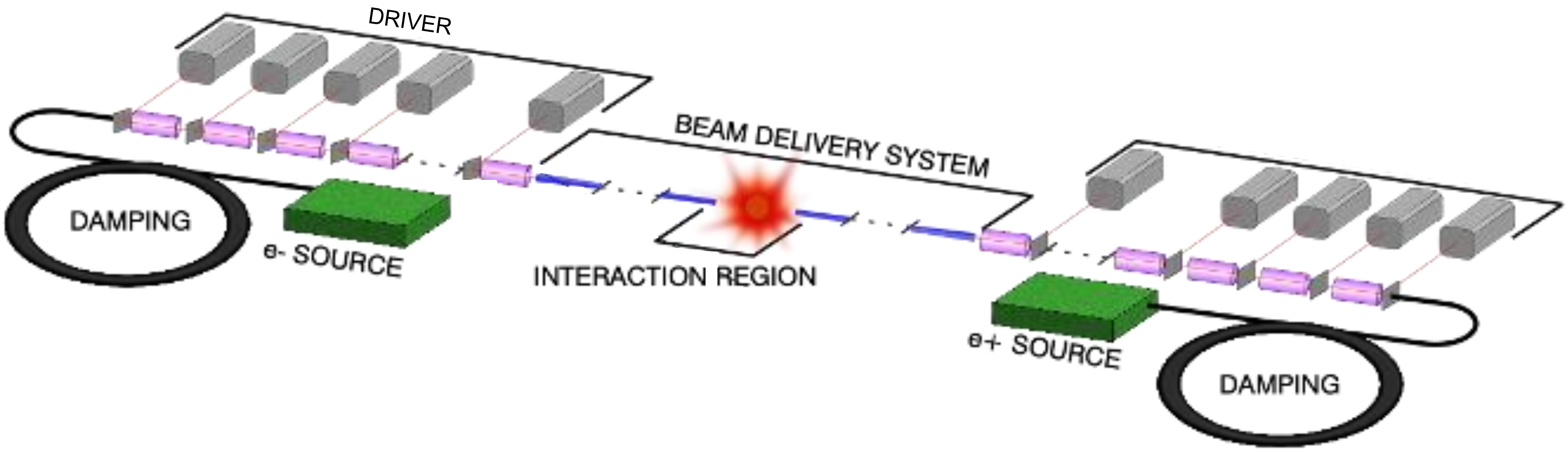
# US perspective on plasma based accelerators and future colliders




2024

Cameron G.R. Geddes, Director
Accelerator Technology & Applied Physics Division
Lawrence Berkeley National Laboratory

# Outline

Introduction to US planning

- Snowmass community study highlights

P5 decadal plan (panel member)

- High level recommendations
- Accelerator and Collider plan
- Wakefield accelerator highlights

Personal perspectives (not P5 content)

- Wakefield accelerator next steps

# Thank You to the AF6 & Advanced Accelerator Community

**Report of the Accelerator Frontier Topical Group 6 on Advanced Accelerator Concepts for Snowmass 2021**
https://arxiv.org/abs/2208.13279

Includes citations for this talk

Also: novel RF, other concepts (not the focus of this talk)

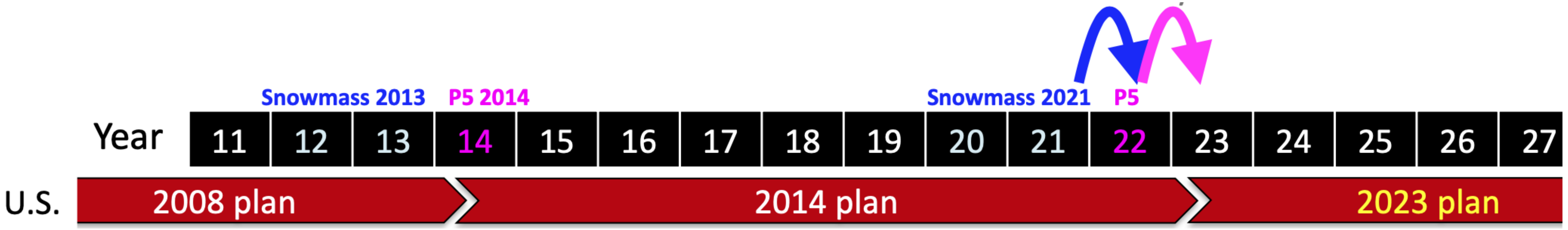
Thanks for coordination and work with the Implementation Task Force and AF4 collider Groups, the overall AF, EF, TF, CoF, CmF, and the overlapping European Strategy for Particle Physics Roadmap group.


**AF6 Conveners:**
C.G.R. Geddes [1], R. Assmann [2],M. J. Hogan [3], and P. Musumeci [4]

**Community Contributors:**
E. Adli[5], C. A. Aidala [6], F. Albert[7], W. An[8], G. Andonian[4], O. Apsimon[10], R. Ariniello[9], S.-W. Bahk[11], S. Barber[1], C. Benedetti[1], R. Bernstein[12], C. Boffo[12], M. Buscher[13], J. Bromage[11], S. S. Bulanov[1], E. M. Campbell[11], G. J. Cao[5], I. Chaikovska[16], Z. Chang[15], E. Chowdhury[17], C. Clarke[3], N. M. Cook[14], S. Corde[18], B. Cros[19], R. D'Arcy[2], X. Davoine[20], C. Doss[9], M. Downer[21], Q. Du[1], H. Ekerfelt[3], C. Emma[3], E. H. Esarey[1], A. Faus Golfe[22], D. Filippetto[1], F. Fiuza[3], D. H. Froula[11], M. Fuchs[23], A. Galvanauskas[6], T. Galvin[7], S. Gedney[24], S. J. Gessner[3], M. Gilljohann[25], M. Golkowski[24], A. J. Gonsalves[1], S.A. Gourlay[1], J. Grames[26], A. Grassellino[12], L. Gremillet[20], E. Gschwendtner [54] G. Ha[27], A. F. Habib[28], C. Häfner[29], T. Heinemann[28], R. Hessami[30], B. Hidding[28], D. Hoffmann[29], G. H. Hoffstaetter[31], S. M. Hooker[32], A. Huebl[1], Y. Ivanyushenkov[27], P. M. Jacobs[1], C. Jing[25,33], C. Joshi[4], A. Kanareykin[33], M. Kanskar[34], T. Katsouleas[35], L. Kiani[7], S. Knapen[1], K. Krushelnick[6], S. Kuzikov[33], A. Lankford[36], C. Lee[37], W. P. Leemans[2], R. Lehe[1], A. Lehrach[3], J. Lewellen[3], C. A. Lindstrøm[2], M. Litos[9], I. Lobach[27], G. Loisch[2], I. Low[27], W. Lu[38], W. Lu[39], X. Lu[25,40], S. M. Lund[41], A. R. Maier[2], G. G. Manahan[28], Y. Mankovska[25], P. Meade[42], C. Menoni[43], M. Messerly[7], H. M. Milchberg[44], S. B. Mirov[45], G. Moortgat-Pick[2], W. B. Mori[4], G. Mourou[25], P. Muggli[46], S. Nagaitsev [11,47], K. Nakamura[1], E. A. Nanni[3], T. Nelson[3], B. O'Shea[3], J. Osterhoff[2], H. Padamsee[12], S. Pagan Griso[1], J. P. Palastro[11], M. Palmer[48], M. Peskin[3], H. Piekarz[12], P. Piot[38,27], K. Põder[2], I. Pogorelsky[48], M. Polyanskiy[48], S. Posen[12], E. Power[11], J. Power[27], E. Prebys[49], T. Raubenheimer[3], B. Reagan[7], Javier Resta-Lopez[50], S. Riemann[2], J. Rocca[43], J. B. Rosenzweig[4], M. Ross[3], J. Rothenberg[51], A. A. Sahai[24], P. San Miguel Claveria[25], P. Scherkl[28], B. E. Schmidt[52], C. B. Schroeder[1,53], B. A. Shadwick[23], J. Shah[27], V. Shiltsev[12], P. Sievers[54], E. I. Simakov[37], E. Sistrunk[7], K. Sjobak[5], T. Spinka[7], J. Stohr[3], A. Sutherland[28], M. Swiatlowski[55], P. Taborek[36], T. Tajima[36], C. Tang[38], R. J. Temkin[56], C. Tenholt[57], D. Terzani[1], M. Thévenet[2], A. G. R. Thomas[6], S.Tochitsky[4], F.S. Tsung[4], D. Umstadter[23], J. Upadhyay[37], N. Vafaei-Najafabadi[40,48], A. Valishev[12], J. van Tilborg[1], J.-L. Vay[1], L. Visinelli[10], G. White[3], R. Wilcox[1], L. Willingale[6], G. Xia[60], V. Yakimenko[3], W.-M. Yao[1], K. Yokoya[13], R. Yoshida[27], A. Zholents[27], T. Zhou[1], F. Zimmermann[54], J. Zuegel[10,1]

# Strategic Planning Process for U.S. Particle Physics

- Particle physics is the largest driver of U.S. investment in advanced accelerators
  - Light sources, other DOE nearer term ideas often based from stewardship of collider technology
  - NSF programs are broadly based
- DOE and NSF programs in particle physics are guided by decadal planning
  - Snowmass community science study defined important questions, promising opportunities
  - 2-year discussion across the field developed and provided community input / interest on opportunities
  - Particle physics Project Prioritization Panel “P5” formulates a 10-year plan and 20 year vision within funding constraints. Subpanel of High Energy Physics Advisory Panel, advising DOE & NSF.




  - Parallel: International benchmarking panel
  - Parallel: National Academies studies define overall scientific vision-Elementary Particle Physics
  - Related to: planning for other U.S. science areas such as nuclear physics, fusion and plasmas; international planning including the European Strategy for Particle Physics, Japan, others

# Snowmass Advanced Accelerator Selected Highlights

- Advanced accelerators in beam and laser driven structures/plasmas offer potential for compact, energy efficient future e-e+/$\gamma\gamma$ colliders to the 15 TeV range with few TeV/km geometric gradients

- AF6 key recommendations …vigorous research on advanced accelerators as part of General Accel. R&D
  - A targeted R&D program addressing high energy advanced accelerator-based colliders
  - Recognize near-term applications as essential & providing leverage to colliders-strengthen connections
  - Enhanced driver R&D to develop efficient, high repetition rate high average power… technology
  - Upgrade beam test facilities, continue strong role in workforce, recruiting including high repetition rate facility kBELLA, positrons at FACET-II, integrated SWFA at AWA
  - Pursue 10’s GeV collider demonstration study develop physics experiments at intermediate energy
  - A DOE-HEP sponsored workshop should update and formalize the U.S. Advanced accelerator strategy

# Snowmass Accelerator Frontier

- AF4: No 10 TeV lepton collider at ‘green’ maturity level needed for informed comparison, decision
  - Advanced accelerators ranked less mature
- Implementation task force compared collider properties and potential costs
  - Wakefield collider: limited documentation, next steps include integrated self consistent parameter sets with tradeoffs to advance evaluation of collider options. For future technologies, cost est. likely conservative.
  - Muon collider: substantial documentation
- Accelerator Frontier summary:

‘… discovery machines <u>such as O(10 TeV c.m.e.) muon colliders </u>have rapidly gained significant momentum… R&D is in progress on <u>other concepts such as wakefield </u>based e +e − or γγ systems which <u>may present additional future options</u>... reduce the dimensions and thus potentially reduce the costs and power consumption of future high energy physics machines’

AF4 status summary

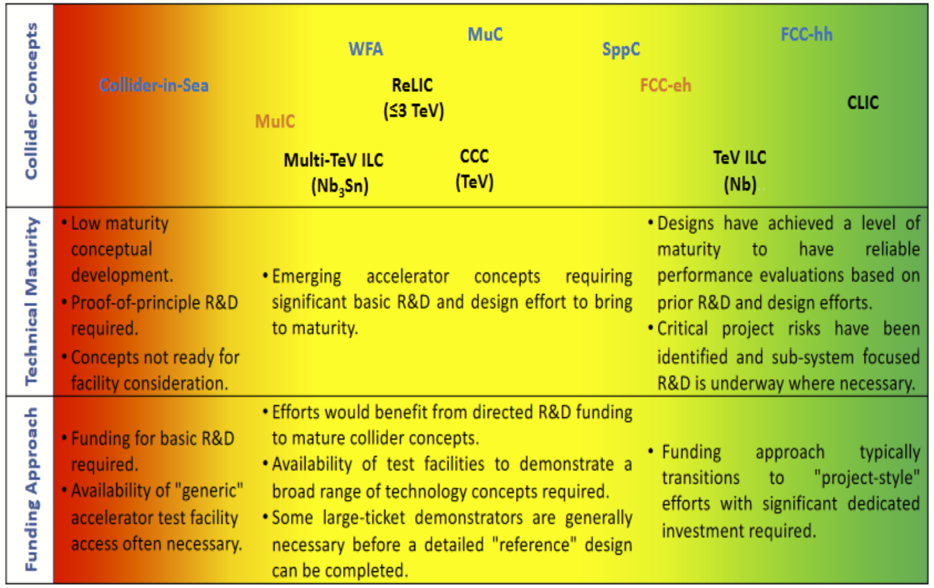



*Figure 1 The AF4 evaluation of the maturity level of various concepts. Further details for the evaluation of the various concepts can be found in the “Concept Assessments” Section. The color code is that the concepts shown in blue offer a path to constituent center-of-mass energies >10 TeV, while those shown in orange are electron-hadron machines, and those shown in black are lepton collider concepts which will reach only into the 1-few TeV range.*

# Strong Snowmass Energy & Theory Frontier Interest in 10+ TeV

- Higher energy scale than emphasized in the last Snowmass driven by evolving data
- Leptons offer clean collisions and strong physics potential
  - Motivates R&D to reduce cost and energy consumption and provide viable options at 10+ TeV
  - Similar physics is anticipated to be accessible with muons or e+e-
  - $\gamma\gamma$ colliders likely access significant portion of physics, analysis less complete
  - Muon collider emphasis: included detailed EF analysis driven by renewed muon collider effort and presence, spanning accelerator, energy, instrumentation (detection) and theory frontiers
  - e+e- collider forum: identified potential for similar signatures, potential LC benefit via AAC methods but not analyzed in detail – stronger engagement on colliders with energy/theory frontiers needed
- Challenging energy scale emphasizes
  - Leverage of nearer term applications
  - R&D on new technologies including carbon efficiency
  - Integrated design and analysis of collider options

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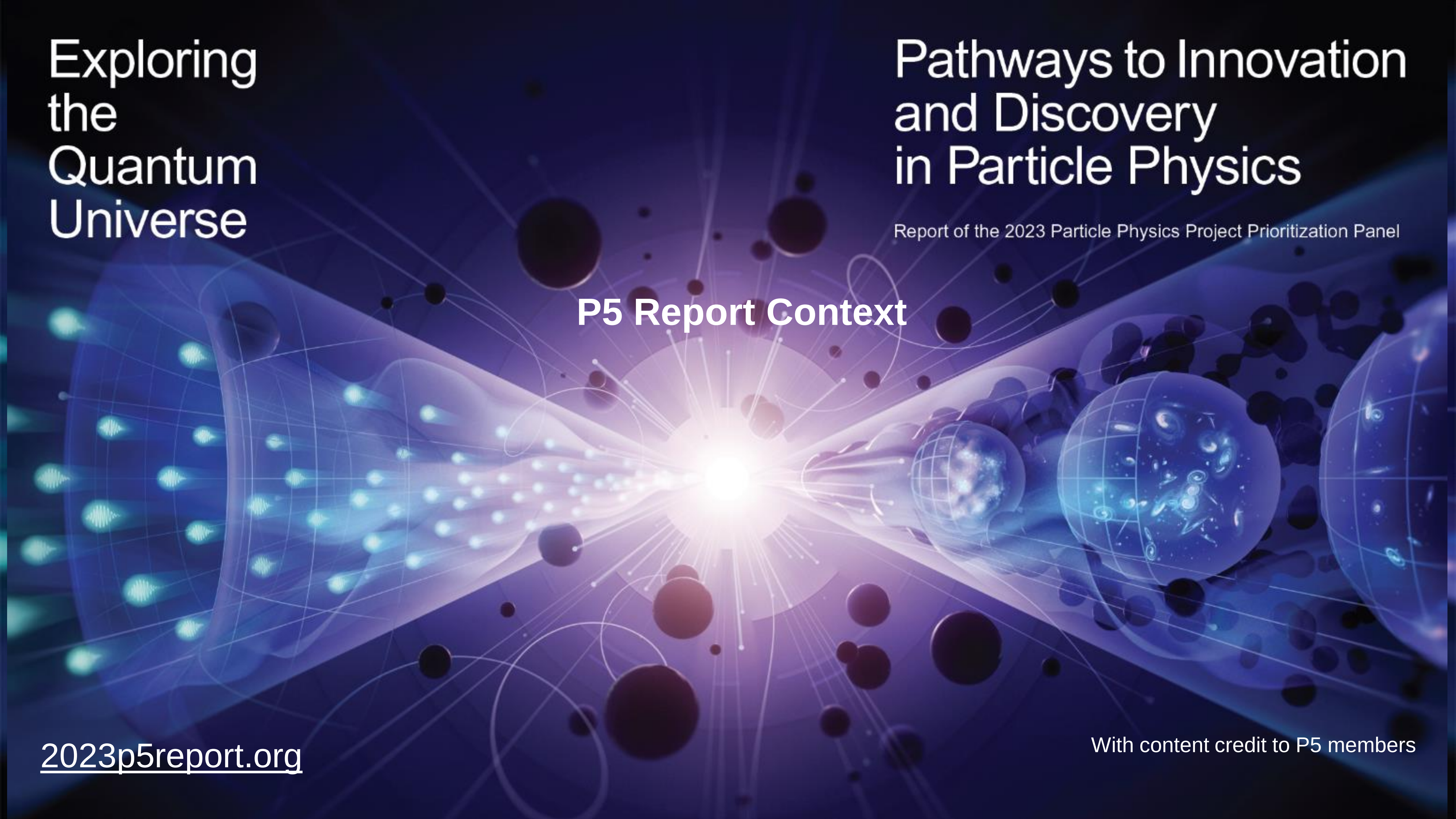



# P5 Report Context

2023p5report.org

With content credit to P5 members

# 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

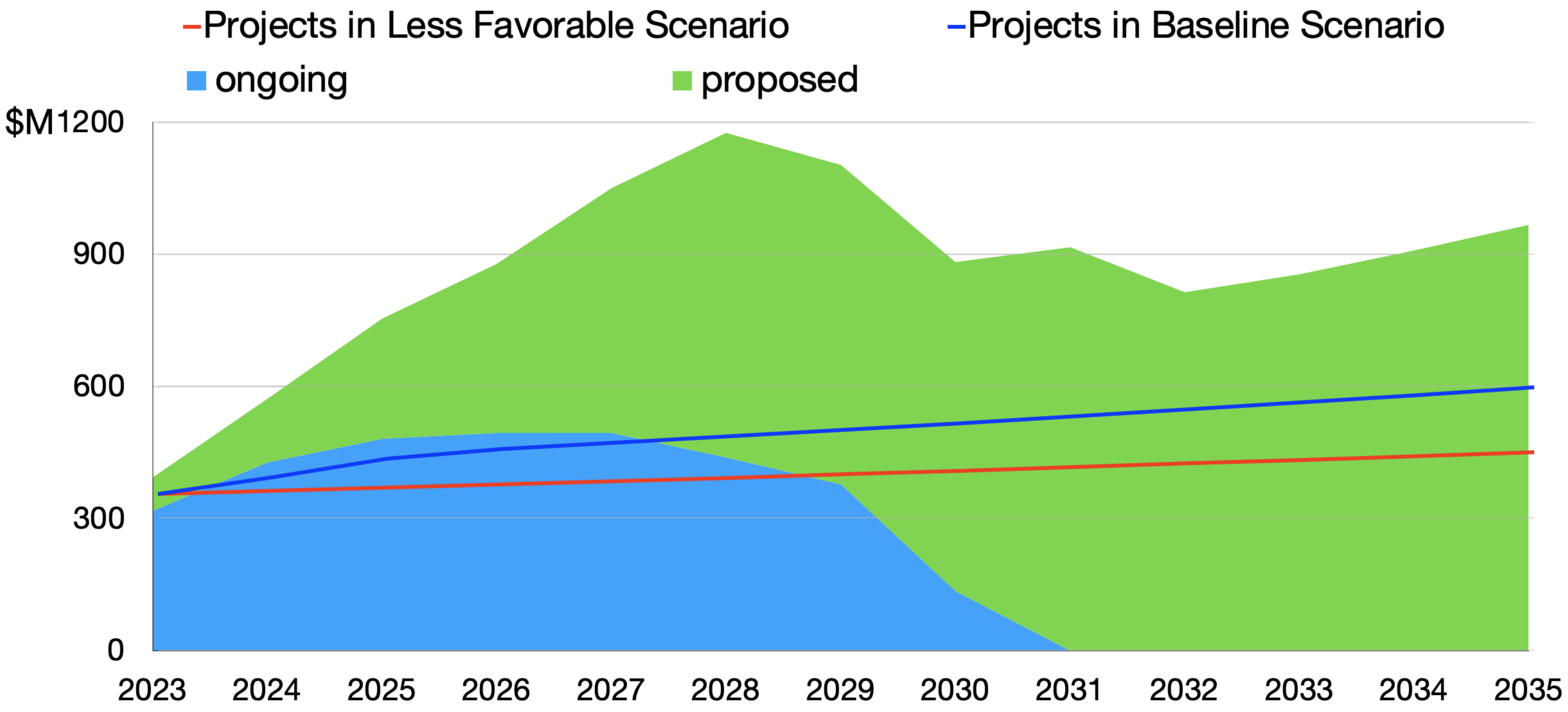
and others

## Today is NOT a full presentation of the P5 report

*Focus on accelerator and collider items relevant to wakefield accelerators*

***Recommendations/highlights shown are a subset and do not represent priority***

# Proposed projects exceeded scenarios




not including on-shore Higgs factory

DOE only

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# Recommendation 2

Rank-Ordered

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 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:

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# 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 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.

# Area Recommendations

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

Collider R&D (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.**

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# Collider R&D: 10 pCM collider

Collider R&D

**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.**

**Targeted detector R&D for 10 TeV pCM machines is needed to address challenges specific to these high energy machines, such as ultrafast timing, radiation hardness, and high rate capabilities.** In particular, detector R&D for a muon collider is needed to address challenges related to the unstable nature of muons. Beam-induced backgrounds due to the in-flight muon decays from the beam line can potentially inhibit the ability of the detectors to successfully reconstruct collision products. While many aspects of detector design and optimization are common to all future collider detectors, this unique feature requires dedicated study and R&D.

# Area Recommendations

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.

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.

# Area Notes - Wakefields

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.**

Use of the existing test facilities should be maximized. […] Key acceleration and beam requirements of a stage for a future collider based on wakefield technology, including energy gain with high brightness beams at high efficiency, can be developed using FACET-II at SLAC, BELLA at LBNL, AWA at ANL, ATF at BNL, ZEUS at University of Michigan (NSF), and other facilities. In addition to demonstrating a single self-contained stage, some of these facilities, particularly BELLA and potentially ZEUS, can demonstrate the next step toward a plasma wakefield collider, operation with two linked stages.

# Test Facilities

General Accelerator R&D

**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).

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# Takeaway Messages

- Interest shifted from the TeV range to emphasize 10+ TeV/parton energies, after a Higgs factory
- Strong progress assessing wakefield 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 e+ accel.
  - Collider conceptual parameter sets developed based on component simulations and models
  - Strengthening R&D and test facilities while leveraging near term applications are important.
- Snowmass science study engaged across the particle physics community and informs the P5 plan
  - Advanced accelerators offer potential for compact, energy efficient future e-e+/gg colliders
  - Viewed as longer term than other options, less engagement
  - Emphasis on the need for designs with documented self-consistent parameter sets and identified technology gaps to guide and assess R&D
  - Engagement with broad accelerator and particle physics communities is needed to advance design & consideration as a collider option

# Towards a Wakefield Collider

- Research progress under GARD is demonstrating fundamental techniques, potential
- Collider studies are critical to define path to address 10 TeV pCM
  - Starting with end-to-end design concept with cost scales, self consistent parameters
  - Integration of detector and physics signatures – muon example – synergy with C3, others
  - Unique interaction point modeling and physics at high energy
  - 10 TeV class is the motive: Near term applications and series of collider steps likely
- Test facilities demonstrate key technical elements, often below final collider specification
  - Energy gain, high brightness & efficiency FACET-II, AWA, BELLA, ZEUS, ATF… (& staging)
  - Additional proposed: kBELLA rate and active feedback for precision alignment

    FACET-II upgrade for positrons; AWA for GeV structures
- Demonstrators could integrate substantial parts of collider approaching design level
  - Multi-stage at significant repetition rate, potentially two arms

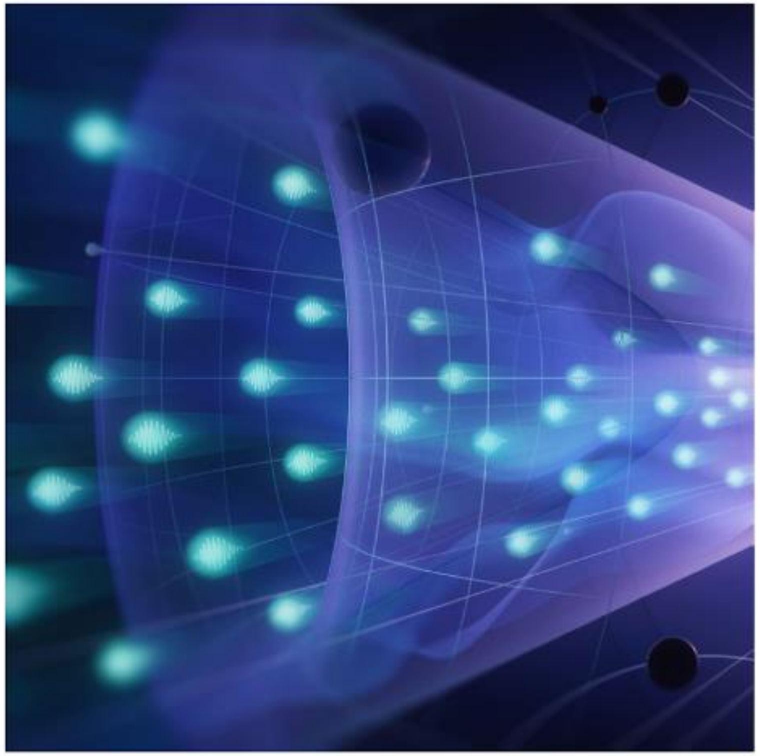
# Collider Planning

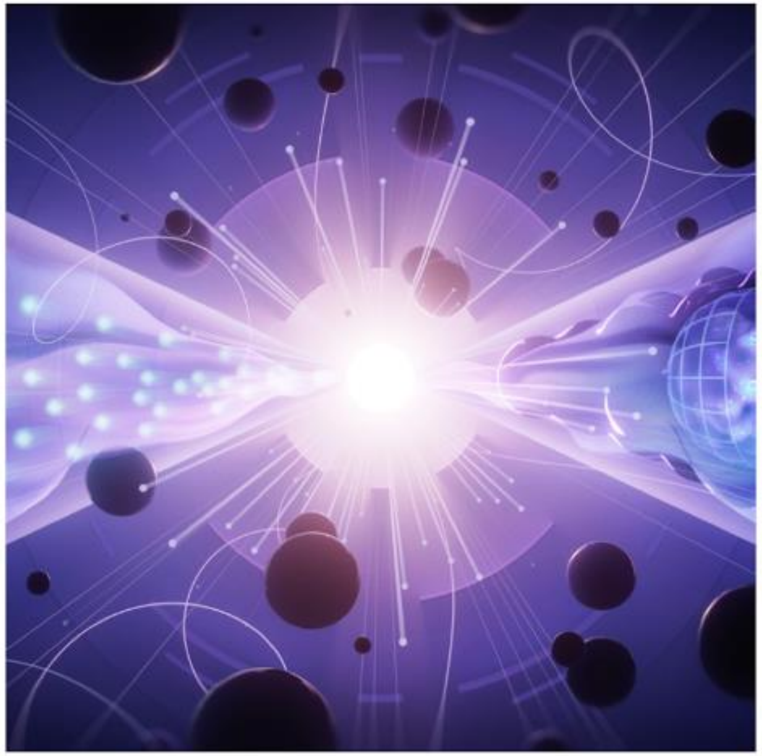
- Move towards integrated design, starting with self consistent parameter sets, cost ranges (muon collider success in the last decade)
  - Integrating: injection, staging, efficiency, cooling alignment and jitter effects and tolerances, matching/coupling between many stages, BDS and Final focus
  - Engaging: the detector, high energy physics and conventional collider communities, & internationally
  - Frame a common wakefield collider concept with techniques offering technical risk mitigation
  - Synchronize R&D across labs, identify challenges and address jointly
  - Engage particle and detector physicists – there are unique issues with WFA detectors e.g. short bunches. Discussing parameters so they have to something to work with is essential.
  - Key to motivating and directing development
- Discussion started 2024: Review concepts, identify gaps, collider community input…
  - Next: broaden and formalize collaboration and integrate with international efforts
- Engage strongly in non-AAC meetings and workshops: IPAC, LCWS, IMCC, C3, APS April…
  - Engage with muon collider, ILC, CLIC, C3 and other collider communities; facility upgrades, injectors…
- Leverage near term applications to advance collider path and reduce cost and risk; cross agency

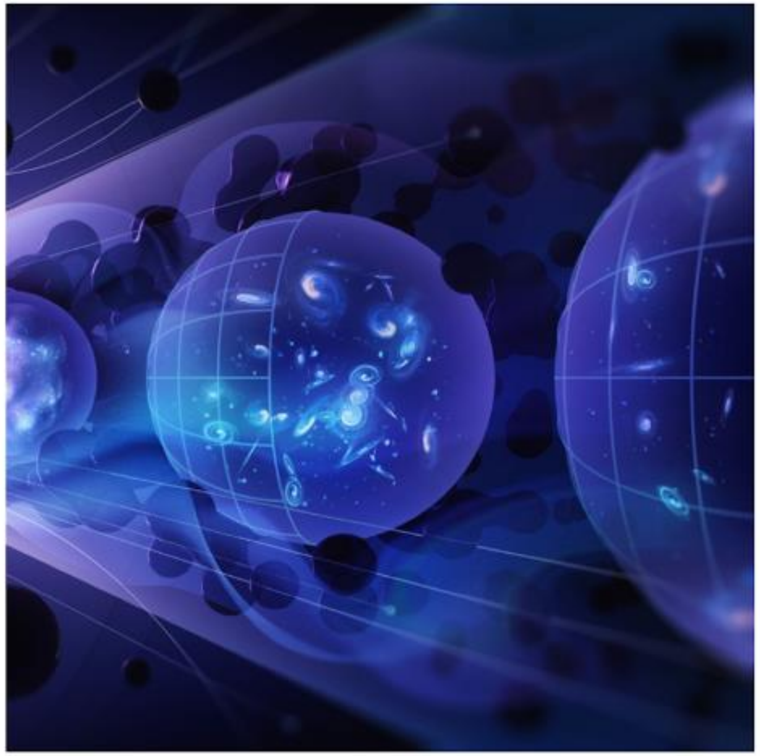
# Snowmass Acknowledgement

We gratefully acknowledge the input of all of the members of the Accelerator Frontier 6 group of Snowmass, and of colleagues in other Accelerator, Energy, Community and other Frontiers, which went into this report. The report in particular draws on the many white papers submitted, as well as presentations at the Summer Study and preceding meetings, and we appreciate their authors. 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 and DE-AC02-76SF00515, and by the National Science Foundation. 


P5 Panel







**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

# Backup material

# 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).

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# 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.

# 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 5

Not Rank-Ordered

The following workforce initiatives are detailed in section 7:

a. All projects, workshops, conferences, and collaborations must incorporate ethics agreements that detail expectations for professional conduct and establish mechanisms for **transparent reporting, response, and training**. These mechanisms should be supported by laboratory and funding agency infrastructure. The efficacy and coverage of this infrastructure should be reviewed by a HEPAP subpanel.

b. Funding agencies should continue to support programs that **broaden engagement** in particle physics, including strategic academic partnership programs, **traineeship programs, and programs in support of dependent care and accessibility**. A systematic review of these programs should be used to identify and **remove barriers**.

c. Comprehensive **work-climate studies** should be conducted with the support of funding agencies. Large collaborations and national laboratories should consistently undertake such studies so that issues can be identified, addressed, and monitored. Professional associations should spearhead field-wide work-climate investigations to ensure that the unique experiences of individuals engaged in smaller collaborations and university settings are effectively captured.

d. Funding agencies should strategically increase support for **research scientists, research hardware and software engineers, technicians, and other professionals** at universities.

e. A plan for **dissemination of scientific results to the public** should be included in the proposed operations and research budgets of experiments. The funding agencies should include funding for the dissemination of results to the public in operation and research budgets.

# 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**

Timeline 2024 – 2034

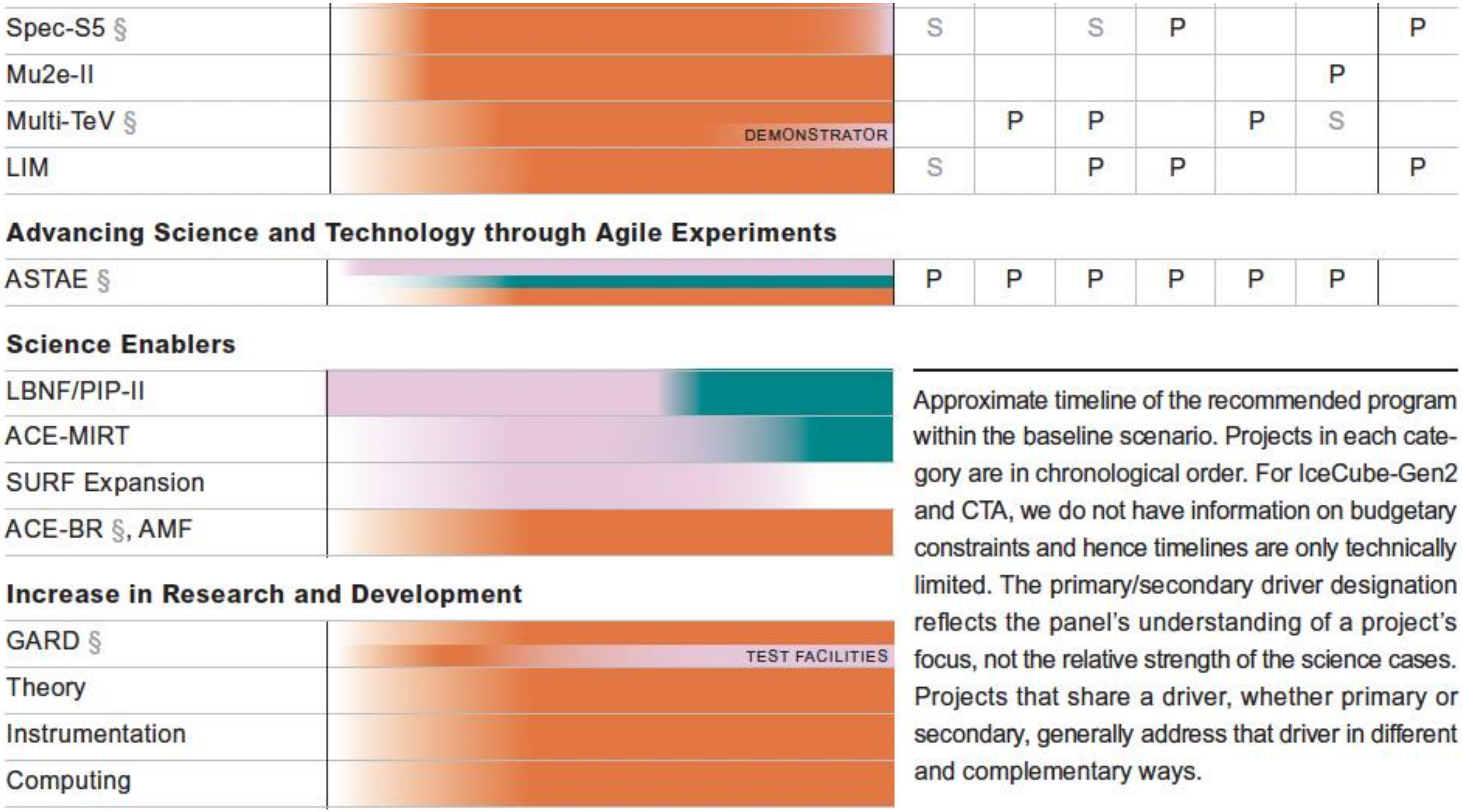
| Science Experiments | Neutrinos | Higgs Boson | Dark Matter | Cosmic Evolution | Direct Evidence | Quantum Imprints | Astronomy & Astrophysics |
|---|---|---|---|---|---|---|---|
| LHC | | P | P | | P | P | |
| LZ, XENONnT | | | P | | | | |
| NOvA/T2K | P | | | | S | | |
| SBN | P | | | | S | | |
| 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 |

Science Drivers

# 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




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.

# 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

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# Pathways to Innovation and Discovery in Particle Physics

**Report of the Particle Physics Project Prioritization Panel 2023**

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