A Perspective on the Heilmeier Catechism

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JUMP 2.0 Heilmeier Catechism Informational e-Workshop

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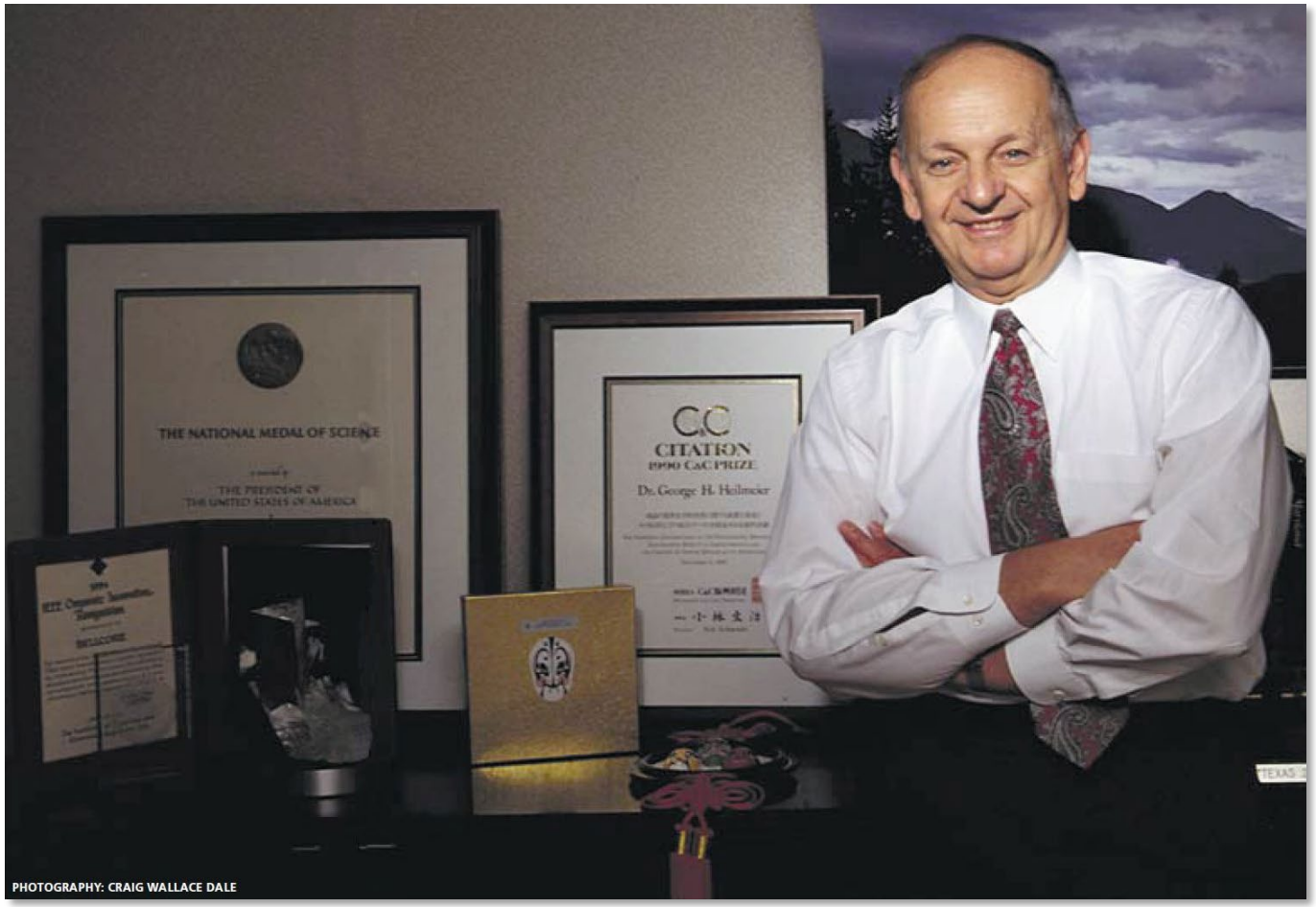
Who was George Harry Heilmeier?



- Inventor of the LCD at RCA (1958-1970)
- Assistant Director for Defense Research and Engineering, Electronic and Physical Sciences (1971-1975)
- Director of ARPA (1975-1977)
  - Stealth
  - IR imaging
  - Artificial intelligence
- VP and CTO Texas Instruments (1977-1991)
- CEO Bellcore (1991-1996)
- CEO SAIC (1996-1997)
- Many boards
  - National Academy of Engineering
  - Defense Science Board (DSB)

https://doi.org/10.1109/MSPEC.1997.591660

"Dr. Heilmeier, the son of a janitor, was the first member of his family to finish high school."

https://en.wikipedia.org/wiki/George_H._Heilmeier

1981 DSB Summer Study

We recommend that DoD adopt such a structured methodology for its decision making. Whether or not the figure of merit or the criteria are exactly adopted is not the point. What is important is that decision makers at all levels should ask the questions which are summarized in the investment strategy catechism:

- What are we trying to do?
- How is it done today and what are the limitations of current practice?
- What is new in my approach and why do I think I can be successful?
- Assuming success, what difference will it make to the user or in a mission area context?
- How long will it take; how much will it cost; what are the "midterm" and "final" exams?

The answers to these questions should be of great value in the resource allocation process.

Sincerely

George H. Heilmeier

George H. Heilmeier

1981 DSB Summer Study p.16

In the course of applying this scenario-based planning approach to the current DoD technology base program, the panel took into account all of the key elements of a comprehensive investment strategy for technology development, the "catechism":

- What is it? What is this effort trying to accomplish? (defining the technology sufficiently well to discriminate it from other similar technologies)
- Why is it important? Assuming success, what difference can it make to the user or in a mission area context? (taking into account the nature and limitation for current practice)
- What is the current status? What is the DoD program? What should it be? What is new about the proposed effort and why will this approach be successful?
- How long will it take? How much will it cost? What are the measures of success?

The panel recommends that the USDRE direct the Services and DARPA to incorporate such an approach in all future technology base planning and in POM guidance so that technologies funded through the allocation process would be more explicitly and consistently related to future operational needs.

Appears 2 more times in the report

Some Reflections on Innovation and Invention

Some Reflections on Innovation and Invention

George H. Heilmeier

One of the reasons technology transfer is problematic in the United States is that "innovation" is misunderstood among members of our technical community

George H. Heilmeier is president and chief executive officer of Bellcore, Inc. Dr. Heilmeier presented this Founders Award Lecture at the NAE Annual Meeting, 29 September 1992, in Washington, D.C.

I am grateful to the National Academy of Engineering for honoring me with the Founders Award. I confess to being awed by this award. The past recipients are among my heroes, and one has strong feelings of inadequacy in such company.

I'd like to share with you some personal experiences in the saga of liquid crystal display technology, along with some lessons they taught me about invention and innovation.

Looking at innovation on the personal level, it seems to me that most successful innovators have something in common with a successful hockey player. Wayne Gretzky once said that he doesn't skate to where the puck is. He skates to where it's *going to be*.

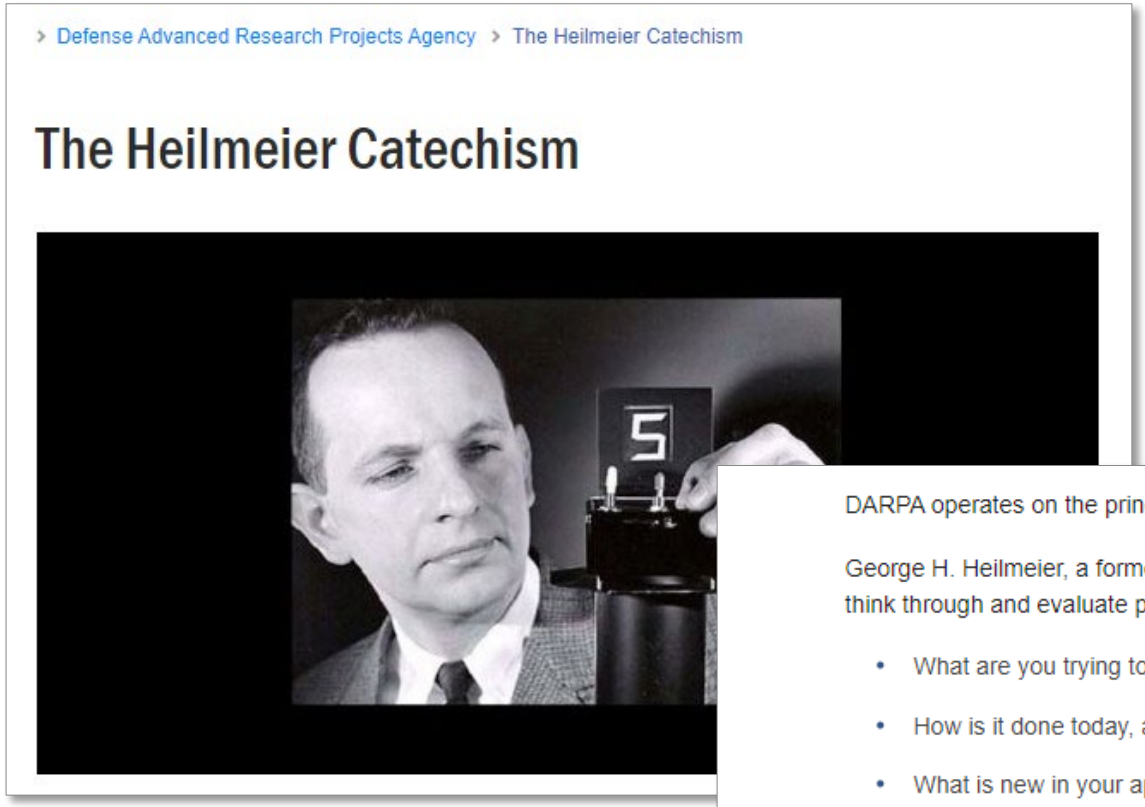
Innovation depends in part on anticipating where technology and its applications are going in the future and daring to trust that intuition. It's rooted in knowledge, skill, practice, experience, and the courage to act. It is often nurtured by the support and guidance of mentors whose intuition and motivation resonate with yours.

https://isi.edu/~johnh/TEACHING/CS551/ARCHIVE/Heilmeier92a.pdf

The "No Excuse" Policy represents a no-nonsense commitment to technology transfer. There are seven basic tenets:

- Formulate a "catechism":
  - What are you trying to do?
  - How is it done today? What are the limitations of the current practice?
  - What is new in your approach and why do you think it can succeed?
  - Assuming you are successful, what difference does it make?
  - How long will it take? How much will it cost? What are the midterm and final exams?
- Recognize productization as a necessary, crucial activity. Allocate capital and personnel to productization early. Technology, however good, is not enough.
- Identify receivers of the technology and ownership of the transfer early. Provide incentives on both sides.
- To the maximum extent possible, use common equipment in the development laboratory and in early manufacturing.
- Begin the transfer process immediately after demonstrating feasibility in the laboratory. Stay close to marketing.
- Manufacturing must prove the methods developed in the laboratory before initiating efforts to improve them.
- Keep the laboratory involved in the productization and manufacturing phases through completion of product qualification and achievement of cost and performance goals.

The DARPA website has yet a different version



https://www.darpa.mil/work-with-us/heilmeier-catechism

DARPA operates on the principle that generating big rewards requires taking big risks. But how does the Agency determine what risks are worth taking?

George H. Heilmeier, a former DARPA director (1975-1977), crafted a set of questions known as the "Heilmeier Catechism" to help Agency officials think through and evaluate proposed research programs.

- What are you trying to do? Articulate your objectives using absolutely no jargon.
- How is it done today, and what are the limits of current practice?
- What is new in your approach and why do you think it will be successful?
- Who cares? If you are successful, what difference will it make?
- What are the risks?
- How much will it cost?
- How long will it take?
- What are the mid-term and final “exams” to check for success?

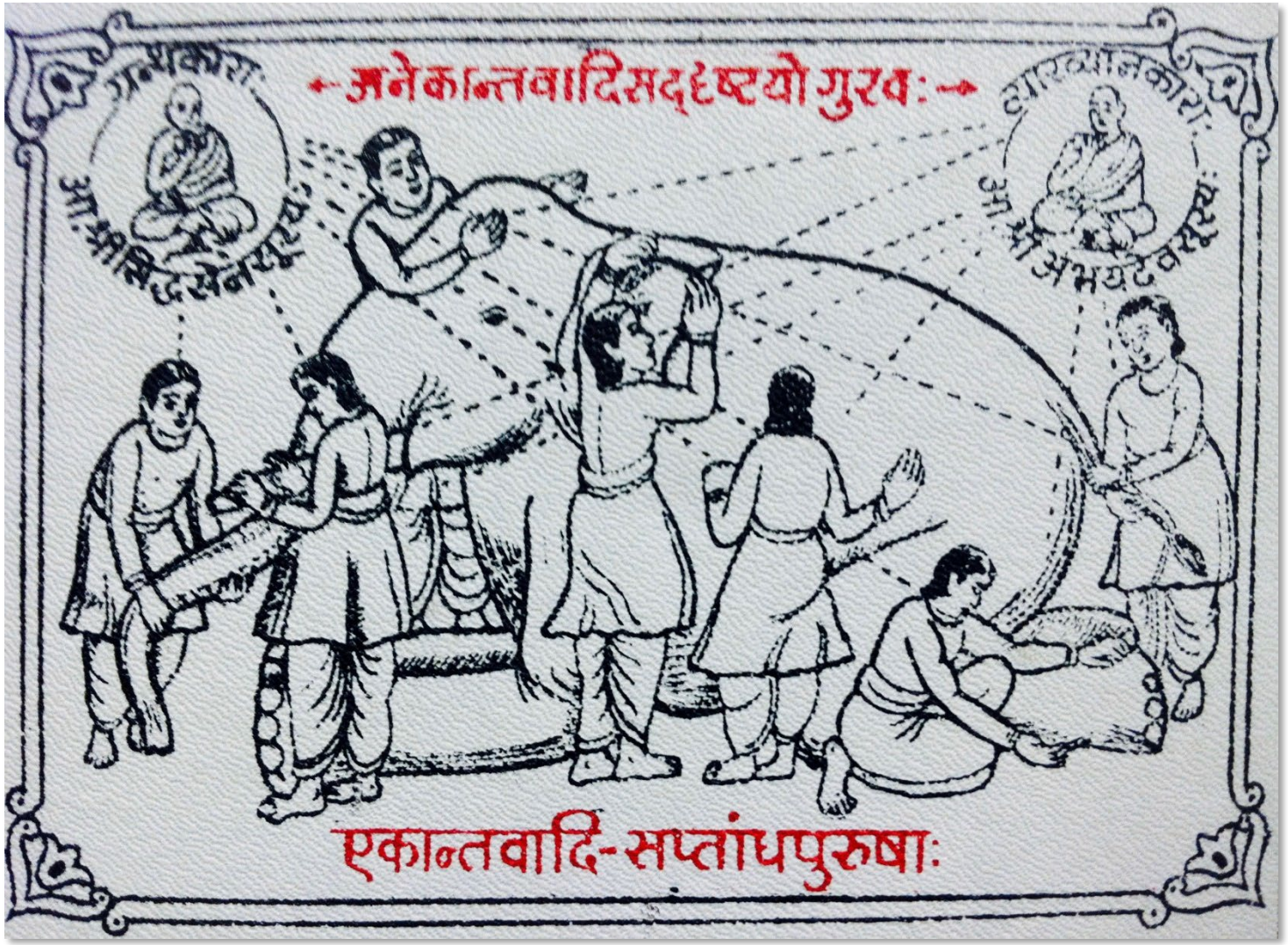
The Heilmeier Catechism(s)

DARPA Web / Wikipedia	13-900	DARPA Lanyard Card	Heilmeier DSB Study, 1981	Peter Highnam (ca. 2020)	IEEE Spectrum June 1994	Alternate Version	MTO's Favorite Version
1. What are you trying to do? Articulate your objectives using absolutely no jargon.	1. What are you trying to do? Articulate your objectives using absolutely no jargon.	1. What are we trying to do?	1. What are we trying to do?	1. What are you trying to do?	1. What are you trying to do? Articulate your objectives using absolutely no jargon.	1. What is the problem, and why is it hard? (Articulate your objectives using absolutely no jargon.)	1. What problem are you trying to solve?
2. How is it done today, and what are the limits of current practice?	2. How is it done today, and what are the limits of current practice?	2. How is it done today? Who does it?	2, How is it done today and what are the limitations of the current practice?	2. How is it done today and who does it? What are the limitations of the present approaches?	2. How is it done today, and what are the limits of current practice?	2. How is it done today, and what are the limits of current practice?	3. What are the key technical challenges that prevent that problem from being solved today?
		3. What are the limitations of the present approaches?					
3. What is new in your approach and why do you think it will be successful?	3. What is new in your approach and why do you think it will be successful?	4. What is new about our approach? Why do we think we can be successful this time?	3. What is new in my approach and why do I think I can be successful?	3. What is new about our approach, and why do we think it will succeed?	3. What's new in your approach and why do you think it will be successful?	3. What's new in your approach, and why do you think it will be successful?	4. What is the new approach and how can it overcome these technical challenges?
4. Who cares? If you are successful, what difference will it make?	4. Who cares? If you are successful, what difference will it make?	5. If we succeed, what difference do we think it will make?	4. Assuming success, what difference will it make to the user or in a mission context?	4. If we succeed, what difference will it make?	4. Who cares? If you're successful, what difference will it make?	4. Who cares? If you're successful, what difference will it make?	2. Why would solving that problem have a large impact (to the DoD)?
5. What are the risks?	5. What are the risks and the payoffs?				5. What are the risks and the payoffs?	5. What are the risks and the payoffs?	
6. How much will it cost?	6. How much will it cost? How long will it take?	7. How much will it cost?	5. How long will it take; how much will it cost; what are the "midterm" and "final" exams?	5. How long do we think it will take, and what are our mid-term and final exams? How much will it cost?	6. How much will it cost? How long will it take?	6. How much will it cost? How long will it take?	5. What is the program plan to solve the problem?
7. How long will it take?		6. How long do we think it will take? What are our mid-term and final exams?					
8. What are the mid-term and final "exams" to check for success?	7. What are the mid-term and final "exams" to check for success?				7. What are the mid-term and final "exams" to check for success?	7. What are the mid-term and final "exams" to check for success?	

More Heilmeier… and Some Others

DARPA Web / Wikipedia	Heilmeier (attributed)	Heilmeier (Interview, 2007)	Larry Lynn Questions (ca. 1998)	R. Leheny (ca. 2004)	T. Tether 2018
1. What are you trying to do? Articulate your objectives using absolutely no jargon.	1. What are you trying to accomplish?	1. What are you trying to do?		1. What is it you want to do and why should I care?	1. What is the problem? Describe it using no jargon.
2. How is it done today, and what are the limits of current practice?	2. How is it done now, and with what limitations?	2. How is it done today and what are the limitations of the current practice?	2. What is the technical challenge? (i.e., why DARPA?)	2. Why can't I do it now?	4. What are the technical and organizational transition challenges that need to be resolved to solve the problem and why are they difficult?
					5. How is your approach different from that tried previously?
3. What is new in your approach and why do you think it will be successful?	3. What is truly new in your approach which will remove current limitations and improve performance? By how much?	3. What's really new in your approach and why do you think it can succeed?	3. What's being done to solve the problem today that is not working. (i.e., why we believe that it would be beneficial to look at this area.) What will the world look like when this technology matures?		3. What is your approach to solve the technical and organizational transition challenges?
4. Who cares? If you are successful, what difference will it make?	4. If successful, what difference will it make?	4. Assuming you're successful beyond your wildest dreams, what difference does it make to national security?	1. What is the compelling military need for the technology?		2. If successful in solving the problem, what difference will it make and why, and who is the ultimate transition organization?
5. What are the risks and the payoffs*?		5. What are the risks that are involved here and do you have a risk reduction plan?			
6. How much will it cost?	7. How much will it cost?	7. How much is it going to cost?		4. Program plan, how does the work proceed, how do the different thrusts feed off each other, with ROM budget?	9. How much will it cost to achieve each milestone criteria and why?
7. How long will it take?	5. What are the mid-term, final exams or full scale applications required to prove your hypothesis? When will they be done?	6. How long is this going to take?			8. How long will it take to achieve each milestone criteria and why?
8. What are the mid-term and final "exams" to check for success?		8. What the midterm and final exams?		3. How will I measure progress on these challenges, and how are the related to the metrics?	6. What are the quantitative or qualitative milestone criteria that must be achieved in order to solve the problem?
	6. What is the DARPA "exit strategy"?				7. What is the priority order in which the specific technical milestones and organizational transition sub-challenges need to be resolved such that if the first priority cannot be solved, there would be no need to do the rest, and so forth and so on?

So, what is the Heilmeier Catechism, exactly?



https://upload.wikimedia.org/wikipedia/commons/e/ed/Medieval_Jain_temple_Anekantavada_doctrine_artwork.jpg

- No clear answer … because even George Heilmeier wasn't consistent
- Variants in the Heilmeier catechism are mostly trivial, but there are some meaningful differences
  - "What are you trying to do?" vs. "What problem are you trying to solve?"
  - "What are the risks and the payoffs?"
  - "Who cares?"
- Not explicitly part of any common formulation, oddly:
  - "What is the key insight?"
  - "Why now?"
  - "Why DARPA?"
  - "Technical challenges"
  - "DARPA-hard"
  - "Transition"

From the meeting invitation

You are invited to participate in a JUMP 2.0
Heilmeier Catechism Informational e-Workshop:

Tuesday, October 17, 2023
2:00 pm ET

Presented by
Dr. Dev Palmer
Deputy Director, Microsystems Techno

Please join us for this informational workshop that will be inclusive of an o

Registration is available

Register Me

The Heilmeier Catechism

George H. Heilmeier, a former DARPA director (1975-1977), crafted a set of questions known as the "H
proposed research programs.

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- How is it done today, and what are the limits of current practice?
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- Who cares? If you are successful, what difference will it make?
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https://www.darpa.mil/work-with-us/heilmeier-catechism

Where is the Heilmeier Catechism most useful?

DARPA uses it to assess the risk versus potential reward during program formation

- PMs are posing a problem, not proposing a solution
- The questions can help structure a program to solve the hardest part first (tech challenges)
- They also help clarify how to evaluate performer progress against goals (metrics)

The Heilmeier Catechism also is useful for structuring proposals to DARPA (and other agency) programs

- The questions can help organize and communicate your technical ideas
- But remember in this case you are proposing a solution to the DARPA-posed problem

Is it at all useful for measuring research progress?

- Some of the questions, yes, but nuances around the answers might be different … let's jump in and deconstruct them one by one …

What are you trying to do? Articulate your objectives using absolutely no jargon.

- Clear and concise communication of complex technical ideas in general is challenging
- But it's very important – particularly when you are reviewing a lot of research in a limited period of time like we do in the JUMP 2.0 annual reviews
- If you can't get your elevator pitch pretty well-clarified and well-articulated to convey the difficulty of the problem you're solving and the potential payoff, you are not communicating the value of your work
- Success in academia requires a high level of specialization, which often makes it even more difficult to avoid using jargon, but it’s worth the effort
- In particular, very quickly articulating what you're trying to do, why, and what the impact is going to be is critical when you have the opportunity to communicate with people in senior leadership positions

How is it done today, and what are the limits of current practice?

- JUMP 2.0 is focused on groundbreaking research
- It is extremely important to adopt an external focus and thoroughly understand what other people are doing in your area
- At the reviews it would be helpful to touch at least a couple of the latest external examples of how people are doing things (“How is it done today?”), and how the area has advanced since the start of the program
- Be sure to compare apples to apples, i.e. don’t compare your proposed scheme for 3D integration to mature techniques for 2D integration, compare it to state-of-the-art 3D work
- From a DARPA perspective, the apples-to-apples comparison is a lot more interesting because we want to know what you're doing differently, and why we should continue to fund you

What is new in your approach and why do you think it will be successful?

- What is your new approach?
  - Is it a material discovery – some new material or new compound that has a property people did not anticipate it having?
  - Is it a new device structure or system architecture?
  - Is it a new way to interpret the laws of physics?
    - Of course you can't break the laws of physics, but sometimes you can look at them in a different way that gives you some insight onto how to do things differently and better than they are being done today
- Question the assumptions that you're working under – often breaking those assumptions gives you insight into what actually is new in your approach
- Then at the annual review, in quantitative terms, tell me why your approach is going to be successful

Who cares? If you are successful, what difference will it make?

- If you're working on a purely defense-related program, the answer to this question is going to be very different than if you're working on a fundamental research program or if you're working on a commercial project
- In the case of JUMP 2.0, the “who cares” part had better be DARPA and the industry partners
- If you're successful in answering “what difference it will make” part that’s good
- It is understandable that sometimes it’s difficult to present that information because it's fundamental research and we're not exactly sure where the research is going to take us

What are the risks?

- You can fold the answer to this question into the third question when you're talking about what's new in your approach and why do you think it will be successful – risks basically are why you think your approach might not be successful

- And if your approach doesn't work, what are you going do about it? How can you change your approach?

- Or do we just need to stop working on it until we come up with a different idea?

How much will it cost? How long will it take?

- This one is easy: JUMP 2.0 will cost around $300M and will take five years
- Not worth our time and attention in the annual reviews in my opinion

What are the mid-term and final "exams" to check for success?

- This is kind of a tricky one when you're talking about fundamental research
- DARPA projects tend to have well defined-goals
  - The P in DARPA stands for Project
  - Proposal structure almost always is something like "I am going to take technology X from point A to point B in Y months with Z dollars"
  - When the actual deliverables and constraints of the program are that well defined, it's straightforward to determine midterm and final exams because you're checking progress against the metrics
- JUMP 2.0 is long-term fundamental research with long term goals – much harder to talk about midterm and final exams
  - JUMP 2.0 does include the mid-program realignment where we're going to evaluate the progress of each task relative to what the task goal is – is it actually moving the needle or not?
  - The mid-program alignment is less of a specific objective midterm and final exam and more of an interpretation of how your approach is working. Are you making progress? Is there a line you can draw from where you are today in the research to where you might be, or where you need to be, in order to actually make the difference when your program is successful? How close are you getting to the fundamental limits of the technology?
- In the fundamental research case, it might be more interesting to hear about how you're evaluating your progress on an ongoing basis – i.e. how you are measuring it and against what benchmark

Takeaways

- The Heilmeier Catechism has guided the formulation of DARPA programs for decades
- It also can be useful as a framework for writing proposals to DARPA and other agencies
- But you have to pay attention to the nuances when using it to evaluate ongoing research
  - Clearly and concisely state the technical problem you are trying to solve
  - Describe what's new in your approach relative to the current state of the art in the technology
    - Why do you think your approach will be successful and what are the risks?
  - Describe how you are evaluating your research progress
    - How are you measuring your progress and against what benchmark?
    - How close are you getting to the fundamental limits of the technology?
  - Tell me who cares and why
- Help DARPA and the industry partners help you be successful!

www.darpa.mil