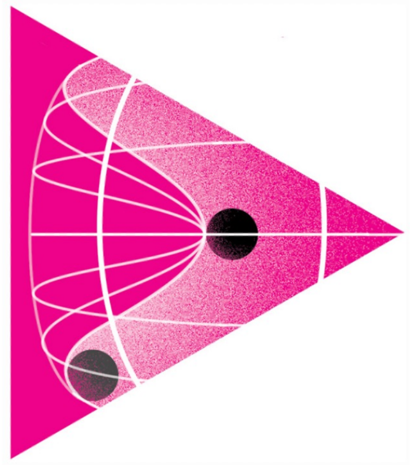
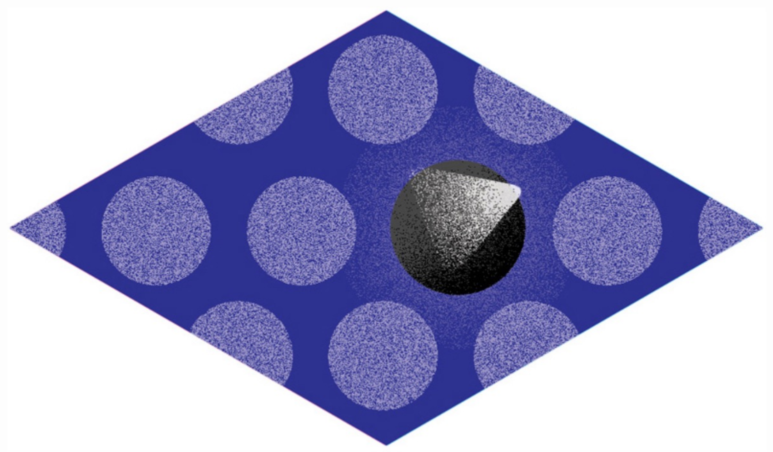
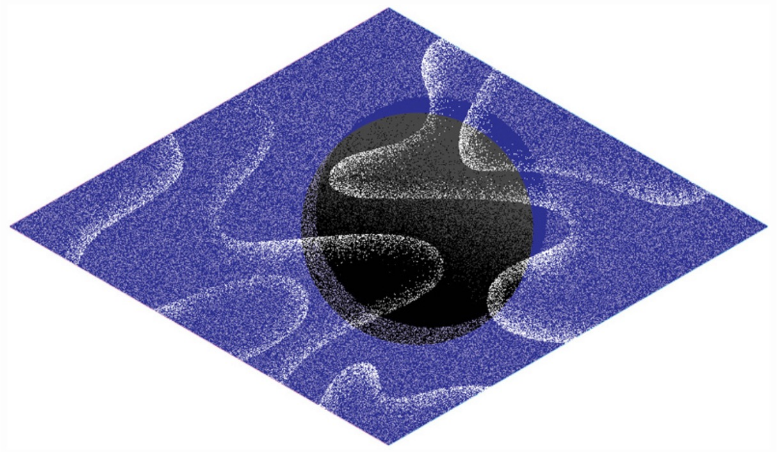
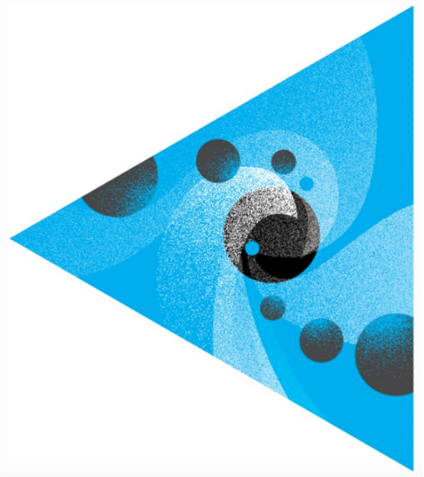
Advanced Accelerator Concepts and the 2023 P5 Report









Cameron Geddes
Non-P5 collaborators: Jens Osterhoff, Carl Schroeder, Jeroen van Tilborg, Remi Lehe,
Axel Huebl, Jean-Luc Vay, Eric Esarey; *Lawrence Berkeley National Laboratory*
Spencer Gessner, Brendan O'Shea, Rachel Margraf, Mark Hogan,
SLAC National Accelerator Laboratory
Philippe Piot, Jing, Chunguang Jing, John Power; *Argonne National Laboratory*
Gwanghui Ha, Xueying Lu, *Northern Illinois University*
Pietro Musumeci, *University of California, Los Angeles*
Ralph Assmann, *GSI Helmholtzzentrum für Schwerionenforschung*

Advanced Accelerator Concepts Workshop, 22 July 2024

Outline

P5 report context (panel member)
  Top level recommendations
  Accelerator and Collider Area Recommendations and Content

Personal perspectives: AAC next steps (not P5 content)

Exploring
the
Quantum
Universe

Pathways to Innovation
and Discovery
in Particle Physics

Report of the 2023 Particle Physics Project Prioritization Panel

P5 Report Context

2023p5report.org

With slide credit to Tulika Bose
and other P5 members

P5 Panel

Shoji Asai, Patrick Huber, Richard Schnee, Amalia Ballarino, Kendall Mahn, Sally Seidel (ex Officio since June 2023), Tulika Bose, Rachel Mandelbaum, Seon-Hee Seo, Kyle Cranmer, Jelena Maricic, Jesse Thaler, Francis-Yan Cyr-Racine, Petra Merkel, Christos Touramanis, Sarah Demers, Christopher Monahan, Abigail Vieregg, Cameron Geddes, Hitoshi Murayama (Chair), Amanda Weinstein, Yuri Gershtein, Peter Onyisi, Lindley Winslow, Karsten Heeger (Deputy Chair), Mark Palmer, Tien-Tien Yu, Beate Heinemann, Tor Raubenheimer, Robert Zwaska, JoAnne Hewett (ex officio until May 2023), Mayly Sanchez

Full P5 presentations …

HEPAP Presentation in DC, December 7-8

Draft report presented to HEPAP for discussion and open community feedback
Unanimous support of the report by HEPAP vote

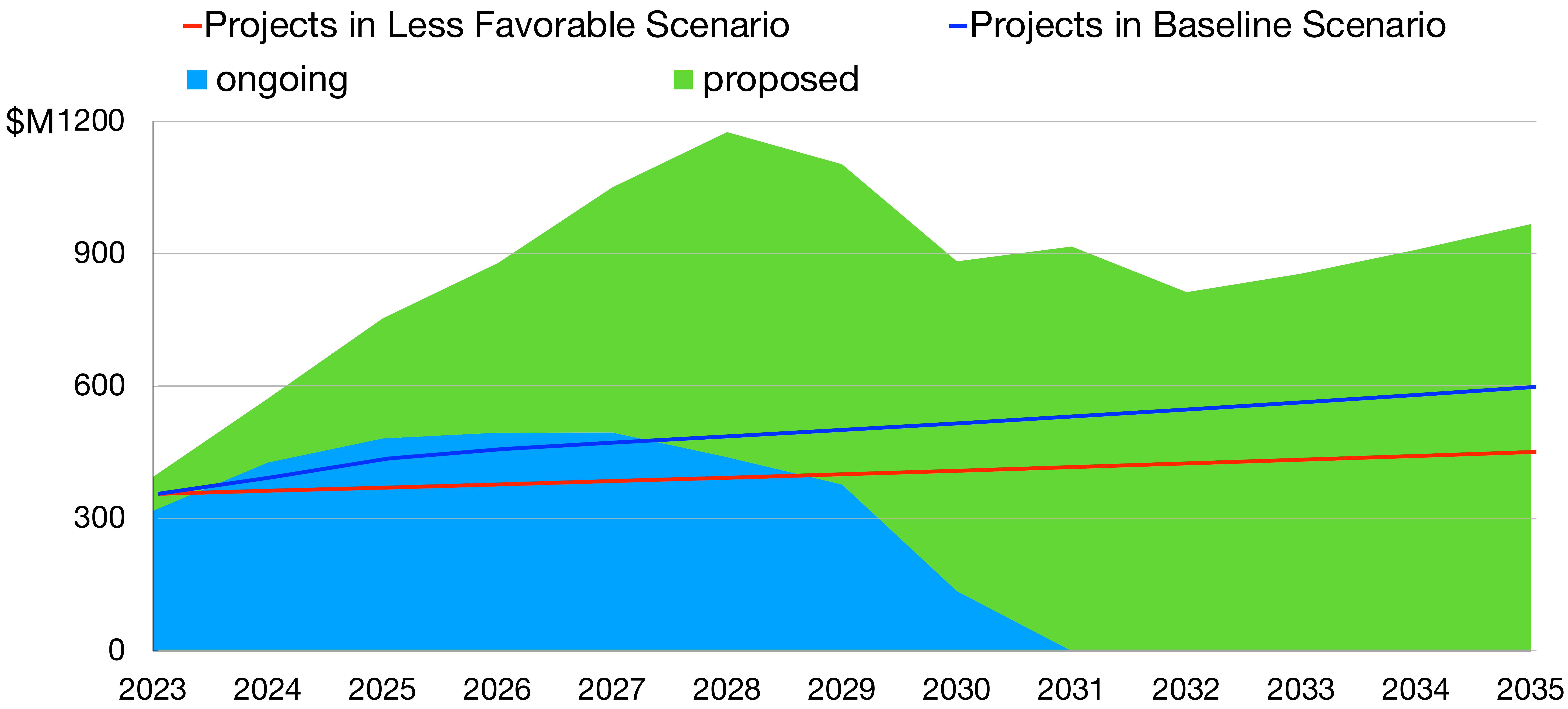
FNAL Town Hall, December 11…

Today is not a full presentation of the P5 report

Today's presentation will focus on accelerator and collider studies and perspectives for advanced acceleration concepts

Recommendations shown are a subset and do not represent priority

Proposed projects exceeded scenarios



not including on-shore Higgs factory

DOE only

Exploring the Quantum Universe

Recommendation 1

Not Rank-Ordered

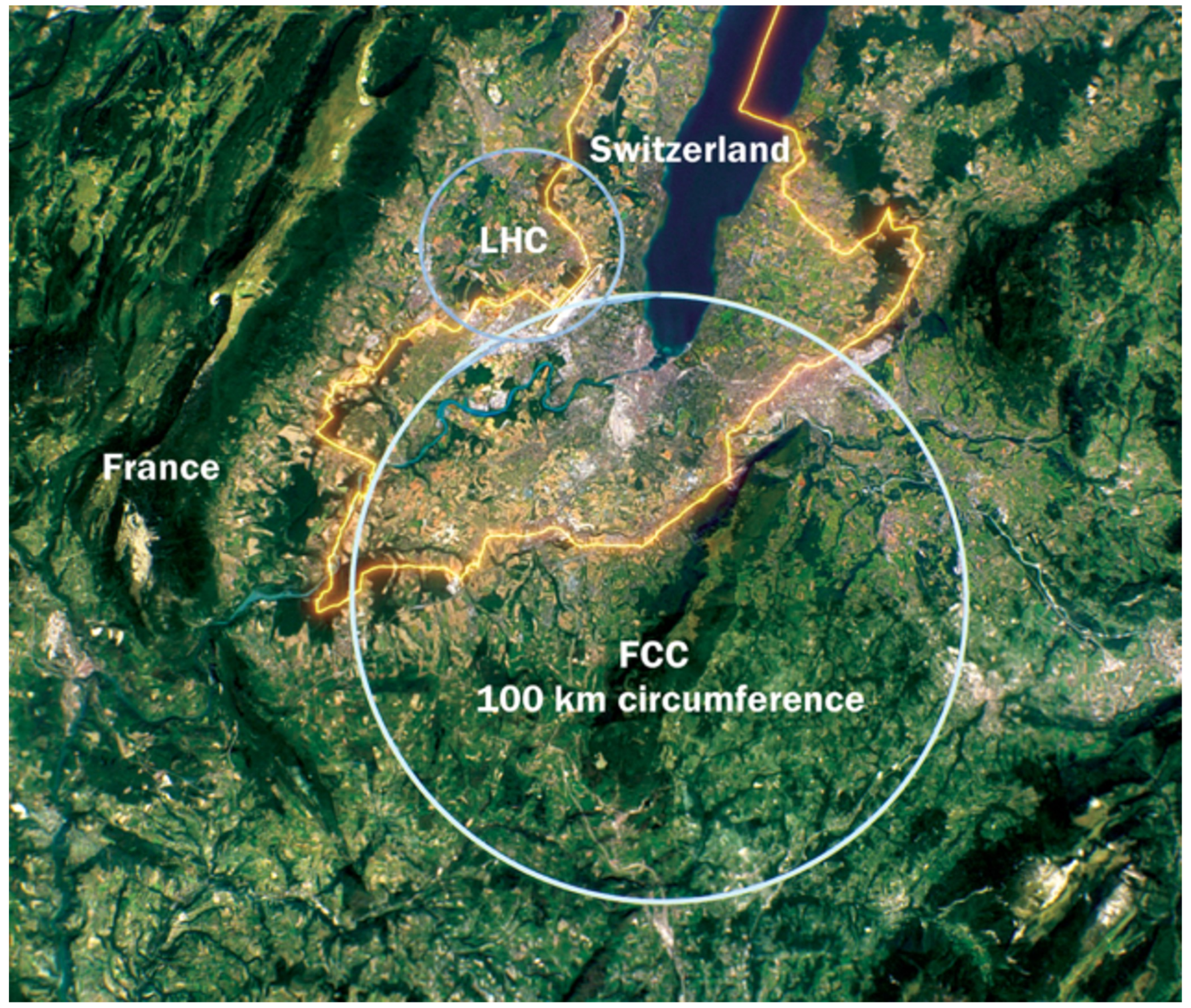
As the highest priority independent of the budget scenarios, complete construction projects and support operations of ongoing experiments and research to enable maximum science. We reaffirm the previous P5 recommendations on major initiatives:

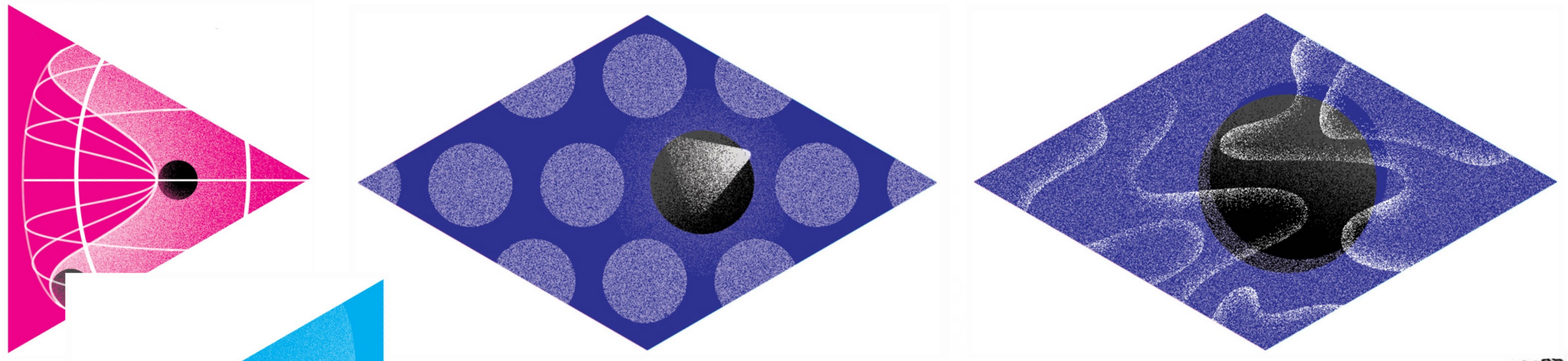
a. HL-LHC (including ATLAS and CMS detectors, as well as Accelerator Upgrade Project) to start addressing why the Higgs boson condensed in the universe (reveal the secrets of the Higgs boson, section 3.2), to search for direct evidence for new particles (section 5.1), to pursue quantum imprints of new phenomena (section 5.2), and to determine the nature of dark matter (section 4.1).
b. The first phase of DUNE and PIP-II to determine the mass ordering among neutrinos, a fundamental property and a crucial input to cosmology and nuclear science (elucidate the mysteries of neutrinos, section 3.1).
c. The Vera C. Rubin Observatory to carry out the LSST, and the LSST Dark Energy Science Collaboration, to understand what drives cosmic evolution (section 4.2).

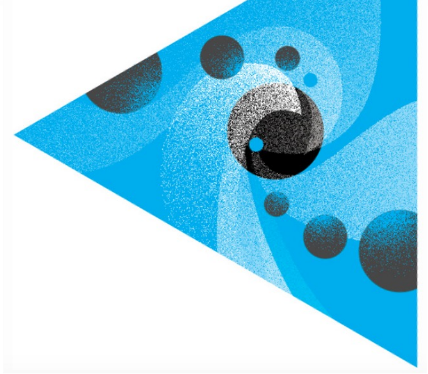
Higgs Factory

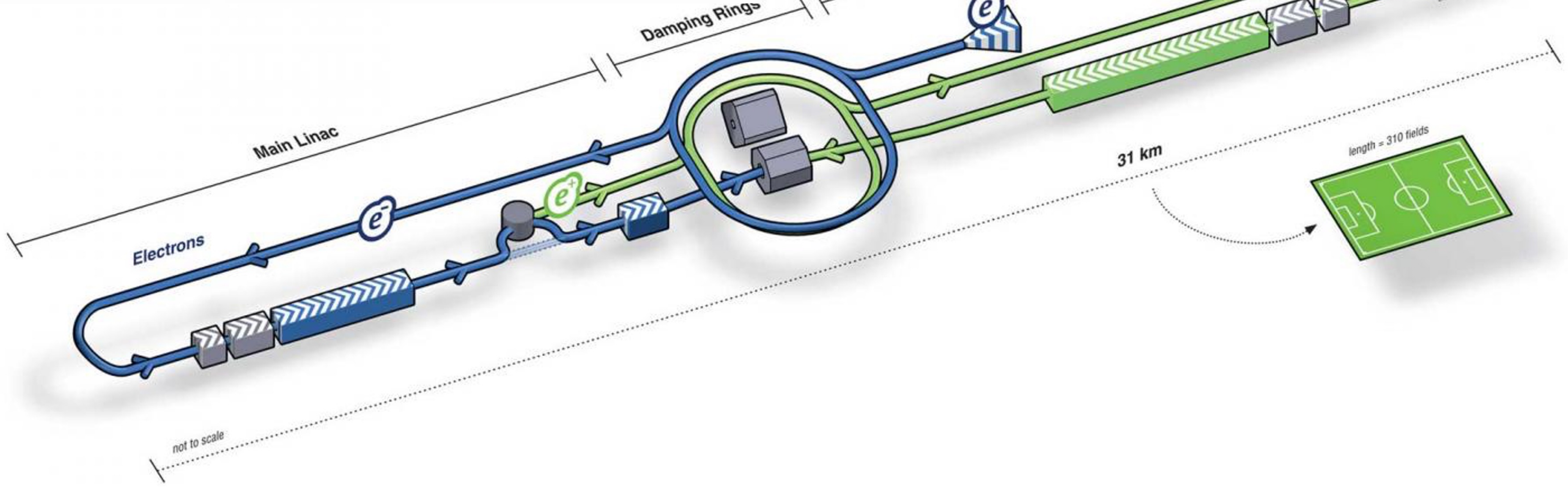
electron-positron collider covering center-of-momentum energy range 90 - 350 GeV

- Precision measurements of Higgs couplings and some production modes
- Improve knowledge of coupling to charm quark, potentially provide access to coupling to strange quark
- Order of magnitude improved sensitivity to Higgs invisible decays
- Improved sensitivity to direct searches for feebly coupled light states, such as heavy neutral leptons and axion-like particles
- EW sector consistency checks, testing through quantum loops that relate W & Z bosons, the top quark, and the Higgs, extend the probed energy scale by a factor of 3–10 beyond the HL-LHC









Exploring the Quantum Universe

Recommendation 2

Rank-Ordered

Construct a portfolio of major projects…

a. **CMB-S4**, which looks back at the earliest moments of the universe to probe physics at the highest energy scales. It is critical to install telescopes at and observe from both the South Pole and Chile sites to achieve the science goals (section 4.2).

b. **Re-envisioned second phase of DUNE** with an early implementation of an enhanced 2.1 MW beam—ACE-MIRT—a third far detector, and an upgraded near-detector complex as the definitive long-baseline neutrino oscillation experiment of its kind (section 3.1).

c. **An off-shore Higgs factory**, realized in collaboration with international partners, in order to reveal the secrets of the Higgs boson. The current designs of FCC-ee and ILC meet our scientific requirements. The US should actively engage in feasibility and design studies. Once a specific project is deemed feasible and well-defined (see also Recommendation 6), the US should aim for a contribution at funding levels commensurate to that of the US involvement in the LHC and HL-LHC, while maintaining a healthy US on-shore program in particle physics (section 3.2).

d. **An ultimate Generation 3 (G3) dark matter direct detection experiment** reaching the neutrino fog, in coordination with international partners and preferably sited in the US (section 4.1).

e. **IceCube-Gen2** for study of neutrino properties using non-beam neutrinos complementary to DUNE and for indirect detection of dark matter covering higher mass ranges using neutrinos as a tool (section 4.1).

Recommendation 3

Not Rank-Ordered

Create an improved balance between small-, medium-, and large-scale projects **to open new scientific opportunities and maximize their results, enhance workforce development, promote creativity, and compete on the world stage.**

In order to achieve this balance across all project sizes we recommend the following:

a. Implement a new small-project portfolio at DOE, **Advancing Science and Technology through Agile Experiments (ASTAE)**, across science themes in particle physics with a competitive program and recurring funding opportunity announcements. This program should start with the construction of experiments from the Dark Matter New Initiatives (DMNI) by DOE-HEP (section 6.2).

b. Continue Mid-Scale Research Infrastructure (**MSRI**) and Major Research Instrumentation (**MRI**) programs as a critical component of the NSF research and project portfolio.

c. Support **DESI-II** for cosmic evolution, **LHCb upgrade II** and **Belle II upgrade** for quantum imprints, and **US contributions to the global CTA Observatory** for dark matter (sections 4.2, 5.2, and 4.1).

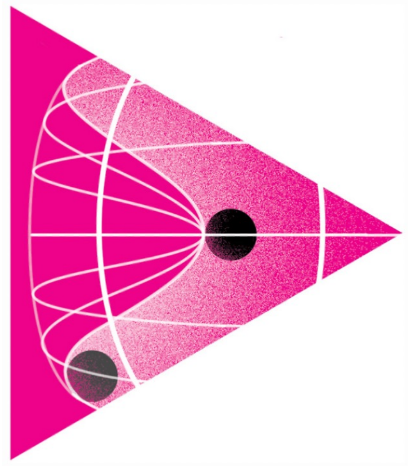
The Belle II recommendation includes contributions towards the SuperKEKB accelerator.

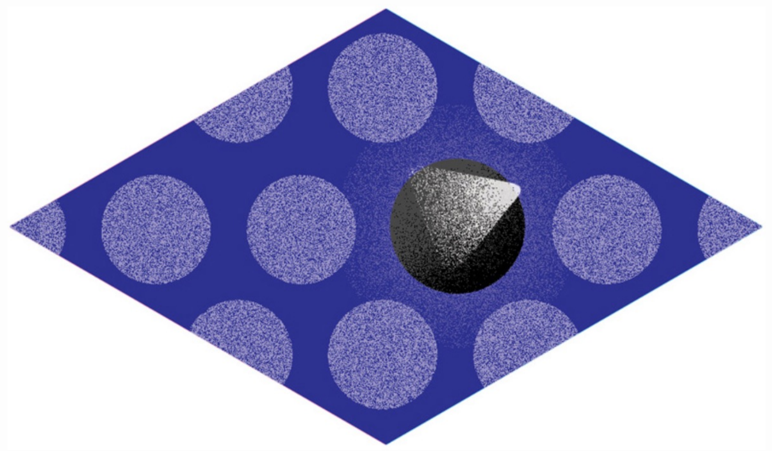
10 TeV pCM collider

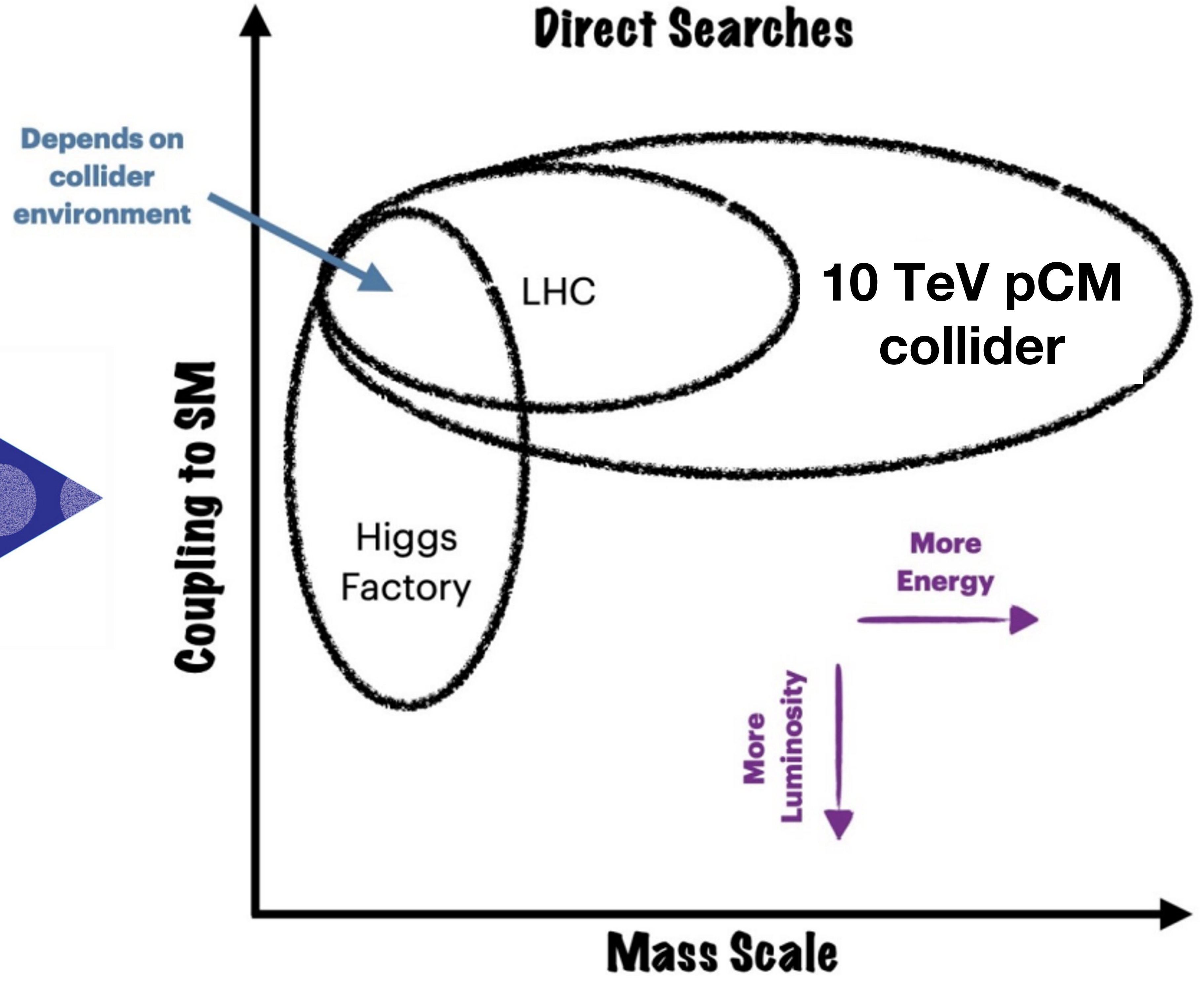
Based on proton, muon, or possible wakefield technologies

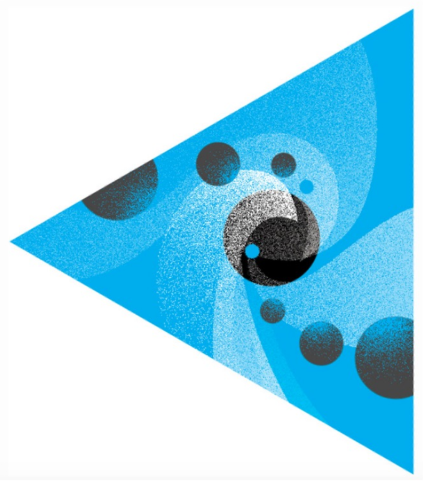
Higgs self-coupling precision of 5%, an order of magnitude better than the HL-LHC

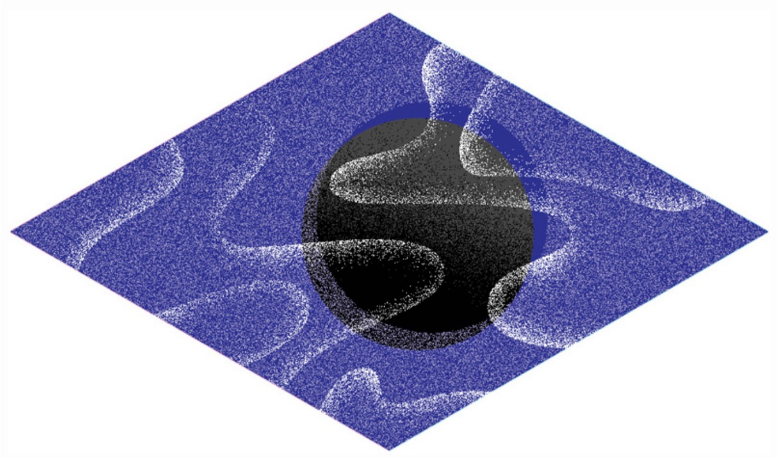
- Further improve precision for Higgs couplings (e.g. top quark-Higgs coupling, muon pairs, Zg)
- Direct discovery of particles responsible for potential deviations observed at Higgs factory
- Sensitivity for new gauge bosons, fermions, or other resonances will be extended by an order of magnitude beyond the HL-LHC
- Access to rare decays and new hidden sectors, furthering the mass reach to new particles well beyond the HL-LHC
- Reach the thermal WIMP target for minimal WIMP candidates











Recommendation 4

Support a comprehensive effort to develop the resources—theoretical, computational, and technological—essential to our 20-year vision for the field. This includes an aggressive R&D program that, while technologically challenging, could yield revolutionary accelerator designs that chart a realistic path to a 10 TeV pCM collider.

Investing in the future of the field to fulfill this vision requires the following:

Recommendation 4

Not Rank-Ordered

a. Support **vigorous R&D toward a cost-effective 10 TeV pCM collider based on proton, muon, or possible wakefield technologies,** including an evaluation of options for US siting of such a machine, with a goal of being ready to build **major test facilities and demonstrator facilities within the next 10 years** (sections 3.2, 5.1, 6.5, and Recommendation 6).

b. Enhance research in **theory** to propel innovation, maximize scientific impact of investments in experiments, and expand our understanding of the universe (section 6.1).

c. Expand the **General Accelerator R&D (GARD)** program within HEP, including stewardship (section 6.4).

d. Invest in R&D in **instrumentation** to develop innovative scientific tools (section 6.3).

e. Conduct **R&D** efforts to define and enable new projects in the next decade, including detectors for an e^+e^- Higgs factory and 10 TeV pCM collider, Spec-S5, DUNE FD4, Mu2e-II, Advanced Muon Facility, and line intensity mapping (sections 3.1, 3.2, 4.2, 5.1, 5.2, and 6.3).

f. Support key **cyberinfrastructure** components such as shared software tools and a sustained R&D effort in computing, to fully exploit emerging technologies for projects. Prioritize **computing and novel data analysis techniques** for maximizing science across the entire field (section 6.7).

g. Develop plans for improving the **Fermilab accelerator complex** that are consistent with the long-term vision of this report, including neutrinos, flavor, and a 10 TeV pCM collider (section 6.6).

We recommend specific budget levels for enhanced support of these efforts and their justifications as **Area Recommendations** in section 6.

Recommendation 5

Invest in initiatives aimed at developing the workforce**,** broadening engagement**, and supporting** ethical conduct **in the field. This commitment nurtures an advanced technological workforce not only for particle physics, but for the nation as a whole.**

Recommendation 6

Convene a targeted panel with broad membership across particle physics later this decade that makes decisions on the US accelerator-based program at the time when major decisions concerning an off-shore Higgs factory are expected, and/or significant adjustments within the accelerator-based R&D portfolio are likely to be needed. A plan for the Fermilab accelerator complex consistent with the long-term vision in this report should also be reviewed.

The panel would consider the following:

1. The level and nature of **US contribution in a specific Higgs factory** including an evaluation of the associated schedule, budget, and risks once crucial information becomes available.
2. Mid- and large-scale **test and demonstrator facilities** in the accelerator and collider R&D portfolios.
3. A plan for the evolution of the **Fermilab accelerator complex** consistent with the longterm vision in this report, which may commence construction in the event of a more favorable budget situation.

Collider R&D

Area Recommendation: To enable targeted R&D before specific collider projects are established in the US, an investment in collider detector R&D funding at the level of $20M per year and **collider accelerator R&D at the level of $35M per year in 2023 dollars is warranted**.

Collider R&D

Targeted collider R&D is required to translate advancements in detector and accelerator technology into the experimental facilities that shape our understanding of the universe. …

Targeted R&D investments are **crucial for developing comprehensive designs with cost models, guiding technology advancements and collider pathways, establishing advanced performance benchmarks for detectors and accelerators, and training the next generation of experts**.

This increased investment complements general detector and accelerator R&D (Section 6.3, 6.4), which focuses on developing the necessary infrastructure and technologies. This synergistic approach is essential for positioning the US as a leader in projects outlined in our 20-year vision. This includes **robust participation in an off-shore Higgs factory** and a **pivotal role in shaping the path towards a future 10 TeV pCM machine, potentially on US soil.**

Collider R&D: Higgs Factory

(accelerator portion)
The decisions related to construction of an off-shore Higgs factory are anticipated to be made later this decade. The **current designs of both FCC-ee and the ILC satisfy our scientific requirements.** To secure a prominent role in a future Higgs factory project, the **US should actively engage in feasibility and design studies** (Recommendation 2c). Engagement with FCC-ee specifically should include design and modeling to advance the feasibility study, as well as R&D on superconducting radio frequency cavities designed for the ring and superconducting magnets designed for the interaction region. These efforts benefit from synergies in workforce development through participation in SuperKEKB and the Electron-Ion Collider.

Maintaining engagement with ILC accelerators through the ILC Technology Network can include design updates and cryomodule construction. These will support significant US contributions to potential projects. A global framework for future collider development, such as the ILC International Development Team as implemented by ICFA for the ILC, is relevant for all future colliders.

… (parallel detector effort not shown here for space)…

Major international decisions on the route to a Higgs factory are anticipated later this decade. Supported by ICFA, the Japanese HEP community remains committed to hosting the ILC in Japan as a global project. The FCC-ee feasibility study is scheduled for completion by 2025, followed by an update by the European Strategy Group and a decision by the CERN Council. Once a specific project is deemed feasible and well-defined, the US should focus efforts towards that technology. A separate panel should determine the level and nature of US contribution while maintaining a healthy US on-shore program in particle physics (recommendation 6). **In the scenario where a global consensus to move forward with the Higgs factory is not reached, the next P5 should reevaluate.**

Exploring the Quantum Universe

Collider R&D: 10 pCM collider

(accelerator portion)

Parallel to the R&D for a Higgs factory, the US should pursue a 10 TeV pCM collider.

End-to-end designs are needed well before a decision can be made on a project in order to understand potential performance parameters and costs. These will guide research priorities and technology development as well as demonstrator facilities. Such early designs will also play a critical role in creating and sustaining the expertise to design such machines. Progress on these end-to-end designs should be evaluated (Recommendation 6).

The 10 TeV pCM energy scale and potential performance benefits **motivate muon collider development, as well as ongoing work to advance proton and possible advanced wakefield accelerator paths (**section 6.4.1). The US should pursue a leading role in the muon collider design effort, in concert with the International Muon Collider Collaboration (IMCC). This includes R&D on relevant technologies and preparations for a demonstrator facility. Delivery of a baseline design later this decade is also a crucial milestone. Development of technologies under accelerator R&D (Section 6.4) are essential to this effort, including superconducting magnets at higher field crucial to both future proton (FCC-hh) and muon colliders, and high temperature superconductors suitable for high field and temperature. Similarly, **progress in advanced wakefield accelerators motivates efforts to develop a self-consistent design to understand feasibility and costs. Each of these research areas will benefit from international engagement to enable timely progress**.

(context)

Wakefield concepts for a collider are in the early stages of development. A critical next step is the delivery of an end-to-end design concept, including cost scales, with self-consistent parameters throughout. This will provide an important yardstick against which to measure progress with this emerging technology path.

General Accelerator R&D

Area recommendation: Increase annual funding to the General Accelerator R&D program by $10M in 2023 dollars to ensure US leadership in key areas.

General Accelerator R&D

Broad generic R&D with a long term focus is critical to extending the reach of accelerators to meet future physics needs. **Technical breakthroughs** are required to enable accelerators to **meet the field's science drivers, to push costs lower than estimates based on current technology, and to reduce environmental impact. There are exciting opportunities in the development of (i) new high average power, efficient drivers (RF, lasers, and electron beams), (ii) accelerating structures that can sustain high average power and gradient (metallic, plasma and dielectric), (iii) high temperature superconducting magnets, and (iv) computing, instrumentation and controls. Normal conducting radio frequency (RF), superconducting RF, superconducting magnets, targets, and advanced acceleration concepts are essential** to develop the next generation of accelerators for particle physics. The normal conducting RF program should incorporate innovative concepts such as cryogenic cool copper and distributed coupling. **Accelerator and beam physics research is also critical, including large-scale computation as machines become more complex.** Superconducting high field magnet R&D is essential to future proton (FCC-hh) and muon collider options; timely execution of magnet R&D would leverage expertise becoming available with the completion of the HL-LHC Accelerator Upgrade Project.

Context

While it will take time to assemble the teams required to inform these decisions, it is imperative that this **R&D is pursued aggressively if we hope to act on our most ambitious goal of initiating a 10 TeV pCM collider shortly after the conclusion of the HL-LHC** program.

Key acceleration and beam **requirements of a stage for a future collider based on wakefield** technology [can be developed], including energy gain with high brightness beams at high efficiency… **operation with two linked stages.**

Robust growth in the field requires **strengthened investment in education, training, and retention** to renew the workforce and develop expertise in accelerator disciplines. A strong and creative workforce is necessary to develop and build new accelerators and colliders. Creating such a workforce is **driven by state-of-the-art R&D** that attracts high-level talent and can execute the machine design, development, and research needed for major new accelerator projects. Such growth is **seeded by development of university groups and targeted training, such as the curriculum at the US Particle Accelerator School**…

Investments in **Accelerator Science and Technology (AS&T) drive innovation that has benefits extending well beyond particle physics**. AS&T efforts that directly support particle physics goals also meet critical needs in other offices, agencies and organizations…. More broadly, these investments lead to **valuable partnerships** with other DOE Science offices, other US agencies such as the Defense Advanced Research Projects Agency (DARPA) and the National Nuclear Security Agency (NNSA), academia, and industry. These partnerships are dynamic drivers of innovation and progress in accelerators that **benefit both particle physics and the nation**.

Exploring
the
Quantum
Universe

Test Facilities

Area recommendation: Support generic accelerator R&D with the construction of small scale test facilities. Initiate construction of larger test facilities based on project review, and informed by the collider R&D program.

Test facilities are ever more important to develop the advanced technology for future machines (Recommendation 4a, 4c). The need is magnified by the small number of training opportunities on operating machines and the significant timescales and technical demands of the next colliders. Use of the existing test facilities should be maximized.

Future test facilities would typically be mid-scale projects. **Technical and scientific plans should be developed for test facility projects that could be launched within the next 5–10 years.**

These could include the second stage cool copper test, which could develop high gradient normal conducting RF technology.

Advanced accelerator test facilities can explore technology and concepts that could significantly reduce cost and risks associated with a 10 TeV pCM collider. An upgrade for **FACET-II e^+** is uniquely positioned to enable study of positron acceleration in high gradient plasmas. New kW-class efficient lasers, and use of their kilohertz repetition rate for active feedback at **kBELLA,** will advance stage performance and enable beam tests. An **AWA** upgrade would support GeV advanced structures.

These, together with muon collider development, will advance the technology and **feed into a future demonstrator facility** to make possible a 10 TeV pCM collider (see Sec. 6.5). Many of these projects may be ready for scientific, technical, and cost reviews within the context of the HEP program toward the middle to end of this decade (Recommendation 6).

Outline

P5 report context (panel member)
    Top level recommendations
    Accelerator and Collider Area Recommendations and Content

Personal perspectives: AAC next steps (not P5 content)

Following content
is not P5 output

Motivation

- Strong AAC progress assessing limits and demonstrating technologies continues motivation
  - Experimental results: 10 GeV class beams, beam loading & efficiency, plasma recovery, staging, high transformer ratio, positrons, and FEL-lasing demonstrating high beam quality
  - Concepts addressing: ion motion, synchrotron radiation, scattering, hosing and positron acceleration advance potential for a future collider
  - R&D and test facility progress, while leveraging near term applications, are important.
- Wakefield collider conceptual parameter sets developed based on component simulations
  - Potential for compact, energy efficient future e-e+/$\gamma\gamma$ colliders
- Development of new acceleration and beam manipulation methods continues to expand options

Personal perspectives (not P5 content)

Takeaway messages

- Interest shifted from the TeV range to emphasize 10+ TeV/parton energies, after a Higgs factory
  - Leptons offer clean collisions and strong physics potential

- Challenging energy scale emphasizes
  - R&D on new technologies
  - Energy efficiency
  - Leverage of nearer term applications

- Advanced acceleration methods have broad potential impact
  - Source: potential direct nm electron emittance, at 10's of kHz for luminosity
  - Accelerator: stages to minimize length & power, with precision alignment via active feedback
  - Interaction region: short bunches, strong focusing, energy recovery - potential to benefit luminosity/power
  - Positron methods: fast cooling potential, non-symmetric acceleration in plasmas. Also $\gamma\gamma$ potential

Personal perspectives (not P5 content)

Collaboration

- HEP is global and a future collider will be built through world-wide collaboration
  - Need to collaborate & build workforce across all concepts to build any collider
  - Advanced accelerators strongly develop workforce: as performance and applications advance, leverage opportunities for integration (beamline elements in plasma FELs, collider design…)

- Learn from and work with broad collider community, participate strongly in non-AAC meetings
  - IPAC, IMCC, LCWS, FCC Week, APS April meeting…
  - Explore potential of AAC technologies as injectors, components or upgrades for other machines such as a linear Higgs factory upgrade to 10 TeV, as well as for light sources

- Engage particle and detector physicists – there are unique issues with WFA detectors e.g. short bunches. Relaying parameters so they can develop the physics case is very important.

Personal perspectives (not P5 content)

Collaboration

- Develop technology in targeted ways that advance collider concept
  - Address key technical gaps identified, refresh technical roadmaps
  - Sustainability is a key metric and wakefields have potential assets to explore
- Partnerships with other DOE Science Offices, other agencies important to build capability
  - BES, FES, NNSA, NSF, DARPA, others…
  - Leverage broad applications to prove technology
- Leverage near term applications to advance collider path and reduce cost and risk, working across funding agencies

Personal perspectives (not P5 content)

Next Steps

- Develop end-to-end design concept (not CDR level) including cost scales, with self-consistent parameters throughout, guiding continued development
  - Integrating: injection, cooling, alignment tolerances, stage matching/coupling, BDS and Final focus
  - Engaging: the detector, high energy physics and broad collider communities, & internationally
  - Framing: a common wakefield collider concept with techniques offering technical risk mitigation
  - Path through near term applications, 10’s GeV multistage demonstrator, series of collider steps

- Continue development of performance, targeting collider needs and using test facilities
  - Key acceleration and beam requirements of a stage for a future collider including energy gain with high brightness beams at high efficiency
  - Operation with two linked stages and high coupling efficiency; Very high brightness beams
  - Methods to reduce cost and risk, guided by collider R&D & applications, potentially motivating new facilities

    Candidates incl.:positrons (FACET-II); GeV structures (AWA); rate and active feedback for precision (kBELLA)

Personal perspectives (not P5 content)

Backup material

P5 report context (panel member)
- Top level recommendations
- General accelerator
- Collider studies
- Cool copper

Personal perspectives (not P5 content)
- Sequence of studies
- C3 overlap with other concepts

These are intended to seed the discussion for this session

Collaboration and Overlap

- Cool copper is a technology with broad potential (as are wakefields, HTS magnets…)
  - Fundamental RF technology, test facilities, training and workforce

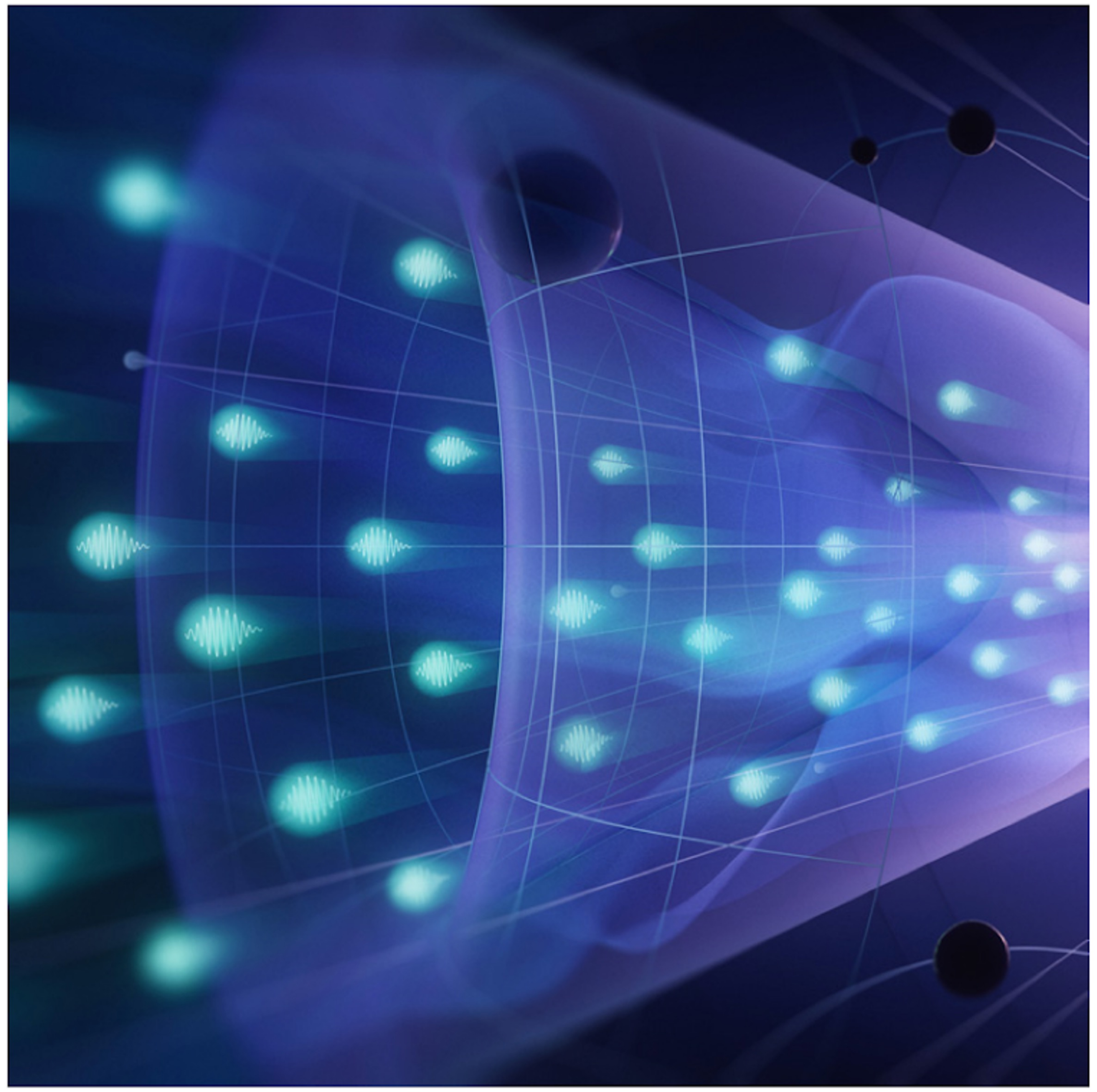
- Many potential opportunities in HEP
  - FCCee injector option?
  - ILC and wakefield commonality: focusing, interaction region, feedback, detectors
  - Linac and related studies – overlap with ATF3, GRIT, others?
  - Wakefield drivers, heat dissipation, positrons (potential mutual test facility?)
    upgrades: can we re-use the accelerator as driver? Re-use facility?
    complementarity – how far can we push the HALHF concept? Others?
  - Other areas, e.g. muon cooling cavities, accelerator?

Personal perspectives (not P5 content)

Acknowledgements

P5: We thank members of the cost subcommittee for their timely and hard work, in particular its chair, Jay Marx. We also thank all the national laboratories that made their staff available for this important task. We thank people at funding agencies for providing us all necessary information and support throughout the process. We thank our peer reviewers for giving us constructive feedback under a tight deadline. We thank Lawrence Berkeley National Laboratory, Fermilab, Argonne National Laboratory, Brookhaven National Laboratory, SLAC National Laboratory, Virginia Tech University, and University of Texas Austin for hosting the town halls. We thank James Dawson and Marty Hanna for professional editing. We thank Michael Branigan, Brad Nagle, Olena Shmahalo and Abigail Malate for providing beautiful graphics and layout. We thank the Yale Physics Department for supporting the development of the website. We thank Kerri Fomby, Jody Crisp, and Taylor Pitchford at ORISE and Stephany Tone at LBNL for logistical support. We thank our families for supporting us during this year-long process. And most importantly, we thank APS/DPF for organizing the Snowmass Community Study, and all members of our community for their bold and creative vision as well as their input to the process.

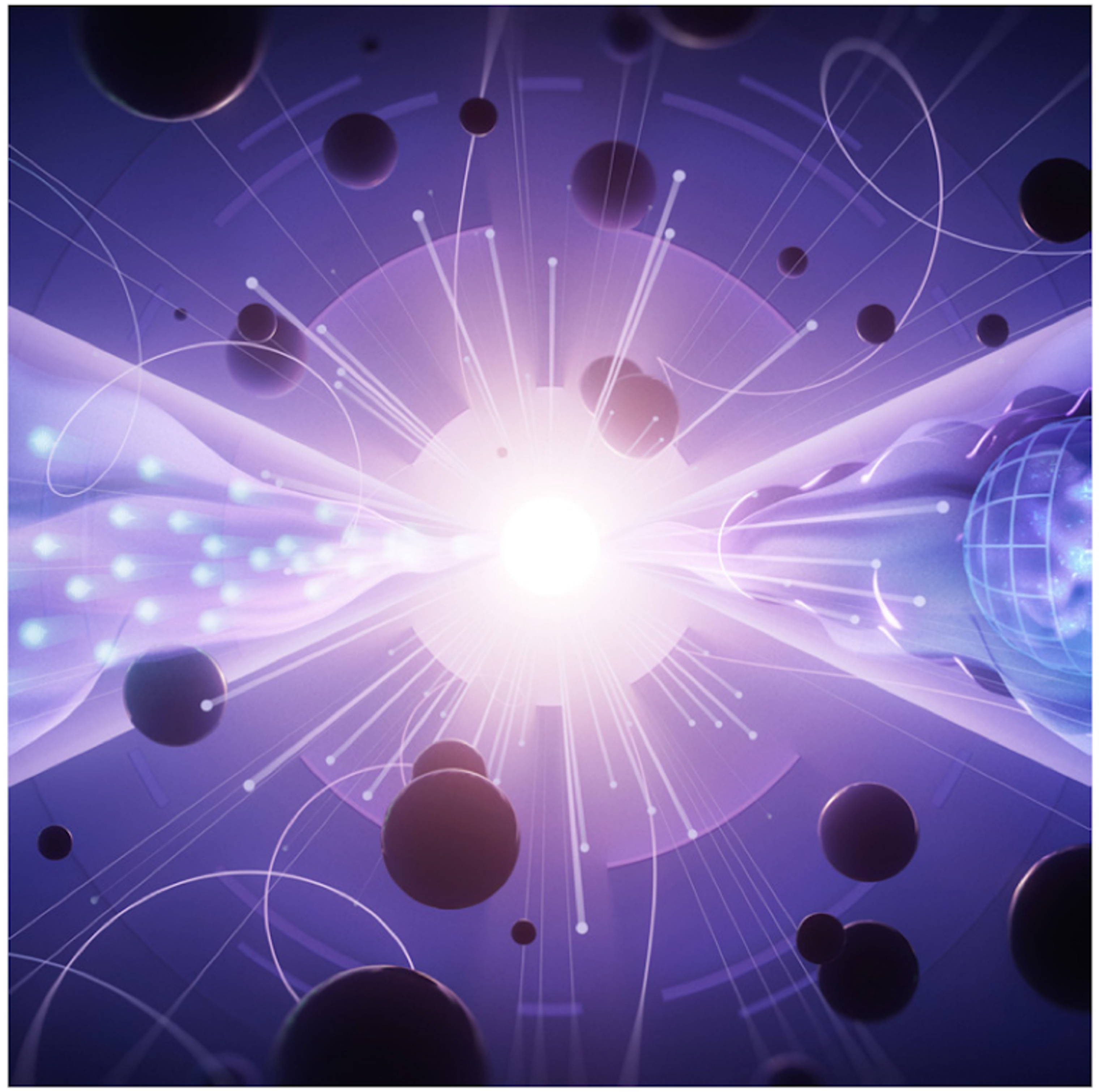
This talk: We gratefully acknowledge the authors of the P5 report, all of the members of the Accelerator Frontier 6 group of Snowmass, and of colleagues in other Accelerator, Energy, Community and other Frontiers. This work was supported by the Director, Office of Science, Office of High Energy Physics, of the U.S. Department of Energy under Contracts including No. DE-AC02-05CH11231, DE-AC02-06CH11357, and DE-AC02-76SF00515, and by the National Science Foundation. The U.S. Government retains and the publisher, by accepting the article for publication, acknowledges that the U.S. Government retains a non-exclusive, paid-up, irrevocable, world-wide license to publish or reproduce the published form of this manuscript, or allow others to do so, for U.S. Government purposes.



Decipher the Quantum Realm

Elucidate the Mysteries of Neutrinos

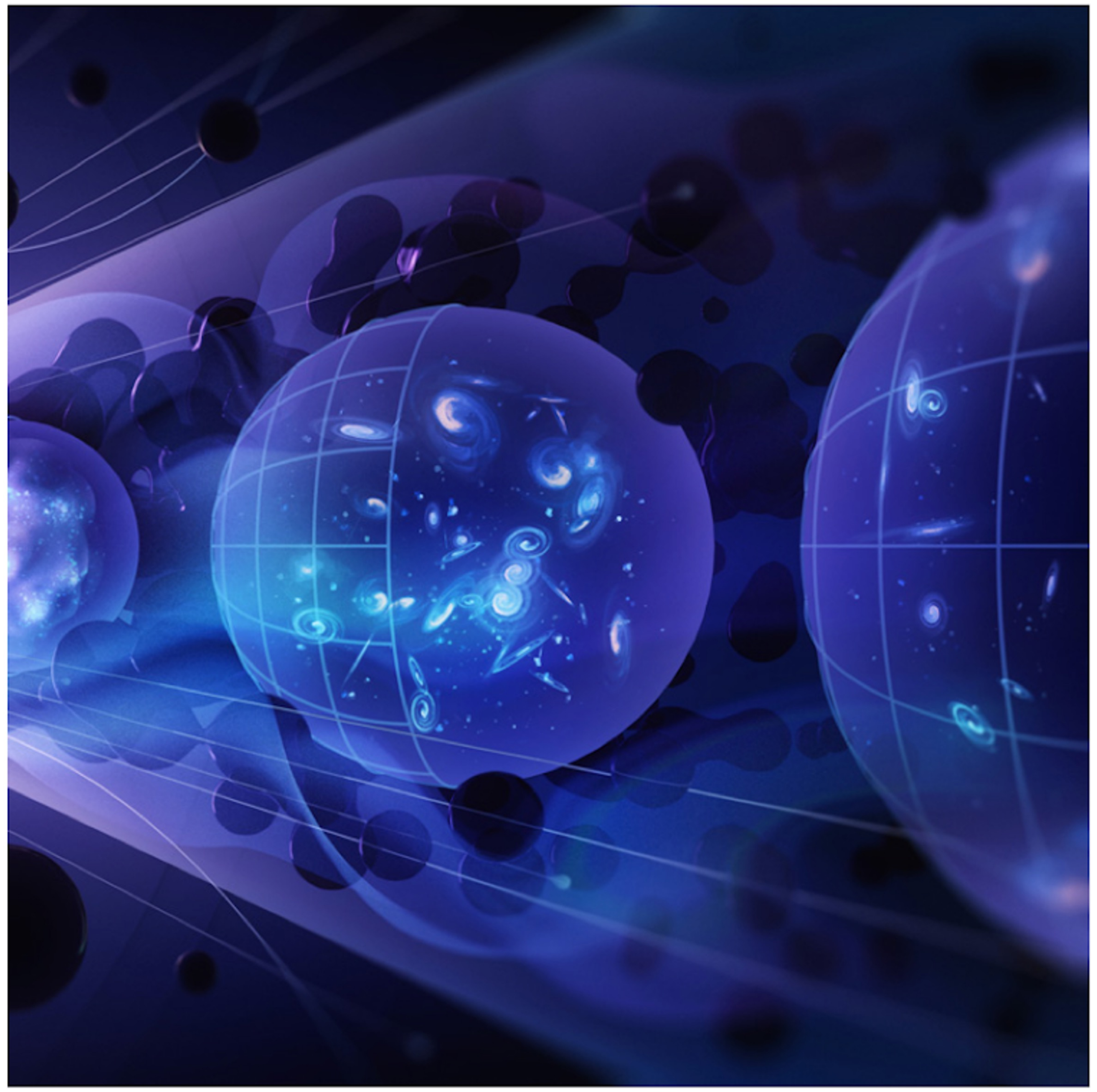
Reveal the Secrets of the Higgs Boson



Explore New Paradigms in Physics

Search for Direct Evidence of New Particles

Pursue Quantum Imprints of New Phenomena



Illuminate the Hidden Universe

Determine the Nature of Dark Matter

Understand What Drives Cosmic Evolution

Difficult Choices

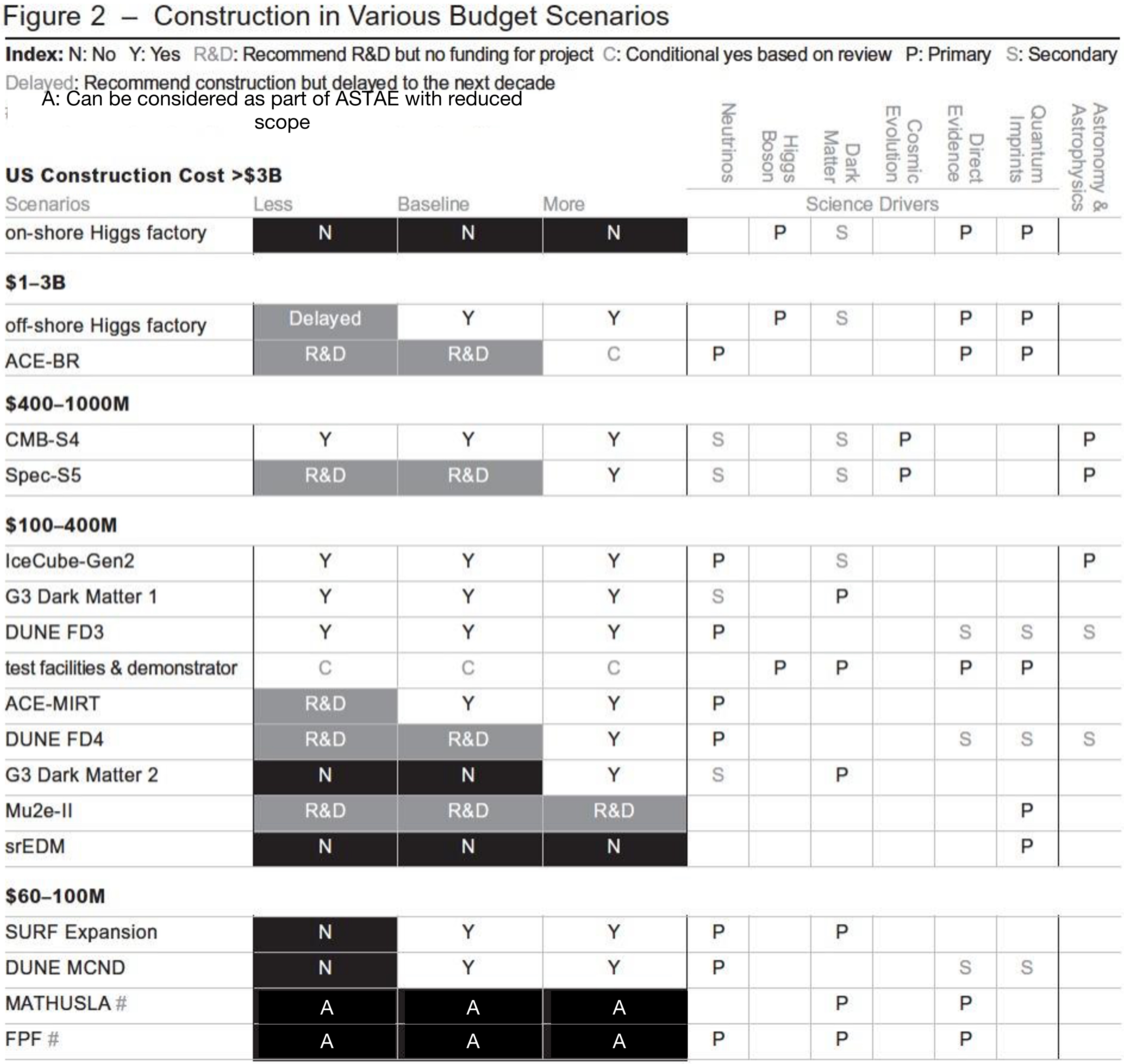


Figure 2 – Construction in Various Budget Scenarios

Index: N: No Y: Yes R&D: Recommend R&D but no funding for project C: Conditional yes based on review P: Primary S: Secondary

Delayed: Recommend construction but delayed to the next decade

A: Can be considered as part of ASTAE with reduced scope

Scenarios	Less	Baseline	More	Neutrinos	Higgs Boson	Dark Matter	Cosmic Evolution	Direct Evidence	Quantum Imprints	Astronomy & Astrophysics
US Construction Cost >$3B										
on-shore Higgs factory	N	N	N		P	S		P	P	
$1–3B										
off-shore Higgs factory	Delayed	Y	Y		P	S		P	P	
ACE-BR	R&D	R&D	C	P				P	P	
$400–1000M										
CMB-S4	Y	Y	Y	S		S	P			P
Spec-S5	R&D	R&D	Y	S		S	P			P
$100–400M										
IceCube-Gen2	Y	Y	Y	P		S				P
G3 Dark Matter 1	Y	Y	Y	S		P				
DUNE FD3	Y	Y	Y	P				S	S	S
test facilities & demonstrator	C	C	C		P	P		P	P	
ACE-MIRT	R&D	Y	Y	P						
DUNE FD4	R&D	R&D	Y	P				S	S	S
G3 Dark Matter 2	N	N	Y	S		P				
Mu2e-II	R&D	R&D	R&D						P	
srEDM	N	N	N						P	
$60–100M										
SURF Expansion	N	Y	Y	P		P				
DUNE MCND	N	Y	Y	P				S	S	
MATHUSLA #	A	A	A			P		P		
FPF #	A	A	A	P		P		P		

Exploring
the
Quantum
Universe

Figure 1 – Program and Timeline in Baseline Scenario (B)

Index: 🟩 Operation ▨ Construction 🟧 R&D, Research P: Primary S: Secondary

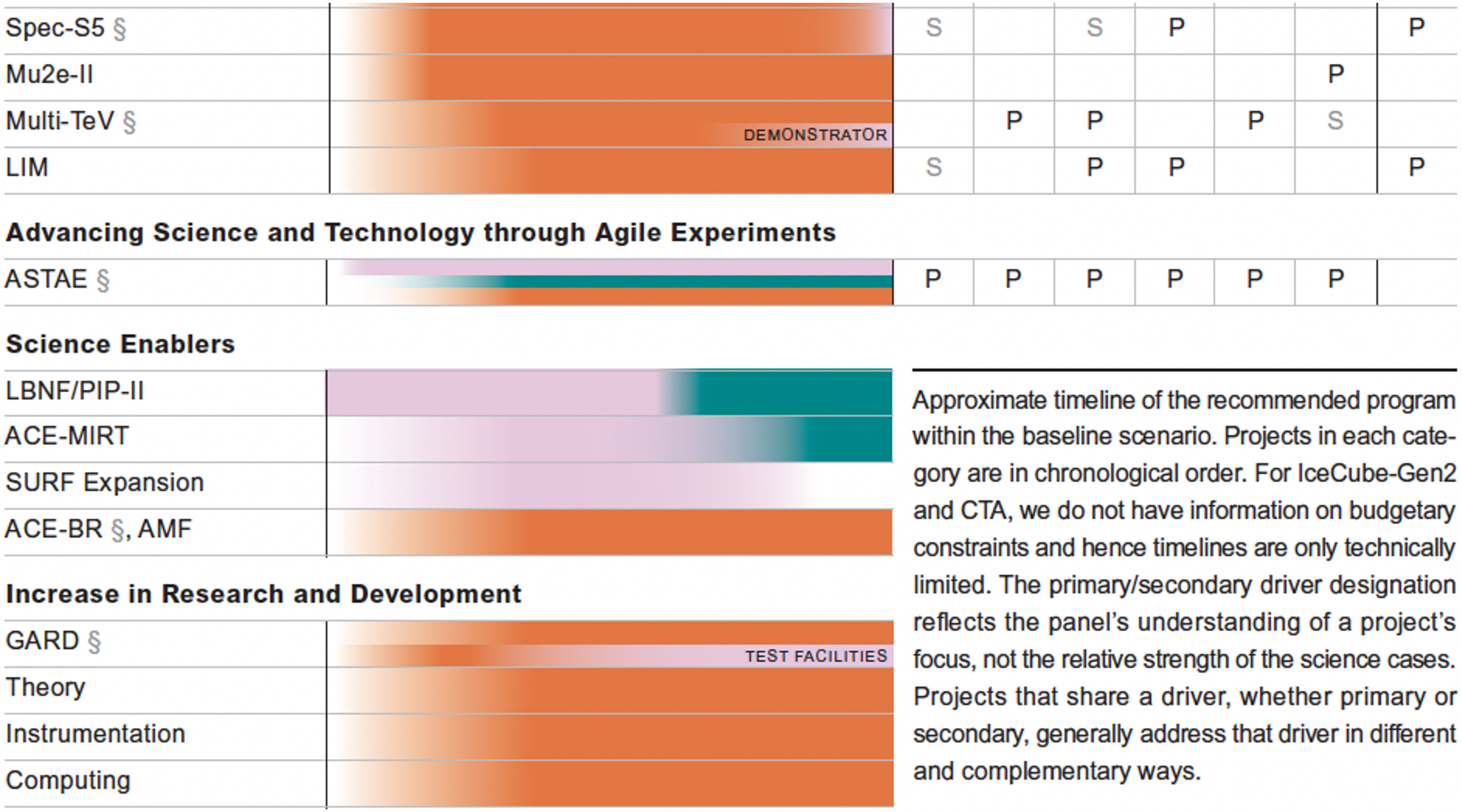
§ Possible acceleration/expansion for more favorable budget situations

Science Experiments	Neutrinos	Higgs Boson	Dark Matter	Cosmic Evolution	Direct Evidence	Quantum Imprints	Astronomy & Astrophysics
LHC		P	P		P	P	
LZ, XENONnT			P				
NOvA/T2K	P				S		
SBN	P				S		
DESI/DESI-II	S		S	P			P
Belle II			S		S	P	
SuperCDMS			P				
Rubin/LSST & DESC	S		S	P			P
Mu2e						P	
DarkSide-20k			P				
HL-LHC		P	P		P	P	
DUNE Phase I	P				S	S	S
CMB-S4	S		S	P			P
CTA			S				P
G3 Dark Matter §	S		P				
IceCube-Gen2	P		S				P
DUNE FD3	P				S	S	S
DUNE MCND	P				S	S	
Higgs factory §		P	S		P	P	
DUNE FD4 §	P				S	S	S

Figure 1 – Program and Timeline in Baseline Scenario (B)

Index: 🟩 Operation ▨ Construction 🟧 R&D, Research P: Primary S: Secondary

§ Possible acceleration/expansion for more favorable budget situations



Spec-S5 §	S		S	P			P
Mu2e-II						P	
Multi-TeV §		P	P		P	S	
LIM	S		P	P			P
Advancing Science and Technology through Agile Experiments							
ASTAE §	P	P	P	P	P	P	
Science Enablers							
LBNF/PIP-II							
ACE-MIRT							
SURF Expansion							
ACE-BR §, AMF							
Increase in Research and Development							
GARD §							
Theory							
Instrumentation							
Computing							

Approximate timeline of the recommended program within the baseline scenario. Projects in each category are in chronological order. For IceCube-Gen2 and CTA, we do not have information on budgetary constraints and hence timelines are only technically limited. The primary/secondary driver designation reflects the panel's understanding of a project's focus, not the relative strength of the science cases. Projects that share a driver, whether primary or secondary, generally address that driver in different and complementary ways.

More Favorable Budget Scenario

Not Rank-Ordered

In a budget outlook more favorable than the baseline budget scenario, we urge the funding agencies to support additional scientific opportunities. Even a small increase in the overall budget enables a large return on the investment, serving as a catalyst to accelerate scientific discovery and to unlock new pathways of inquiry. The opportunities include R&D, small projects, and the construction of advanced detectors for flagship projects in the US. They are listed below in four categories from small to large in budget size:

a. R&D

   i. Increase investment in **detector R&D** targeted toward future collider concepts for a Higgs factory and 10 TeV pCM collider in order to accelerate US leadership in this area.
   ii. Pursue an expanded DOE **AS&T** initiative to develop foundational technologies for particle physics that can benefit applications across science, medicine, security, and industry,
   iii. Pursue **broad accelerator science and technology development** at both DOE and NSF, including partnerships modeled on the plasma science partnership.

b. Small Projects

Expand the portfolio of agile experiments to pursue new science, enable discovery across the portfolio of particle physics, and provide significant training and leadership opportunities for early career scientists.

More Favorable Budget Scenario

Not Rank-Ordered

c. Medium Projects

i. **Initiate construction of Spec-S5** as the world-leading study of cosmic evolution, with applications to neutrinos and dark matter, once its design matures.

ii. Initiate construction of an **advanced fourth far detector (FD4) for DUNE** that will expand its neutrino oscillation physics and broaden its science program.

iii. Initiate construction of **a second G3 dark matter experiment** to maximize discovery potential when combined with the first one.

d. Large Projects

Evolve the infrastructure of the Fermilab accelerator complex to support a future 10 TeV pCM collider as a global facility. A positive review of the design by a targeted panel may expedite its execution (Recommendation 6).

Collider R&D

Collider R&D

Overall, an iterative co-design process that integrates accelerator, detector, and simulation expertise is crucial for addressing challenges specific to 10 TeV pCM machines and for demonstrating their technical and costing feasibility. **R&D efforts in the next five years will inform test facilities as discussed in Section 6.4 for the mid-to-late decade time period and collider design results will set the stage for initiating a demonstrator facility** (Recommendation 6), that would feed into future decisions on a potential collider project.

Area Recommendation: To enable targeted R&D before specific collider projects are established in the US, an investment in collider detector R&D funding at the level of $20M per year and collider accelerator R&D at the level of $35M per year in 2023 dollars is warranted.

For the targeted detector R&D, **we suggest initially allocating 70% of the funds for Higgs factory detector R&D, with about 30% reserved for 10 TeV pCM detector R&D**. Once detector R&D for a Higgs factory is funded and coordinated as a project, targeted detector R&D for a 10 TeV pCM machine should be ramped up.

Prioritization Principles

Overall program should

- **enable US leadership** in core areas of particle physics
- leverage **unique US facilities and capabilities**
- engage with **core national initiatives** to develop key technologies,
- develop a **skilled workforce** for the future that draws on US talent
- realize **effective engagement, partnership, and leadership in international endeavors**

We also **considered the uncertainties in the costs, risks, and schedule** as part of our prioritization exercise. The prioritized project portfolios were chosen to **fit within a few percent of the budget scenarios** and to ensure a reasonable outlook for continuation into the second decade, even though that is beyond the purview of this panel.

Balance of program in terms of

- Size and time scale of projects
- Inside or outside the US
- Project vs research
- Current vs future investment

Exploring the Quantum Universe

Pathways to Innovation and Discovery in Particle Physics

Report of the Particle Physics Project Prioritization Panel 2023

https://www.usparticlephysics.org/2023-p5-report/