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Breakthrough Propulsion Physics Project: Project Management Methods

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Abstract

To leap past the limitations of existing propulsion, the NASA Breakthrough Propulsion Physics (BPP) Project seeks further advancements in physics from which new propulsion methods can eventually be derived. Three visionary breakthroughs are sought: (1) propulsion that requires no propellant, (2) propulsion that circumvents existing speed limits, and (3) breakthrough methods of energy production to power such devices. Because these propulsion goals are presumably far from fruition, a special emphasis is to identify credible research that will make measurable progress toward these goals in the near-term. The management techniques to address this challenge are presented, with a special emphasis on the process used to review, prioritize, and select research tasks. This selection process includes these key features: (a) research tasks are constrained to only address the *immediate* unknowns, curious effects or critical issues, (b) *reliability* of assertions is more important than the *implications* of the assertions, which includes the practice where the reviewers judge *credibility* rather than *feasibility*, and (c) total scores are obtained by *multiplying* the criteria scores rather than by adding. Lessons learned and revisions planned are discussed.

Introduction

The NASA Breakthrough Propulsion Physics (BPP) Project was funded from 1996 through 2002 to assess the prospects from emerging science that might lead to breakthrough methods of spaceflight. Since this objective is more visionary than typical propulsion research, special attention is given to how such a Project is managed. Balancing credibility and vision evokes special challenges, as does the requirement to produce near-term progress toward goals that are beyond foreseeable fruition. The management challenges and resulting methods are described.

Project Goals

As its name suggests, the Breakthrough Propulsion Physics Project is specifically looking for propulsion *breakthroughs* from *physics*. It is not looking for further *technological refinements* of existing propulsion methods. Such refinements are being explored in other NASA projects (2003 NASA Strategic Plan, Bachtel 1997). This Project seeks further advances in *science* from which genuinely new *technology* can eventually be derived - technology to surpass the limits of existing methods (Millis 1999a).

The research is focused toward the following three specific breakthroughs—the “Grand Challenges” that could revolutionize spaceflight and enable interstellar missions:

Grand Challenge 1: Mass.—Discover new propulsion methods that eliminate or dramatically reduce the need for propellant mass. This implies discovering fundamentally new ways to create motion, presumably by interaction with the properties of space, or by the interaction of matter, energy, and space-time, including the possibility of manipulating gravity or inertia.

Grand Challenge 2: Speed.—Discover how to circumvent existing limits to dramatically reduce transit times. This implies discovering a means to move a vehicle at or near the actual maximum speed limit for motion through space, or by some other means, such as by interaction with space-time itself to circumvent normal limits.

Grand Challenge 3: Energy.—Discover fundamentally new modes of onboard energy generation to power these propulsion devices. This third goal is included since the first two breakthroughs could require breakthroughs in energy generation, and since the physics underlying the propulsion goals is closely linked to energy physics.

The physics underlying these ambitions includes experiments and theories regarding the coupling of gravity, electromagnetism and space-time, properties of the space vacuum and inertial frames, quantum level effects, warp drives, wormholes, and anomalous effects of force-production or energy-exchange.

Project Implementation

This Project was invited as part of a 1996 reassessment of NASA propulsion research efforts (Bachtel 1997 and Millis 1999a), yet its implementation was proposed as a sequence of conditional steps, where continuation was contingent upon successful completion of the prerequisite steps (Millis 2000). This conditional posture was proposed to ensure that there were enough foundations for launching into, and sustaining, such a visionary project. These conditional milestones and the documentation summarizing their findings (cited in parentheses) are as follows:

1996—Determined that sufficient scientific foundations exist (Millis 1996).
1997—Determined that affordable research candidates exist (Millis 1999b).
1998—Devised means to prioritize and select research tasks (Millis 1999a).
1999/2000—Solicited and selected 1st round of research tasks (NASA Solicitation NRA–99–LeRC–1).

In October 2002, the sponsoring Program, the Advanced Space Transportation Program, was reorganized. All research was deleted that was at less than Technology Readiness Level 3 (conceptual design tested analytically or experimentally), including the BPP Project. In November 2002, the Final Report of Aerospace Commission, whose task was to assess the health of the Nation's aerospace industry, recommended: " . . .that the federal government significantly increase its investment in basic aerospace research . . ." (Walker 2002). More specifically, in Chapter 9, the Commission recommended: "In the longer-term, breakthrough energy sources that go beyond our current understanding of physical laws… must be credibly investigated in order for us to practically pursue human exploration of the solar system and beyond. These energy sources should be the topic of a focused basic research effort." At this time it is uncertain if support for the BPP Project will resume.

A recent compilation of the findings of 16 individual research tasks, from this Project and several parallel programs, indicates that about a third of the approaches were found not to be viable, a quarter have clear opportunities for sequels, and the rest remain unresolved. It is expected that new approaches will continue to emerge (Millis 2004).

This Report

This report is intended for two audiences; mangers of other cutting-edge projects, and researchers interested in breakthrough propulsion physics. The management methods are offered as a guide for others that face similar challenges. Many of the methods presented here can be adapted to other long-range research projects. Also, researchers can benefit from examining the research selection criteria, to improve their future proposals, should funding resume for research solicitations.

The special challenges of visionary research are outlined, as are the mitigation strategies for dealing with these challenges. A special emphasis is placed on the process to review, rank, and select research options. The lessons learned over the course of the Project are also offered, as well as the planned changes that respond to the lessons learned.

Special Challenges and Mitigations

A normal challenge of any research project is to prioritize tasks to efficiently direct resources to the best prospects. In addition to this typical challenge, the BPP Project faces the challenge of making credible progress toward the incredible ambition of breakthrough spaceflight. Because the desired propulsion breakthroughs are presumably far from fruition, and perhaps even impossible, specific strategies have been devised to mitigate the risks and maximize the progress of such visionary research.

Exploring the edge of knowledge for profound discoveries evokes special challenges. In addition to the normal challenges of scientific research—discovering how nature truly works—the provocative character of Grand Challenges can encumber research. First, by pursuing truly profound improvements in the human condition, the stakes are higher and accordingly emotions run higher. Second, by operating on the genuine edge of knowledge, instead of exploring refinements of established knowledge, controversial ideas are encountered. This combination of heightened emotions and controversy can encumber the normal, productive discourse of scientific study. Both skeptics and optimists can prematurely reach conflicting conclusions and, in their zeal, fail to communicate with the impartiality needed to rigorously identify, test, and resolve the real issues. To mitigate these difficulties and to focus on more constructive operating practices, the BPP Project employs the following operating strategies, each of which is explained in a subsequent paragraph:

BPP Operating Strategies

- Reliability—Success is defined as acquiring *reliable* knowledge, rather than as achieving a breakthrough.
- Immediacy—Research is focused on the *immediate* unknowns, make-or-break issues, or curious effects.
- Measured—Progress is measured using a combination of the scientific method and applicability.
- Iterated—Overall progress is achieved by repeating cycles of short-term, incremental tasks.
- Diversified—Multiple, divergent research topics are explored simultaneously.
- Impartial—Reviewers judge *credibility* and *relevance*, but are not asked to predict *feasibility*.
- Empirical—Preference is given to experiments and empirical observations over purely analytical studies.
- Published—Results are published, regardless of outcome.

Reliability

Although it is a common practice when advocating research to emphasize the ultimate technical benefits, this practice is not constructive on topics as visionary and provocative as BPP. Instead, it is more constructive to emphasize the *reliability* of the information being offered. Compared to other space propulsion research, new propulsion physics is at its infancy. It is expected, therefore, that any practical embodiment is years, perhaps decades away, if not impossible. 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 on where it is needed, the BPP Project does not consider any approach unless the credibility criteria are satisfactorily addressed, regardless of the magnitude of claimed benefit. Success is defined as acquiring *reliable* knowledge, rather than as achieving a breakthrough.

Immediacy

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 comparing the established and emerging physics to the BPP Grand Challenges. 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 approach. This near-term focus for long-range research also makes the research tasks more manageable and more affordable. Specifically, the Project requests that any proposed research be configured as a task that can reach a reliable conclusion in one to three years. Should the results be promising, a sequel can be proposed in the next solicitation cycle.

Measured

To help guide researchers to identify a suitable research increment, and to provide managers a means to measure progress, the Scientific Method has been adapted as a readiness scale in a manner similar to how the Technology Readiness Levels are used to measure *technological* progress (Hord 1985). These new readiness levels are detailed in appendix A. Specifically, the readiness scale developed for the BPP Project consists of three stages that gauge the *applicability* of the work (reflecting how research can evolve from the more general, to the more specific application), and within each of these 3 stages, 5 steps of the *scientific method* are repeated (from recognizing the problem, through testing the hypothesis). This equates to 15 levels of relative maturity, with the most advanced level being equivalent to Technology Readiness Level 1 (basic principles observed and reported). Once the maturity of a research objective has been identified relative to this scale, the next logical increment of research would propose to advance that topic to the next higher level. This is consistent with the *incremental* research strategy.

Iterated

To accumulate progress over the long term, the original intent of the Project was to solicit a suite of proposals every two to three years, and to let the lessons learned from 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. At any point, if a research task leads to the discovery of a new propulsion or energy effect, it can be pulled out of this process into its own advancement plan. At the time of this writing, however, funding for future BPP solicitation cycles has been deferred. Only one solicitation cycle was completed during the funded tenure of the Project. The next cycle was to be implemented as an Internet based system, which is discussed in the "Revisions Initiated" section.

Diversified

In addition to the techniques already mentioned, the BPP Project aims deliberately to address a *variety* of research approaches in each review cycle. In simple terms, this is to diversify the research portfolio. It is far too soon, in the course of BPP, to down-select to just one or two hot topics. This is in contrast to the more common research on advanced propulsion that deals with topics above Technology Readiness Level 1 (basic principles observed and reported). In those cases, further advancements are sought on technical approaches already under study. Although this more common strategy can produce advances on the chosen topics, it faces the risk of overlooking emerging alternative approaches, and faces the risk that support will wane unless the chosen topics produce unambiguous positive results. For the BPP Project, a more strategic approach was chosen, where cycles of peer-reviewed solicitations examine a diverse portfolio of options, and where the decisions build on the lessons learned from the prior cycle of research. Furthermore, each of the BPP research options only focuses on an immediate critical issue, to systematically chip away at the unknowns toward the overall Project objective.

Impartial

In addition to the mandatory emphasis on credibility and the management utility of short-term, measured progress, the BPP Project also seeks visionary, breakthrough gains. This presents a special challenge, since this invites the kind of controversial ideas that are typically encountered at the edge of emerging knowledge. Considering that most historic breakthroughs originally sounded like fringe ideas, it is expected that many of the proposed approaches to BPP might sound too visionary at first, or at least unfamiliar. It is therefore difficult to sort out the fringe ideas that may one day evolve into tomorrow's breakthroughs from the more numerous, erroneous fringe ideas. Typically, when confronted with such unfamiliar ideas, many reviewers will reflexively assume that the idea will not work. Given the kind of fundamental investigations sought by this Project, it is difficult to reliably determine such technical feasibility during a proposal review. Such an assessment would constitute a full research task itself. Instead of judging technical feasibility, proposal reviewers are asked to judge if the task is leading to a result that other researchers will consider as a reliable conclusion on which to base future investigations. This includes seeking tasks that can reliably demonstrate that certain research approaches are not feasible. This posture of judging credibility rather than prejudging correctness is one of the ways that the BPP Project is open to visionary concepts while still sustaining credibility.

Empirical

Since this NASA Project is interested in advancements that can eventually lead to new technology, and since empiricism is necessary to validate theories, there is a decided preference toward empirical observations over purely analytical studies—*all other factors being equal*. Experiments, being hardware, are considered closer than theory to becoming technology. Also, experiments are considered a more direct indicator of how nature works. Theories are interpretations to explain observations of nature, while the empirical data *is* nature.

Published

The final requirement within the BPP strategies is that the research findings are 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 following the same dead-ends. In the course of the BPP Project, it was learned that more than one organization had independently pursued similar dead-end research ideas, because none of these organizations ever published credible results to educate the greater community.

Evaluation System

Adaptation of Multiplicative Evaluation Method

The actual BPP evaluation process was adapted from a procedure developed by Banks of NASA Glenn Research Center (appendix B). This procedure starts by assembling a team of representative experts and customers of the desired technology. Through brainstorming and voting, the team defines the relevant evaluation criteria, and narrows these criteria down to a minimal list with weighting factors for each. The group also must distinguish between criteria that are mandatory (criteria that *must* be met), and those criteria that are just enhancing. It is essential that both the customers and the practitioners of the research concur with the criteria before applying the criteria to evaluate options. Appendix B offers a synopsis of this more general process as a guide to other research Project Managers.

A recent example of an application of Bank's process is the selection of the replacement thermal control materials for the Hubble Space Telescope (Townsend 1999). The process, as adapted for the BPP Project, is detailed next.

The key features applied to the BPP evaluation process from the Banks procedure are:

- Multiplicative scoring
- Familiar scholastic score gradations
- Independent, minimum criteria, concurred by team

Multiplicative scoring.—To quickly filter out substandard submissions, it is desired to have a feature whereby *any* failure to meet a *mandatory* criteria will eliminate the entire submission from competition. To provide this feature as an integral part of the 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* criteria will result in a total score of zero.

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

$$\text{Total Score} = \left(\frac{A}{N_A}\right)^a \left(\frac{B}{N_B}\right)^b \left(\frac{C + C_{\min}}{N_C}\right)^c$$

where

- A, B, C… represent criteria scores,
- a, b, c… are weighting factors, where 1 is the maximum value, and lower priorities are fractions of 1.
- N_A, N_B, N_C … are normalizing functions,
- $C_{\min}$ is a preset value to prevent the parenthetical term from equaling zero, thereby making criteria C nonmandatory.

To allow nonmandatory criteria into a multiplicative system, two different approaches can be employed. The easiest, and the way employed with the BPP process, is to just assign a score range for that criteria where the lowest possible score is not zero. The alternate approach, shown in the equation above, is to include a nonzero value in the criteria's equation. This second approach, however, complicates how normalizing functions are included.

To accommodate weighting factors, *exponents* are used analogously to the way *coefficients* are used for additive systems. It is recommended to use positive values equal to or less than 1 for these exponents, where an exponent of 1 represents the highest priority.

In practice, the effect of the weighting functions also is tied to the maximum-point-value that each criteria can attain. Therefore it is necessary that each criteria be normalized to the same maximum-point-value (the terms within the parentheses) prior to applying the weighting exponents. For normalization, which means equalizing each criteria prior to applying its weighting function, 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 nonmandatory 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 nonmandatory criteria have a nonzero value as their minimum possible score, then no additional constants or associated normalization functions are required.

Scholastic grading.—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 criteria, such as with the Small Business Innovative Research (SBIR) evaluations (NASA SBIR homepage: http://sbir.nasa.gov), 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 points)	=	Excellent or outstanding, meeting the criteria to the maximum amount.
B (3 points)	=	Good, or well above average.
C (2 points)	=	Average, or the score to use if there is no reason to score high or low.
D (1 point)	=	Poor or well below average.
F (0 points)	=	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 total final scores even with such limited gradations.

Team concurrence on criteria.—As mentioned before, the Banks evaluation process includes a stage where a team of representative experts and customers develop and eventually concur on the set of evaluation criteria. In the case of the BPP Project, the criteria were evolved over several years and amongst a variety of teams. These criteria reflect the BPP strategies described earlier. In the early stages of the Project, a "Product Definition Team" helped devise the first set of criteria (Millis 1996). This team included a mix of both customers of the research (NASA, DOD, DOE) and practitioners of research (physicists from a variety of organizations). A revised set of criteria were tested during a workshop in 1997 (Millis 1999b), and a further revised set (Millis 1999a) was finally adapted for use in the formal solicitation in 1999 and 2000. (NRA–99–LeRC–1). The lessons learned through this process were incorporated into the version presented in this report.

Two-Stage Evaluation Process

The actual research prioritization and selection process employed by the BPP Project follows a two-stage peer-review process. In the first stage, proposals are numerically scored relative to the criteria, and in the second stage a diverse suite of proposals are selected from the top-ranking candidates.

In the first stage of the review process, proposals are subject to scientific review by discipline specialists in the area of the proposal. Proposals are reviewed by a combination of in-house and selected external reviewers, with due regard for conflict of interest and protection of proposal information. These external reviewers may be from other NASA centers, other Government Labs, Universities, or Industry. Each proposal is evaluated by several evaluators, who assign numeric scores for each criteria of the BPP Project. By having multiple reviewers per proposal, the standard deviations in their scores can be used as an indicator of reviewer agreement.

To streamline this process, the proposal submitters were asked to encapsulate the key points of their proposal onto a summary form. This form was configured to pull together the information that pertained most directly to the evaluation criteria, and to present this information in a standard format to make it easier for the reviewers to find the key information. An example of the Research Increment Summary Form, being drafted for a future solicitation, is presented in appendix C.

The second stage of the review process involves the compilation and review of these scores by a smaller team of reviewers, again with due regard for conflict of interest and protection of proposal information. This team examines the top ranking proposals and checks for any disparity in review scores on any given proposal. After a suite of top ranking proposals is identified through this secondary review, a diversified set of research tasks is selected up to the limit of available funding.

A suite of different approaches is desired to "diversify the portfolio" since it is still too early to tell which research paths will lead most directly to breakthroughs. This means that the proposals selected for award may not necessarily be selected contiguously from the highest ranking set. For example, if the top two ranking proposals are both to perform an experimental test of a given theory, "A", and the third-ranking proposal is to test a different theory, "B", then it is the prerogative of this selecting team to award the best proposal on "A" and to the third-ranking proposal on "B" while skipping the second-ranking proposal on "A"; if this supports diversification of research.

Selection Criteria

To evaluate and select research topics for the BPP Project, the criteria described below were developed from the methods cited previously. These criteria are a derivative of the criteria used in the first BPP research solicitation (NRA–99–LeRC–1).

Of these, "Technical Relevance" relate directly to the BPP Grand Challenges, the "Credibility" criteria judge the reliability of the research, and the "Resource" criteria address affordability and timeliness. The total composite score is achieved by multiplying the individual criteria scores as illustrated in the equation at the end of this section. This provides the feature whereby a failure to meet any mandatory criteria (zero score on criteria) will result in a total score of zero.

As mentioned earlier, technical feasibility is not being judged with these criteria. Given the kind of fundamental investigations sought by this Project, it is difficult to reliably determine technical feasibility during a proposal review. Such an assessment would constitute a full research task itself. The burden of addressing feasibility, via a discriminating test, is a required part of the research proposal, and adherence to this requirement is addressed by criteria 7. Instead of judging technical feasibility, proposal reviewers are asked to judge if the task is based upon credible foundations, and leading to a result that other researchers will consider as a meaningful and reliable conclusion on which to base future investigations. This includes seeking tasks that can reliably demonstrate that certain research approaches are not feasible. This posture of judging credibility rather than prejudging correctness, is one of the ways that the BPP Project is open to visionary concepts while still sustaining credibility.

Technical Relevance:

1. Gain—Magnitude of performance improvement relative to all three of the BPP Grand Challenges, assuming the approach under consideration ultimately reaches fruition.
2. Empiricism—Does the topic deal with tangible physical effects or just theory?
3. Readiness—The present maturity of the topic/concept under study, as measured using the Applied Science Readiness Levels (appendix A).

4. Progress—Magnitude of progress to be achieved by the research task, as measured by the difference in the readiness now (criteria 3), and the anticipated readiness level to be reached upon completion of the task, as measured using the Applied Science Readiness Levels (appendix A).

Credibility:

5. Foundations—Based on credible references.
6. Contrasts—Compared to current credible interpretations.
7. Tests—Leading toward a discriminating test.
8. Results—Probability that the task will result in knowledge that will be a reliable foundation for future decisions.

Resources:

9. Triage—Will it be done anyway or is it unique to this Project?
10. Cost—Funding required (reciprocal scoring factor).
11. Time—Time required to complete task (reciprocal scoring factor).

Composite equation.—The total composite score is achieved by multiplying the individual scores, as illustrated in the equation below. This has the feature whereby a failure to meet any mandatory criteria (zero score on criteria) will result in a total score of zero.

$$\text{Total Score} = G^{WG} E^{WE} \left(RN/_{RNV}\right)^{WRN} \left(P/_{PNV}\right)^{WP} CF^{WCF} CC^{WCC} CT^{WCT} CR^{WCR} TR^{WTR} C^{WC} TI^{WTI}$$

Criteria	Variable Name	Score Range	Equation	Normalizing Variable	Weighting Variable
1: Gain	*G*	0 to 4	$G = (GM + GS + GE)/3$	(1)	*WG*
Gain, goal 1—Mass	*GM*	0 to 4	-	-	-
Gain, goal 2—Speed	*GS*	0 to 4	-	-	-
Gain, goal 3—Energy	*GE*	0 to 4	-	-	-
2: Empiricism	*E*	1 to 4	-	-	*WE*
3: Readiness (now)	*RN*	0 to 15	-	$RNV = 15/4$ (2)	*WRN*
4: Progress	*P*	0 to 15	$P = RA – RN$	$PNV = 1/2$ (3)	*WP*
Readiness (after)	*RA*	0 to 15	-	-	-
5: Credible foundations	*CF*	0 to 4	-	-	*WCF*
6: Credible contrasts	*CC*	0 to 4	-	-	*WCC*
7: Credible tests	*CT*	0 to 4	-	-	*WCT*
8: Credible results	*CR*	0 to 4	-	-	*WCR*
9: Triage	*TR*	1 to 4	-	-	*WTR*
10: Cost (discrete bands)	*C*	0 to 4	-	(4)	*WC*
11: Time (discrete bands)	*TI*	1 to 4	-	(4)	*WTI*

Notes:

1. The 1/3, embedded in the Total Gain equation, is to normalize the total gain to a maximum value of 4.
2. The Readiness Normalization Value (*RNV*) is set to 15/4 so that a ranking of Technology Readiness Level 1 (score = 15) equates to a score of 4.
3. The Progress Normalization Value (*PNV*) is set to 1/2, so that the typical progress of incrementing up one level of the Applied Science Readiness Levels (appendix A) equates to a score of 2 (an average condition).
4. For Cost and Time, the scoring gradations are set to discrete bands rather that directly entering time or cost, and where the higher scores reflect lower cost and time. Specific scoring gradations examples are in appendix D, "Research Increment Evaluation Instructions."

Criteria explained.—The following text more thoroughly describes each prioritization criteria. To see how these apply to a review, including the scoring gradations, refer to appendix D, "Research Increment Evaluation Instructions."

Criteria 1: *Gain on goals*

This criteria grades how the proposal relates to all three of the BPP Grand Challenges, assuming that the concept behind the proposed increment of research ultimately reaches fruition. Each Grand Challenge is graded separately and the final Gain criteria involves the sum of these three subset scores.

It is mandatory that the proposed work seeks advances in science that are in some way relevant to the Project's three propulsion challenges or any critical issues or unknowns related to these goals. The scope is limited to further advances in *science* from which genuinely new technology can eventually emerge—to surpass the limits of existing methods—as opposed to further developments of known *technology*. This means that if the proposed work *only* builds on known technology, then it fails this criteria.

Criteria 2: *Empiricism*

Does the topic deal with tangible physical effects or just theory? Since this NASA Project is interested in advancements that can eventually lead to new technology, and since empiricism is necessary to validate theories, there is a decided preference toward empirical observations over purely analytical studies—*all other factors being equal*. Experiments, being hardware, are considered closer than theory to becoming technology. Also, experiments are considered a more direct indicator of how nature works. Theories are interpretations to explain observations of nature, while the empirical data *is* observed nature. The most desired research task is an experiment that is coupled with theory. Experiments that are backed by a sound theoretical foundation provide a means to numerically assess the utility and scalability of the effects beyond just a single demonstration experiment. The next preference is experimental work by itself; for example, to independently test a claimed anomalous effect. After that, the next preference is theoretical work by itself. Lowest on this priority scale is work that only involves comparative studies of existing approaches or literature searches.

Criteria 3: *Readiness*

This criteria measures the present maturity of the topic/concept under study, as measured against the Applied Science Readiness Levels (appendix A).

Criteria 4: *Progress*

This criteria measures the anticipated progress to be achieved by the research task, calculated by the numeric difference between the *before* and *after* readiness scores, as measured using the Applied Science Readiness Levels. The level *before* is simply criteria 3, Readiness. The level *after* is the readiness level expected to be achieved upon completion of the research task. Appendix A contains a full description of the meaning of these readiness levels.

Criteria 5: *Credible foundations*

This criteria grades how well the proposed work is grounded in credible foundations. The proposed work must be based in some way on data or theories that are in the peer-reviewed literature. Note: requiring reference citations is one of the techniques to filter out "fringe" submissions. This is a mandatory criteria, which means that a failure to meet this criteria (zero score) will result in a total score of zero.

Criteria 6: *Credible contrasts*

This criteria grades how well the authors articulate how their proposed work compares to existing credible interpretations, relative to the BPP Grand Challenges. This is to ensure that an idea is oriented toward the goals of the Project, and to ensure that the authors have done their homework on the existing literature. This not only checks for relevance to BPP, but also positions the work to address the next criteria of a discriminating test. Note: requiring reference citations is one of the techniques to filter out "fringe" submissions. This is a mandatory criteria, which means that a failure to meet this criteria (zero score) will result in a total score of zero.

Also, recall from the objectives of the BPP Project, that the perceived *correctness* of the author's alternative interpretations is *not* being judged with this criteria. Unless there is some obvious error, it is considered too difficult to reliably determine such feasibility during a proposal review. Such an assessment would constitute a full research task unto itself. The burden of addressing feasibility, via a discriminating test, is addressed by criteria 7. For this criteria, it is important that the author demonstrates an understanding of the current, credible interpretations that are cited and is able to contrast this prior knowledge to the approach they are offering.

Recall that the BPP Project examines emerging physics in the context of propulsion and power, and as such, there is latitude to consider alternative perspectives beyond that from *general* physics. Even though the current credible interpretations have already passed their own rigorous tests, this does not imply that such interpretations are a complete or best representation of the actual underlying physics, especially in the context of propulsion and power. Conversely, however, if the proposed interpretation has already been raised and dismissed in the open literature, then the author must cite these references and address the issues raised.

Criteria 7: *Credible tests*

This criteria judges how well the research advances the topic toward a discriminating test. It is required that the proposed work be leading toward a discriminating test or actually be a discriminating test. If a discriminating test can be completed within the budget and time guidelines requested of proposals, it is necessary that the test actually be proposed. Otherwise, it is sufficient to propose the design of an experiment for a make-or-break test, or to further advance a theory toward testable predictions.

This requires that the author must identify the critical make-or-break issues for their immediate area of investigation. Also, the proposed next-step must be consistent with the scientific method, with due consideration for the current status of the topic as specified by the author. Further note that, depending on the status of the proposed task, independent verification may be warranted. In such a case, the vested interests of the Principle Investigator must be taken into account. This is a mandatory criteria, which means that a failure to meet this criteria (zero score) will result in a total score of zero.

Criteria 8: *Credible results*

This criteria grades the expected fidelity of the conclusions to be reached at the end of the proposed task. Will the task result in knowledge that will be a reliable foundation for future research decisions?

Successful completion of the research task is defined as learning more about reaching the breakthrough, rather than actually achieving the breakthrough. Negative test results are considered progress. *What is required for successful completion is that the work reaches a credible resolution that is clearly communicated*. This criteria is judged on a combination of the realism of the proposed work, its cost and schedule, and on the credentials of the proposed research team and their facilities. If it is likely that the work can be completed within the funding and time allocations specified, and that the results will be accepted by other researchers as a credible foundation for future work, then a high score is warranted. Note too that, depending on the status of the proposed task, independent verification may be warranted. In such cases the vested interests of the Principle Investigator must be taken into account to ensure that there is no conflict of interest in the outcome of the device, phenomenon, or theory under test. This is a mandatory criteria, which means that a failure to meet this criteria (zero score) will result in a total score of zero.

Criteria 9: *Triage*

Will this research be done anyway or must this Project support it? This criteria addresses the possibility that the BPP Project can save its resources if the topic is likely to be explored without support of the BPP Project.

Criteria 10: *Cost*

This is a reciprocal scoring factor that addresses practical resource concerns. The more costly the work, the lower the overall score, *all other factors being equal*.

Criteria 11: *Time*

This is a reciprocal scoring factor that addresses practical resource concerns. The longer to reach a reliable conclusion, the lower the overall score, *all other factors being equal.*

Lessons Learned

Overall, the strategy and prioritization methods of the BPP Project were found to be effective, although areas of improvement have been identified. The Project strategy has met with positive reviews, the selection process has worked well in practice, and the publication of research proves that incremental progress can be affordably produced on BPP topics. Some of the improvements, particularly refinements to the research criteria themselves, have already been incorporated. Each of these lessons are explained in more detail next, along with descriptions for other improvements that were being implemented at the time that funding for the Project was deferred. The planned improvements, which included shifting the operation of the Project to a nonprofit consortium of government, university and industry, were intended to improve the ability of the Project to respond to the large number of researchers and research approaches, to identify the most pressing research issues needing attention, and to provide continuity to better direct progress. This is discussed in the "Revisions Initiated" section.

Strategy Lessons

The overall BPP Project Management strategy, specifically to identify diverse near-term issues and to concentrate on acquiring reliable information rather than grand claims, has met with positive reviews. Quoting from an independent review panel of the Space Transportation Research Program, of which the BPP Project was a subset:

"[The BPP] approach was unanimously judged to be well thought out, logically structured and carefully designed to steer clear of the undesirable fringe claims that are widespread on the Internet. The claim that the timing is ripe for a modest program of this sort was agreed to be justified: Clues do appear to be emerging within mainstream science of promising new areas of investigations. The team concurred that the 1997 BPP kickoff workshop did identify affordable candidate research tasks which can be respectably pursued, and that the proposed research prioritization criteria were a valid way to select from amongst these (and future) proposals. The program approach was deemed to be sound: emphasizing near-term progress toward long-term goals; supporting a diversity of approaches having a credible scientific basis; holding workshops to exchange ideas; solicit constructive criticism and assess progress; aiming toward testable concepts." (Merkle 1999)

Solicitation Process Lessons

Regarding the utility of the solicitation process, specifically the process formally implemented over 1999 and 2000 (NRA–99–LeRC–1), the following lessons were learned, each of which is explained in a subsequent paragraph:

- Multiple reviewers per proposal is vital
- Two-stage review process is effective

- Summary sheets are effective
- Some criteria needed rearranging (revisions already incorporated)

On topics as visionary as BPP, it is prudent to have numerous reviewers to provide well rounded assessments. For the 1999–2000 solicitation, at least 4 reviewers per proposal were secured for all 60 proposals, with virtually all proposals being scored by 5 reviewers. Although it was challenging to secure the volunteer services of the 50+ reviewers required for this coverage, having this many reviewers allowed several important functions to be employed. First, it was easy to avoid conflicts-of-interest by not having any proposals scored by people from their own, or competing, institutions. Next, it was possible to match the areas of expertise of the proposals and reviewers, which varied greatly since there is a wide span of physics underlying the BPP ambitions. And finally, it was possible to calculate the standard deviations of reviewers' scores to flag any disparate reviews for further scrutiny. This feature, of assessing the relative agreement amongst the reviewers of a given proposal, was found to be an effective tool for screening out questionable proposals and to identify problems with any reviews.

Having a two-stage review process was found to be effective. The first stage allowed each proposal to receive a thorough review, while the second stage allowed a smaller team to effectively compare the proposals against one another. The first stage produced a large amount of data that was, by itself, not sufficient to identify the best proposals. Even after this data was analyzed to obtain an average score for each proposal and the value for the standard deviation between the reviewers' scores, further discussion was required to effectively review and distill these results. For this, a smaller team of reviewers was assembled. This team consisted of 10 government employees from multiple government labs, including NASA, the Department of Energy, and the Department of Defense. With such a smaller, but still diverse, group of reviewers, it was relatively easy to sort through the total scores, evaluate the disparate reviews, and then select a diverse suite of research from top-ranking proposals. This second-stage of the process required about 2 1/2-days.

The use of proposal summary sheets, like that in appendix C, was also found to be effective. These are the forms where the submitters encapsulate the key points of their proposal to streamline the review process. This made it very easy to quickly filter out noncompliant proposals and to focus on the key points needing further scrutiny. This might also become a useful tool for pre-proposal screening, where submissions of just the summary sheets could be used as a first screening, where only those submissions with acceptable summary sheets are invited to submit a full proposal. Also, a similar form was required from the reviewers, which made it easy to compile the scores. An example of a Research Increment Summary Form, specifically the one being drafted for a future solicitation, is presented in appendix C. This format is also being considered as the header for archiving the BPP research findings and opportunities in an electronic database.

Regarding the criteria themselves, some revisions were needed based on the lessons learned. First, the Readiness Scales needed to be revised. In the original solicitation, the applicability criteria (called "Directness" in the NRA solicitation) and the Scientific Method readiness scale were separate. When submitters and reviewers were scoring readiness, they often overlooked how readiness and applicability were linked, leading sometimes to contradictory assessments of readiness and progress. This is why the revised readiness scale has these two criteria explicitly interwoven. The other problem was that the "Lineage" criteria was found to be redundant to the "Probability of successful completion" criteria. Lineage has now been deleted since it is integral to what is now called "Credible Results" (criteria 8) described in this report.

Cost-Effectiveness

In regards to whether the strategic approach of the BPP Project was cost effective, data on the cost and products of the Project have been compiled into table 1 so that Project Managers of other similar topics can compare this data to their own practices. It is difficult to directly compare such productivity to other similar efforts because there are so few efforts where the data is publicly available. The one example for a related topic that is publicly available, in addition to the BPP Project, is from a congressionally mandated task to the West Virginia Institute for Software Research. Data for this single research task is also provided in table 1.

Summarizing the data in table 1, the BPP Project addressed 8 different research topics, produced 13 journal publications on those findings, and produced numerous other publications and information exchange opportunities for roughly 1-1/2 million dollars, spread out over 7 years. A summary of these and other research tasks has been recently compiled (Millis 2004). In addition, preliminary work for converting the operation of the Project into a non-profit consortium was supported.

TABLE 1—COST-EFFECTIVENESS DATA

NASA Breakthrough Propulsion Physics Project (1996–2002)[a]				**$1,554K**
Activity	**Publications**	Item Cost	Category Cost	
Research			$721K	
Macaly: Experimental and theoretical investigations into quantum vacuum energy.	– 9 Journal Publications – Numerous Conference Publications	$283K		
Malloy: Experimental and theoretical investigations into superluminal quantum tunneling.	– 3 Journal Publications – Numerous Conference Publications	$170K		
Ringermacher: Experimental test of EM torsion theory.	– 1 Conference Publication	$100K		
Cramer: Experimental and theoretical test of transient inertia claims.	– 1 Conference Publication	$49K		
Robertson: Experimental Cavendish balance test of RF pumped superconductor claims.	– 1 Conference Publication	$37K		
Deck: Theoretical assessment of deep Dirac energy levels.	– 1 Journal Submission (in review)	$13K		
Fralic: Experimental test of Schlicher thrusting antenna claims.	– 1 Conference Publication	$5K		
Millis: Theoretical problem definition of "space drives.".	– 1 Journal Publication (publication fees)	$1K		
Miscellaneous research support purchases		$63K		
On-Site Contractor (3 yrs.) to handle unsolicited correspondence and Project documentation			$176K	
Consortium initiation			$163K	
Kick-off Workshop (1997)	– 1 Workshop Proceedings		$50K	
Program Support (overhead, averaged $63K/yr.)	– 1 Journal Publication – 6 Conference Sessions		$444K	
Congressionally Earmarked Project (2001–2001)				**$2,000K**
Institute for Scientific Research: Theoretical and experimental tests of Heaviside force for propulsion	– 1 Journal Submission (in review) – 1 Conference Publication			

[a]Of the $1,554K funds allocated to the BPP Project, $1,354K came from the Advanced Space Transportation Program managed at Marshall Space Flight Center, and $200K came from the Office of Space Science, managed by NASA Headquarters.

Areas Needing Improvement

Overall, the strategies and review process were found to be effective, but further improvements are warranted. Over the course of the Project it became apparent that there are many more published research options than have been credibly assessed, and there has been no overall comparative assessments of these options to identify the most promising and cost-effective research avenues. Even though a solicitation process inherently includes such a review, such solicitation reviews are limited to whatever proposals are received. It is likely that not all of the critical issues will be represented by quality proposals. To counter this situation, the following four strategies are under consideration:

- Conduct a study to contrast the BPP challenges and emerging physics to identify research connections.
- Establish an Internet-accessible comparative research database.
- Provide better networking with the geographically dispersed physics practitioners.
- Provide means for NASA to address those areas that are overlooked via in-house research capability.

Ideally, it would be useful to have an overall assessment of the emerging physics and how it compares to the goals of the NASA Project. This is a daunting task as it covers much diverse ground and relevant questions have only begun to be systematically raised. One approach is to divide this ambition into the three BPP Project goals and for each goal identify the most critical physics issues needing resolution. The closest example of such an analysis, to date, was an assessment of the Project's 1st goal—to eliminate the need for propellant mass (Millis 1997). Similar studies on the 2nd and 3rd goals are desired. Further still, it is desired to have an overall assessment that links the critical unknowns related to the Project's propulsion goals to unresolved issues of general physics.

To allow comparison of the variety of research options, it would be prudent to establish a comparative database of all proposals and research suggestions received. Since the relative value of these, with respect to the BPP Project, could be judged by the BPP selection criteria, these criteria would serve as the logical fields with which to summarize, sort, and comparatively rank this information. The Research Increment Summary form, like that shown in appendix C, could address this essential information to allow this kind of ranked archiving. Further links could also be added to provide access to the details of the research options. The implementation of this strategy is one of the features of the nonprofit consortium discussed in the "Revisions Initiated" section.

Even with such a ranked archival database, however, the information is still dependent on receiving quality proposals into the system. It is desirable to have a study that contrasts emerging physics to the goals of the BPP Project to identify the pertinent unknowns, critical issues, and curious effects that still require resolution. With the exception of the one paper that defined specific challenges for the first "Grand Challenge" of the BPP Project (Millis 1997), there have not been other assessments to identify the unresolved areas of physics that pertain to the BPP challenges. "Problem definition" studies for the second and third grand challenges are needed as well. One approach to provide the structure for organizing this type of complex interconnections is the "Traceability Tree" (figure 1) discussed in one of the prior BPP studies (Millis 1996), and further refined in a work that suggests applying the tools of fault-tree analysis to this challenge (Zampino 2003).

Another area needing improvement is to provide better access to the various researchers that can add value to this topic. The difficulty is that such researchers are geographically scattered across the world and do not necessary know which BPP problems need more rigorous attention. The Internet database is one means to improve this feature, as are the problem definition studies. But even beyond these strategies, it would be prudent to simply increase the number of capable researchers that are interested in addressing the topic. Even though this outreach has been happening over the course of the Project, a more deliberate

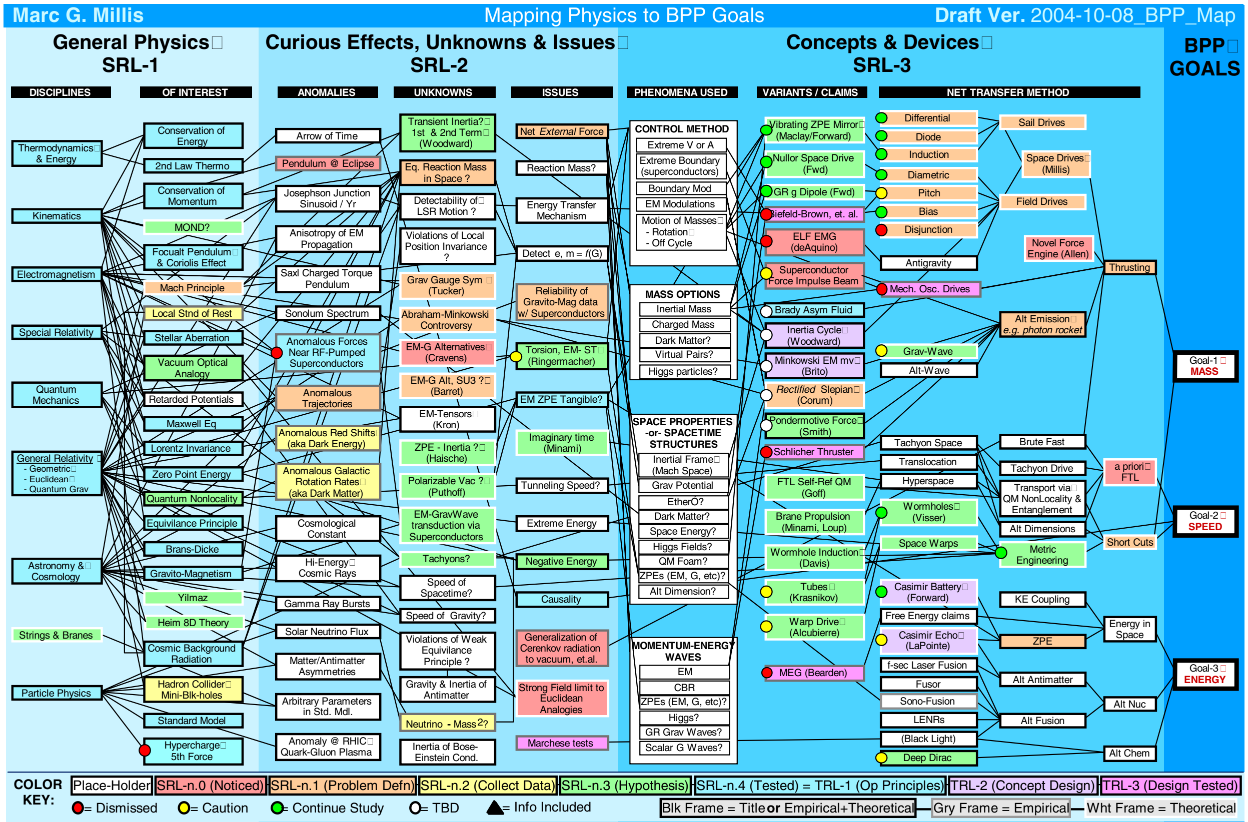

Figure 1.—Breakthrough propulsion physics project traceability map. This figure is presented here only to introduce the concept of the Traceability Map as opposed to being a substantive example. It is a work in progress. Conceptually, this tool provides a framework against which to plot the correlations between emerging physics and the desired propulsion breakthroughs. In the left-most column, the various disciplines of physics are listed, which then branch out into specific items of interest relative to propulsion physics. In the right-most column, the desired goals of the project are listed, which then branch out into a variety of hypothetical concepts for achieving the desired effects. In-between these two extremes are where the relevant effects, unknowns, and issues are posted. These are linked to the physics and to the propulsion concepts with which they pertain. When done thoroughly, this linking can determine which effects, unknowns, and issues warrant the most attention to help prioritize research. In the interim it simply serves as a way to plot the various activities and their relations. In the color version, color-coding indicates the progress of each block relative to the scientific method readiness levels described in appendix A.

attempt to interest more physics students in this endeavor would increase the overall progress toward answering the BPP Project challenges.

Many of these improvement strategies were intended to be addressed with the implementation of a research consortium, which is discussed next.

Revisions Initiated

Traceability Map

One of the areas needing improvement was to have a better way to identify the physics issues that pertain most to the goals of breakthrough propulsion. One tool being explored by NASA to address this need is the “Traceability Map” shown in figure 1. This map provides a means to compare the links between topics in emerging physics and the critical issues related to the desired propulsion goals. By applying techniques similar to a fault tree analysis to such a mapping, the topics that have the *most* importance can be more easily identified (Zampino 2003). Such a framework can also serve as a map against which to plot known research. Specifically, if a particular item (block on the map) has been, or is being investigated, links to that more detailed information can be added to the block. In this way, the map becomes an introductory reference to grasp the range and diversity of research related to the topic.

Research Consortium

To more easily access the large number of geographically dispersed participants in BPP research and to utilize the unique advantages of government, universities, and industry, a Breakthrough Propulsion Physics Research Consortium is the next evolution planned for the BPP Project. In 2002, a Cooperative Agreement between NASA and the Ohio Aerospace Institute (OAI) was established to implement this consortium. OAI is a nonprofit organization whose charter involves fostering collaboration between government, universities, and industry on the sciences and technologies of mutual interest.

The consortium will consist of an Advisory Council, invite membership from the large number of geographically dispersed participants in BPP research, will conduct research solicitations, provide management oversight of selected research, and maintain an electronic database of research proposals and results. The planned functions of the consortium are briefly described next.

Although on hold due to the deferment of BPP funding, many of the operating methods for the consortium have already be set into motion. Should the NASA funding resume, or funding be secured from another source, the consortium can be readily reactivated.

BPP Advisory Council.—It is envisioned that the Advisory Council would consist of several renowned representatives of the physics community; a representative from each of the following government organizations: NASA, the Department of Defense, and the Department of Energy; a representative for any other consortium sponsors; and an administrative, nonvoting, member from OAI. The BPP Advisory Council will manage reviews, solicitations, and selections of research tasks; manage the electronic database of the consortium; oversee workshops to foster information exchange on propulsion physics; and propose additions, replacements or terminations of members of the consortium. The Advisory Council’s recommendations will be formalized through a voting membership structure, as will decisions regarding activities funded with resources from multiple nongovernment sources.

BPP consortium membership.—In addition to NASA as the prime sponsor for the BPP consortium, other groups and individuals may become participating members, subject to the recommendation and approval of the Advisory Council. There would be different levels of membership, including Sponsors and the Researcher Network. Membership can include participating universities, industries, other government organizations, or individual researchers. Participation may be as a sponsor or as a recipient of research funds. Also, a network of BPP researchers will be part of this membership. This network can be geographically dispersed and will consist of established researchers who can provide reviews of materials sent to the BPP consortium, as well as identify and/or propose candidate research. Intellectual Property and Nondisclosure Agreements for the members will be patterned after methods used in existing research consortia managed by the OAI.

BPP consortium research solicitations.—The consortium would issue regular, formal research solicitations. All research submissions will be evaluated by a peer review using a two-stage review process similar to the first BPP research solicitation (NRA–99–LeRC–1). In the first stage, a network of reviewers from members of the consortium, will numerically score the proposals through the Consortium’s electronic submission and review database, which will use the BPP Research Prioritization Criteria. In the second stage, the numeric results will be compiled by the consortium to indicate the top ranking candidates. These results will be submitted to the Advisory Council for their review. The Advisory Council will select which tasks shall get consortium support and make recommendations to the

NASA BPP Project Manager for when NASA in-house tasks could be supported outside the consortium. The review process for proposals will assess credibility of the proposed research, not the likelihood that the areas of investigation will produce a breakthrough. Even research that fails to produce a breakthrough has the valuable consequence of identifying where future investments are not warranted. The Advisory Council is at liberty to not select any tasks in the event that no credible research proposals are received.

BPP electronic submission and review database.—To enable the participation of geographically dispersed members, and as a means of providing "clearinghouse" information about BPP research, the BPP Research Consortium would implement an electronic database. The database will be accessible through the Internet, regardless of the user's platform. Appropriate security measures will be incorporated to ensure that access levels are aligned with user categories. The publicly-accessible portion of the database will list currently sponsored research, a bibliography of completed research articles, and allow submission of research proposals, suggested topics, or research results. The Researcher-Network-accessible portion of the database will provide access to submitted research proposals, suggested topics, or research results so that these can be reviewed and scored. This portion of the database will be tailored to fit the specific Prioritization Criteria of the BPP Project and will have automatic ranking functions based on submission information and review scores. The Sponsor-accessible portion of the database will provide access to the results of the reviews, where research options are comparatively ranked. An example of how this summary information might be displayed, is included in appendix C.

BPP workshops and conferences.—When the BPP Research Consortium and its electronic submission/review database are established, regular BPP conference sessions can be reestablished and managed by the consortium. The electronic submission and review database will facilitate the peer-review functions needed to screen these conference paper submissions. Workshops will be convened as appropriate. For both workshops and conferences, proceedings or other forms of reference-able documentation are required.

Resumption of In-House Research

Another planned revision to the BPP Project is to reactivate NASA in-house research. The NASA in-house tasks are planned as a separate budget line item within the BPP Project to ensure that NASA has at least one BPP research task in addition to the tasks that are competitively selected through open solicitations. The candidate NASA in-house research tasks will be subject to the same peer-review selection process as those of the open solicitations. The BPP Research Consortium will conduct these reviews, and the BPP Project Manager will select the NASA in-house tasks after considering the recommendations from the Advisory Council. NASA tasks will be subject to the same conditions of all BPP research—that the research tasks are affordable, near-term, and credible research that will make incremental progress toward the Project's Technical Challenges. By having a sustained in-house capability, NASA will have the means to address approaches not covered by the externally proposed research. Such an activity also helps maintain continuity and core-competency at NASA for the BPP topic.

Concluding Remarks

On topics as visionary as seeking breakthroughs in propulsion physics, it is especially challenging to proceed in a credible and constructive manner. From the experiences of the BPP Project over its initial 7 years, some of the most useful strategies have been:

- 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.

- Putting the emphasis on the *reliability* of conclusions rather on than their implications.
- Having reviewers concentrate on the *reliability* of the information to be gained from research, rather than on whether the concept under investigation is feasible.
- Devising a numerical means to impartially compare research options and readily reject “fringe” submissions.
- Addressing a diversified portfolio of research approaches.

It is expected that many of these strategies could be adapted to other leading-edge research projects that face similar challenges.

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Appendix A
Applied Science Readiness Levels

Using the Science Method as a Research Management Tool

The following readiness levels were devised to provide a ranking system for *applied science* in an analogous manner to how the Technology Readiness Levels (Hord 1985) are used to rank relative maturity of *engineering* developments. Specifically, these Applied Science Readiness Levels (SRLs) consist of 3 stages for *applicability* (reflecting how research evolves from the more general understanding to the more specific applications), and within each of these three levels, 5 steps of the *Scientific Method* are repeated. This equates to 15 levels of relative maturity, with the most advanced level being equivalent to Technology Readiness Level 1 (basic principles observed and reported).

APPLIED SCIENCE READINESS LEVELS

SRL–1.0	General Physics—Prescience (unconfirmed effect or new information connection)
SRL–1.1	General Physics—Problem formulated
SRL–1.2	General Physics—Data collected
SRL–1.3	General Physics—Hypothesis proposed
SRL–1.4	General Physics—Hypothesis tested and results reported
SRL–2.0	Critical Issues—Prescience (unconfirmed effect or new information connection)
SRL–2.1	Critical Issues—Problem formulated
SRL–2.2	Critical Issues—Data collected
SRL–2.3	Critical Issues—Hypothesis proposed
SRL–2.4	Critical Issues—Hypothesis tested and results reported
SRL–3.0	Desired Effect—Prescience (unconfirmed effect or new information connection)
SRL–3.1	Desired Effect—Problem formulated
SRL–3.2	Desired Effect—Data collected
SRL–3.3	Desired Effect—Hypothesis proposed
SRL–3.4	Desired Effect—Hypothesis *empirically* tested and results reported (equivalent to **TRL 1:** Basic principles observed and reported)

Three stages of applicability.—The scope of scientific research can span from the very general, broad-sweeping considerations all the way down to specific details of a given application. The more focused that a research increment is toward a desired application, the greater its applicability level, as reflected in the following 3 stages:

Stage 1: *General physics*
The research topic deals with general underlying physics related to the desired application.

Stage 2: *Critical issues*
The research topic deals with an immediate unknown, critical make or break issue, or curious effect relevant to the desired application.

Stage 3: *Desired effects*
The research topic deals with a specific effect or device for answering the goals of the application.

***Scientific method*.—**Within each of these ranges of focus, the scientific method can gauge progress. The following definitions for the steps of the scientific method have been tailored to reflect *applied* research progress. Since applied research implies a tangible product, these steps distinguish between *empirical* approaches (those based on the emergence of empirical effects) and *theoretical* approaches (those based on theory). The most significant distinction is with the final stage, where the hypotheses are tested. For applied science, again emphasizing a tangible product, this final stage can only be satisfied with an *empirical* test. Another noteworthy distinction from the common definitions of the Scientific Method is the inclusion of the zeroth step. This gives a placeholder for emerging opportunities that have not yet been addressed.

Step 0: Prescience

Empirical: Observations of an unconfirmed anomalous effect have been reported (includes observations of natural phenomena or claims of unverified devices), or

Theory: A correlation between a desired goal (or unsolved problem) and the existing knowledge base has been articulated.

Step 1: Problem formulated

Empirical: An experiment has been defined that can collect the data required to isolate and characterize the anomalous effect, or

Theory: A goal (or problem) has been defined specifically enough to identify the specific remaining knowledge gaps toward achieving the goal (or solving the problem).

Step 2: Data collected

Empirical: Data has been collected and analyzed from experiment to isolate and characterize the anomalous effect, or

Theory: The relevant data to fill the critical knowledge gaps, identified in the previous step, have been collected through experiment, observation, or mathematical proof (this level includes assessments of theory using mathematical analysis).

Step 3: Hypothesis proposed

Empirical: A mathematical representation of the physical principles underlying an effect has been offered to explain the effect and predict additional (testable) effects, or

Theory: A mathematical representation of the relation between physical phenomena has been offered that addresses the goal (or problem) formulated previously.

Step 4: Hypothesis tested and results reported

The hypothesis has been tested by comparison to observable phenomena or by experiment sufficiently to determine if it appears viable, and the results reported. NOTE: In the context of applied research, testing of a hypothesis must be empirical; that means it must be done by comparison to observable phenomena or by experiment, rather than just by mathematical proof. Although mathematical proof can be used to test the consistency of a theory against the known science, such a mathematical test alone is not sufficient to warrant achievement of Step 4. Instead, a mathematical test of a theory reflects achieving Step 2.

Once a research objective has been defined and its maturity identified according to the Applied Science Readiness Levels, the next logical increment of research would be to advance the topic the next level of the Applied Science Readiness Levels.

Appendix B
Basic Research Evaluation System

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This appendix summarizes the decision-making process devised by Bruce Banks of the NASA Glenn Research Center that was used as a foundation for the research prioritization process of the Breakthrough Propulsion Physics Project. It is designed for the prioritization of plans where there are many issues of varying importance and degree of influence that tend to make such selections complex. A multiplicative scoring method is employed, based on principles of probability and statistics, that is significantly more sensitive than additive scoring methods.

This procedure starts by assembling a team of representative experts and customers of the desired technology. 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 customers for the research concur with the criteria and it is crucial that the other participants concur before applying the criteria to actually evaluate the options.

A recent example of an application of this process is the selection of the replacement thermal control materials for the Hubble Space Telescope (Townsend, J.A., Hansen, P.A., McClendon, M.W., de Groh, K.K., and Banks, B.A. (1999), "Ground-based testing of replacement thermal control materials for the Hubble Space Telescope," In *High Performance Polymers*, vol. 11, pp. 63–79).

The process has the following advantages and characteristics:

- It is ideal for making the best decision when many complex and often conflicting issues must be considered.
- The process is applicable to prioritizing choices and making decisions on almost any topic such as:
  - Strategic planning (determining which of several potential plans is best)
  - Personnel selections or promotions
  - Career options
  - Contractor selections
  - Major purchases
- Every issue and every opinion is considered.
- One individual or a team can perform the process, but the team approach is recommended on those decisions where a team consensus on the final decision is paramount.
- The decision-making process results in excellent "buy-in" by those using the process, because all issues and everyone's opinions can be taken into account.
- The process employs multiplicative scoring that is significantly more sensitive to critical issues than additive scoring methods.
- The decisions are prioritized in a quantified manner.
- The resulting decisions are highly defendable.
- The process minimizes debate or skewing from overly-assertive people.
- The process can be performed remotely, without an actual team meeting, by simple, electronic transference of information files via e-mail.
- The mathematical methods of the process can be automated in software.

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. With these up for discussion, it is easier to start collecting a list of evaluation factors. When the committee discusses the evaluation criteria, every proposed criteria should be listed, regardless of how many people feel it is meritorious, because after all criteria are listed, individual or group voting will be done to determine the relative importance of each.

Evaluation criteria should be:

- Phrased in a positive sense (express as desired characteristics or freedom from undesirable characteristics)
- Phrased such that there is majority acceptance of the wording
- Independent of each other (no duplication of issues)
- Address single issues (issues should not contain the word “and”)
- Able to be numerically scored (or graded)
- Include all relevant issues

Relative Importance of Criteria

Once the criteria have been selected, the committee decides, by consensus or majority vote, what the most important criteria is. The “relative importance” of this criteria is assigned a value of 1. (Comment: The committee can decide that this is a fixed maximum and that no other criteria can be given a value greater than 1 by any committee member. This would prevent “skewing of the system” by any one individual.)

Voters decide their values for relative importance of each evaluation criteria knowing that:

- The most important criteria is rated 1, and is not changeable.
- Because the most important criteria has been rated 1, the relative importance of the remaining criteria will have proportional importance values between 0 and 1. For example, a criteria half as important as the most important criteria should be rated 0.5
- Values can be entered which have up to three decimal places to the right of 0.

Scholastic Grading

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 criteria, such as with the Small Business Innovative Research (SBIR) evaluations, 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 points)	=	Excellent or outstanding, meeting the criteria to the maximum amount.
B (3 points)	=	Good, or well above average.
C (2 points)	=	Average, or the score to use if there is no reason to score high or low.
D (1 point)	=	Poor or well below average.
F (0 points)	=	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 total final scores even with such limited gradations.

Span of Influence of Criteria

Each prioritization criteria is able to influence the overall score, from a possible maximum of outstanding to some lower level, depending upon how critical the criteria is. This includes identifying which criteria are mandatory, meaning those for which a zero-score would eliminate the option from consideration. For each criteria, the committee members determine the overall prioritization score that would result if a option were rated a perfect “4” with respect to every other criteria, but rated “0” for the specific criteria being considered. For example, when choosing to buy a car, the trim color could contribute to the overall desirability, but even if it were the worst color possible (score “0” for that criteria on that car), an overall good “3” rating would be appropriate for the whole car if all other criteria were outstanding (grade = “4”). In this example, the criteria identified as *trim color* would receive a *span of influence* value of “3.”

Composite Scoring Equations

After this step, the data can be tabulated and calculations performed to determine the normalizing coefficients for the composite scoring equation.

RELATIVE IMPORTANCE, SPAN OF INFLUENCE, AND NORMALIZING COEFFICIENTS

Criteria Scores	Relative Importance (score between 0 and 1)	Span of Influence (score on a scale of 0 to 4)	Normalizing Coefficient for Score	Normalizing Coefficient for Span of Influence
A1	R1	S1	k1	c1
A2	R2	S2	k2	c2
A3	R3	S3	k2	c3

The normalizing coefficients, kn and cn, mathematically scale scores for each criteria to assure the span of influence is properly accounted for within the 0 to 4 scoring scale. Or stated differently, the coefficients are set so that the parenthetical terms of the composite scoring equation have a maximum value equal to 4.

For this example, the basic composite scoring equation is as follows:

$$\text{Composite Score}(A) = (k1 \cdot A1 + c1 \cdot S1)^{R1}(k2 \cdot A2 + c2 \cdot S2)^{R2}(k3 \cdot A3 + c3 \cdot S3)^{R3}$$

Where *A* represents the option being scored, *An* reflects the score given for the n^{th} criteria on option *A*, and the other variables are as shown in the table above.

The mathematics to determine the normalizing coefficients can be automated in software.

Scoring of Candidates

Once the relative importance, span of influence, and normalizing coefficients are determined for each issue, then the candidates can be scored with respect to each of the criteria using the scholastic 4-point scale cited previously.

Appendix C
Research Increment Summary Form

REVIEW COLUMN

A: Submitted by:

Submission ID#:

Reviewer ID#:

B: Title of submission (for published results, list full reference citation here)

C: Submission Type:

- Published Results (blocks R,S optional))
- Proposal (Skip blocks E,F)
- Suggested Inquiry (Fill in at least A-D,G,K,N)

SCORES

Calculated Composite: F D C B A 0-4

Subjective Score: F D C B A 0-4

Automated Pre-Score: F D C B A 0-4

D: Central issue, unknown, or observation under study (paragraph)

E: Findings [Only required when citing published results] (paragraph & check box F)

F: Sequels Expected? No Maybe Yes

SEQUELS JUSTIFIED? Red Yel Grn

G: Relevance to Project (paragraph & check-boxes H)

H: Ultimate Improvement

Mass 0 1 2 3 4

Speed 0 1 2 3 4

Energy 0 1 2 3 4

RELEVANCE

1: Gains toward Goals?

M 0 1 2 3 4 0-4

S 0 1 2 3 4 0-4

E 0 1 2 3 4 0-4

2: Empiricism S T E T&E 1-4

I: Increment of work proposed or reported (paragraph & check boxes J,K,L,P,S)

J: Type of Research: Study Theory Expmt Ex&Th

KL: Readiness & Progress

K: Before **L:** After

=TRL2 =TRL2

Desired Effects: Test, Hypoth, Data, Defn, PreSci

Critical Issues: Test, Hypoth, Data, Defn, PreSci

General Phys: Test, Hypoth, Data, Defn, PreSci

Science Readiness Levels

3-4: Readiness & Progres

Before After

=TRL2 =TRL2

Desired Effects: Test, Hypoth, Data, Defn, PreSci

Critical Issues: Test, Hypoth, Data, Defn, PreSci

General Phys: Test, Hypoth, Data, Defn, PreSci

(4) Calculated Progres: F D C B A 0-4

M: Contrasting or skeptical challenges to the proposed (or reported) approach (paragraph)

N: Founding References (list)

O: Representative Graphic (optional)

P: Related Disciplines (Check all that apply)

- Basic Motion & Energy
- Electromagnetism
- Special Relativity
- Quantum Mechanics
- QM: Vacuum
- QM: Non-Locality
- QM: Gravity
- Particle Physics
- Micro/Nano Physics
- Cryogenic Physics
- Superconductors
- Cosmology/Astron
- General Relativity
- GR: Metric Engr
- GR: GravitoMagnetic
- Strings/Branes

CREDIBILITY

5: Foundations F D C B A 0-4

6: Contrasts F D C B A 0-4

7: Testability F D C B A 0-4

8: Results F D C B A 0-4

Q: Contrasting or Skeptical References (list)

R: Prior publications to reflect proposed researchers' qualifications (list)

S: Resources

Triage: Likely other ? BPP unique

Cost: $ K

Time: =3y 2y 1y <1y

RESOURCES

9: Triage: D C A 1-4

10: Cost F D C B A 0-4

11: Time (yrs) D C B A 1-4

T: Performing Organization

U: Other Sponsors (optional)

Appendix D
Research Increment Evaluation Instructions

The following text more thoroughly describes each prioritization criteria, including the scoring gradations. This text is presented in the context of reviewer instructions, to more clearly convey how these criteria would be employed during a review. The descriptions that follow also identify which criteria are mandatory and which are not.

Criteria 1—Gain on Goals

This criteria grades how the proposal relates to all three of the BPP Grand Challenges, assuming that the concept behind the proposed increment of research ultimately reaches fruition. Each Grand Challenge is graded separately and the final Gain criteria involves the sum of these three subset scores.

It is mandatory that the proposed work seek advances in science that are in some way relevant to the Project's three propulsion challenges or any critical issues or unknowns related to these goals. The scope is limited to further advances in *science* from which genuinely new technology can eventually emerge—technology to surpass the limits of existing methods—as opposed to further developments of known technology. This means that if the proposed work only builds on known technology, then it fails this criteria.

For each of the three Grand Challenges, specify which of the statements best describes the ultimate achievable performance of the concept being addressed by the proposal, while entertaining the assumption that a final embodiment ultimately functions as desired. Granted, it may be difficult to predict this ultimate impact since the concepts may be far from fruition or the concept may appear not to be viable. For grading this criteria, assume for the moment, that the concept is viable and will reach fruition. Other criteria will grade readiness and credibility.

Note that this is a mandatory criteria, which means that a failure to meet this criteria (a zero score on all three subcriteria) will result in a total score of zero. Since the scores for all three Grand Challenges will be added, it is only mandatory that one of the three goals be addressed.

BPP Grand Challenge 1: Mass.—Discover new propulsion physics that eliminates or dramatically reduces the need for propellant.

Scoring gradations:

0 Not applicable to this goal (default answer if no answer specified).
2 Applicable, but potential impact unknown.
3 Intended to significantly reduce propellant requirement.
4 Intended to eliminate the need for propellant and the need for directed energy. (The term, "directed energy" means any form of energy sent from a central location such as from the Earth or Sun.)

BPP Grand Challenge 2: Speed.—Discover how to circumvent existing speed limits to dramatically reduce transit times.

Scoring gradations:

0 Not applicable to this goal (default answer if no answer specified).
2 Applicable, but potential impact unknown.
3 Intended to eliminate speed constraints caused by limits of propellant or energy supply.
4 Intended to circumvent the light speed limit.

BPP Technical Challenge 3: Energy.—Discover new energy physics to power these propulsion devices at levels sufficient for interstellar flight.

Scoring gradations:

0 Not applicable to this goal (default answer if no answer specified).
2 Applicable, but potential impact unknown.
3 Better energy conversion physics, but still limited to a consumable onboard supply.
4 Intended to provide energy sources and conversion methods accessible in flight.

Criteria 2—Empiricism

Does the topic deal with tangible physical effects or just theory? Since this NASA Project is interested in advancements that can eventually lead to new technology, and since empiricism is necessary to validate theories, there is a decided preference toward empirical observations over purely analytical studies—*all other factors being equal*. Experiments, being hardware, are considered closer than theory to becoming technology. Also, experiments are considered a more direct indicator of how nature works. Theories are interpretations to explain observations of nature, while the empirical data *is* nature. The most desired research task is an experiment that is coupled with theory. Experiments which are backed by a sound theoretical foundation provide a means to numerically assess the utility and scalability of the effects beyond just a single demonstration experiment. The next preference is experimental work by itself; for example, to independently test a claimed anomalous effect. The next preference is theoretical work by itself. Lowest on this priority scale is work that only involves comparative studies of existing approaches or literature searches.

Scoring gradations:

1 Comparative study, data collection, or literature search.
2 Theoretical work only, without empirical investigations.
3 Experimental tests or empirical observations only.
4 Experiment or empirical observations coupled with theory.

Criteria 3—Readiness

This criteria measures the present maturity of the topic/concept under study, as measured against the Applied Science Readiness Levels. This criteria and criteria 4 are answered jointly in the BPP Readiness Level Scoring Gradation Table, shown below. The specific value entered for this criteria is the score (left column) corresponding to the appropriate readiness level (right column).

Criteria 4—Progress

This criteria measures the anticipated progress to be achieved by the research task, as measured by the numeric difference between the *before* and *after* readiness scores. The *before* level is simply criteria 3, Readiness. The *after* is the readiness level expected to be achieved upon completion of the research task. Both of these use the numeric scores associated with Applied Science Readiness Levels, shown in the Progress Scoring Gradation Table below.

Scoring gradations for criteria 3 (readiness) and criteria 4 (progress).—Using the BPP Applied Science Readiness Level Table below, specify the status to be achieved upon completion of the proposed task ("after"), and its status today ("before") for the work being proposed. Answer this question *within the limits of the specific increment of research being addressed* by the proposal. Note that "successful completion" is defined as completing the proposed work and learning more about reaching the breakthrough, rather than actually achieving the breakthrough. Negative test results are still progress.

Important note: Be sure that the score value assigned for *after* is greater than or equal to the score value for *before*, and furthermore, note that an *equal* value for both *before* and *after* means *zero* progress, which will fail the proposal. Only score this way if, indeed, the proposal is *not* offering any real progress. Appendix A contains a full description of the meaning of these readiness levels.

BPP READINESS LEVEL SCORING GRADATION TABLE

Score *Before* (Readiness)	**Score *After***	**BPP Applied Science Readiness Levels**
16	16	**≥ TRL–2:** Application conceptual design formulated (or higher readiness level)
		Applicability Stage 3—Desired Effect: Deals with a specific effect or device for producing net force, motion, or energy-exchange that addresses the BPP Grand Challenges
15	15	**SRL–3.4** Desired Effect—Hypothesis tested empirically and results reported (equivalent to **TRL–1** Basic principles observed and reported)
14	14	**SRL–3.3** Desired Effect—Hypothesis proposed
13	13	**SRL–3.2** Desired Effect—Data collected
12	12	**SRL–3.1** Desired Effect—Problem defined
11	11	**SRL–3.0** Desired Effect—Prescience (unconfirmed effect or new information connected to application)
		Applicability Stage 2—Critical Issue: Deals with a critical unknown, make or break issue, or curious effect directly related to the BPP Grand Challenges
10	10	**SRL–2.4** Critical Issue—Hypothesis tested empirically and results reported
9	9	**SRL–2.3** Critical Issue—Hypothesis proposed
8	8	**SRL–2.2** Critical Issue—Data collected
7	7	**SRL–2.1** Critical Issue—Problem defined
6	6	**SRL–2.0** Critical Issue—Prescience (unconfirmed effect or new information connected to application)
		Applicability Stage 1—General Physics: Deals with general physics pertinent to the BPP Grand Challenges in some manner
5	5	**SRL–1.4** General Physics—Hypothesis tested empirically and results reported
4	4	**SRL–1.3** General Physics—Hypothesis proposed
3	3	**SRL–1.2** General Physics—Data collected
2	2	**SRL–1.1** General Physics—Problem defined
1	1	**SRL–1.0** General Physics—Prescience (unconfirmed effect or new information connected to application)

Criteria 5—Credible Foundations

This criteria grades how well the proposed work is grounded in credible foundations. The proposed work must be based in some way on data or theories that are in the peer-reviewed literature. Note: requiring reference citations is one of the techniques to filter out “fringe” submissions. This is a mandatory criteria, which means that a failure to meet this criteria (zero score) will result in a total score of zero. Grade this criteria on how well the author identifies the most relevant references for their topic of investigation. A variety of specific guidelines are provided below on how this criteria maps to different proposed situations.

New, unreported effect.—In cases where an unconfirmed anomalous effect is being investigated (where the effect has not yet been independently reported nor confirmed in the peer-reviewed literature), the author must cite peer-reviewed references to indicate why the newly observed phenomenon would be considered anomalous. For example, in the case of anomalous thrust observations, where no reaction mass is readily apparent, it would be appropriate to cite references on momentum conservation. Note: requiring an admission, that the effect does not match the physics known to date, is one of the techniques to filter out “fringe” submissions. If the author assumes that the effect is genuine, despite not having been independently confirmed, or despite the appearance of contradicting known physics, then the author fails this criteria. For consistency in this case, it is expected that the readiness level specified under criteria 3, should match "Scientific Method Step 0 (prescience)," at whatever "Applicability Stage" fits the proposed work.

Known, unconfirmed effect.—In cases where an unconfirmed anomalous effect is being investigated, that has been reported in the literature, then these references must be cited. It is expected, in this case, that the existing literature would already list suspect causes for a "false positive" to help guide independent confirmation or refutation. For consistency in this case, it is expected that the readiness level specified under criteria 3, should match "Scientific Method Step 1 (problem formulated)," at whatever "Applicability Stage" fits the proposed work. If the suspect causes have not yet been articulated in the literature, then the readiness level specified under criteria 3, should match "Scientific Method Step 0 (prescience)." If the author assumes that the effect is genuine, despite not having been independently confirmed, or despite the appearance of contradicting known physics, then the author fails this criteria.

Known, confirmed effect.—In the case where the proposed work builds on an effect that has already been confirmed in the peer-reviewed literature, then the author must cite those references. For consistency in this case, it is expected that the readiness level specified under criteria 3, should match "Scientific Method Step 2 (data collected)," at whatever "Applicability Stage" fits the proposed work.

New theory.—In the case where work involves a theory that is not yet in the peer-reviewed literature, then the author must cite peer-reviewed references of the data or phenomena with which they are claiming consistency. It is not necessary that the author agree with current interpretations of this data, but it is mandatory that the theories are consistent with credible empirical evidence. For consistency in this case, it is expected that the readiness level specified under criteria 3, should match "Scientific Method Step 2 (data collected)," at whatever "Applicability Stage" fits the proposed work.

Known theory.—In the case where the proposed work builds on a theory that is already in the peer-reviewed literature, then the author must cite those references. For consistency in this case, it is expected that the readiness level specified under criteria 3, should match "Scientific Method Step 3 (hypothesis proposed)," at whatever "Applicability Stage" fits the proposed work.

Scoring gradations:

0	F	Fails to meet.
1	D	Poor or well below average.
2	C	Average.
3	B	Good or well above average.
4	A	Excellent or outstanding, meeting the criteria to the maximum amount.

Criteria 6—Credible Contrasts

This criteria grades how well the authors articulate how their proposed work compares to existing credible interpretations, relative to the BPP Grand Challenges. This is to ensure that an idea is oriented toward the goals of the Project, and to ensure that the authors have done their homework on the existing literature. This not only checks for relevance to BPP, but also positions the work to address the next criteria of a discriminating test. Note: requiring reference citations is one of the techniques to filter out "fringe" submissions. This is a mandatory criteria, which means that a failure to meet this criteria (zero score) will result in a total score of zero. Grade this criteria by how well the author identifies the most relevant literature and on their understanding of this literature.

Also, recall from the objectives of the BPP Project, that the perceived *correctness* of the author's alternative interpretations is *not* being judged with this criteria. Unless there is some obvious error, it is considered too difficult to reliably determine such feasibility during a proposal review. Such an assessment would constitute a full research task unto itself. The burden of addressing feasibility, via a discriminating test, is addressed by criteria 7. Instead, judge how well the author demonstrates an understanding of the current, credible interpretations that are cited, and the author's ability to contrast this prior knowledge to the approach they are offering. Recall that the BPP Project examines emerging physics in the context of propulsion and power, and as such, there is latitude to consider alternative perspectives beyond that from *general* physics. Even though the current credible interpretations have already passed their own rigorous tests, this does not imply that such interpretations are a complete or best representation of the actual underlying physics, especially in the context of propulsion and power. Conversely, however, if the proposed interpretation has already been raised and dismissed in the open literature, then the author must cite and address these references.

A variety of specific guidelines are provided below on how this criteria maps to different proposed situations.

New, unreported effect.—In cases where an unconfirmed anomalous effect is being investigated, where the effect has not yet been reported nor confirmed in the peer-reviewed literature, then the author must focus on comparing the effect with other, credibly known effects that might lead to a false-positive conclusion. In the prior criteria ("foundations"), the author had to acknowledge that the effect was anomalous. In this criteria, the author must demonstrate that they are astute to the conventional interpretations that must be tested to determine if the effect is genuinely new. References that cover these conventional interpretations must be cited. Also, the author must explain why the effect (if genuine) might be advantageous to the BPP challenges. If the author does not address the issue of ruling out the suspect causes, then the author fails this criteria.

Known, unconfirmed effect.—In cases where an unconfirmed anomalous effect is being investigated, that has already been reported in the literature, two different scenarios can apply. If the existing published report (that the author had to cite under "foundations") did *not* list a well-rounded set of suspect causes for a "false positive", then judge adherence to this criteria in the same manner stated above for the case of *new unreported effects*. On the other hand, if the existing published report did sufficiently list suspect causes for a "false positive," then the original report will suffice for the required "contrast" citation, but the author must still demonstrate that they are astute to the conventional interpretations necessary to determine if the effect is genuinely new. Also, the author must explain why their proposed investigation is more applicable than the existing or past investigations into the effect. In the case where *null* results were previously published, the author must cite these and explain why these prior tests were incomplete, in error, or why a reinvestigation is warranted. Reference citations for these other investigations are required. Also, the proposal must explain why the effect (if genuine) might be advantageous to the BPP challenges. If the author does not address the issue of ruling out the known, suspect causes, then the author fails this criteria.

Known, confirmed effect.—In the case where the proposed work builds on an effect that has already been confirmed in the peer-reviewed literature, then the author must explain why the effect might be relevant or advantageous to the propulsion challenges and why their investigation is more applicable to BPP than the prior or ongoing investigations into the effect. Reference citations for the confirmation publication, and for any prior or ongoing investigations, are required. If the author is challenging the current interpretations of the effect, then also judge this criteria by the guidance offered under "new theory," below.

New theory.—In the case where a theory is proposed that is not yet in the peer-reviewed literature, then it is mandatory that the new theories be compared to the contemporary theories that address the same phenomena. Reference citations for the contemporary theories are required. The comparison must explain why the new theory would be more advantageous to the propulsion challenges than the contemporary theories. Judge this criteria by how well the author demonstrates an understanding of the existing theories, and on the author's ability to identify the unresolved issues of both theories with respect to the goals of breakthrough propulsion or power. The author must also demonstrate a willingness to consider that their theory might be in error, by identifying its weak points. If the author assumes that their theory is correct, without it having been confirmed with rigorous empirical tests, then the author fails this criteria.

Known theory.—In the case where the proposed work builds on a theory that is already in the peer-reviewed literature, then the author must describe how their work will be more applicable to BPP than the prior or ongoing work on the same theory. Reference citations for the contemporary theories are required. If the theory is still under debate in the open literature, then the author must acknowledge its potential weaknesses, and cite references that highlight these issues. Judge this criteria by how well the author demonstrates an understanding of the known theory, its debated issues, and on the author's ability to identify how the theory applies to the goals of breakthrough propulsion or power.

Scoring gradations:

0	F	Fails to meet.
1	D	Poor or well below average.
2	C	Average.
3	B	Good or well above average.
4	A	Excellent or outstanding, meeting the criteria to the maximum amount.

Criteria 7—Credible Tests

This criteria judges how well the research advances the topic toward a discriminating test. It is required that the proposed work be leading toward a discriminating test or actually be a discriminating test. If a discriminating test can be completed within the budget and time guidelines requested of proposals, it is necessary that the test actually be proposed. Otherwise, it is sufficient to propose the design of an experiment for a make-or-break test, or to further advance a theory toward testable predictions.

This requires that the author must identify the critical make-or-break issues for their immediate area of investigation. Also, the proposed next-step must be consistent with the scientific method, with due consideration for the current status of the topic as specified by the author. Further note that, depending on the status of the proposed task, independent verification may be warranted. In such a case, the vested interests of the Principle Investigator must be taken into account. This is a mandatory criteria, which means that a failure to meet this criteria (zero score) will result in a total score of zero. A variety of specific guidelines are provided below on how this criteria maps to different proposed situations.

Unconfirmed effect (reported or not).—In cases where an unconfirmed anomalous effect is being investigated, a discriminating test must be suggested that could distinguish between possible conventional explanations or whether this is a genuine new effect. The task should propose to at least design a discriminating experiment, or to actually conduct an experimental test. The work will be considered more credible if the proposal concentrates only on the experimental methods rather than on speculating on a new cause for the effect. For consistency in this case, it is expected that the completion readiness level (the level anticipated after the task is completed), will be at least at "Scientific Method Step 1 (problem formulated)" for an experiment design, or "Scientific Method Step 2 (data collected)" if an experimental test is actually planned.

Known, confirmed effect.—In the case where the proposed work builds on an effect that has already been confirmed in the peer-reviewed literature, a logical next step would be to develop a theory to describe the anomaly. It would also be appropriate to propose a reconfiguration of the effect so that its propulsive or energy implications could be assessed. For consistency in this case, it is expected that the completion readiness level could be anywhere between "Scientific Method Step 2 (data collected)" through "Scientific Method Step 4 (hypothesis tested)," depending on the breadth of the proposed work.

Theory.—In the case where the proposed work deals with theory, it is mandatory that the new theories are at least matured to the point where mathematical models are offered (this is one of the "fringe" filters). Then, either further mathematical analysis, to predict testable effects; comparison to credible empirical observations; or experimental tests must be proposed that can bring the theory closer to a correctness resolution. An actual empirical test is preferred. For consistency in this case, it is expected that the completion readiness level could be anywhere between "Scientific Method Step 1 (problem formulated)" through "Scientific Method Step 4 (hypothesis tested)," depending on the breadth of the proposed work

Scoring gradations:

0 F Fails to meet.
1 D Poor or well below average.
2 C Average.
3 B Good or well above average.
4 A Excellent or outstanding, meeting the criteria to the maximum amount.

Criteria 8—Credible Results

This criteria grades the expected fidelity of the conclusions to be reached at the end of the proposed task. Will the task result in knowledge that will be a reliable foundation for future research decisions?

Successful completion of the research task is defined as learning more about reaching the breakthrough, rather than actually achieving the breakthrough. Negative test results are considered progress. What is required, for successful completion, is that the work reaches a credible resolution that is clearly communicated. If it is likely that the work can be completed within the funding and time allocations specified, and that the results will be accepted by other researchers as a credible foundation for future work, then a high score is warranted. Base this assessment on a combination of the realism of the proposed work, its cost and schedule, and on the credentials of the proposed research team and their facilities. If cost-sharing is mentioned in the proposal, judge this criteria on the total resources to be devoted, not just the amount to be charged to NASA. Consider the clarity and quality of the proposal and any prior publications from the authors as a good reflection of the clarity and quality of the final product. Note too that, depending on the status of the proposed task, independent verification may be warranted. In such cases the vested interests of the Principle Investigator must be taken into account to ensure that there is no conflict of interest in the outcome of the device, phenomenon, or theory under test. This is a mandatory criteria, which means that a failure to meet this criteria (zero score) will result in a total score of zero.

Scoring gradations:

0 F Fails to meet.
1 D Poor or well below average.
2 C Average.
3 B Good or well above average.
4 A Excellent or outstanding, meeting the criteria to the maximum amount.

Criteria 9—Triage

Will this research be done anyway or must this Project support it? This criteria addresses the possibility that the BPP Project can save its resources if the topic is likely to be explored without support of the BPP Project.

Specify which statement best describes the situation. Note that this is not a mandatory criteria. A minimum score here will only result in demoting an overall “A” grade to a “C” grade.

Scoring gradations:

1 (D) Certain to be credibly done without the support of the BPP Project.
2 (C) Unknown.
4 (A) Exclusively suited to the BPP Project.

Criteria 10—Cost

This is a reciprocal scoring factor that addresses practical resource concerns. The more costly the work, the lower the overall score, *all other factors being equal*.

Scoring gradations:

0 (F) If the cost is outrageous, then assign a failing grade.
1 (D) Cost ≥ $400K—Below average.
2 (C) Cost = $200K—Average.
3 (B) Cost = $100K—Good or well above average.
4 (A) Cost ≤ $50K—Excellent (but verify that this is realistic for the work offered).

Criteria 11—Time

This is a reciprocal scoring factor that addresses practical resource concerns. The longer to reach a reliable conclusion, the lower the overall score, *all other factors being equal*.

Scoring gradations:

1 (D) Duration ≥ 3 years—Below average.
2 (C) Duration = 2 years—Average.
3 (B) Duration = 1.5 years—Good or well above average.
4 (A) Duration ≤ 1 year—Excellent (but verify that this is realistic for the work offered).

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13. ABSTRACT ***(Maximum 200 words)***

To leap past the limitations of existing propulsion, the NASA Breakthrough Propulsion Physics (BPP) Project seeks further advancements in physics from which new propulsion methods can eventually be derived. Three visionary breakthroughs are sought: (1) propulsion that requires no propellant, (2) propulsion that circumvents existing speed limits, and (3) breakthrough methods of energy production to power such devices. Because these propulsion goals are presumably far from fruition, a special emphasis is to identify credible research that will make measurable progress toward these goals in the near-term. The management techniques to address this challenge are presented, with a special emphasis on the process used to review, prioritize, and select research tasks. This selection process includes these key features: (a) research tasks are constrained to only address the immediate unknowns, curious effects or critical issues, (b) reliability of assertions is more important than the implications of the assertions, which includes the practice where the reviewers judge credibility rather than feasibility, and (c) total scores are obtained by multiplying the criteria scores rather than by adding. Lessons learned and revisions planned are discussed.

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