Group #16: Community of Interest on the Future of Scientific Methodologies

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| --- | --- |
| Date | November 2, 2020 |

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1 Day One - November 2, 2020

1.1 Breakout 1 - Define the Scope of the Problem.

Participants: 0

**Question or instruction for the discussion:**
Breakout 1 - Define the Scope of the Problem.
The purpose of this session is to lay the foundation for the next 5 sessions. That is, each breakout group will define a key piece of technology, a new device, or methodology that would have an impact on how the labs/scientists operate. The details should include answers to the questions below.

**Sticky points:**

 Top Takeaways (5 points per participant)

* What is the problem, issue, technology, device, methodology?
	+ Inability to communicate effectively between basic and applied research. (#1)
		- and to leverage new modes of communications... ie the present and future internet to bring the correct minds together. (#6)
	+ Incomparable incentive structures that prevent forming good working relationships. (#2)
		- ensure that the value of basic research is rewarded. Career, funding, recognition. incentivize cross pollination. (#11)
	+ Definitions of basic and applied depends on your perspective. (#3)
		- Example: computational science users want a stable HPC platform with a lot of cycles, while computer scientists want an HPC platform that is easily reconfigured, rebooted, measured, and debugged. (#14)
	+ (1) Problem: we need to have a systematic way to balance basic research versus applied research (or development?). Otherwise, applied research and development often canabilizes basic research. (#4)
	+ (2) How do we structure and connect the value creation chain from basic theoretical foundation all the way to societal and commercial applications. (#5)
		- Each stage needs different skills from theoretical foundations to business development. Will we teach future scientist to be proficient in all these skills (is that possible?) ? Or will we rely on handing off projects between "stages" but then whose responsibility is it to make the connection, establish communications, etc. to make this successful (#24)
			* (2) one model that was developed in WWII to deal with the german Uboats was called a fusion cell - wherein researchers sat with operators side-by-side, working together. they identified problems in real time together and the researchers were able to solve those specific problems quickly. while not exactly a "futures" sort of approach, this same kind of idea could be applied to a futures perspective - with basic researchers sitting side by side with the futures folks in the mission - those folks defining the next few generations of systems. (btw if memory serves, this WWII relationship resulted in development of sonar...) (#38)
	+ (1) Lack of targeted initiatives/forums where solutions and problems can be brought together to be applied to specific problem (e.g., we tend to have pure research topical conferences, and domain science conferences, but not venues that target solutions) (#7)
	+ mission needs should be at a high level so as to allow new and creative solutions. (#8)
	+ (1) The explosion of scientific publications can make it hard to find and connect with the right people. (The publishing industry and/or incentives for promotions are a problem.) (#9)
	+ (1) Problem: no defined path for basic to applied to practice, nor for the reverse (#10)
		- No real career path for someone that straddles both (#13)
		- Does there have to be or could this be "solved" through better communication and collaboration between somebody on the basic and somebody on the applied side? Or do we fundamentally need people that straddle? Maybe management needs to make this connection? (#39)
	+ Gap between basic and applied research is partially due to lack of understanding on both sides. From the basic research side a lack of understanding of the real problems, real constraints, what is needed. From the applied side a lack of understanding of what is possible. Often, real problems can be solved with a simple solution, but the basic researchers don't know this. (#12)
	+ (1) Finding and connecting with people having overlapping skills and interests. (#15)
	+ Don't let applied research, limit or filter what basic research is done. (#16)
		- Conversely, don't let basic research limit what the applied researchers dream about. (#30)
	+ Model from Universities: Research Park/Spinoff model - combines a few people expert in the basic research (e.g., the faculty leader, but sometimes a student) with new staff (venture funding for programmers, lab techs, engineers). Drawback - funding from private sector (mostly short term returns) (#17)
	+ Identifying groups in mission areas who are not responsible for day-to-day firefighting is also key - those folks don't have the bandwidth or risk tolerance for absorbing new, somewhat unproven approaches to solving problems. Must identify the groups at the labs (e.g.) who are responsible for defining the future systems, future needs, future capabilities. Those are the folks who have more latitude for working with basic researchers to develop and mature their approaches. They can also have some additional influence on what the constraints of future systems might be.... (#18)
	+ Having the time to overcome the language barrier and build a relationship, given that we are typically getting more and more fragmented among projects is an issue. (#19)
	+ Bureaucratic problems also exist - skunkworks has been a great model for unleashing staff from risk-averse-driven constraints that hinder development and maturation of novel, higher-risk kinds of technologies. (#20)
	+ Whose mission is it to connect basic and applied research (at the level of a program office)? You can argue that DARPA has such a mission, and funds it. (#21)
	+ There can be disconnects between applied and basic research due to a lack of understanding about the constraints of deployed environments. As an example, one might have an environment when a sensor is background noise limited while the basic research goal is to improve sensitivity which would simply be lost in the background noise. (#22)
	+ (2) Realizing that it is sometimes okay to fail, learn from the failure, and move on into a more productive avenue for collaboration. Program managers do not want to hear the failure word. (#23)
		- One also wants to be agile and fail fast rather than draw it out. (#26)
		- it is actually necessary to fail. this doesn't seem to be valued. (#27)
		- Absolutely. Failure should be an option, and probably the norm for difficult basic research. The quest for dark matter, solving the mathematics of primes. These are all moon shots in a 30 year career, but they should be encouraged, not discouraged. (#28)
	+ (1) Support for exploring alternative solutions in parallel and learning from them (#25)
	+ Properly classify existing funding into basic research, applied research, development, and use. TRL classifications are a good start but miss some concerns like manufacturing and economics. (#29)
		- and human usability (#41)
	+ More integrated leverage of technology in scientific communication (e.g., publishing quality software makes it easier to adopt solutions) (#31)
	+ Time to think, rather than constantly writing proposals and always looking to the next thing to keep your group funded. (#32)
		- Freedom to fail as well - can you do something bold that might not work out, if failure threatens the group? (#35)
	+ People are incentivized for quantity (e.g., number of papers) vs. quality (impact) as quality is not as easily measured. (#33)
	+ Restore the large industry driven research eco system. Bell Labs , Xerox Parc etc.. are to some extent now Google , Microsoft but not at the same scale in terms of basic research. (#34)
	+ Continued focus by the labs and DOE on accounting, legalities, auditing, management, and program development distracts from the overall time allocated to doing basic research (even when the funding does exist). (#36)
	+ Anticipating the future needs, so that we get ahead of the curve, rather than ride the wave. (#37)
	+ Working on a 30 year time horizon, while being funded on a 3 year cycle is nuts. (#40)
	+ Remove publishing gatekeepers whilst maintaining academic integrity. arXive , wikipedia should count as much as journal publications on good content. (#42)
	+ (1) Crossing from basic to applied research can often cross a line into export controlled areas. (#43)
		- and can quickly cross into classified areas. (#45)
	+ Technology in particular.... a call out to the mathematics in support of quantum physics. (#44)
	+ (2) 1. .Structure and incentives don't support basic/applied (#46)
	+ 2. Communications needs to be improved - terms, exchanges, more than papers; quality not quantity (#47)
	+ (1) 3. Failures are not allowed. (#48)
* Who would develop it (basic research to advanced deployment)?
	+ there are people in organizations who are able to both think broadly and get deep technically where needed. they are identifiable if one uses the right tools (psychometrics) but one has to know those tools exist. these people are fairly rare, though.... another way to think about them is they are both converging and diverging in terms of their thinking. (#49)
	+ Top down drive for change in the government. NSF / DOE etc.. hopefully each with the same or aligned philosophy. (#50)
	+ Program managers would have to be willing to implement any structural changes. (#51)
	+ (5) Funding agencies, program managers, congress, and ultimately the public need to consider a different value proposition for research: to allow basic research to fail; to enable parallel, collaborative efforts instead of competition; To provide incentives to cross TRL levels; (#52)
	+ (2) AI that can track and rank basic to applied (automatically assign TRL levels instead of asking humans to do it): We (DOE) would need to develop it. It would inform as a histogram the investments, so a portfolio approach to basic and applied could be tuned for specific outcomes. (#53)
	+ perhaps this could be phrased as who sponsors the work as it reaches different TRLs? (#54)
	+ (1) Program Managers. Rethink the management model and have the courage to implement it. ( Failure / Quality of work / Value of work ) (#55)
	+ (2) management must be aligned with the vision for building bridges. putting the bridge building onto research staff or onto staff who are responsible for putting out mission fires won't work. management must be incentivized to actually do this hard thinking. again - people who can do this are rare. (#56)
	+ (1) Idea Fairs -- a conference format that would allow participants to work across problems and solutions and support an active format where things can be discussed or even tried (#57)
	+ this can't be a box-checking exercise.... (#58)
	+ (3) Some of the academic fields (sociology, psychology, behavioral economics) could help transform the current state. How can we involve them in applying their work? (#59)
		- ask them for their help.... (and don't discount the work if it seems "fuzzy" or "fluffy" - it is actually very rigorous, but very different from mathematical, physical, computational sciences) (#63)
	+ Prize driven events. DARPA and others had some fun with large award prizes that seemed to get a lot of excitement (#60)
	+ (2) AI for "scientific social networking" to link Basic and Applied: The AI would find and tune for specific outcomes the arxiv articles I should be exploring, IEEE conferences, etc. The broader Scientific community would need to develop it (#61)
	+ (2) Incentives (and marketplace) for idea sharing that is different than traditional style publications: results that can be explored and extended easily can lead to faster synergies and easier adoption of research (#62)
	+ (3) DARPA model: Use funding opportunities to provide incentive to take idea to practice with understanding that could fail (but fail fast) (#64)
	+ (2) A group of "futurists" across the labs who focus on these issues may help. (#65)
		- Some small set of people that have a broader overview (#75)
	+ (2) micro-grants. small, fast turnaround grants with a brief proposal and overhead. Think $100k for three months from a 5 pg proposal with a review time of 1 week. (#66)
		- sandia has this and it often turns into "i don't have coverage for the whole year, so i'll propose something small to cover myself..." (#70)
		- How would we manage the implied funding insecurity of this model? Who would set aside a 3-5 year grant to work on a 100K grant and then potentially get stuck without funding ? (#72)
		- Also, how would we manage the relatively high ratio of overhead on these efforts (#78)
	+ University spin-off model: Provide options to take basic research to practice using subset of team; allow for "re-entry" into basic research (#67)
	+ (1) Programs or laboratory management structure builds a "Reverse DARPA" model that can incentivize basic research directions as opposed to accelerating basic to applied research. (#68)
	+ (1) Labs could implement a joint LDRD program that allows funding a team that crosses lab boundaries. (#69)
	+ (1) Early Career Grants are a good example of seeding long term strength ! (#71)
		- (1) What about mid career grants? Is that a missed opportunity? (#77)
			* The way these are typically funded right now they are effectively mid career awards. The majority of people with successful grants at least on the ASCR side are at the very end of their eligibility (8-10 years from phd) (#79)
	+ (2) more consistency across DOE directives and lab management directives. ASCR mandates open source yet lab management doesn't want to give away anything of value :) (#73)
		- To open source something, we have to tell the lab lawyers that our work is of zero value... (#76)
* Who would use it and what skills would they need to use it effectively?
	+ The basic and applied communities. (#74)
* When would it be expected to be in production use (N years in the future)?
* Where, and how widely, would it be deployed?
* What is the setup time and/or process for using it?

1.2 Breakout 2 - Implications of this Problem.

Participants: 0

**Question or instruction for the discussion:**
Breakout 2 - Implications of this Problem.
Each group will now develop a list of issues and implications for the issue/technology/community they settled on. There are lots of implications for how a technology can be used, or further developed.

**Sticky points:**

 Top Takeaways (5 points per participant)

* What other/companion technologies, services, software/hardware must also be developed and deployed?
	+ How we would organize the process in 2050 for basic + applied research? (#2)
	+ We want to enable two paths of basic to applied: 1) Enable the basic researcher(s) to follow their idea; 2) Hand off ideas between teams (#3)
		- 1) Implies funding, job flexibility, and job security for the researchers to follow a different path for a while (#7)
		- 2) Implies better communication structures (of management?) to create and maintain the necessary connections to find possible hand offs and facility the necessary communications to make them successful (#12)
		- Recognize different time scales - it may take a few years to demonstrate an idea (the basic research) but a decade to reduce it to practice (or vice versa, depending on the topic). (#26)
	+ DARPA model: programs to take risks in moving basic research results to practice. (#5)
		- On the basic research side, (block) fund a lower bound on effort with the researcher responsible for getting funded for the "summer months". (#6)
		- Include job security - failure (especially fast failure) is not a negative (#9)
			* Repeated failures may become a negative though. You do not want to keep funding people or activities that fail all the time. (#32)
	+ Take a team science approach, so context of how basic research could be applied is included in its formation. (#10)
		- Ensure there is a baseline level of mission understanding for all staff and that is something that people can refresh on reasonably often. briefings, short classes, etc. at the classified level. A mandatory "core curriculum" so to speak. (#4)
		- Develop methods for research and mission staff to interact often enough to develop relationships. ultimately, these basic-mission research teams are built on relationships. Periodic internal workshops (max of a day), periodic briefings with the same groups of people, technical exchanges... (#16)
		- staff who are good at building these bridges should be rewarded for mentoring other staff who want to learn how to do this. (#23)
		- Develop models based on research teams where individuals who prefer to work on pure research are paired with complementary expertise (i.e., recognize "research delivery" as an important factor in generating impact) (#22)
		- Have workshops without pre-defined conclusions that bring the groups together? (#20)
		- Foster venues and collaboration mechanisms where problems meet solutions alongside topical venues/discussions (#14)
		- Restructure efforts: Remove the terms "basic" and "applied" from discussions (especially funding). Use at least some TRL to provide a finer grain (#19)
		- Reward teamwork and teach/respect teams and group efforts. (#28)
		- May take a different funding model to support team vs. traditional PI model. (#29)
		- Recognize and promote teamwork across disciplines and TRLs. Our awards systems are largely focused on individual accomplishments rather than team science. (#30)
		- Be more intentional in doing work that supports the mission, rather than the field of dreams approach. (Or some mixture.) (#33)
		- It is important for researchers who are wanting to do basic research to understand the operational environment(s) into which they are hoping their basic research is adopted. There is something to be said for doing basic research for its own sake, and often truly innovative work will be used in ways that were not imagined by the inventors/discoverers. However, if basic researchers really want to have a hope of impact, they have a responsibility to understand the mission spaces, their needs, their constraints and try to listen to those. This doesn't mean that basic researchers should be turned into a job shop to solve near-term problems that mission people don't have bandwidth to solve. But, random-walk research in the national labs that actually have mission to support should be minimal. However, basic research is not necessarily random-walk research - there seems to be a tendency to conflate basic research with random-walk. There is also a tendency to make up mission links for random-walk research in order to get the latter funded - mission tie becomes a box checking exercise this is just as ineffective. (#36)
	+ Invest into mechanisms to produce research results in more consumable form (e.g., software, consumable datasets, etc.) and then provide incentives to share research via those mechanisms (#11)
		- Journal publish applied + basic papers. (#13)
			* - Use new media as well as Journals. Since the goal is getting the idea out and curating for scientific integrity, how can we do it beyond the journal model. (#17)
			* Reproducibility in basic research can lead to more confidence/less risk in attempting to translate to applied problem. (#24)
	+ These efforts can't be box-checking exercises - there must be a way to empower staff to develop relationships so that the applied folks have staff to whom they can reach back and the basic folks have applied folks from whom they can get up-to-date information on mission needs and problems. (#18)
	+ Create a way for basic and applied researchers to see the need for their work (#27)
		- Make sure that managers who are managing research portfolios (LDRD, ASCR, etc.) have ties into mission so that they can help staff connect to mission owners and can mentor those staff to help them identify mission problems. (#8)
		- Technology aids: Use AI to identify possible connections between basic/applied research (#15)
			* Virtual research assistants; clippy for the research age. (#25)
		- Aggregate and generalize/elevate applied research problems to core basic research challenges (every problem is a special flower → study of how pollination works) (#21)
	+ Have a portion of the research portfolio with a longer term focus. (#34)
	+ Acknowledge that basic research need not have an identified application; manage this accordingly (#35)
	+ DOE takes big risks on large scale. $150M - $500M moon shots. Is this the right ratio, or should we have more mid-scale projects ? (#72)
* Who is/will develop this companion technology/service?
	+ Program managers will contribute to the developments. (#31)
		- Shepherd research areas and projects (#51)
		- Encourage project calls that require team delivery for projects. (#63)
		- the cadence of funding for important topical (#65)
	+ lab management (#37)
		- Provide a funding and job security structure to ensure the best efforts in basic and applied research. (#53)
		- Provide help to staff to make connections to mission (if research staff) or to research staff (if mission staff). (#58)
		- Take responsibility to develop understanding of mission needs and to develop relationships with mission problem owners. (#61)
			* management needs to be responsible for helping build these bridges and developing staff knowledge. should be on their performance review forms. (#45)
				+ and connecting different orgs so they work together (including inter-agency coordination and collaboration) (#48)
		- lab management vs. doe management. Does the lab's desire to protect IP serve or hinder basic and applied research vs. DOE's approach to generally open source and publish. (#70)
	+ Scientist (#38)
		- Should take responsibility to have at least a minimum understanding of mission needs. Management needs to help with this. (#57)
		- Take the initiative towards team science (#59)
	+ doe management (#39)
	+ congress (#40)
		- Needs to enable different funding model that allows more flexibility outside of direct project funding and is willing to accept the risk implied with job security (#52)
	+ The public (#41)
		- Ideally should value even failed research projects rather than demanding consequences in hindsight. (#54)
	+ the community (#42)
	+ Industry (#43)
		- Provide commercialization opportunities and pathways (#71)
	+ humandkind (#44)
	+ future generations (#46)
		- Become scientifically literate (#66)
		- Further the mission and carry on the research (#67)
	+ white house/ostp (#47)
		- omb, gao (#60)
		- Promulgates high-level government value statements and strategy (#62)
	+ academia (via facilities, collaborations, students) (#49)
		- educate future basic and applied scientists (#68)
			* perform small scale high-risk basic research (#69)
	+ bureaucrats (#50)
		- Should be applied selectively where process and risk adversity is the goal. (#55)
		- Distract with other less important problems (#56)
		- stop pulling up the carrots to see how they are growing! let researchers work on their work instead of providing constant info to bean counters. (#64)
* What skills/knowledge does the end user require?
* What are the training/support requirements?
	+ Understand the basics of the overall mission. (#1)

1.3 Day 1 Reflections

Participants: 0

**Brainstorm question or instruction:**
Day 1 Reflections
This area is for the Moderator to note key discussion points to summarize what was accomplished in Day one. Remember that day one is focused on Identifying a new technology or methodology and identifying the implications and possible consequences of it. The moderator can populate this individually at the end of the day or request input from the group here.

* 1. Funding and management model for securing basic research, given the uncertain nature of it's output. For applied research risk/reward/success should also be balanced.
Communications. So that applied and basic researchers can find each other and collaborate.
Growing an eco system to increase and maintain funding. ( NSF / DOE / etc.. ) ( Industry ) science as a national priority.
Incentive structures that lead to collaboration behavior.

2 Day Two - November 5, 2020

2.1 Breakout 3 - Signposts

Participants: 0

**Brainstorm question or instruction:**
Breakout 3 - Signposts
What we are looking for is technology or social trends that would give us clues that we are on the right track. o How would precursor technologies/services be identified? o What are the precursor technologies/services? o Is there a rank order for when specific technologies/services need to be available? o What DOE or Lab policies need to be in place now, in 5 years? o What facilities need to be in place now, in 5 years?

**Sticky points:**

 Top Takeaways (5 points per participant)

* 1.  Good ideas we can do now
	+ Comments
	+ Fund some ideas at random - don't just try to pick the "best" proposals (and we could look at how accurate and effective those choices were). This is done already in some countries to make decisions less conservative (#9)
	+ Bring back 5-10 year funding in some areas - not everything (ether basic or applied) can be completed in 3 years. (#10)
	+ Start funding people not ideas? Startup companies are supposedly majorly funded based on the team not necessarily based on the idea. That was also the model of Bell labs, Xerox, etc. Pick a good team, given them money and lock the door (#13)
* 2. A proposal for 2050
	+ Comments
	+ A new office of translational science, funded to carry out the translation of discoveries in basic science to practice (#3)
	+ The Airplane example covers both early research into flight, but also engineering at the end to make them prevalent. Similarly we can think of 30 years as beginning with a wild idea, but the last 15 years are a transition to perfecting the implementation. (#4)
		- In the next 10 years ( early phase ) important new topics include: 1. Quantum. 2. Genomics and the ecology 3. Cosmology Each leads to a different airplane. DOE should have such airplanes identified, with a 30 year horizon, and apply early ( Basic research ) and ( Applied research ) in a continuous long term vision. (#6)
	+ Encourage team science by changing organizational assignments to multidisciplinary teams. Encourage pairing so people of different educational background are cross-training (continued work force development). (#5)
	+ Reorganize the offices in DOE to be interdisciplinary rather than domain focused, with crosscuts by domain (teams by goal rather than by expertise) (#7)
	+ Create integrated teams that include researchers from the entire span of basic to applied research and jointly reward success. - The goal being to give the basic research folks an incentive to make the applied research successful and vice versa. (#8)
	+ New educational models where one could get a PhD equivalent degree that requires mastery of multiple topics which is not tied to a single academic department. The idea is to break the cycle of academics creating a workforce which are essentially clones of their advisors. (#11)
	+ Create new (virtual) DOE laboratory(ies) for translational science that integrates university, government, and corporate entities. (#12)
	+ Program offices and laboratories collaborate on problems of national importance issuing joint programs that require diverse technical expertise. (#14)
	+ Acknowledge different research models, and organize around them. They include (1) curiosity-driven basic research. No immediate goal for application, though important applications have emerged (note: is there a separate org that looks for these?) (2) basic research driven by application needs. Inspired and funded by the application need. (3) Translational research and applied research - intended to address a mission goal by solving different problems (see TRLs for a spectrum of such applied research). (#16)
	+ We need to find a better way to assess and reward scientific impact to society beyond one's most recent publications. (#17)
	+ Developing a team-oriented approach for science which recognizes the role of pure research, applied research, development, deployment, and adoption in creating innovation. The key value that such approach recognizes is the fact that the evaluation and continued improvement of some of the research ideas often has to take place in the context of their application -- and thus the application is critical to its success. In some ways an approach like this would reflect a product lifecycle pipeline in industry where a company might have separate departments dealing with produce lifecycle from innovation to deployment - though with greater weight towards innovation, greater reward structure for failed approaches that contribute to overall development of understanding, and a path forward for spinning off to actual industry adoption. (#18)
* 15. Developing a team-oriented approach
* 19. We could exploit greater networking and collaboration to build bigger, broader teams
* 20. We could have 100's of labs, with a clearer, more narrow focus, instead of a handful of multiprogram labs
	+ Comments
	+ This includes "pop up" labs (or centers with sufficient autonomy) (#21)
	+ Look to where competition happens - encourage constructive for best ideas, don't in a way that prefers safer, more conservative approaches (#22)
* 23. Bold proposal: Change to a team-based approach to achieve goals (whether curiosity-driven basic science or specific mission application). Create an administrative structure that supports these teams (100s of labs) rather than a structure that contains pools of expertise and facilities.
	+ Comments
	+ Is this a replacement or a complement to the current model? (#30)
	+ The feeling is that the current structures meet many needs but are less effective at parts of the research ecosystem (esp. basic to applied to basic). (#31)
	+ Also raised the issue of small scale but long term research (#32)
	+ Office of Team Science (#33)
	+ This would complement, not replace, the existing discipline offices in DOE (#34)
	+ Acknowledge that "team science" is given lip service - the difference here is that the focus here is on the team aspect of achieving a goal, rather than organized around a single domain or facility. This office focuses on team science \*first\*, rather than as a check box in its other metrics. (#51)
* 24. Metrics:
	+ Comments
	+ Lots of ideas that succeed or fail quickly (#25)
	+ (need to demonstrate that you can fail fast) (#26)
	+ Need to be smart about which problems to fund - especially if the timeline is short (#27)
	+ Which need an integrated team vs a smaller, narrower domain team (#28)
	+ How do we support the long development time of some ideas? (#29)
	+ We know that we have succeeded because: (#35)
	+ Metrics for "failure" (that nevertheless generates better understanding) (#36)
		- For example, no-go theorems, better quantitative constraints on aplicability of potential solutions (#45)
	+ Greater (how is that measured) success in translating basic research results into practice (#37)
	+ Greater (ditto) success in solving mission needs by developing the necessary ideas (#38)
	+ Reward true integration and collaboration independent of success or failure (#39)
	+ Ability to hire and retain best people (#40)
	+ Can use traditional metrics (patents, R&D awards, IP license) (#41)
	+ $$ \* Time consumed to get to a continue / stop milestone. (#42)
		- small $$ \* long time = ok. large $$ \* short time is ok. large $$ \* long time is not good, unless the system has checkpoints along the way for potential impact. (#46)
	+ Can monitor other projects for greater value (e.g., if the applied tech is to address mission needs, measure that improvement) (#43)
		- Is there a way to apply AI to detect/expose patterns of success/failure? (#44)
		- Will this lead again to an emphasis on applied research since ultimately success is defined by whether the end-"product" is successful (#49)
	+ A recursive metric? A successful team is one that can generate the next successful proposal? (#47)
	+ need metrics to capture personal growth, e.g., learning/cross-training throughout the team (#48)
		- Value teams that grow/educate/mentor junior researchers (#57)
	+ quality of people attracted to and still working in the "swarm". If it is successful, we will have the best minds. If not, they will go elsewhere in DOE or out. (#50)
	+ One metric could be the number of diverse problems to which the science result could be applied. (#52)
	+ transitioning to widespread adoption/industry (#53)
		- tech transition is a good one, licenses, patents, open source software and data sets. (#56)
	+ Success is the formulation of an applied problem in terms of fundamental research. Reward the teams that can formulate the "right" questions (#54)
	+ Number of interdisciplinary projects/results (#55)

2.2 Breakout 4 - Signpost Plausibility

Participants: 0

**Brainstorm question or instruction:**
Breakout 4 - Signpost Plausibility
Now that we have the list of signposts, the groups need to consider how plausible they are and what DOE needs to do to either ensure they happen or the implications of them not happening. o Who is actively working on these precursors? o When would these precursor technologies/services be needed? o What active or pending research programs need to be in place now? In 5 years? 10? o What existing or planned facilities need to be in place now? In 5 years? 10? o What software services or capabilities need to be in place now? In 5 years? 10? o How successful has the community been in meeting previous goals?

**Sticky points:**

 Top Takeaways (5 points per participant)

* 1.  Create an office of transformative research
	+ Comments
	+ Establish metrics and behaviors they want to see. (#2)
	+ How do we bootstrap bottom up collections of scientists and science topics ? (#3)
	+ We can use the COVID response model to do top down matching of a science need ( COVID) and capable scientists. (#4)
	+ Some form of workshop or online infrastructure to first bring together birds of a feather. (#5)
	+ Create a position for leaders who know how to inspire a small group to work together, and love their science. (#6)
		- example of community leaders in big open source projects. who aren't writing code, but inspiring collaboration. (#7)
* 8. Goalpost for success
	+ Comments
	+ Real measure of success is changing the culture. (#9)
	+ Note that we need some sign of progress, noting that getting to an end result may take decades (#10)
	+ In 5 years, we should see a measurable community and culture change that work in distributed / collaborative / bespoke science domains. (#11)
	+ Note that Social/Behavioral scientists study this (#12)
	+ (We really need such social/behavior experts for this) (#13)
	+ 3 phases: early conceptual phase; mid with applied scientists; later with engineering to deliver. (#14)
	+ How do we reduce the cost to deliver on the promise of new ideas? (#15)
	+ Our metric of success is that the technologies from the \*other\* discussion groups can be developed and brought to fruition. (#17)
	+ Measure the size of the community supported by the office. How big is the community of scientists? (#18)
	+ Measure sustainability of work being funded. (#19)
	+ What is the sustainability plan? Does a popup group become sustainable? What is their exit plan? (#20)
	+ Measure exit as appropriate: - industry adoption. - Science adoption of a theory or proof - successful construction of a DOE instrument (#21)
	+ What is the incentive structure for making scientists want to join our "pop up research" model. (#22)
* 16. We are proposing change in how research is organized. Other groups in the ORISE conference are producing 30 year ideas. How many would benefit from our radical new structure for research ?

3 Day Three - November 10, 2020

3.1 Breakout 5 - Pitfalls and Roadblocks

**The following participants have not been active:**
Bruce, Pete, Aric Hagberg, Kate Keahey, Casee - MS, Casee Meyer - MS, Todd Munson, Kate Shattuck, Ann Speed, Jeffrey Vetter

**Brainstorm question or instruction:**
Breakout 5 - Pitfalls and Roadblocks
Detailed discussions on identifying pitfalls and potential roadblocks. If possible, list in rank ordering. o What could prevent the technology/service/device from being developed (funding, materials, policies, researchers, operations staff, etc.)? o How will progress be measured/evaluated? o How will lack of progress be measured/evaluated? o Who will decide if progress is being made? o What are the consequences of not engaging in this area?

**Sticky points:**

 Top Takeaways (5 points per participant)

* Unsorted (0)
* Pitfalls and Roadblocks (26)
	1. 1. The funding model needs to change to provide continuity for scientist who take a risk
		+ Comments
		+ Failure should not be a risk. (#3)
		+ Funding model can't give unlimited tenure - some failure is ok; constant failure is (probably) not (#9)
		+ One could be successful (not fail) but still might not have the best solution. When should downscoping of competing solutions occur? (#16)
	2. 2. Motivating long term researchers to work hard, when there are no good measures of success.
	3. 4. How to balance stability and turn over. How to define success and maybe more importantly how to identify failure in order to provide a mechanism for turn over.
	4. 5. It is unclear what is the right level of turnover to retain institutional knowledge yet incorporate sufficiently many new ideas, expertise, and perspectives.
	5. 6. Career paths and job security for individual researchers: it is not clear how they would migrate between projects and how their contributions would be assessed
	6. 7. Ensure that the entire basic research to practice "workflow" doesn't stall, especially from a failure to handoff work/staff/expertise/funding/etc.
	7. 10. There currently exist no people / job / funding to close the gaps between research projects at different TRL levels. Somebody needs to be responsible to make the connection and/or to articulate gaps and the necessary skill set might be very different from the traditional "scientist"
	8. 8. Who and how we judge ideas worth pursuing can go horribly wrong.
	9. 11. Without good turnover, this system can become very closed in terms of new people not having space that is filled by current scientists.
	10. 12. There is an implication of increased movement of researchers between teams and institutions -- it is not clear how this mobility would be reflected in practical terms, would it imply more virtual collaboration for example
	11. 14. Personality begins to trump scientific merit in terms of who leads these efforts.
		+ Comments
		+ But continuity in funding in some respects implies funding "personality" in the sense that you fund people not ideas ... that is the basis of the tenure system (#35)
	12. 15. Need to avoid 1-dimensional evaluations - there are different ways that projects can succeed. (Even though eventually funding will provide a ranking - that should be a last step, rather than having a 1-dimensional ranking as the first step).
		+ Comments
		+ One additional evaluation criterion should/could be staff development. Which project was able to attract / train / promote the most successful junior staff (#30)
	13. 17. The office of disruptive science is shunned by other DOE offices. Leading to poor transitions of ideas.
		+ Comments
		+ Another common perception is that "disruptive ideas" are sometimes equate with free money as there is a perceived lack of pressure to succeed (#32)
		+ How do we define failure of a disruptive research project since by design many of the ideas will not pan out? Could we use duration as a metric of success? Meaning independent of potential outcome (good or bad) if it takes too long to get to the outcome the project is unsuccessful. So you either want to demonstrate success fast or demonstrate failure fast, with both outcomes equally acceptable. What would not be acceptable to work for 3 years without knowing whether to stop or not (#33)
	14. 18. How do you find program managers that can handle such a broad view, and work collaboratively with managers in other, more narrowly focused, offices?
		+ Comments
		+ Another challenge is that the broader you make each PMs portfolio the less deep they will be in any one aspect. That means they likely will not be able to technically evaluate most projects as they can be an expert in only so many things (#29)
	15. 19. We assume "enough" money to have a fairly generous set of projects that people can get assigned to. As opposed to a very competitive small pool.
	16. 20. Random selection of projects from a pool of "plausible" projects may be seen as unfair or a misuse of funds. And how is "plausible" decided?
		+ Comments
		+ A new ranking process in the committees ? Must fund - could fund - must not fund (#27)
		+ Is plausible a preliminary result, an general idea to explore, some combination or something else? (#37)
	17. 21. There is a potential for "management" priorities to conflict with "project" priorities across the different participating organizations.
	18. 22. Will basic and applied research scientists collaborate or compete ?
		+ Comments
		+ I think the answer "has to be" collaborate because there seems to be no common scale on which they could compete (#23)
		+ Assuming collaborate is the answer the implied potential pitfall is - what if this collaboration does not happen ... is that a sign of failure in itself ? (#24)
	19. 25. Funding for the "disruptive tech" office gets prioritized not for basic to applied to practice projects but to supplement basic OR applied but not basic TO applied
	20. 26. Multi-institution teams could cause intellectual property management to become overly complex, slowing the transition of basic research to applications.
	21. 28. Pitfall: focus on common problems of research funding, rather than on the problems faced in basic research to practice
	22. 31. How is intellectual and other credit distributed? Who is included? Who is not?
	23. 34. How do we learn about "gaps" in basic to practice? Whose job is it to look at both successful and unsuccessful projects to identify missing elements, whether it is open basic or applied research problems or missing engineering investment or something else? Who funds them? Who evaluates them, and on what metrics?
		+ Comments
		+ Perhaps this is an area where public-private partnership is needed, maybe OTT could help. (#38)
	24. 36. What is the timescale for success? How long will funding need to be secure to give the office a chance? If this time is too short, the office will have the wrong incentives, and may be too conservative.
		+ Comments
		+ Is it possible to scope different timescales for different TRL stages, if so, what is reasonable? (#39)
		+ Should the office evaluate portfolios of projects rather than individual ones (#43)
	25. 40. Where does commercialization fit into this? Which items get reduced to practice but are never commercialized (e.g., Open Source software)? Which items become commercial products, and how does funding for that work (see University models for spinoffs and IP licensing)?
		+ Comments
		+ Do funding pressures encourage commercialization, even where it doesn't make sense? E.g., we'll pay the maintenance on supporting this Open Source software by commercializing that support. (#41)
		+ Is there a preference for open-source or commercially licensed outputs? (#42)
	26. 46. Explain how this is different from ARPA-E (it is, but there is overlap)
* Consequences of not doing this (2)
	1. 44. Without this, basic research to practice remains an ad hoc, accidental process, and opportunities will be lost to realize the benefit of the basic research, and the ideas and challenges uncovered in the applied research.
	2. 45. At a nation scale the US could fall behind on overall scientific leadership.

3.2 Breakout 6 - Keys to Success

**The following participants have not been active:**
Bruce, Pete, Aric Hagberg, Kate Keahey, Casee - MS, Casee Meyer - MS, Todd Munson, \*fac- Nami, Kate Shattuck, Ann Speed, Jeffrey Vetter

**Brainstorm question or instruction:**
Breakout 6 - Keys to Success
Identify who needs to be engaged, research communities, domain science communities, staff, management. Identify needed skills and knowledge (give examples) o What benefits would society obtain? o What benefits would the science/research community obtain? o What research communities need to be involved? o What domain science communities need to be involved? o What staff and management communities need to be involved? o What kind of management structure is required? o How broadly will this impact society and/or the science community?

**Sticky points:**

 Top Takeaways (5 points per participant)

* Unsorted (0)
* Top Down Keys (12)
	1. 2. First director must be an inspirational leader with a compelling vision
	2. 3. Interagency collaboration is a key to success as different agencies often have different strengths at moving through different TRLs.
	3. 4. Have a good single or small set of leadership people who will bootstrap this new office of disruptive science.
	4. 5. The office must be seen as bringing \*new\* funding, not reprogramming existing funding
	5. 6. Establish early that other non-DOE agencies are not threatened by this.
	6. 7. Get buy in from all the lab directors early in the process.
	7. 8. Need broad endorsement from congress, other agencies, and DOE leadership (including lab directors)
		+ Comments
		+ Especially for congress this means a willingness to trust technical experts as an evaluation for especially basic research efforts by lay people will be difficult and likely highly biased by salesmanship (#28)
	8. 10. It is essential that any significant changes in research management are started as a pilot project in parallel or as an addition to existing efforts rather than as a replacement -- this will allow not only for trying and tweaking the approach and allow new workflows to develop in an organic fashion but eliminate a potential natural threat that often comes from resistance to change and wanting to prove that change wrong
	9. 11. Need to be able to document and communicate the success and value of the work
	10. 12. Make sure the human factors and team issues are addressed. Draw on experts in managing humans.
	11. 24. Must be recognized as risk-taking - fail-fast is success (fail-slow is not). Must be funded, managed, and evaluated to encourage risk taking
	12. 36. Need a charter that is clear, concise, and narrow enough (it should not claim all of basic-applied; just what makes sense for DOE). It should be clear what is \*not\* included
* Bottom Up Keys (10)
	1. 1. Organize workshops that motivate a large pool of basic and applied researchers to help shape this vision.
		+ Comments
		+ Work to break down the "basic" vs "applied" pigeon-holes and develop a better community appreciation of a spectrum from basic to applied. (#31)
			- Create a culture where it is clear what constitutes a success in "basic" and "applied" research so they are valued and rewarded by their broad communities. (#33)
	2. 9. Start small if needed. Do some groups in the first year or two, rather than waiting for a big launch.
		+ Comments
		+ Could be a small-scale pilot that validates the approach with respect to others. (#16)
	3. 13. bring best practices from Open Source efforts on how to build communities around a technology.
	4. 14. This effort will require significant community buy-in and organization at many stages, it is key that community support is solicited
	5. 15. Take advantage of collaboration technologies to build stronger multidisciplinary teams
		+ Comments
		+ Improvements in tech remove or reduce remote collaboration barriers. This could increase candidate pool as well as help with job satisfaction by letting people live where they desire. At the same time, we will need to better utilize distributed experimental facilities. (#18)
	6. 17. As scientists lobby for this approach. Convince the highest level of management that this is needed.
	7. 19. Building this up as an effort parallel to exploration going on might help with the evaluation of progress: comparing results from two alternative approaches and examining how they differ might suggest meaningful metrics
	8. 20. Engage experts on organization structure to help envision a new framework for how science collaborations especially between applied and basic could be organized.
	9. 21. Need to have a well defined career arc and rewards structure to attract and retain the best and brightest.
	10. 29. New media collaboration can start today, regardless of this office. Organizing this is good for bootstrap motivation of the ultimate vision we suggest.
* Final Thoughts (6)
	1. 22. We have concern about the use of AI to help manage these efforts. Current systems do not have the fairness, transparency, or explainability, and it is unclear that the issue of bias is solvable even in 30 years. At the same time, we recognize that people are also not very effective at "picking winners", and have their own issues, so there may be a role for AI for some insight but not for making or even strongly influencing decisions.
	2. 23. In the most broad sense we DO have a problem:
	   1. Top students do not choose DOE as a career. Most prefer Google. This is changeable.
	   2. There is a nation scale competition for broad and effective science. The number of scientists involved are growing exponentially, so the system must adjust to manage this scale.
		+ Comments
		+ Competition for top science talent seems poised to increase globally as societies rely more on science to solve major challenges. (#25)
		+ What are alternative reward structures we could envision for science careers as the government will likely never compete on salary and benefits? (#30)
	3. 26. It is possible -- and a good idea -- to design the deployment of the proposed changes as a series of pilot projects, on a relatively small scale and parallel to status quo -- this would both allow to hone the process and get the community buy-in if they prove successful
	4. 27. This new office / effort has to be additive, not in competition with other offices within DOE or across agencies.
	5. 32. Much of this is a "people problem" and will succeed or fail based on peoples motivations, fears, insecurities, cognitive biases, and human nature in general
	6. 34. One way to address DOE vs other agencies is to make creating such offices, with incremental (additional) funding, part of \*every\* research-oriented agency. That does raise the price tag, but if the argument is that this is a way to accelerate science, it would be valuable everywhere. For example, such an approach (and a redistribution of the incremental funding) would go a long way to address some of the concerns with the "Endless Frontier" act to add a technology directorate to NSF (with more funding than all of the rest of NSF combined).
		+ Comments
		+ And then the price tag can be made less concerning by organizing this as pilot projects, and staggering start dates across several years. (#35)

Appendix

Live chat

**The following messages were exchanged via Live chat.**

* Nov 2, 2020, 16:57 UTC
	+ Hmm ... I assume that there is a zoom breakout room attached to this ? (Peer-Timo Bremer | Nov 2, 2020, 16:57 UTC)
	+ Am I supposed to find this somewhere or will they place us in breakouts from their end? (Peer-Timo Bremer | Nov 2, 2020, 16:57 UTC)
	+ made it here (Aric Hagberg | Nov 2, 2020, 17:11 UTC)
	+ They will assign breakouts. If anyone is lost in the shuffle, please text here or on slack. (Yatish Kumar | Nov 2, 2020, 17:12 UTC)
	+ this meeting sphere is apparently available overnight (Jeffrey Vetter | Nov 2, 2020, 21:05 UTC)
	+ not sure if we get notifications - (Jeffrey Vetter | Nov 2, 2020, 21:05 UTC)
	+ please go ahead and add to or organize the content. (Yatish Kumar | Nov 2, 2020, 21:08 UTC)
	+ Please vote. We can get a tally tomorrow. (Yatish Kumar | Nov 2, 2020, 21:09 UTC)