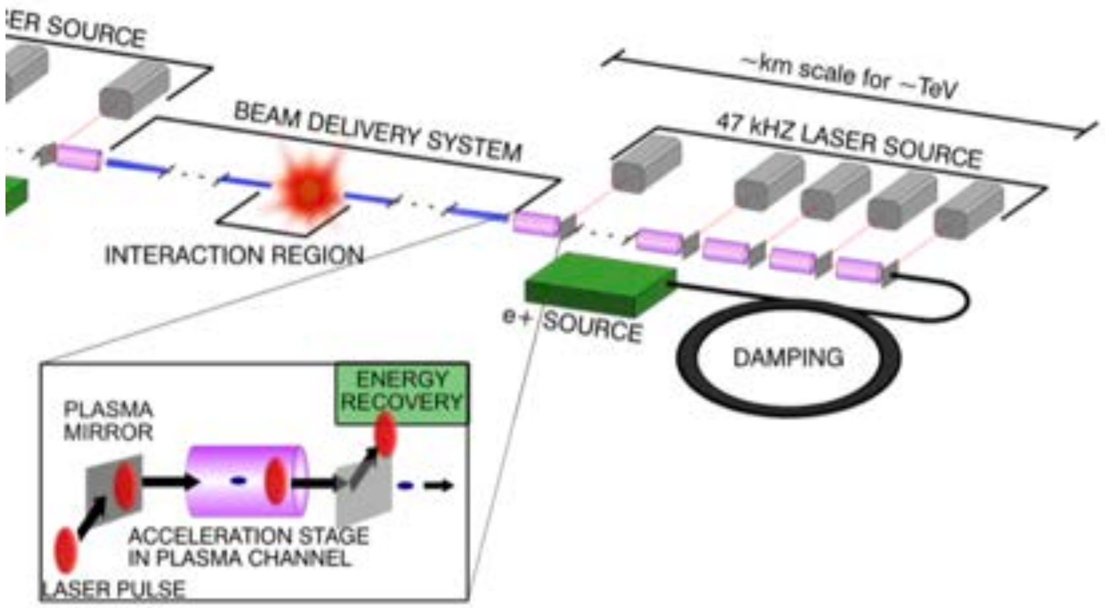
Thinking about
10 TeV pCM Colliders



M. E. Peskin
April 2025

It is time to start thinking about particle physics at the 10 TeV energy scale.

This is not a statement that physics at 1 TeV scale is finished.

LHC and HL-LHC have considerable discovery potential:

search for top quark partners to 1.5-1.8 TeV
search for gluinos to 2.5-3.0 TeV
search for long-lived particles, degenerate scenarios
machine learning-guided search for anomalies

An e+e- Higgs factory is the important next step:

model-independent survey of Higgs couplings to the 1% level and below
search for exotic Higgs decays to the 0.01% level and below

and much more: see arXiv:2503.19983 .

We can carry out these programs with technologies that are mature today.

However, these programs bring current accelerator technologies to their limits.

And, it is unlikely that these or other near-term discoveries will solve the major questions of particle physics. For that, we will need to push onward. In this talk, I will discuss physics at the 10 TeV parton-parton energy scale.

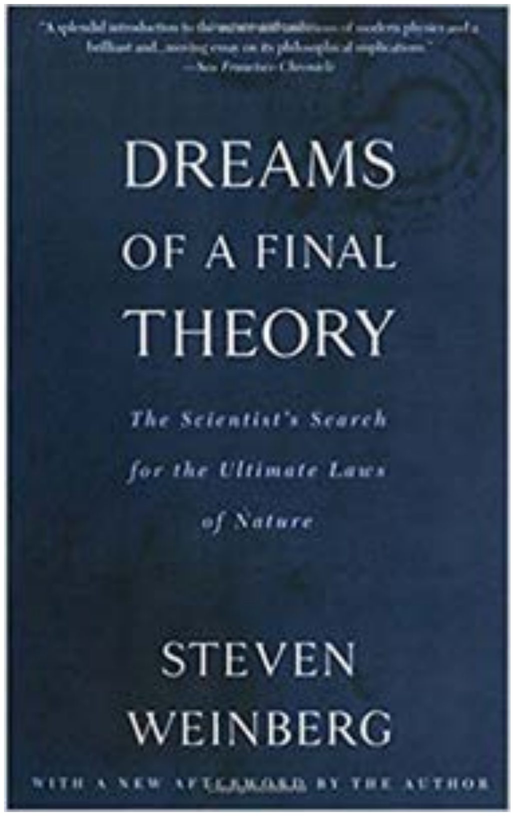
For the TeV scale, serious community involvement started at Snowmass 1982. The first data from the LHC came in 2010.

So, for the 10 TeV scale, we had better start thinking now.

Before the LHC, particle physicists dreamed of a direct connection between the physics at the Higgs boson mass scale and the physics of Grand Unification and String Theory.

This idea seems much less plausible now.

ATF at KEK


DREAMS OF A FINAL THEORY

The Scientist's Search for the Ultimate Laws of Nature

STEVEN WEINBERG

WITH A NEW AFTERWORD BY THE AUTHOR

A key recommendation of the recent P5 report is the visionary statement:

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.

pCM = parton center of mass energy

Such a collider might be based on “proton, muon, or possible wakefield technologies”.

There are many challenges to be met for this program:

Energy: 10 TeV pCM exceeds the limits of all current accelerator technologies. Simply scaling up existing facilities is not an option.

Luminosity: Cross sections for producing new particles of mass M decrease as $1/M^2$. Thus, luminosities growing as E_{CM}^2 are needed. These are $\mathcal{L} \sim 10^{36}/\mathrm{cm}^2\mathrm{sec}$, $10\ \mathrm{ab}^{-1}/\mathrm{yr}$ for 10 TeV.

Backgrounds: Techniques to reach these high luminosities create very large backgrounds generated by beam particles.

Physics: Envisioned machines have price tags in the $ 10B range (many x LHC). Thus, a sharp physics case is needed: “curiosity” and “exploration” will not be enough.

Timescale: Developing solutions to the technical problems will take decades. Nevertheless, I think it is not too early for theorists to study the technical solutions and engage with the planning for physics.

In this talk I will discuss the physics case for 10 TeV pCM colliders — such as it is. I will then review the various technologies that are under consideration.

What is the physics case for experiments at 1 TeV?

The most fully worked source on this is the “Muon Smasher’s Guide”, arXiv:2103.14043, written for Snowmass 2021.

This report gives three key elements:

1. Precision study of the Standard Model, in particular, improved measurements on the Higgs boson.

2. Search for thermal WIMP dark matter candidates in the TeV mass region.

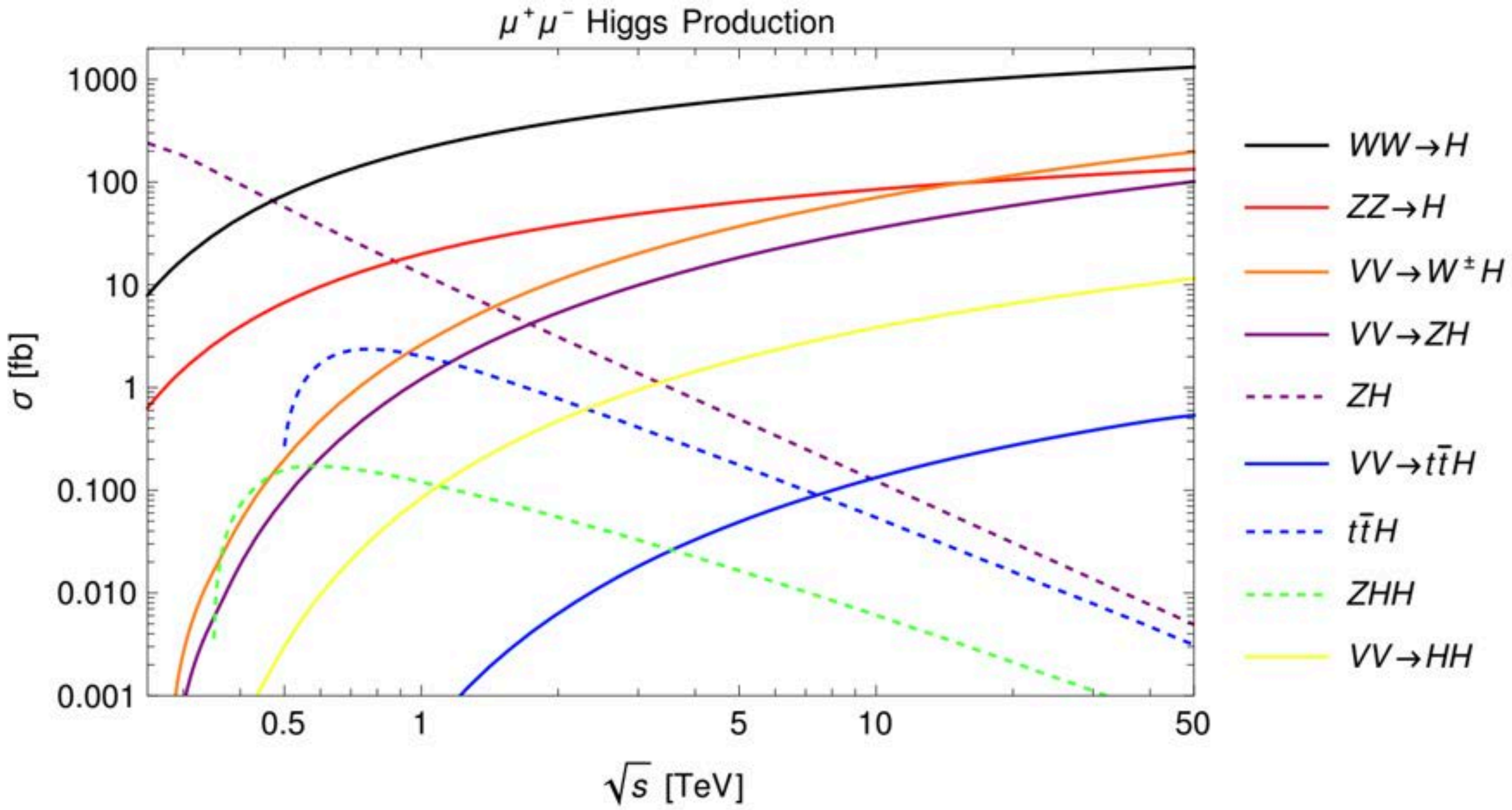
3. Pursuit of possible flavor anomalies.

These points apply to all 10 TeV pCM colliders. I find them interesting but not nearly strong enough.

Begin with the Higgs boson.   An important theory reference is

Forslund and Meade, arXiv:2203.09425

To begin, we must understand the CM energy dependence of parton cross sections:



As we go to high energy, point cross sections fall as $1/E_{CM}^2$ but the WW fusion cross section rises logarithmically. This process is as clean as it gets at a 10 TeV collider, so part-per-mil measurements of $\sigma \cdot BR$ should be possible. Also, the WW fusion cross section to HH rises in a parallel way.

This information can be combined with the precision Higgs mass (10^{-4}) and Higgs width (10^{-2}) measurements at 240-250 GeV.

The sum of this information is a high-precision test of the SM.

Although searches for WIMP dark matter have emphasized the ~100 GeV mass region, there are good candidates for WIMPs in the multi-TeV region.

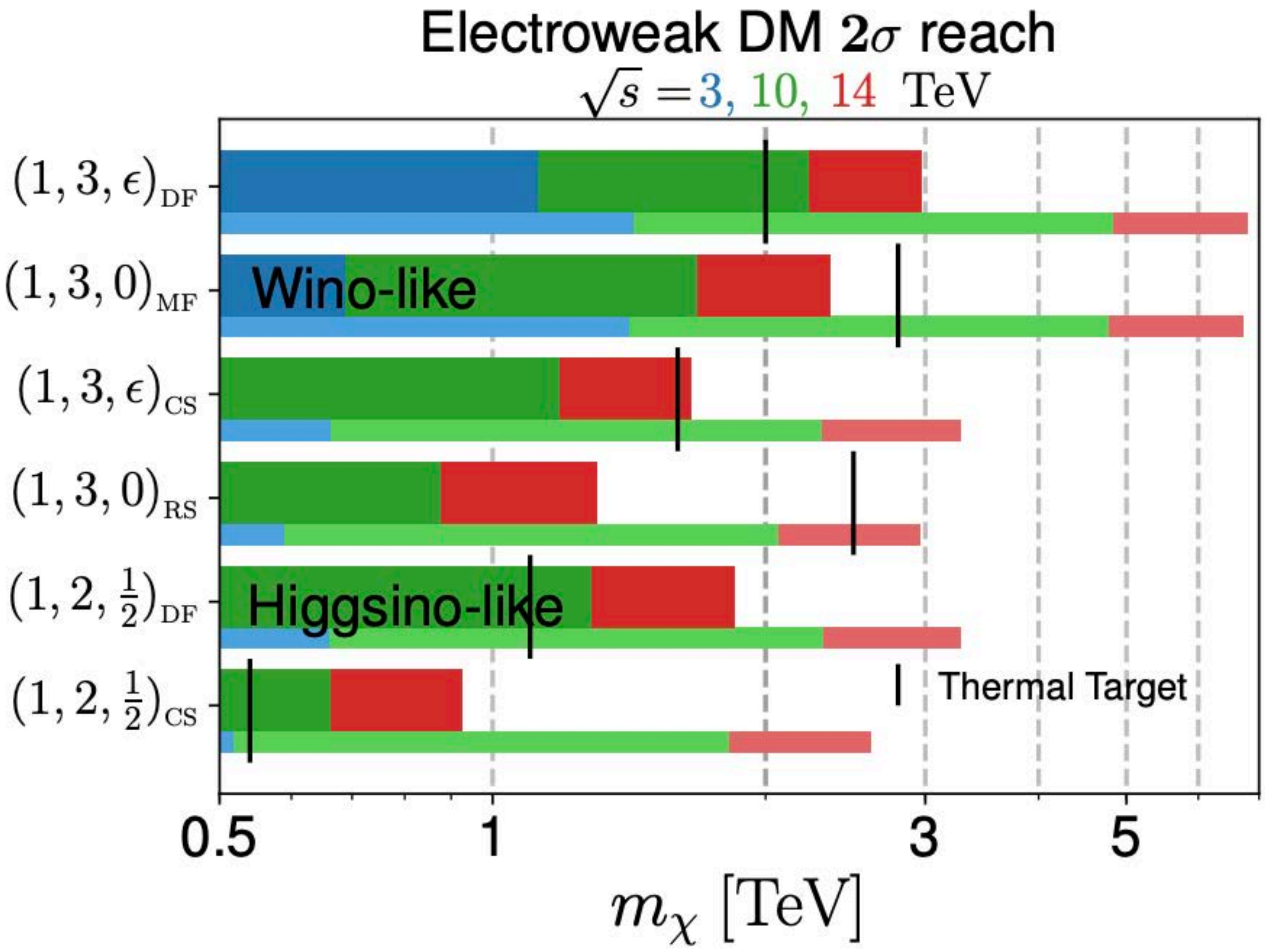
In particular, any heavy fermion in a single electroweak multiplet (e.g., pure Higgsino) is a likely candidate.

Vectorlike fermions, which get most of their mass from sources other than the Higgs, obtain mass splittings of 200 - 350 MeV from SM interactions (Wells and Thomas, hep-ph/9804359). Their direct detection cross sections are very small, and detection of the largest production processes at hadron colliders is very difficult.

These can be found in boosted topologies, or when the charged partner is long-lived.

Their detection at muon colliders has been studied by

Bottaro et al. arXiv:2107.09688
Han, Liu, Wang, and Wang, arXiv:2203.07351



Han, Liu, Wang, Wang

10 TeV brings us close to the point where arbitrary flavor-violating contact interactions are allowed. (Stronger constraints come from $\mu \to e\gamma$, $K^0 - \overline{K}^0$ mixing.) General searches for these is an important part of the experimental program.

Flavor anomalies can provide extra motivation; the strongest ones surviving at the moment are in $B \to D\tau\nu$ decays.

Excuse me that I do not think that any of these arguments go far enough.

As I will discuss, 10 TeV pCM colliders will be, at best, in the $10B cost range (1 x LHC). At this price, we compete with the most prominent projects in science. We need a concrete positive goal: the discovery of new fundamental interactions beyond those in the Standard Model.

Tests of the Standard Model? A part-per-mil anomaly at the scale of m_h would come from new physics at the scale

$$(v/\Lambda)^2 \sim 10^{-3} \quad \to \quad \Lambda = 8 \text{ TeV}$$

which is arguably within the direct reach of the collider.

Discovering this ought to be the goal.

For me, the key unsolved problem of the Standard Model is the mechanism of electroweak symmetry breaking.

In the SM, we make a parameter choice: $\mu^2 < 0$. This is neither a mechanism nor an explanation.

It is now fashionable to say that, with no discoveries of new particles—beyond the Higgs boson—at the LHC, we live in a "post-naturalness era" (Giudice, arXiv:1710.07663). There are now many theories of EWSB where this is generated at a much higher scale, where μ^2 is chosen randomly (N-naturalness) or by early cosmology (relaxion). For a review, see Anson Hook, Ann. Rev. Nucl. Part. Sci. 73, 23 (2023).

I am someone who still believes that there is a mechanistic explanation for EWSB, analogous to the BCS theory of superconductivity or the Hund's rule explanation of magnetism.

Such a mechanism requires new fundamental forces that act at short distances. If you believe that the Higgs mass and VEV are computable, such new forces must exist.

We know how to write models of such forces that act at the scale of the Higgs VEV. But, after much searching, we have not discovered the evidence for these models at the LHC.

If we believe that EWSB has a physics mechanism, we need to explain why the particles responsible are so far above m_h^2 . This is the “Little Hierarchy Problem”.

I find this a very relevant theoretical problem. There are solutions, but they are complicated. We need better ones.

There are models in which the new forces can be found at the 10 TeV scale. These models rely on perturbative feed-down from high scales:

Little Higgs models: Arkani-Hamed et al arXiv:hep-ph/0206021

new strong interactions at 10 TeV
→ effective Lagrangian at ~TeV with vectorlike quarks and
the Higgs doublet as Goldstone bosons

$$\rightarrow \qquad \mu^2 = -\frac{3\alpha_w y_t^2}{8\pi^2} f^2$$

Dirac gauginos: Fox et al, arXiv:hep-ph/0206096

gaugino masses at 10 TeV from a hidden-sector D-term
→ effective Lagrangian with scalar masses from “supersoft” operators

$$\rightarrow \qquad \mu^2 \approx \frac{\alpha_w M_1^1}{\pi} - \frac{3\lambda_t^2 m_{\tilde{q}}^2}{4\pi^2} \log \frac{M_3}{m_{\tilde{q}}}$$

These models not only contain interesting new physics at 10 TeV, but also experiments at 10 TeV are necessary to understand and prove their structure.

It is difficult to feed down from a higher scale perturbatively by a factor of more than 10 .

In principle, one can feed down by an exponentially small factor, but there is no known mechanism for scalar masses.

This makes the 10 TeV scale a definite target.

These solutions to the little hierarchy problem are not very elegant.

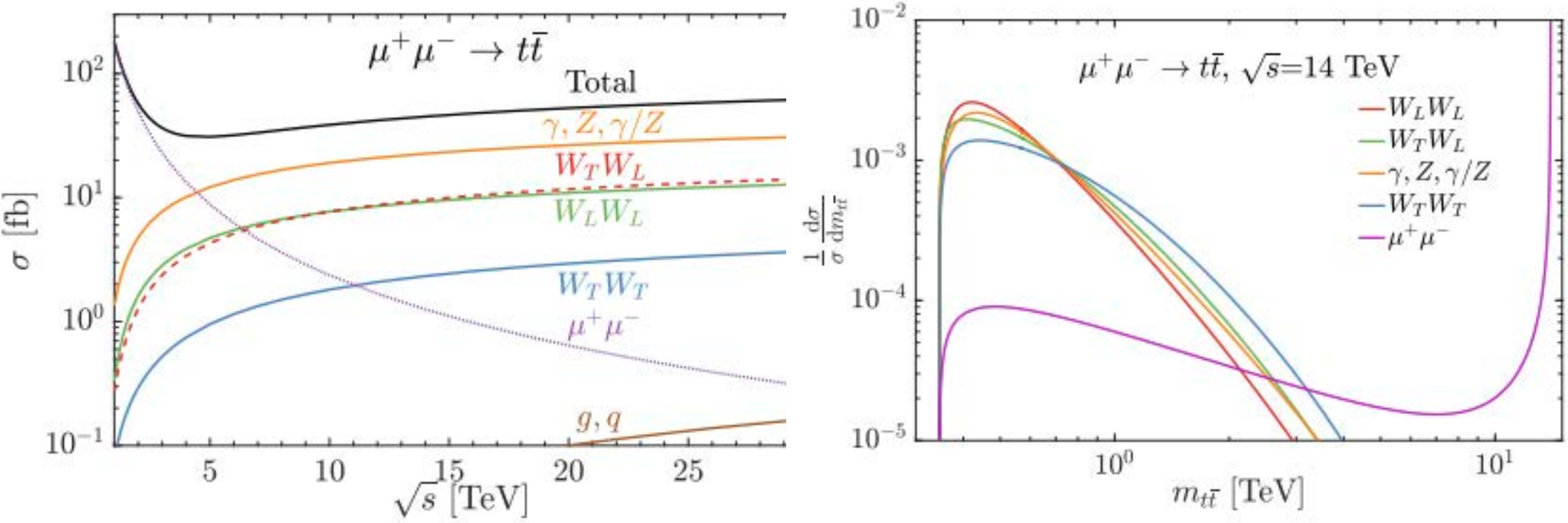
Maybe there are better ones. Maybe there are principles that make these solutions unique and simple.

We theorists need to do better.

But, if you do not believe in models of this type, the next scale in particle physics could be at 1000 TeV, or 1,000,000 TeV. Then asking for 10 TeV collider is a roll of the dice.

Our funding agencies will always ask, why 10 TeV? We need to have an answer.

a comment: This attitude toward the goals of a 10 TeV pCM collider has implications for the collider design. Let’s look again at the cross section dependences, this time for top quark processes.



$\mu^+\mu^- \to t\bar{t}$ processes (Han, Ma, Xie, arXiv:2007.14300)

If we must actually use a 10 TeV collider to access 10 TeV physics, this puts a very challenging requirement on the needed luminosity.

With this as a theory introduction, let’s now review the status and major issues of the 3 suggested technologies for a 10 TeV pCM collider:

Protons: FCC-hh or SppS, 100 TeV pp colliders in ~ 100 km rings

Muons: Muon Collider

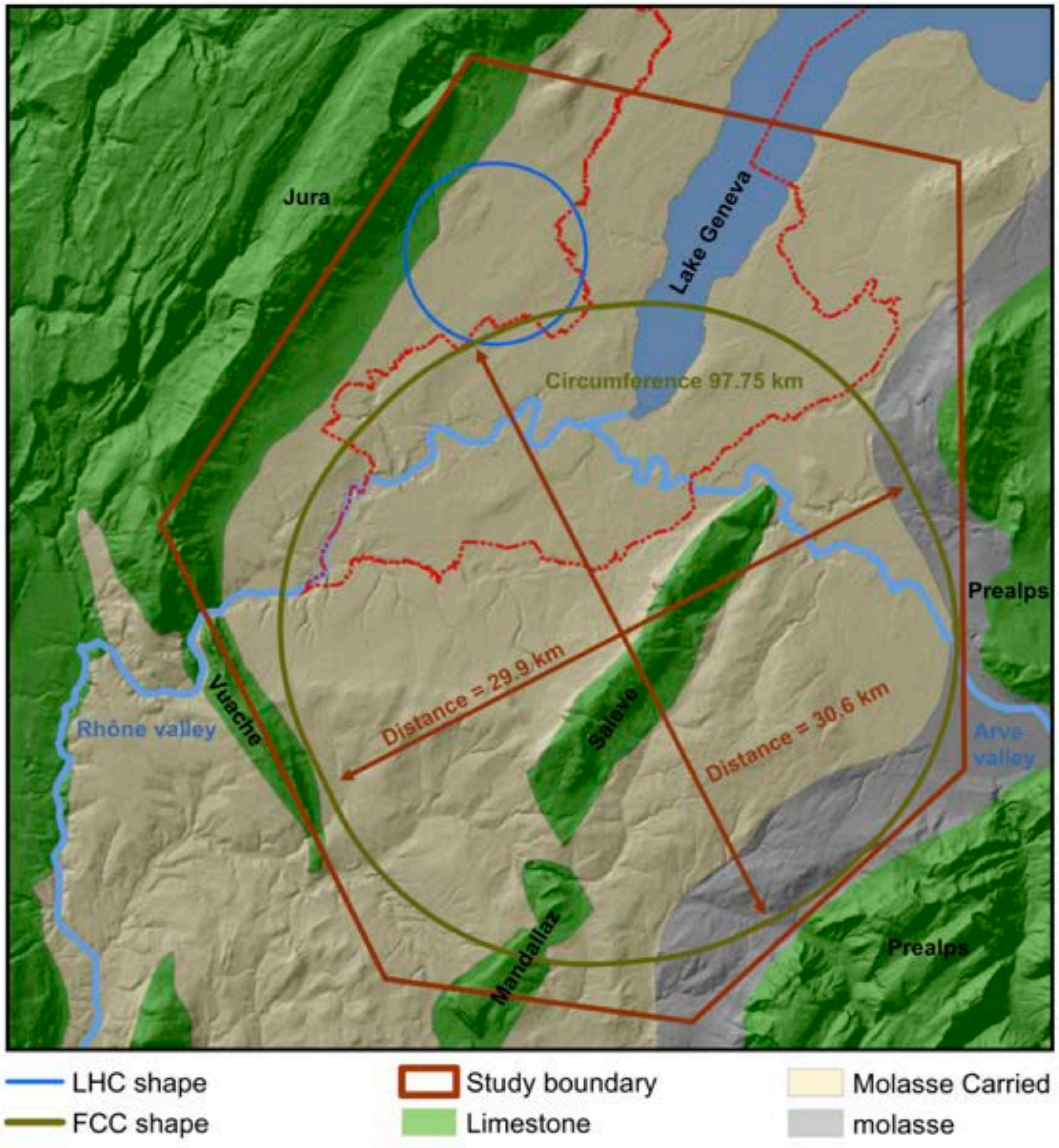
Electrons: beam- or laser-driven plasma wakefieild accelerators

I will quote costs from the report of the Snowmass Implementation Task Force (ITF), Roser et al, arXiv:2208.06030. These authors impressively evaluated and costed 30 future collider proposals. Costs are based on the current status of the accelerator design.

pp colliders — the unfortunate facts of life:

1. The proton is not an elementary particle, so $E(\mathrm{parton}) \sim 0.1 E(p)$
2. Ring size is limited by the available magnetic bending field
3. $\sigma_{tot}(pp)$ is constant as production cross sections ~ $1/M^2$

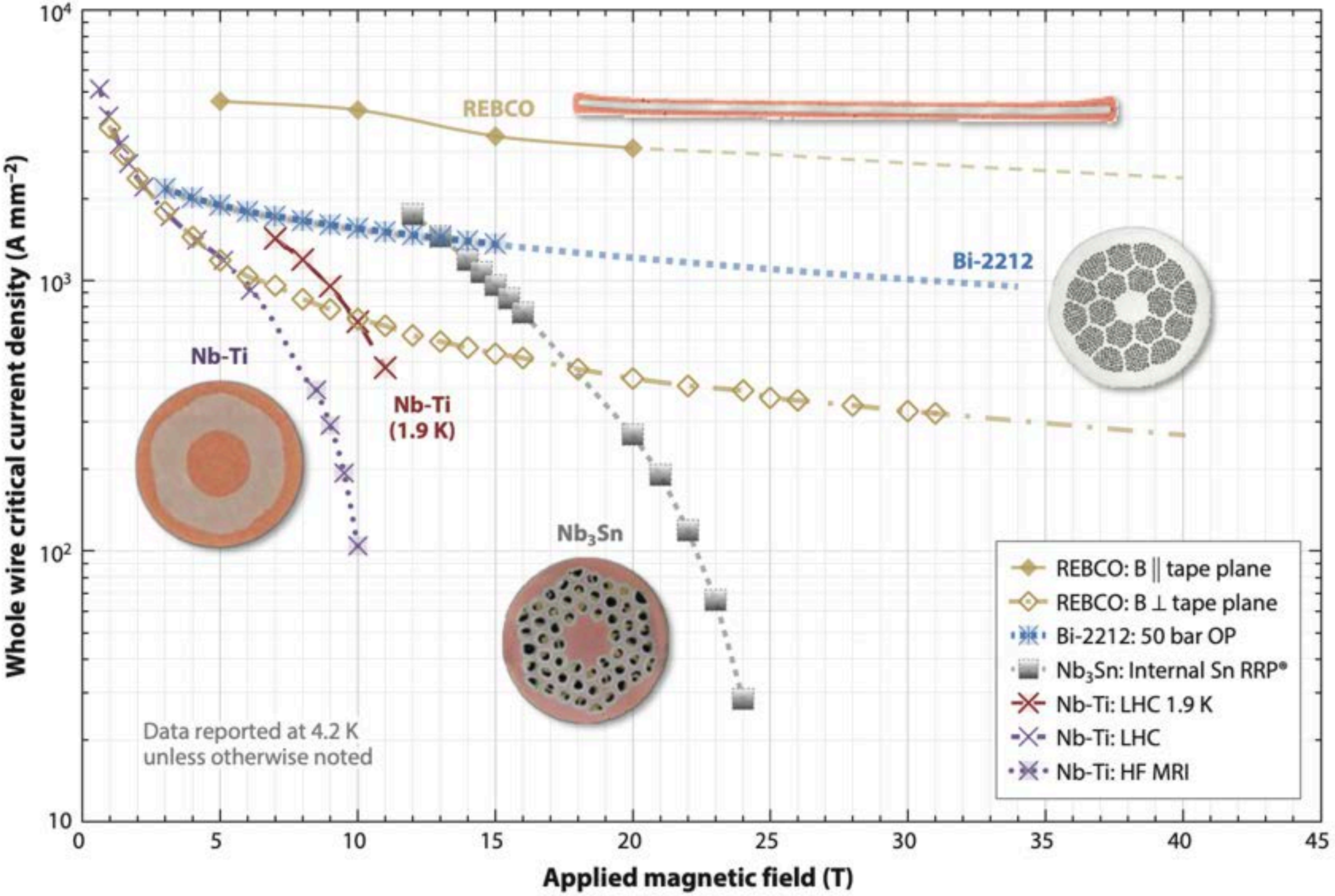
This is the most conservative approach, but also, likely, the most expensive.



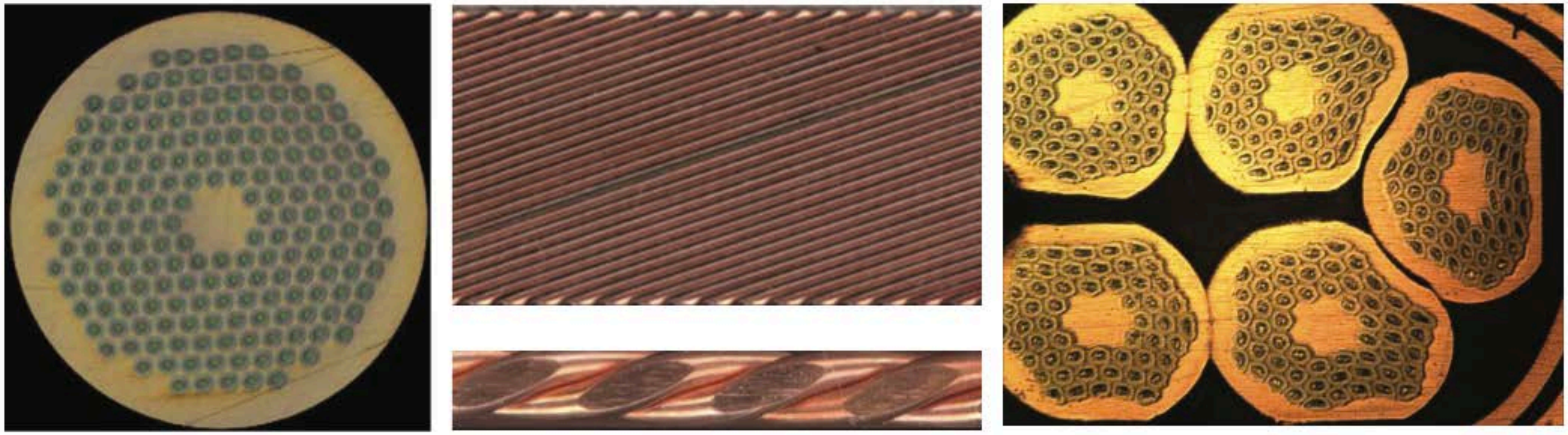
FCC feasibility
study
https://cds.cern.ch/
record/2928193
record/2928793
record/2928194

FCC-hh 91 km ring x 14 T -> 85 TeV in pp CM
estimated cost (Snowmass ITF panel) 20 - 60 B$ US
official cost: 19 B ChF + ~7BChf

The current 8 T is close to the limit for the NbTi magnets used for LHC. For higher fields, new superconductors must be used.



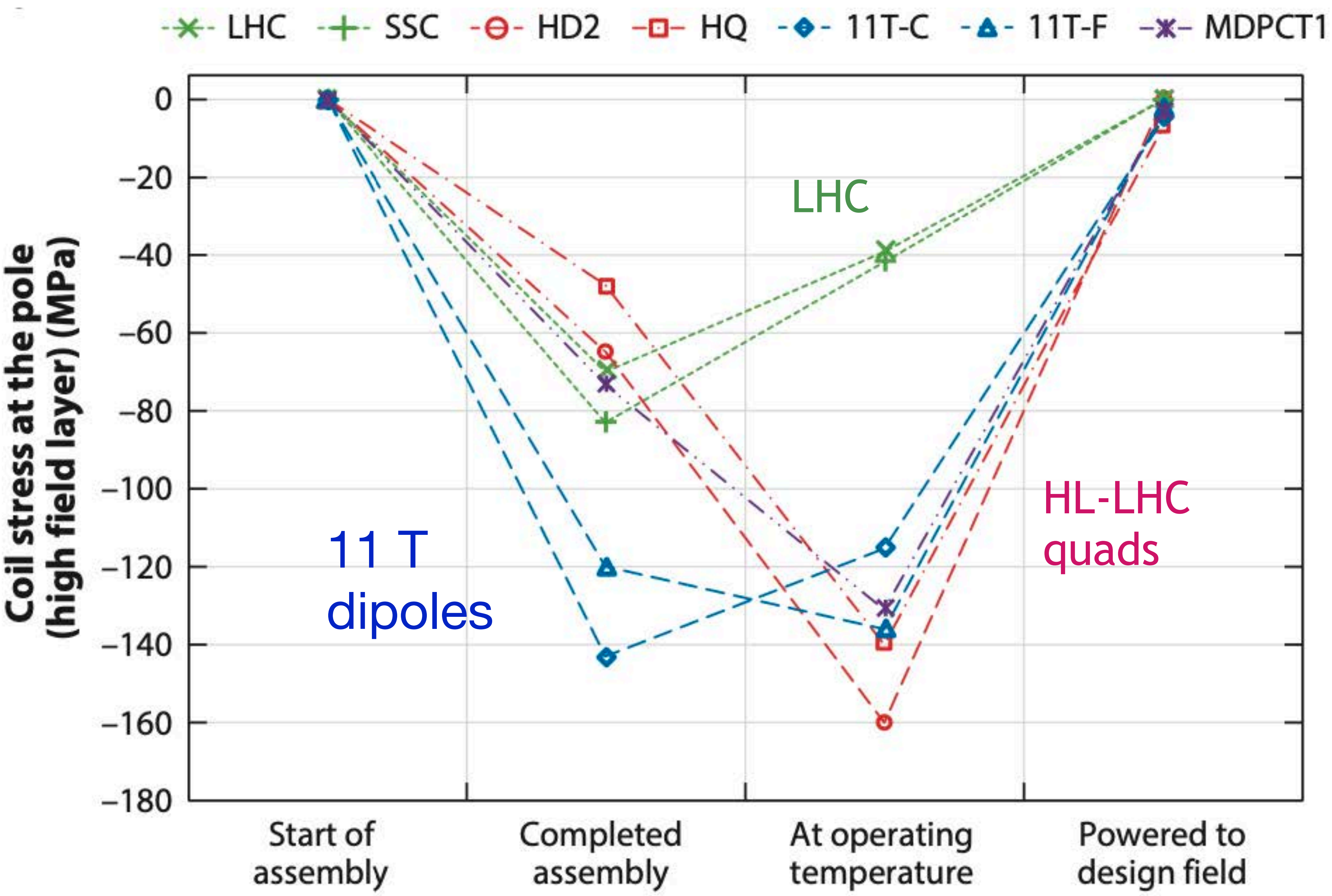
Nb3Sn and High-TC superconductors are brittle materials. To manufacture magnets, it is necessary to wind a precursor material, then heat-treat to make the final product.



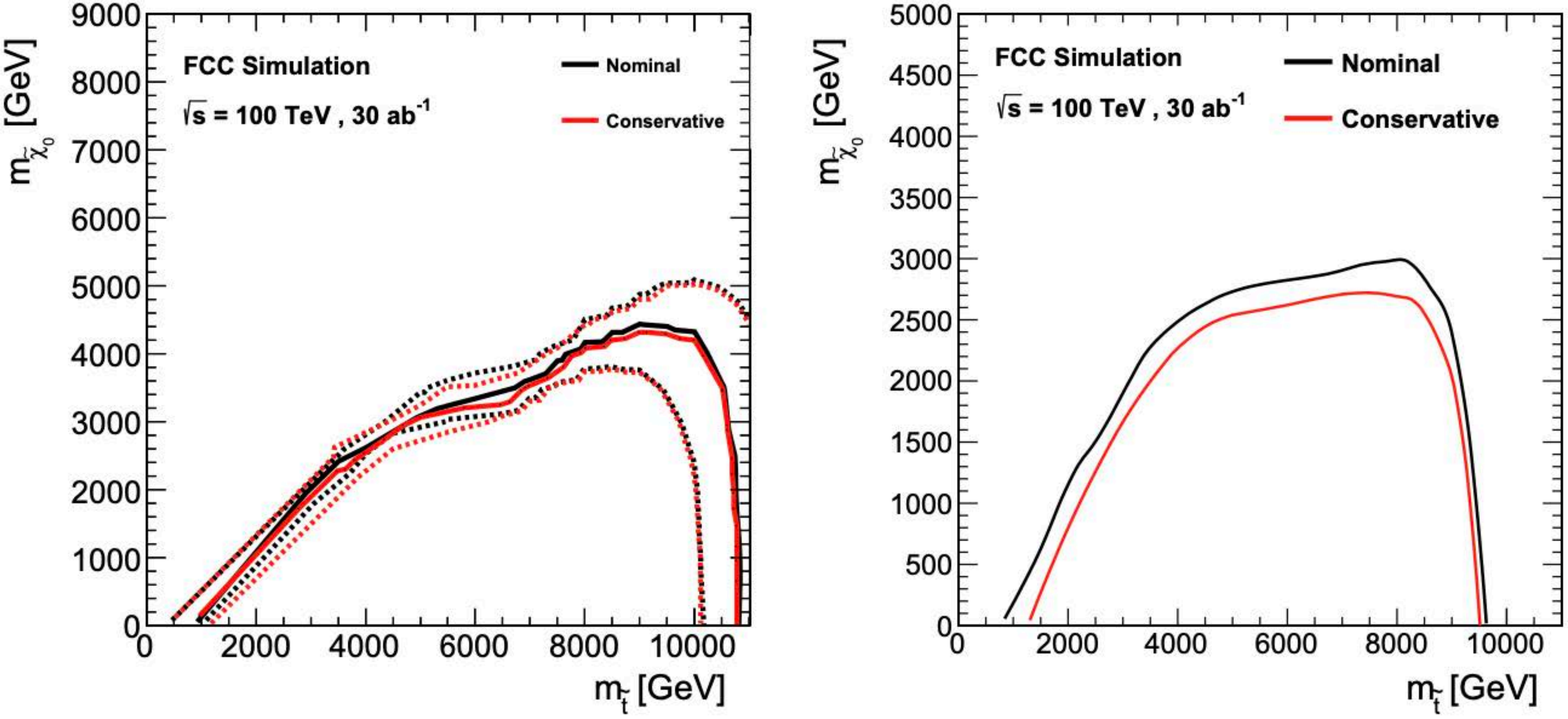
12 T Nb3Sn quadrupoles have been made for the HL-LHC final focus magnets. There are a few examples now of 14 T Nb3Sn. All of these are made 1-by-1. Industrialization of the manufacturing technique is well in the future.

G. Sabbi, Ann. Rev. Nucl. Part. Sci. 74, 369 (2024)

Superconducting magnets undergo high stress at various stages of their construction. This is also an issue for the manufacturing process:



FCC-hh plans for luminosities up to 3×10^{35} , corresponding to 1000 pileup events per bunch crossing. Physics studies assume that future detectors will resolve this. Then the physics reach for new particles is impressive. Example: stop search:



A. Abada, et al. Eur. Phys. J C 79, 474 (2019)

muon colliders — the unfortunate facts of life:

1. Muons are unstable, $\tau = 2\ \mu\mathrm{sec}$
2. Muons are produced in a large phase space that must be reduced by a factor 10^6.
3. When muons are guided to the collision point, their decay products are guided there also.

This approach is the most power- and cost-efficient, if it can work at all.

Muons have a number of advantages for a high-energy collider. Electron synchrotrons are limited to energies below 200-300 GeV because of synchrotron radiation. At the highest energies of LEP, each electron would lose ~1% of its energy every time it went around the ring. For muons, this effect is smaller by

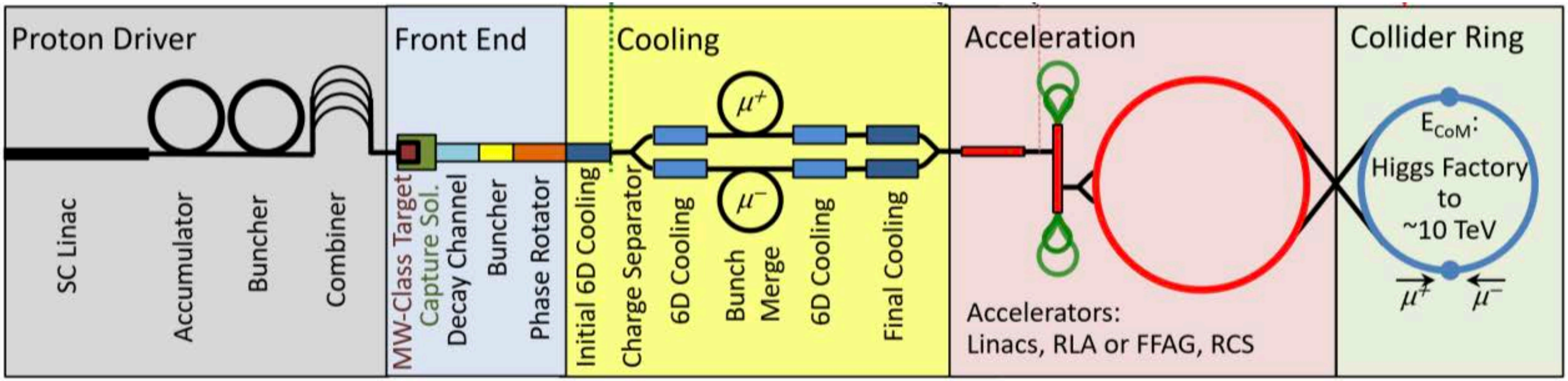
$$(m_e/m_\mu)^4$$

enabling muon colliders in the 10s of TeV range.

In a synchrotron, the luminosity typically increases as $\mathcal{L} \sim E_{cm}$

However, in a muon collider, because the muons live longer, one can have $\mathcal{L} \sim E_{cm}^2$, as we really wish.

The difficulty is that muons must be made on the spot, and then "cooled" to reduce their phase space enough to inject into a synchrotron.



layout for the ionization cooling scheme of muon production

Snowmass Muon Collider Forum report,
Black et al, arXiv:2209.01318

estimated cost for a 10 TeV collider
Snowmass ITF panel: 12-20 B$

idea: 10% reduction per stage x 120 stages = 10^{-6}

but, muon cooling is subtle:

Cooling affects the longitudinal phase space, but we need to reduce the transverse phase space.

Phase space increases in the absorber; eventually, there is an equilibrium. This must be postponed as long as possible.

For a total efficiency of 10^{-3}, require only 4% loss per stage.

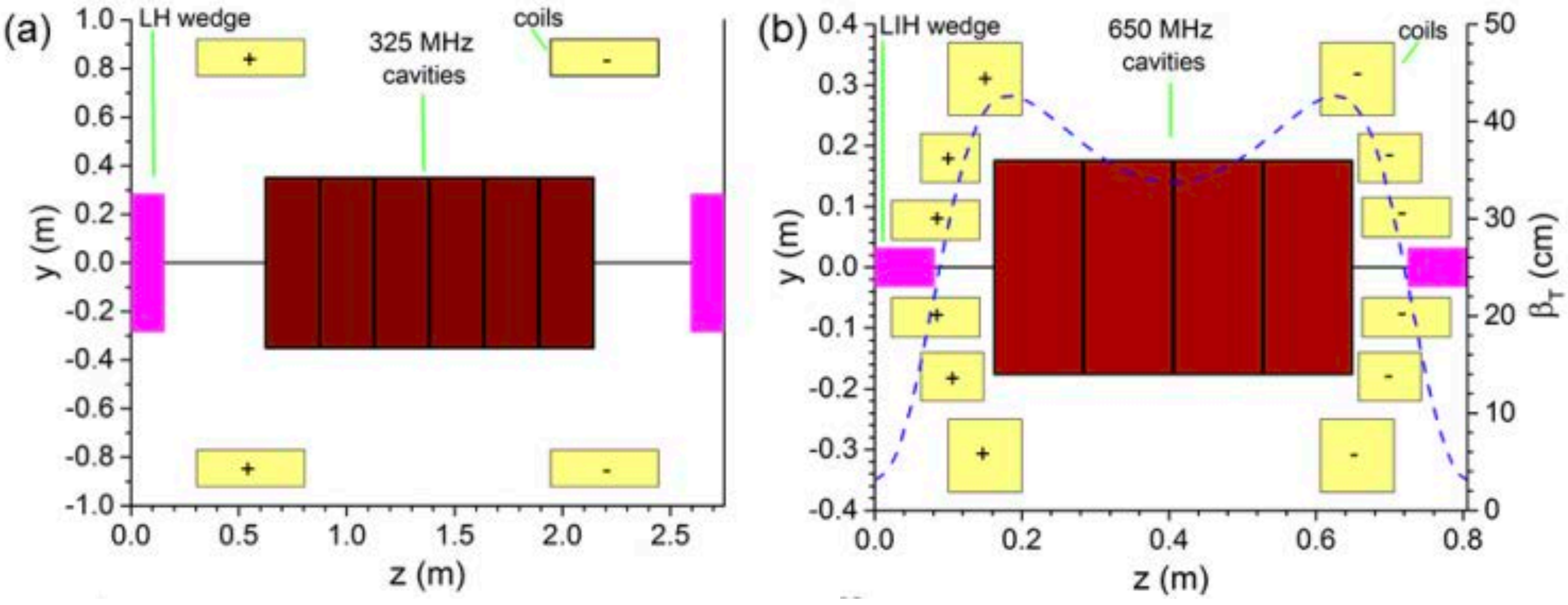
This gives $\mathcal{L} = 2 \times 10^{35}$ at 10 TeV.

MICE experiment: 7% reduction in 1 stage with 10% efficiency.

arXiv:1907.08562

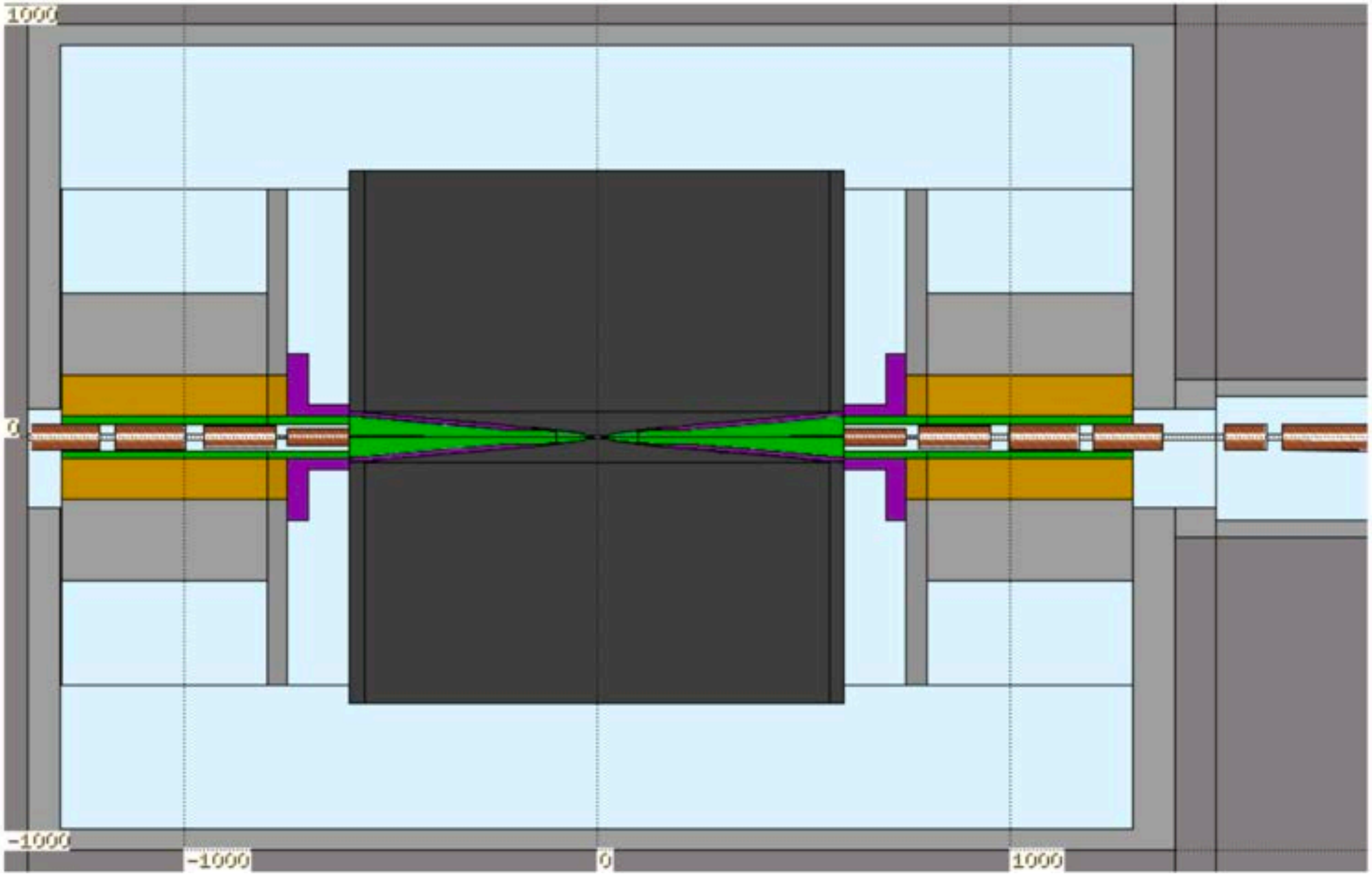
initial stage

final stage



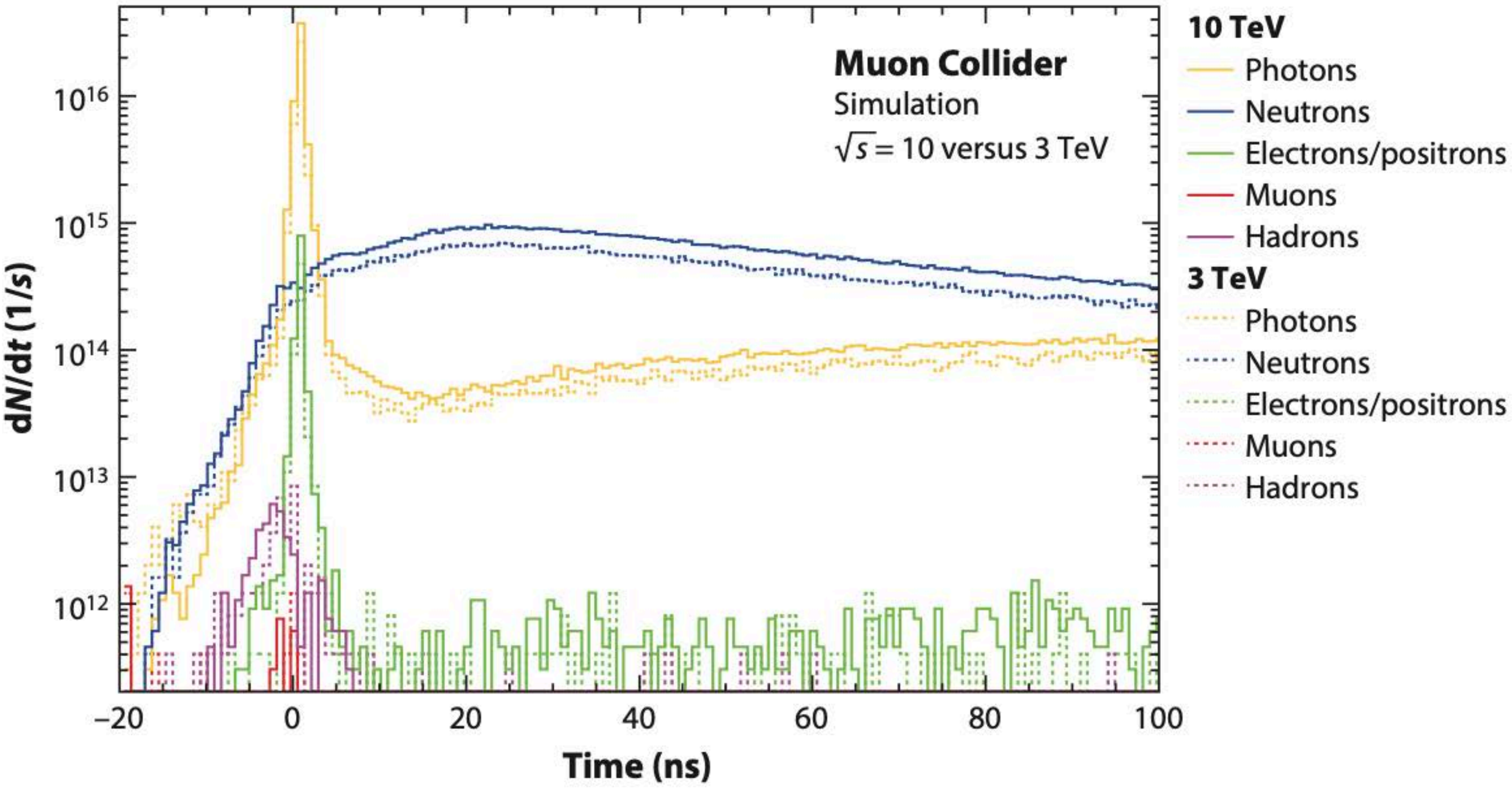
Stratakis and Palmer, Phys. Rev. ST AB 18, 3 (2015)

On the collision path, muons will decay into the detector. This must be considered in the evaluation of physics capabilities.



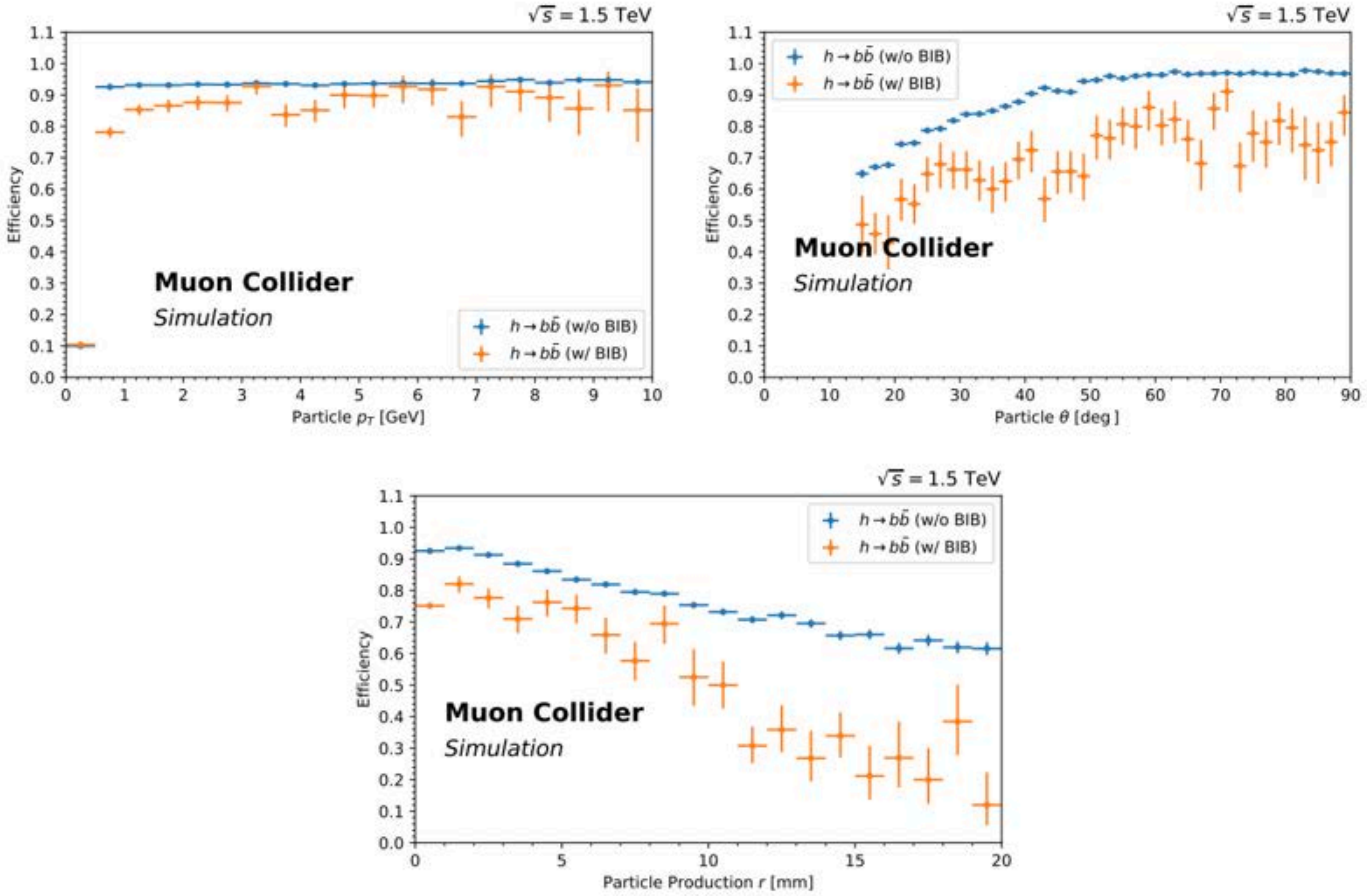
green: Tungsten beam shields (6 m)

Beam induced background particle fluxes



Casarsa, Lucchesi, Sestini, arXiv:2311.03280

efficiency for reconstruction of $h \to b\bar{b}$ events at 1.5 TeV without/with BIB (Bartosik et al arXiv:2203.07964)



benefits greatly from 30 ps timing on tracker hits, and general LHC experience on pileup mitigation

Muon colliders have one more unique hazard: the radiation dose due to neutrinos from decaying muons

$$\text{Dose (mSv/yr)} = 1.5 \frac{N_\mu \ (10^{13}/\text{sec}) \ E_\mu^3 \ (\text{TeV}^3)}{R(\text{km})}$$

For muon colliders in the LHC tunnel, for $\mathcal{L} \sim 10^{35}$
this is close to 1 mSv/yr, the legally allowed dose to the general population.

Current plans are to ameliorate this by wobbling the beam to distribute the neutrino trajectories. Will the public understand and accept this ?

e+e- colliders — the unfortunate facts of life:

1. Due to synchrotron radiation, circular colliders cannot be used above 350 GeV.

2. For linear colliders, $\mathcal{L} \sim P/\sigma_x\sigma_y$, so power efficiency is crucial.

3. With small spot sizes, the beam-beam interaction reaches extreme conditions for $E > 3$ TeV

Plasma wakefield acceleration gives very high accelerating fields, so this approach minimizes facility size, if not cost.

ALEGRO collaboration (B. Cros et al) arXiv:1901.10370

For linear colliders,

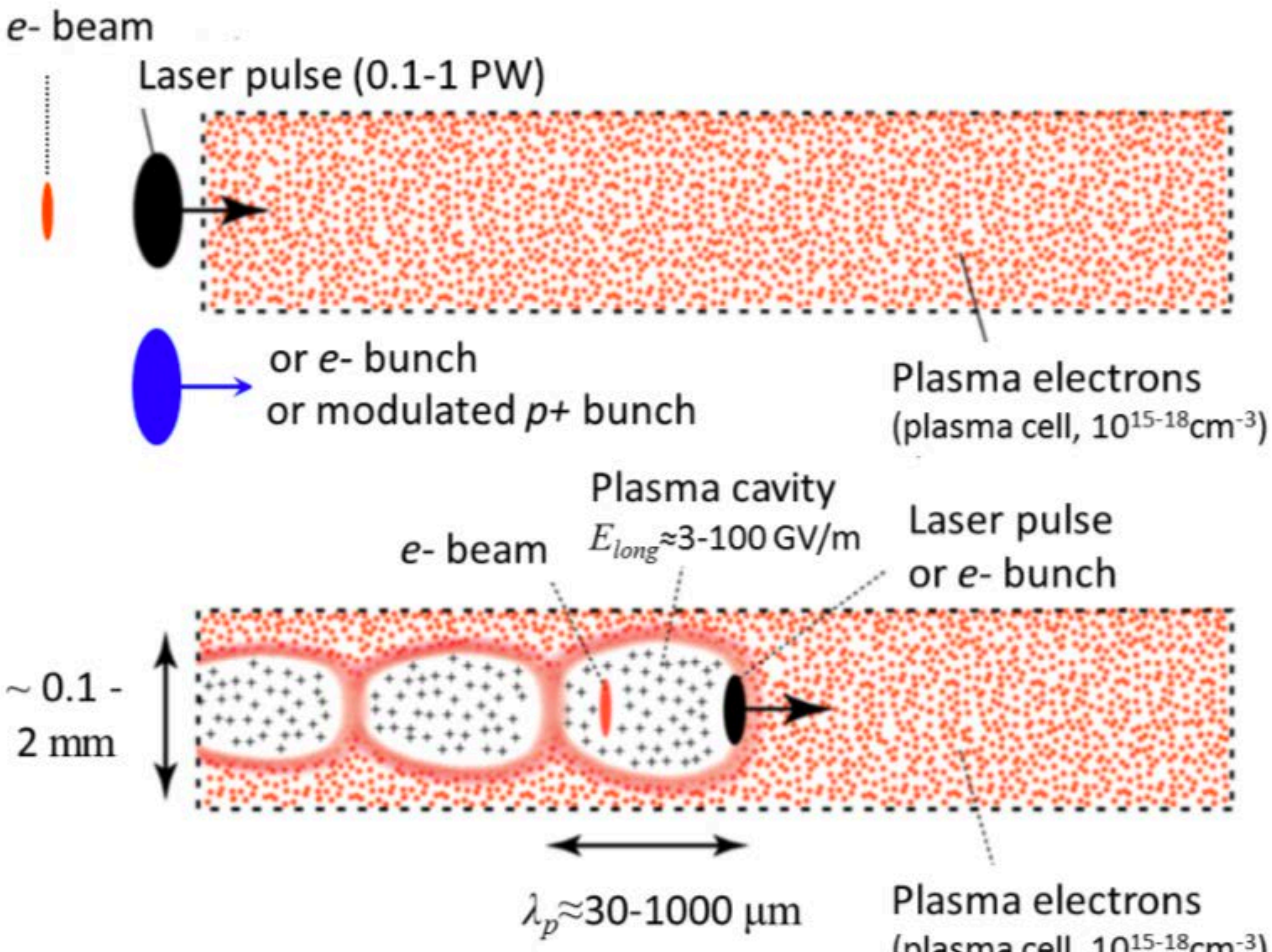
For ILC at 500 GeV, this scaling is

$$2 \times 10^{34} \sim \frac{10 \text{ MW/beam}}{500 \times 6 \text{ nm}^2}$$

Scaling to a 10 TeV collider

$$10^{36} \sim \frac{10 \text{ MW/beam}}{2 \text{ nm}^2}$$

It is already understood how to produce beams of few-nm vertical size for the next-generation ILC and CLIC e+e- colliders.

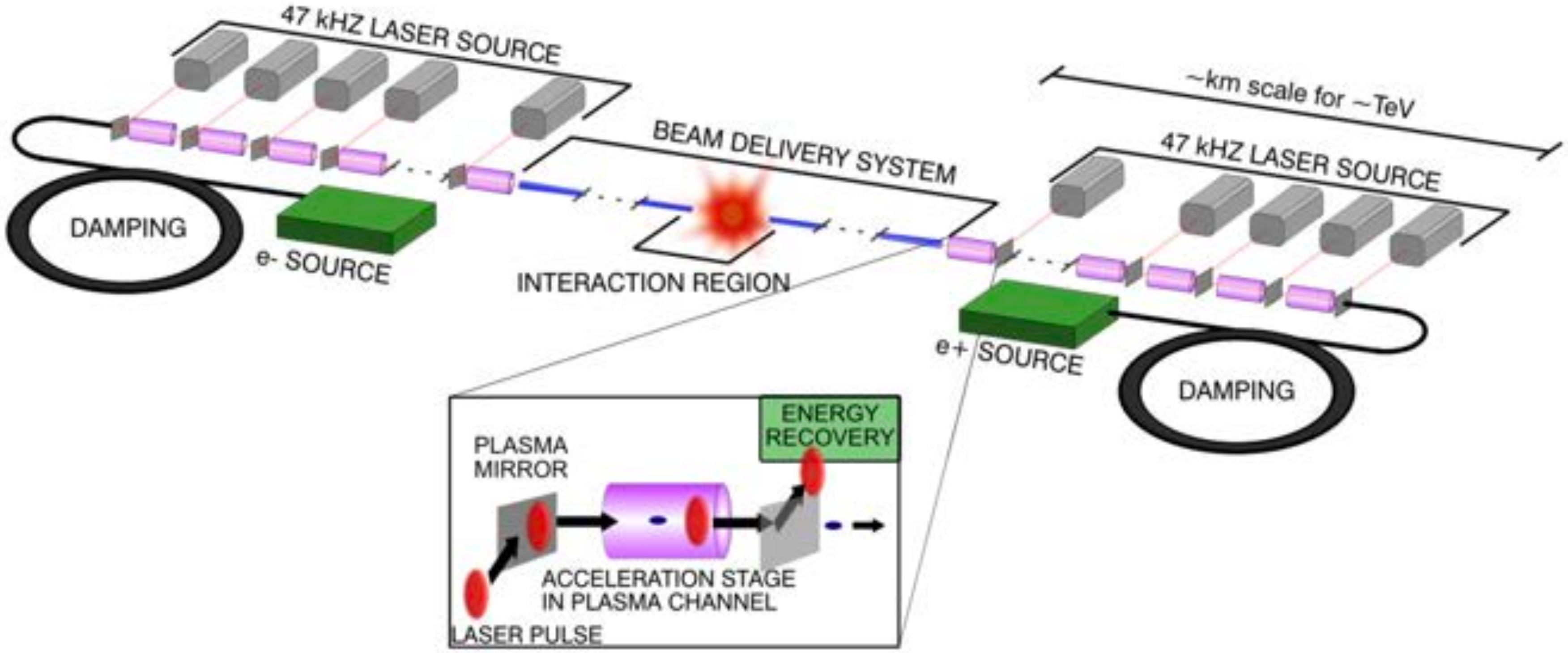


Shiltsev-Zimmermann RMP review - arXiv:2003.0984

Wakefields in a plasma can be driven by beams (e- or proton) or by lasers. The central idea is that an electromagnetic pulse ejects electrons from its path, creating a cavity and longitudinal accelerating fields dynamically.

A pulse that is small transversely leads to a narrow active region, with efficient energy transfer.

The method naturally produces high gradients; gradients of 150 GeV/m have been observed. Controlled acceleration has been achieved with gradients of ~ 5 GeV/m. In contrast, SLAC is 17 MeV/m.



issues for PWFA:

Energy efficiency: wall plug to drive beam or laser

Reproducibility shot to shot, needed to maintain
small emittance by feedback control

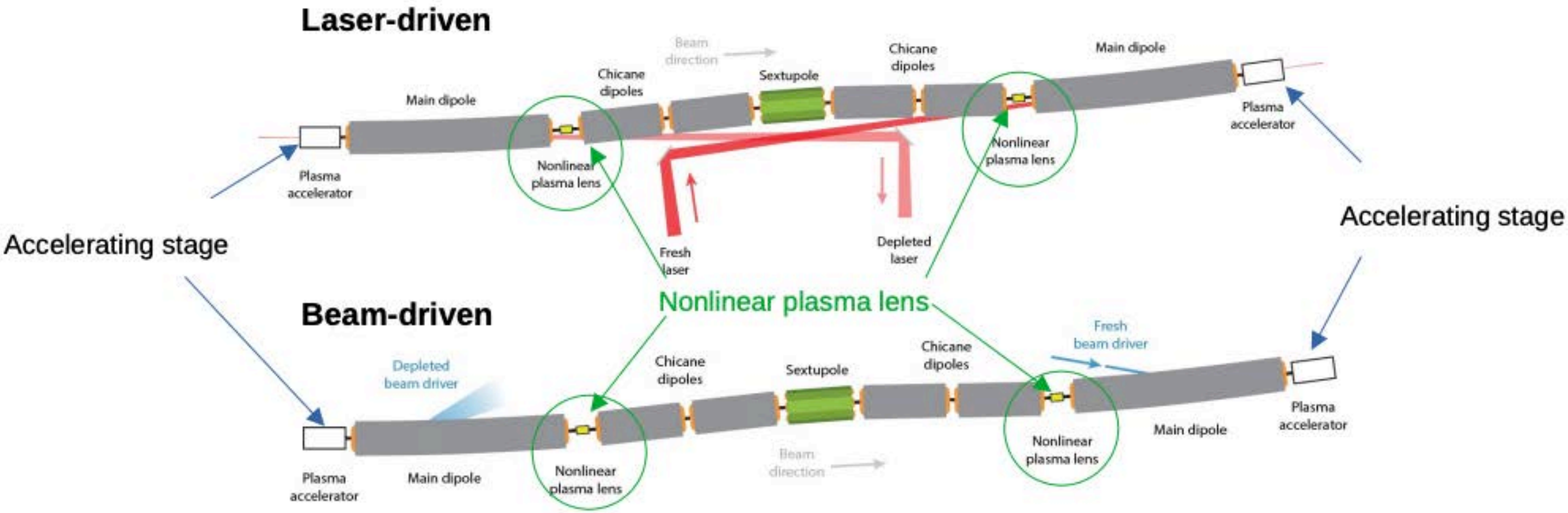
Control of beam instabilities

Transport of the accelerated beam from each
stage to the next with emittance preservation

5,000 / 50 = 100 stages per side

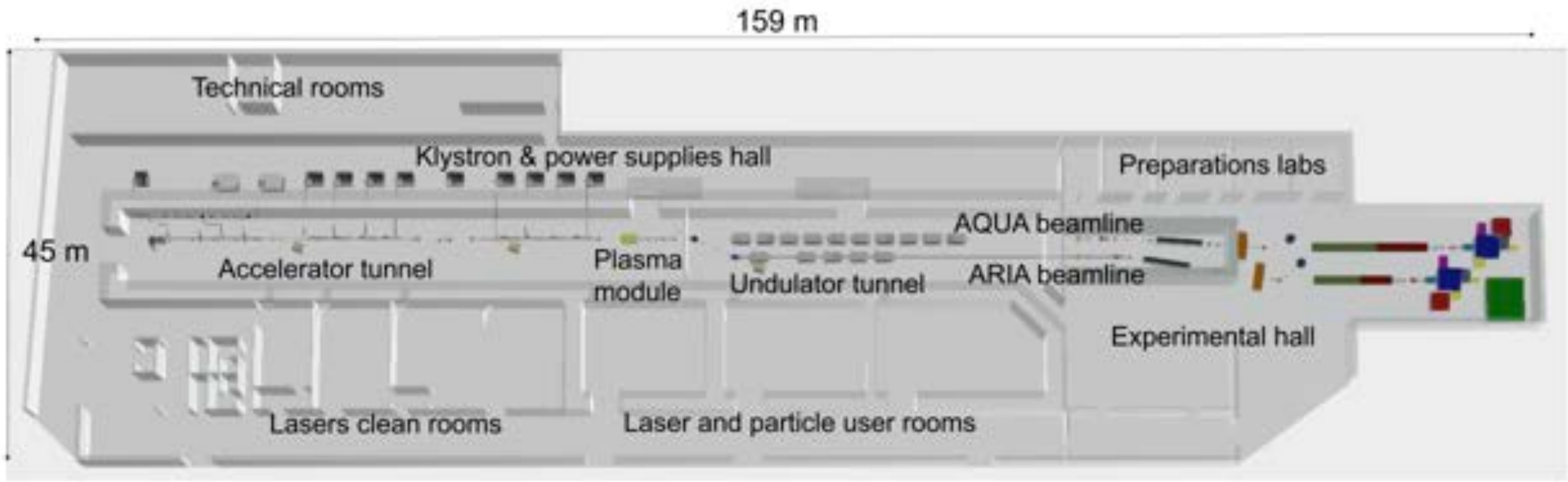
Acceleration of e+: a plasma accelerator is not particle-antiparticle symmetric. If positrons cannot be accelerated efficiently, another solution is to build a photon-photon collider based on e-e- beams. Tim Barlow has new ideas for this option.

Designs for passing the accelerated beams from one stage to the next are still at an early stage. Here is one concept now being studied at DESY for the HALHF Higgs factory project (Lindstrom, Drobniak, Kalvik):



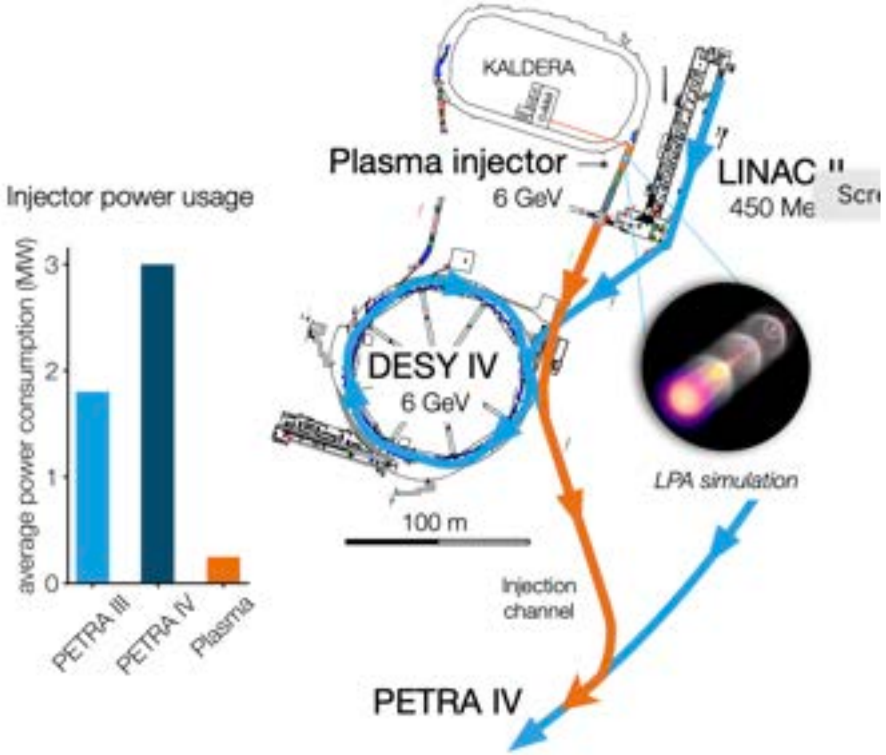
The next step for plasma accelerators is to incorporate them into Photon Science accelerators:

EuPRAXIA at INFN Frascati 1 GeV alternately plasma/X-band
F. Villa, arXiv:2307.0216



Plasma alternate injector
for PETRA IV 6 GeV

M. Thevenet at LCWS 2024



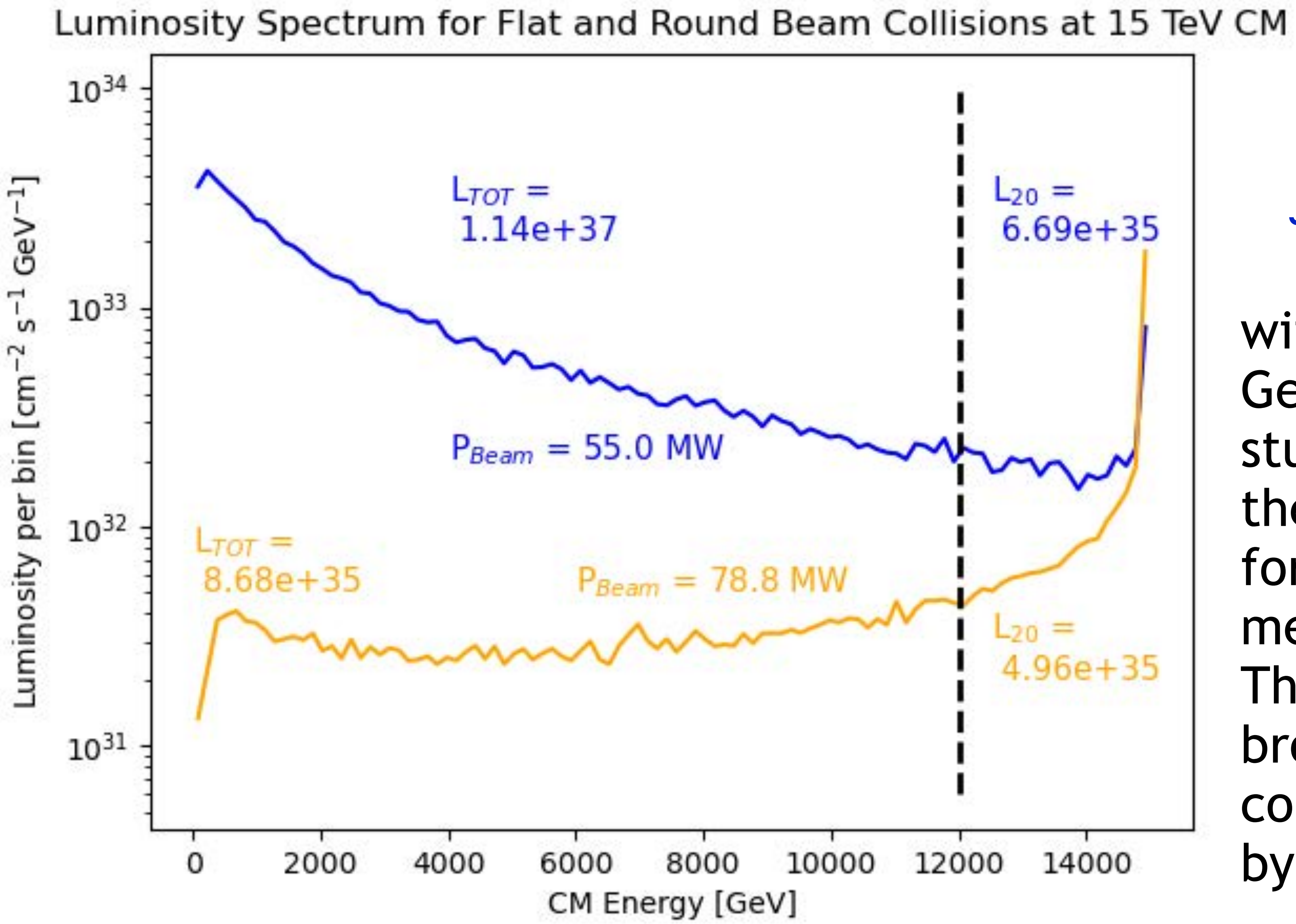
In e+e- collisions with very small, intense spot sizes, the charge of each bunch has a strong effect on the particles of the other bunch.

This leads to hard synchrotron radiation (beamstrahlung) and additional forward pair creation during the bunch-bunch collisions.

The electromagnetic fields are strong enough that Nonlinear QED effects must be taken into account.

simulation of the e+e- collision CM energy spectrum for 15 TeV colliders,

assuming ß function and normalized emittance comparable to ILC designs, scaling up the beam energies:

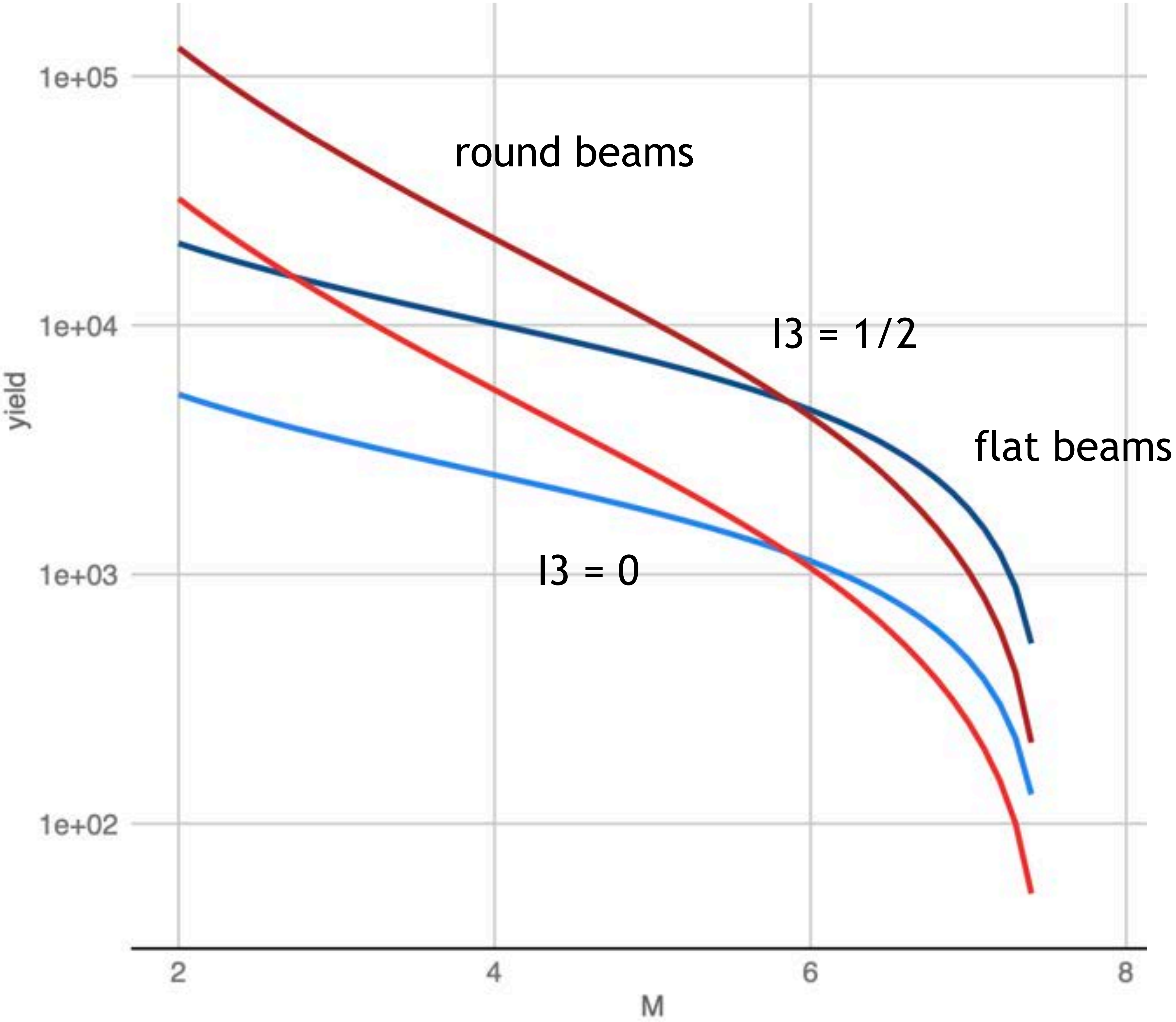


Jaden He

with He and Gessner, we are studying the use of these distributions for physics measurements. This is part of a broader collaboration led by Simon Knapen.

see Barklow et al arXiv:2305.00573

as an exercise, use these spectra to compute the yield/yr for pairs of heavy leptons:



Conclusions: (for experimenters / for theorists)

10 TeV pCM colliders are more than a vision.   There are multiple strategies to reach the 10 TeV scale.   All of these have difficulties that may take decades of R&D to overcome.   But we can solve these problems.

We must make the search for the new ideas needed for 10 TeV colliders a priority for particle physics.

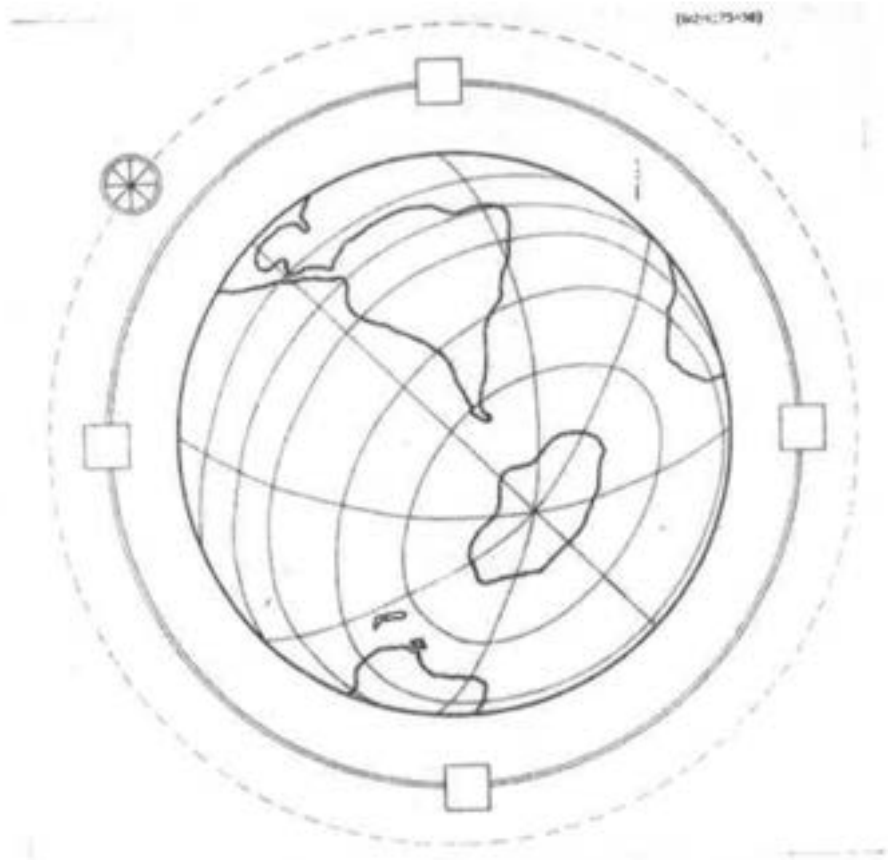
Are the secrets of electroweak symmetry breaking and the Higgs field to be found at 10 TeV ?   If we believe in this, we must still find arguments to convince our skeptical scientific colleagues. If we don’t believe in it, we are believing that there is no point in making the next step in collider physics.

We cannot imagine the future of particle physics without grappling with this question.

a footnote: Fermi's Globatron

see J. Cronin, in "Fermi Remembered" (U Chicago Press, 2004)

address at the APS meeting, January 1954



"Following Fermi's extrapolation, the Globatron would have arrived in 1994 with an energy of 5×10^9 MeV… A dramatic view of this accelerator is shown in Fermi's second slide. In his first slide, Fermi also plots the evolution of cost of the accelerators. The cost of the ultimate accelerator was 170 billion 1954 dollars!"

All of this is very impressive. But the pp center of mass energy of the experiments would only have been 3 TeV.

Within 30 years, after the discovery of alternating gradient focusing, colliding beams, superconducting magnets, particle cooling,

the W and Z were discovered and the blueprint for the LHC was laid out.

We can invent our way out of our current difficulties. This needs to be the first priority in particle physics now.