issue. This near-term focus for long-range research also makes the tasks more manageable and more affordable. Specifically, it is recommended that any proposed research be configured to reach a reliable conclusion in one to three years. Should the results be promising, a sequel can be proposed in the next solicitation cycle.

This is a departure from the Phase-I and Phase-II practice of both the Small Business Innovative Research (SBIR) [35] and the original NIAC [4, 5] programs. In these systems, Phase-I awards are 6-12 month feasibility studies, whereas Phase-II are larger and longer-term awards to move from feasibility toward an application embodiment. While this may be a prudent strategy for technology that is approaching fruition, such a two-stage approach is premature for basic research, even for applied basic research. For example, the Phase-I and Phase-II approach are based on a system where success is based on the feasibility of the concept as opposed to revolutionary research where success is based on the lessons learned.

C. Iterated Research

To accumulate progress, it is recommended to support a suite of proposals every two to three years, and to let the findings of the prior suite influence the next round of selections. This provides an opportunity for new approaches, sequels to the positive results, and redirections around null results.

Again, for basic research aimed at revolutionary advances, this iterative strategy is recommended over the Phase-I and Phase-II strategy of SBIRs and NIAC projects. The distinction is that revolutionary research requires taking risks beyond seemingly feasible approaches. This is also tied to shifting the definition of success from that of feasibility to that of accurately learning the realities behind the desired achievements.

D. Diversified Portfolio

It is far too soon, in the course of seeking revolutionary advancements, to down-select to just one or two hot topics. Instead, a divergent mix of research approaches should be investigated in each review cycle. This is different than the more common practice where further advancements are primarily sought on the technical approaches already under study. Although this more common strategy can produce advances on the chosen topics, it faces the risk of overlooking potentially superior emerging alternatives and the risk that support will wane unless the chosen topics produce unambiguous positive results.

E. Publishing Results

Research findings should be published, regardless of outcome. Results, pro or con, set the foundations for guiding the next research directions. Although there can be a reluctance to publish null results – where a given approach is found not to work – such dissemination will prevent other researchers from repeatedly following dead-ends. Again, by defining success as gaining reliable knowledge, such dissemination of lessons-learned becomes easier.

VIII. MEASURING PROJECT PERFORMANCE

When managing a project it is helpful to specifically convey how the progress on individual tasks combines to make progress toward the project's goals. To that end, the following Project Metrics are recommended:

- Number of visionary notions or grand challenges converted into "important problems";
- Number of "important problems" converted into research tasks;
- Technical progress per task. This can be quantified using the same ranking system used for evaluation. By calculating its score (again) after the task is complete, there should be a difference between the readiness level before and after that task is completed. That difference, combined with the other criterion values, is a reflection of the amount of progress made;
- Percent of "important problems" solved;
- Number of findings published in peer-reviewed literature;
- Number of students inspired (can only count those that send comment);
- Number of spin-offs.

To measure the overall performance of a pioneering research project, all of the productivity measures above can be tallied, and then divided by the amount of resources (funding and time) consumed to achieve them:

$$\text{Relative Performance} = \frac{\sum \text{Project Metrics}}{(\$) \bullet (\text{Duration})} \qquad (2)$$

By including resources, this provides a means to gauge efficiency over time and to compare the effectiveness of this project to others (provided that the other projects are judged according to similar metrics). In colloquial terms, this process can quantify "bang for buck".

IX. CONCLUSION

Although pioneering research is difficult, enough lessons have accumulated from history to guide the management of projects devoted to revolutionary research.

A key recommendation is to combine vision with rigor. Vision is needed to extend beyond existing knowledge, whereas rigor is needed to impartially compare those visions to accrued knowledge. The intent from that contrast is to identify the critical issues, make/break questions, and curious effects related to the desired goals. Once articulated, these become the important problems for pioneering research.

Another key recommendation is not to attempt to judge technical feasibility during proposal reviews, since that approach would constitute a research task unto itself. Instead, focus attention on judging if the proposed work will reach a reliable conclusion upon which other researchers and managers can make sound decisions for the future.

Additionally useful strategies include: breaking down the long-range goals into near-term immediate "go/no-go" research objectives that can each be assessed within 1 to 3 years; devising a numerical means to impartially compare research options and inherently reject non-rigorous submissions; and addressing a diversified portfolio of research approaches.

It is hoped that by articulating these suggestions, other leading-edge research projects can improve their prospects for success.

REFERENCES

[1] Millis, M. (2009). Chapter 22: Prioritizing Pioneering Research. Frontiers of Propulsion Science, **227** of Progress in Astronautics and Aeronautics, Reston VA: American Institute of Aeronautics and Astronautics (AIAA). 663-717.

[2] Merkle, C. L. (Ed.). (1999). Ad Astra per Aspera, Reaching for the Stars, Report of the Independent Review Panel of the NASA Space Transportation Research Program, (January 1999).

[3] Millis,& Davis, (Eds.). (2009). Frontiers of Propulsion Science, **227** of Progress in Astronautics and Aeronautics, Reston VA: American Institute of Aeronautics & Astronautics

(AIAA).

[4] Cassanova, R., Jennings, Turner, Little, Mitchell, & Reilly. (2007). NASA Institute of Advanced Concepts, 9th Annual & Final Report, 2006-2007, Performance Period July 12, 2006 - August 31, 2007.

[5] Bradley, A. (Ed.). (2004). NASA Institute of Advanced Concepts, the first five years and beyond, color brochure. Retrieved 2008-Feb-29 from <http://www.niac.usra.edu>.

[6] Wislon, J.R. (2008). Fifty Years of Inventing the Future, 1958-2008 (DARPA), Aerospace America, **46**(2). American Institute for Aeronautics and Astronautics,. 30-43.

[7] Millis, M. (2004). Breakthrough Propulsion Physics Project: Project Management Methods, NASA/TM–2004-213406.

[8] Foster, R. N. (1986). Innovation: The Attacker's Advantage. New York NY: Summit Books.

[9] Millis, M. (1993). What is Vision 21? In Vision 21: Interdisciplinary Science and Engineering in the Era of Cyberspace, Symposium Proceedings, March 1993, NASA CP 10129. pp. 3-6.

[10] Shepherd, D. A. & Shanley, (1988), Common Wisdom on the Timing of the Entry, CH. 1 in New Venture Strategy. Sage Publications Inc.

[11] Henderson, R. M. & Clark. (1990). Architectural Innovation: The Reconfiguration of Existing Product Technology and the Failure of Established Firms. Administrative Science Quarterly, **35**. 9-30.

[12] Stone. (2003). Bezos in Space. Newsweek on MSNBC News, May-5, Retrieved 2003-May-29 from <http://www.msnbc.com/news/9o4842.asp>.

[13] Race to blast tourists into space. (2006). CNN.com, 21 Mar. Retrieved 2006-May-23 from

<http://www.cnn.com/2006/TECH/space/03/20/space.tourism.ap/index.html>.

[14] Millis (2010) Predictions for Civilian Space Flight Based on Patterns from History. JBIS, **63**, pp. 406-418.

[15] Utterback, J. M. (1994). Dominant Designs and the Survival of Firms. Ch. 2. in Mastering the Dynamics of Innovation. Harvard Business School Press.

[16] Kuhn, T. S. (1970). The Structure of Scientific Revolutions, 2nd ed. Chicago: University of Chicago Press.

[17] Clarke, A. C. (1972). Profiles of the future: An inquiry into the limits of the possible. Bantam Books.

[18] Hartz, J. & Chappell. (1998). Worlds Apart: How the Distance Between Science and Journalism Threatens America's Future, 2nd ed.. Nashville TN: First Amendment Center, Retrieved from

http://www.firstamendmentcenter.org/about.aspx?id=6270.

[19] Park, R. L. (2000). Voodoo Science: The Road from Foolishness to Fraud. New York: Oxford University Press.

[20] Langmuir, I. (1953). "Pathological Science," Colloquium at The Knolls Research Laboratory, 1953-Dec-18, transcribed and edited by R. N. Hall. Retrieved 2008-Feb-27 from <http://www.cs.princeton.edu/~ken/Langmuir/langmuir.htm>.

[21] Sagan, C. & Druyan, (1997) The Demon-Haunted World: Science as a Candle in the Dark, Ballantine Books.

[22] Baez, J. (1998). Crackpot Index. Retrieved 2008-Feb-27 from <http://math.ucr.edu/home/baez/crackpot.html>.

[23] Kruger, J. & Dunning. (1999). Unskilled and Unaware of It: How Difficulties in Recognizing One's Own Incompetence Lead to Inflated Self-Assessments. Personality and Social Psychology, **77**(6): 121-1134.

[24] Forward, R. L. (1995). Indistinguishable from Magic, Baen Books.

[25] Hamming, R. (1986). "You and Your Research." Lecture at Morris Research & Engineering Center (7-May-1986). Retrieved 2006-Feb-17 from

<http://www.cs.virginia.edu/~robins/YouAndYourResearch.pdf>.

[26] Anderson, J. L. (1996). Leaps of the Imagination: Interstellar Flight and the Horizon Mission Methodology, Journal of the British Interplanetary Society, **49**. 15-20.

[27] Millis, M. G. (1997). Breakthrough Propulsion Physics Research Program, NASA TM 107381.

[28] Millis, M. G. & Williamson, G. S. (1999), NASA Breakthrough Propulsion Physics Workshop Proceedings, NASA–CP–1999-208694, Proceedings of a workshop held in Cleveland Ohio, August 12-14, 1997.

[29] NASA Research Announcement: Research and Development Regarding 'Breakthrough' Propulsion, (1999), NASA Lewis Research Center, NRA-99-LeRC-1.

[30] Miller, W. C. (1987). The Creative Edge: Fostering Innovation Where You Work, 2nd printing. Reading MA: Addison-Wesley Publishing Co. Inc.

[31] Tabrizi, B. & Walleigh, R. (1997), Defining Next-Generation Products: An Inside Look, Harvard Business Review, (Nov-Dec) pp. 116-124.

[32] Chesbrough, H. (2000), Designing Corporate Ventures in the Shadow of Private Venture Capital, California Management Review, **24**(3). 31-49.

[33] Cohen, W. M., and Levinthal, D. A, (1990) Absorptive Capacity: A New Perspective on Learning and Innovation, Administrative Science Quarterly, **35**. 128-152.

[34] Townsend, J. A., Hansen, McClendon, de Groh, & Banks. (1999). Ground-based testing of replacement thermal control materials for the Hubble Space Telescope, High Performance Polymers, **11**. 63-79.

[35] Small Business Innovative Research, Evaluation Instructions. Retrieved 2008-Feb-27 from

http://sbir.nasa.gov.

[36] Hord, R. M. (1985). CRC Handbook of Space Technology: Status and Projections. Boca Raton, FL: CRC Press.