Physics Motivations for Future Colliders

A theorist's perspective

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INFN - Sezione di Genova

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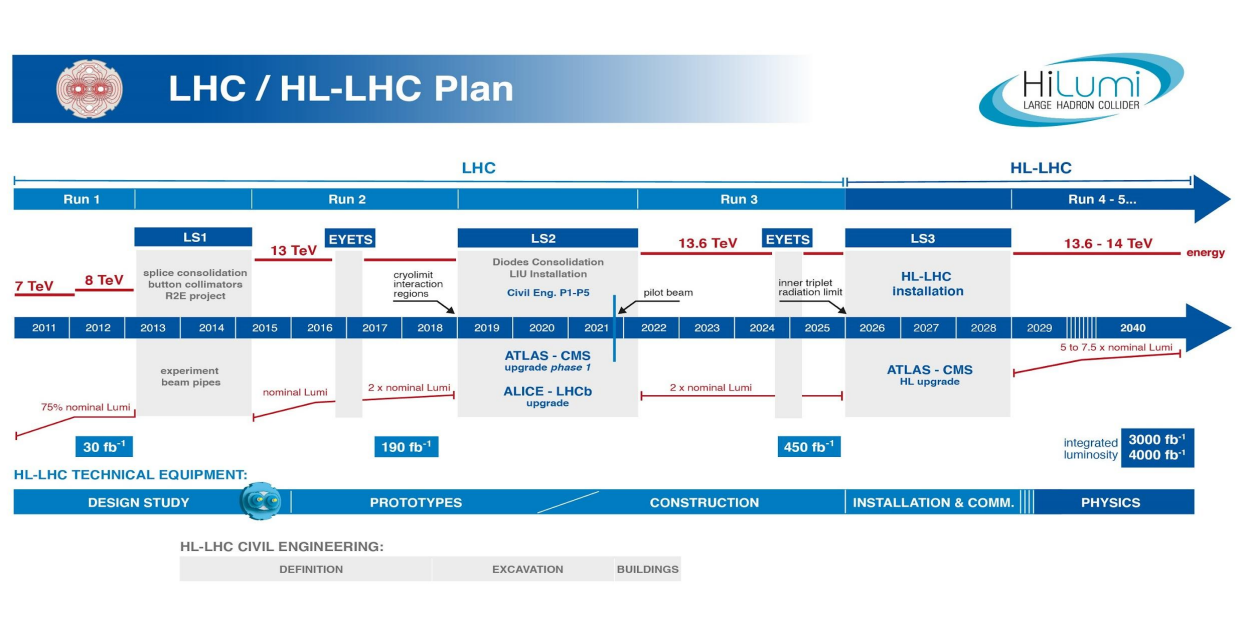
Corfu Workshop on Future Accelerators

Looking after what?

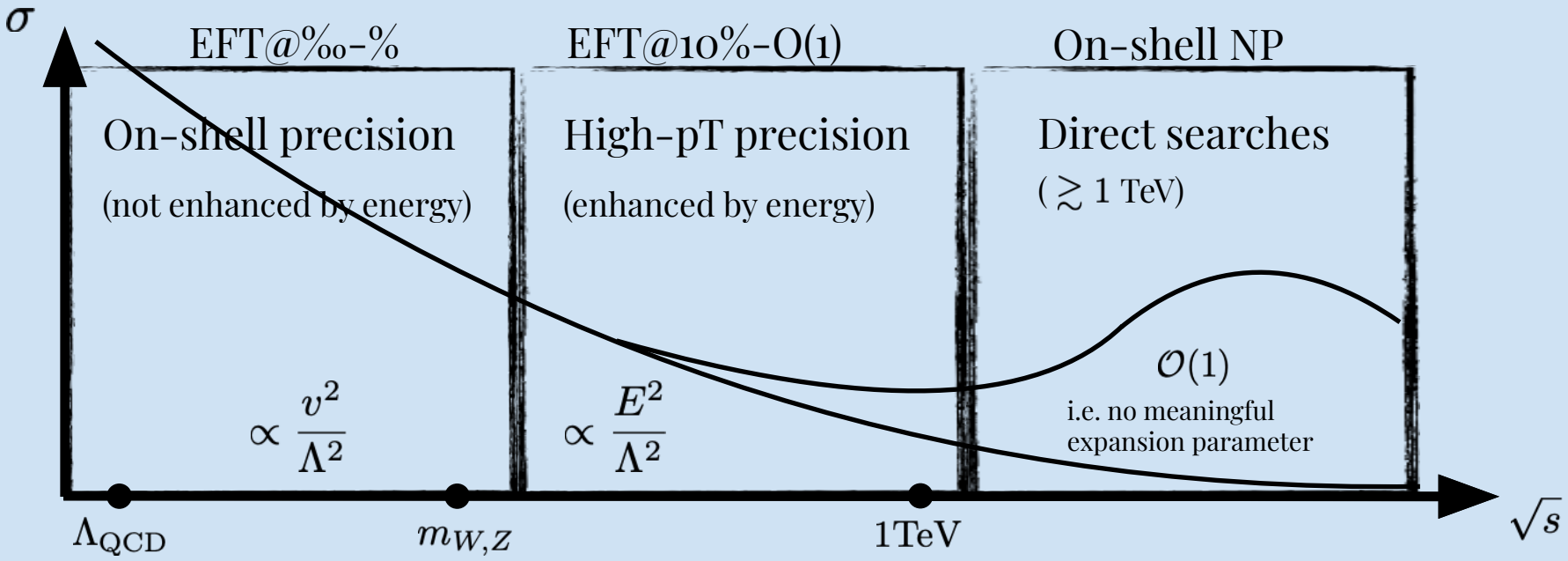
Where do (will) we start from?



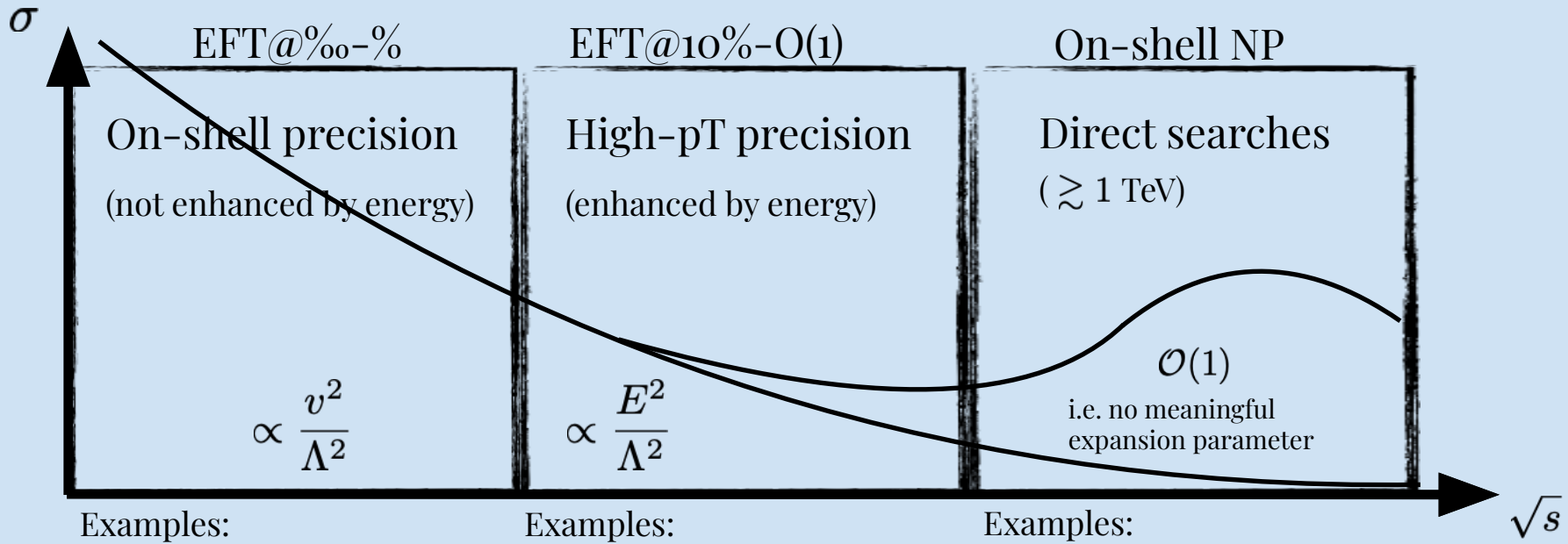
Main goal: find signs of new physics

- Directly: probing on-shell NP
- Indirectly: probing the effect of NP on precisely measured SM observables

Which NP?


σ
EFT@‰-%
EFT@10%-O(1)
On-shell NP
On-shell precision
(not enhanced by energy)
High-pT precision
(enhanced by energy)
Direct searches
($\gtrsim$ 1 TeV)
$\propto \frac{v^2}{\Lambda^2}$
$\propto \frac{E^2}{\Lambda^2}$
$\mathcal{O}(1)$
i.e. no meaningful
expansion parameter
$\Lambda_{\rm QCD}$
$m_{W,Z}$
1TeV
$\sqrt{s}$

Which NP?



Examples:

- SM parameters
- Pole observables
  e.g. S, T, etc.
- Higgs observables

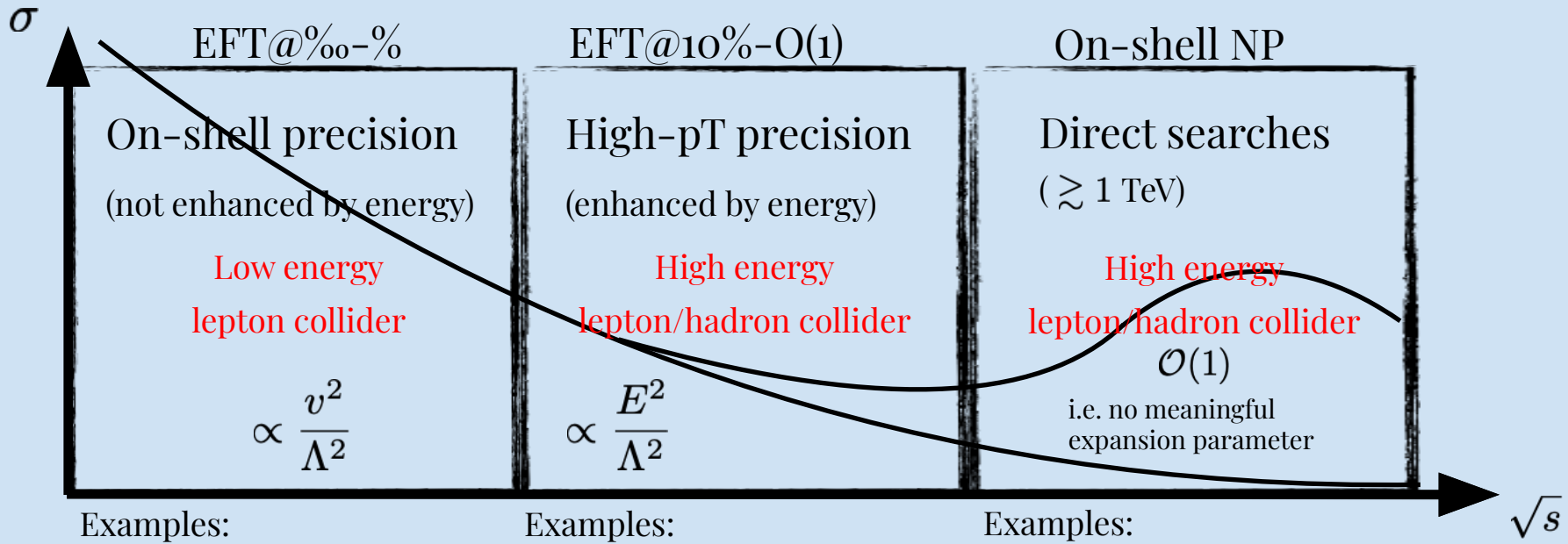
Examples:

- DY (e.g. W, Y, etc.)
- Di-bosons
- Di-jets
- Heavy quarks ($t\bar{t}$, etc.)

Examples:

- SUSY
- Top partners
- Resonances

Which NP?



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On-shell vs off-shell precision

Compare for instance LEP and LHC sensitivity to interactions of the form

Z-pole observable

$$-\frac{\hat{S}}{4m_W^2}(H^\dagger \tau^a H) W^a_{\mu\nu} B^{\mu\nu}$$

LEP	LHC
Energy: ~100 GeV	Energy: ~1 TeV
Accuracy: ~‰-%	Accuracy: ~10%
New physics effects not enhanced by energy	New physics effects not enhanced by energy

LHC "cannot" compete with LEP

off Z-pole observable

$$-\frac{Y}{4m_W^2}(\partial_\rho B_{\mu\nu})^2$$

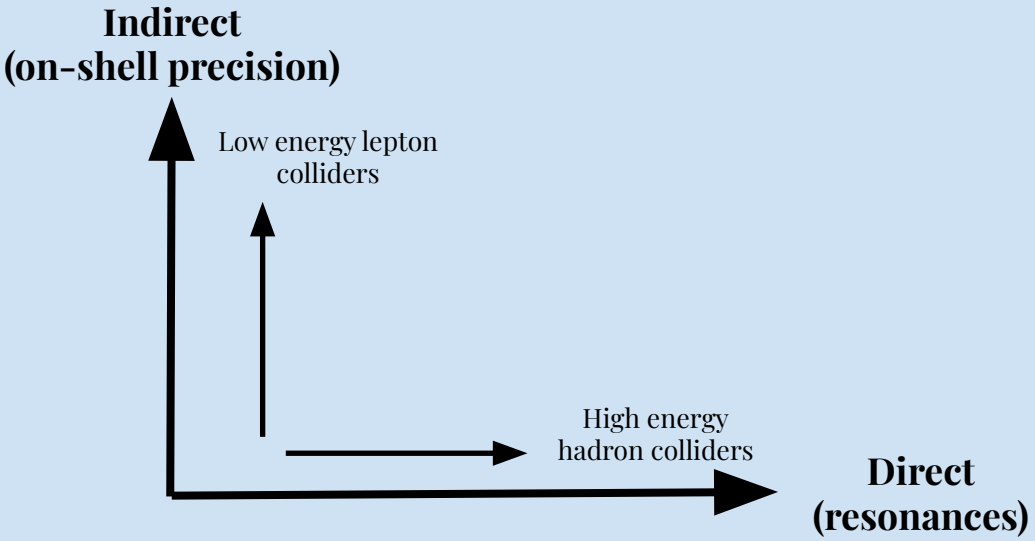
LEP	LHC
Energy: ~100 GeV	Energy: ~1 TeV
Accuracy: ~‰-%	Accuracy: ~10%
New physics effects not enhanced by energy	New physics effects enhanced by $E^2_{\rm LHC}/E^2_{\rm LEP} \sim 100$

LHC comparable with (or better than) LEP

This comparison defines two orthogonal directions in the "precision program"

Which machines for which signatures?

Future colliders in 3D



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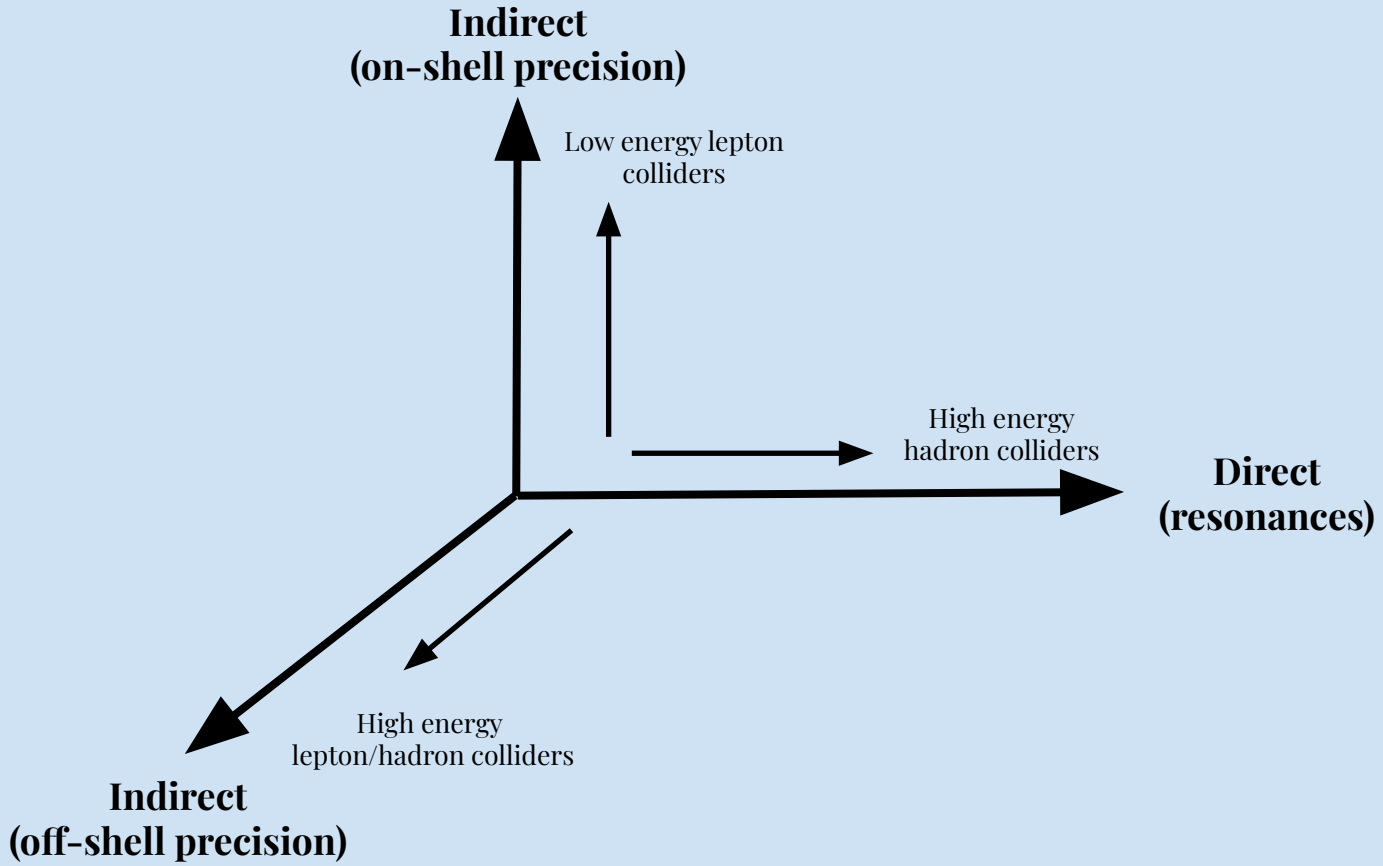

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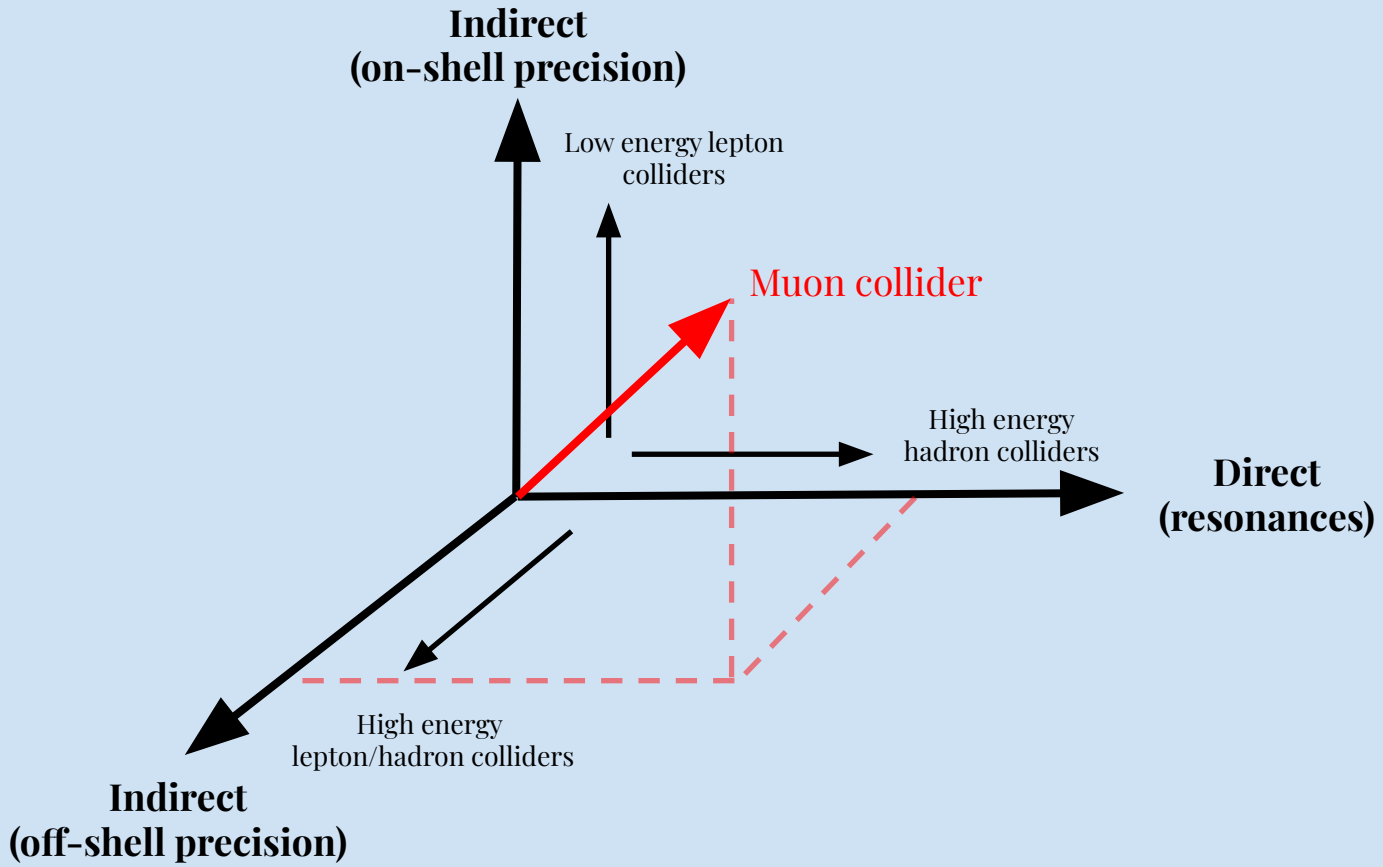

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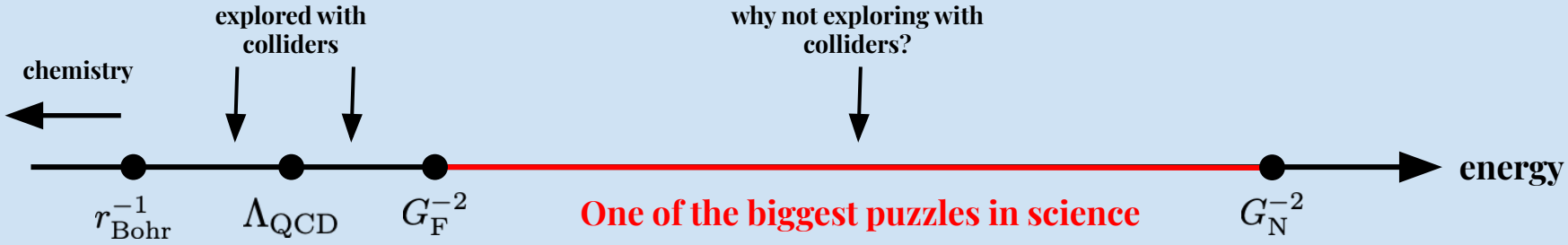

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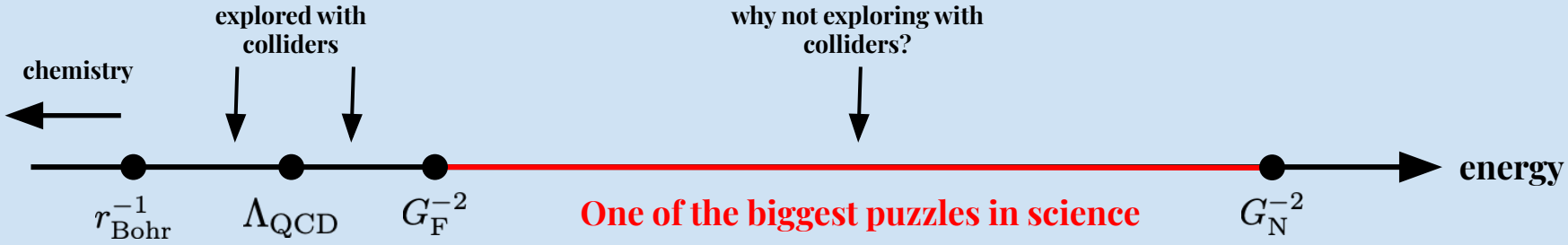

Advantages

- Only technique to probe higher energies model independently (agnostically)
- Experimenter defines experimental setup
- Repeatable (experimental ≠ observational)
- Measurement of SM in new energy regime and to unprecedented precision
- Answer well posed questions about SM and BSM
- Tremendous impact on science and technology

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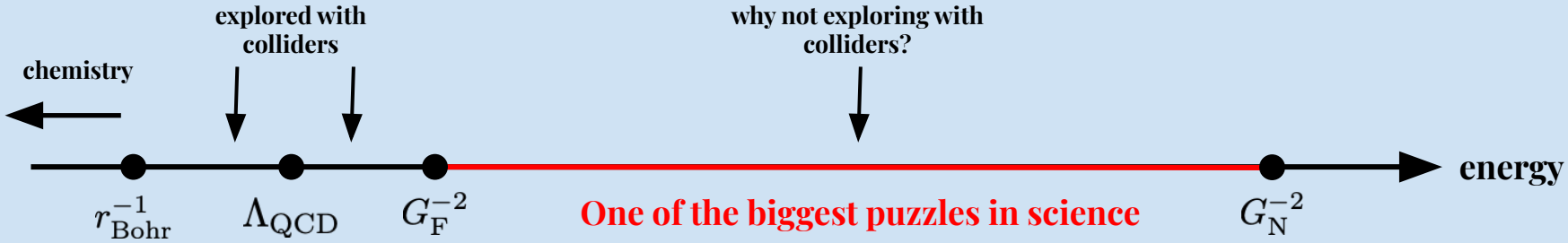

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Challenges

- Energy increases very slowly with time/money (driven by tech evolution, but not only)
- Need to convince funding agencies (the physics case is as simple as above, but it requires understanding of the scientific method and large funds)
- Long term planning is becoming harder due to the increasing speed of tech advance
- Keep the community engaged (or even alive)

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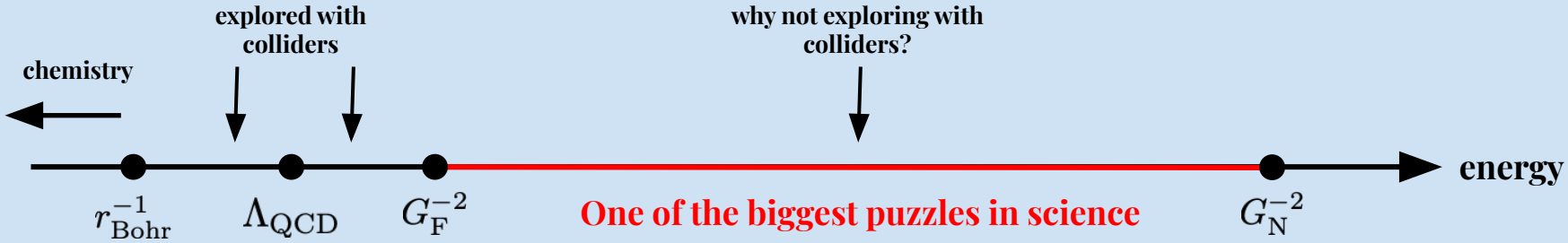

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We have no guarantee of any discovery, but
guarantees cannot be a criterion for fundamental research.
What we can certainly guarantee is a spectacular physics program!

Are particle colliders observatories?

Some colleagues have suggested the analogy of colliders with observatories, defining the former “particle observatories” (a very smart analogy to discuss with funding agencies!)

I would argue that colliders are much more than observatories, because respect the basic requirement of experimental science (compared to observational science), that is the possibility for the experimenter to set up the experiment (including the system under study), to repeat it, and to decide when to stop it

Colliders are the most advanced, both conceptually and technically, incarnation of Galilei's scientific method based on the "Sensate esperienze e necessarie dimostrazioni" (meaningful experiences and necessary demonstrations)

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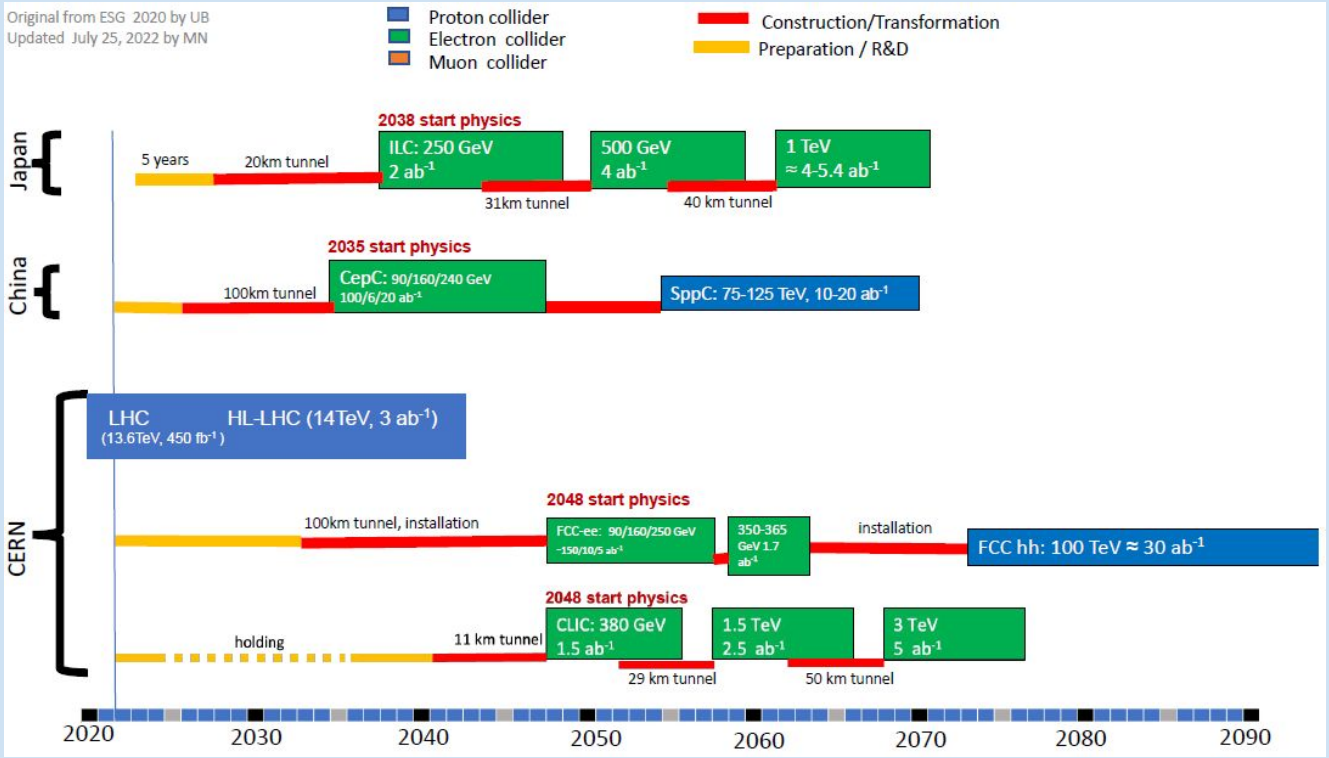
Collider physics is the only general-purpose experimental research field in fundamental science

.. and the only one who delivered revolutionary results!

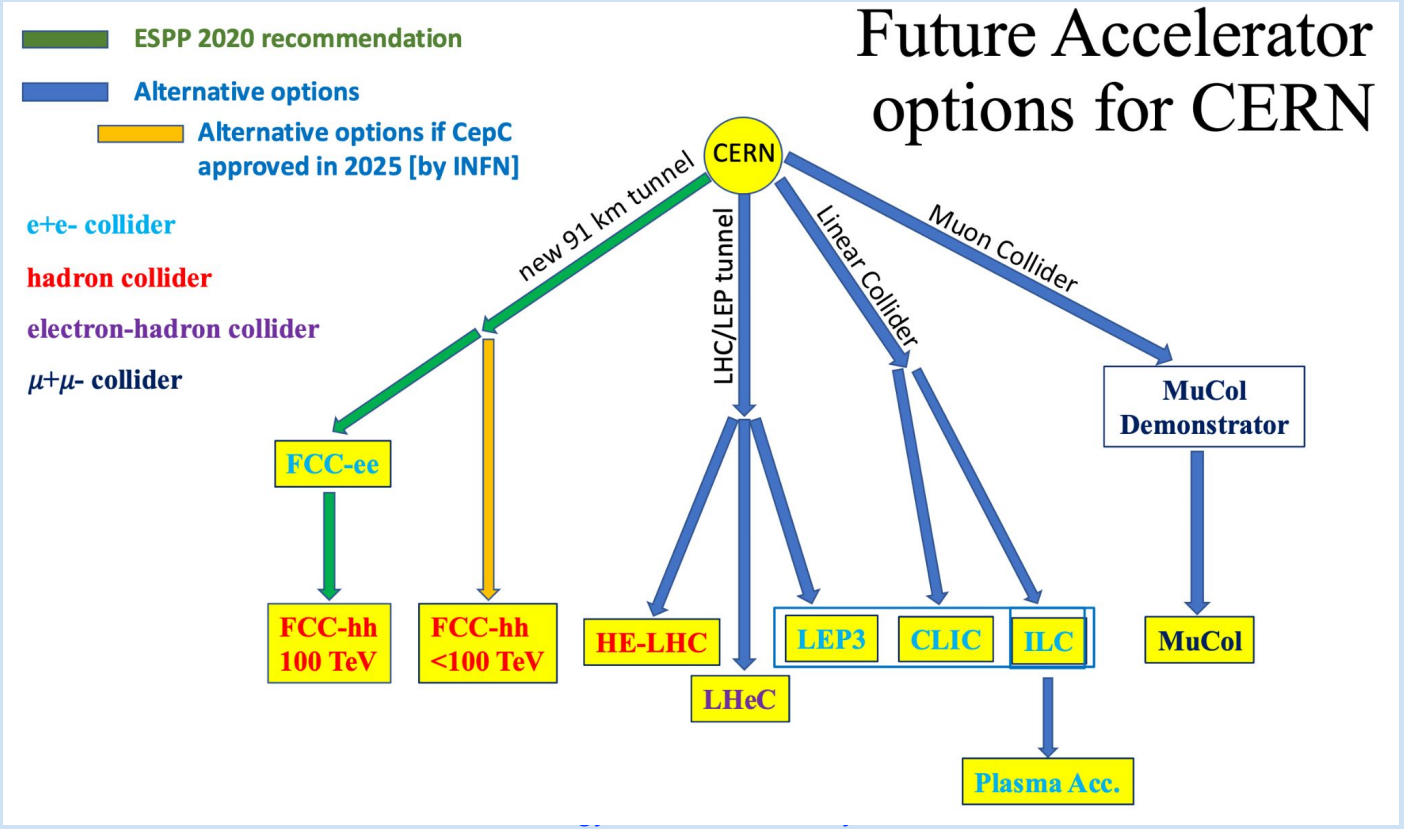
Which colliders?

Which colliders?



Snowmass Energy Frontier summary, 2211.11084

Which colliders?



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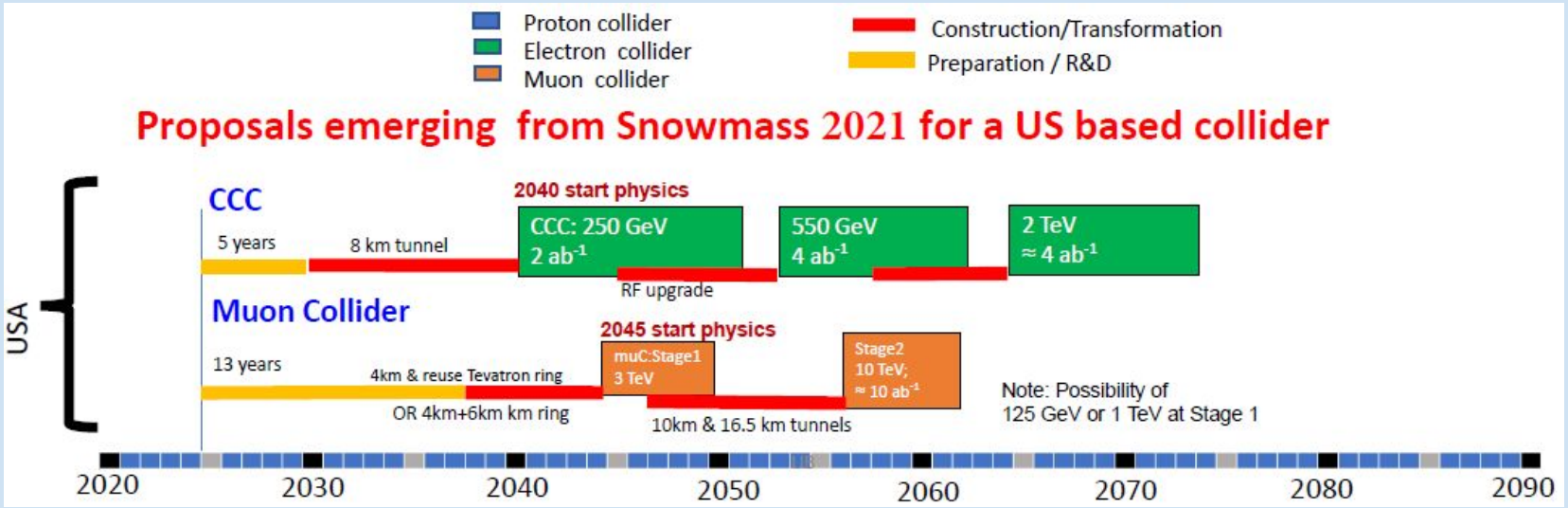

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Some perspective

Measurements, searches, and global fits: a statistical perspective

- **Measurement**
  - Can be defined as parameter estimation within the SM hypothesis
  - Quantifies precisely “what you see”, but says nothing about “what you do not see”
  - Used to extract SM inputs to searches and global fits
- **Search (or direct search)**
  - This usually refers to “direct searches” where, through a statistical hypothesis test, the SM is confronted with a specific alternative hypothesis
  - It gives some information on how much your data prefer the SM vs a well-defined alternative model
- **Fit (or global fit or indirect search)**
  - This consists of either parameter estimation beyond the SM or a hypothesis test with a general enough alternative hypothesis (e.g. EFT)
  - It gives information on “what you see” and “what you do not see”
  - Notice that usually only BSM parameters are fit, while SM ones are taken from measurements

The (HL-)LHC Legacy

- **“BSM measurements” (aka global fit v2.0: SM+EFT)**
  - It is known that uncertainties on some SM inputs is what limits the extraction of BSM parameters and, conversely, the presence of NP may affect extraction of SM parameters

    Examples: PDFs vs DY, multi-jet vs alphaS, etc.
  - As the knowledge of the SM increases (better predictions and more analyses become available) and the large EFT parameter space gets a “good coverage” (several channels are measured and can be combined with each others) one can build a combined likelihood for SM+EFT
  - Analyses that were targeting direct searches need to be turned into “measurements”, which require a higher level of precision (e.g. di-bosons)

    A simple (and interesting) example is given by fitting EFT and PDFs together using DY data (see e.g. Greljo et al. 2104.02723 and subsequent literature)

Building the LHC legacy (in the next ~20 years) for indirect searches means to design and accomplish the final BSM measurement (which includes the SM!)

(New) Challenges

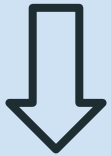
- **Combination and correlation**: combining experimental analyses is still a big issue at the LHC, where uncertainties are parametrized differently, and correlations are not known (there is a slow progress but huge work ahead)
- **Observables definition**: observables related to precision measurements are often targeted on “SM measurements”. It is necessary to extend and optimize them towards multi-differential “BSM measurement” oriented observables (e.g. recent triple differential DY cross section). Multivatiate and ML could also provide a solution.
- **Large parameter space**: when the number of parameters > a few, many studies become unfeasible (a lot of work in this direction: MEM, ML techniques, MadMiner, analytic reweighting, etc.)
- **EFT in backgrounds**: EFT effects may be relevant, especially for reducible BGs
- **Theory uncertainties**: a further complication arises when statistical uncertainties become “negligible” and theory errors start to dominate (e.g. PDFs, HO, etc.). Including theory errors in statistical analysis presents conceptual issues that need to be addressed
- **Result presentation/preservation/distribution**: not only experimental analyses, but also theory results are still shown in an ad-hoc and incomplete way (e.g. 2D contours, etc). For experiments the issue is more severe, but theorists should try to get used to always deliver the full likelihood leading to their fits, that could be used by others and as input to global fits

The EFT direction(s)

EFT for the SM seems like a rather “new” (~10 years) topic for theorists

Many theorists have abandoned model building in favor of EFT

This is not a psychological effect due to the "absence" of new physics



- Absence of new physics (and the existence of precision measurements) is a requirement for EFT to be interesting, relevant, and applicable!
- EFT is the simplest and most consistent way of parametrizing the different directions in which deviations from the SM can appear (SM deformations)
- It is incredibly powerful at determining what “is possible”, what “is impossible”, what “is likely” and what “is unlikely”
- Measurements (and especially precision measurements) in high energy physics have little meaning if one cannot quantify the above in a consistent way
- In other words, EFT provides the “alternative hypothesis” necessary for a robust statistical hypothesis test of the SM

Community POV

Is there consensus in the HEP community?

There is currently no consensus in the community about FC
HEP is (fortunately) also a matter of "taste"

The "negationist"

"Collider physics is dead, we don't need a new collider!"

The "conservative"

"We obviously need a new lepton collider, such as FCC-ee!"

The "progressivist"

"We obviously need a new hadron collider, let's go for the FCC-hh!"

The "dreamer"

"The only innovative collider is a HE muon collider, go for that!"

- Physicists tend to have "strong" opinions
- Geo/political factors have a large impact

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- Physicists tend to have "strong" opinions
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The negationist (no FC)

"Collider physics is dead, we don't need a new collider!"

Arguments

- LHC found nothing
- There are several other fields, e.g. related to astro-particle, who would take advantage from dropping the FC program
- Too hard to keep the community engaged for a long time during construction, especially if we go for "existing" technology and "modest" discovery reach (FCC-ee)
- S&T challenges are moving to fields other than HEP

Counter-arguments

- LHC delivered the most incredible experimental program to date, which is a great success!
- Other fields need to be followed "independently" of the decision of building a new collider: good research should go on in parallel, not compete
- Worries about community engagement are overestimated: people will always like to do research to answer fundamental questions
- New fields at the edge of S&T research will be new weapons to defend our research, not to kill it

The conservative (FCC-ee/CepC)

"We obviously need a new lepton collider, such as FCC-ee!"

Arguments

- Any path towards continuation of a collider physics program will first require new precision measurements
- Unprecedented opportunity for precision measurements
- LHC could not have existed without LEP
- Technology "already exists", so we will have simpler life convincing funding agencies that this would lead to a successful program
- Economically "possible"
- Maybe a higher energy linear collider such as ILC/CLIC can be another option

Counter-arguments

- Building something that falls within "existing technology" will lower the level of new challenges compared to the past
- Harder to attract "the best people" to do something roughly within present technology
- While the opportunity for precision measurements is incredible, it cannot match the discovery potential of a hadron collider, so we should go directly for the latter, maybe with limited energy (~30/40/50/60/70TeV?)
- Linear colliders do not have neither the FCC-ee lumi, nor the FCC-hh energy, not worth investing on them

A personal remark

FCC-ee should NOT be thought of as the new LEP: with new physics knowledge, techniques, technology, tools, we could get much more than at LEP and doing physics at FCC-ee will be radically different than doing physics at LEP

The progressivist (HE-LHC/FCC-hh)

"We obviously need a new hadron collider, let's go for the FCC-hh!"

Arguments

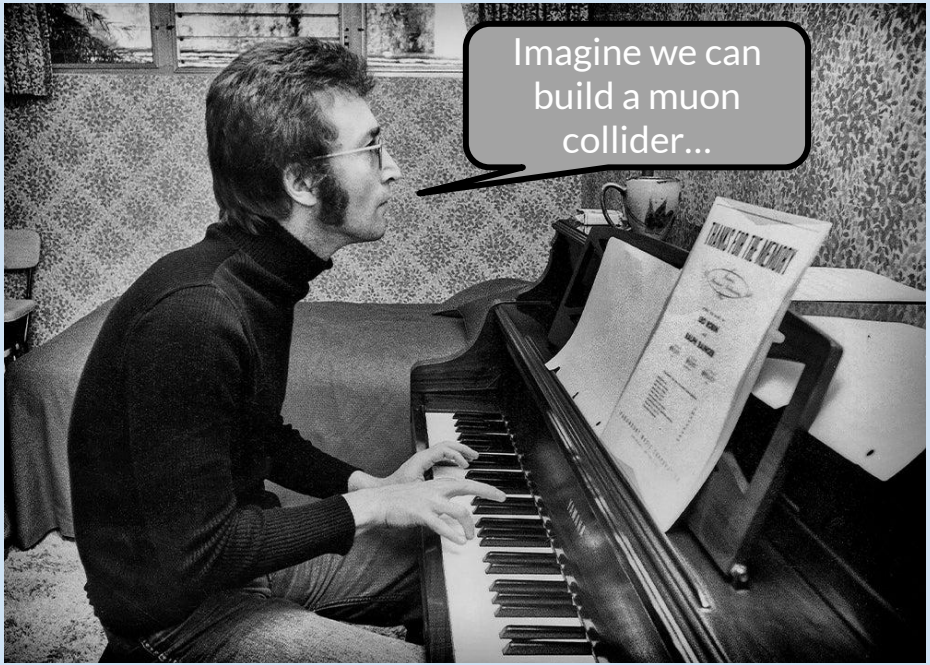
- Two (FCC-ee/CepC) or even more (ILC/CLIC) lepton collider programs around the world are too much. China could focus on CepC while Europe should directly head towards FCC-hh
- FCC-hh is a discovery machine
- FCC-hh could also be a precision machine
- Since technology for $\gtrsim 40$ TeV may require long time, one can envisage a preliminary "HE-LHC" phase
- The program is ambitious enough and with enough discovery potential that keeping community engaged will not be an issue

Counter-arguments

- Europe (CERN) cannot rely on the choices that China/Japan will make and anybody should go on independently
- FCC-hh "useless" without precise SM input measurements at a lepton collider
- Technology does not exist and will take a long time (first physics run > 2050)
- Going for an intermediate phase (HE-LHC) would make it "even more" expensive for a "modest" gain
- FCC-hh economically unsustainable as "first FCC phase"
- Harder to get funds compared to FCC-ee, while simpler after FCC-ee (tunnel already built)

The dreamer (muon collider)

"The only innovative collider is a HE muon collider, go for that!"



Arguments

- This is the hardest challenge, so we should undoubtedly go for it
- Joins the high energy reach of FCC-hh and the high precision of FCC-ee
- A conceptually new direction in collider physics (nothing ever seen before)

Counter-arguments

- Technology does not exist and will probably not exist for a long time
- Too ambitious and with too little know-how to be allocated the huge funds it would need to make real progress
- Need to keep R&D alive, but not commit to it
- May become an inconclusive fund draining project that is not the standard in HEP

Personal opinion: impact of AI

The impact of the current AI revolution may be O(1) on our FC planning

- Machine/detector R&D and design
- Data taking and monitoring (what if triggers will not be needed anymore?)
- Analysis techniques (also for projections and extrapolations)
- Object reconstruction
- Theory predictions (e.g. decrease theory errors by increasing efficiency/speed of numerical integration/sampling, etc.)
- Physics interpretations (meaningful fits with hundreds/thousands of parameters?)

Conclusions

Conclusions

- Collider physics is the **only general-purpose experimental research field in fundamental science** and the only one who delivered revolutionary results
- **Astroparticle/cosmology cannot replace collider**, neither can tens of "smaller" particle physics experiments (they are all complementary to colliders)
- **Collider physics established the SM**, the best theory of Nature we have so far, and is the only experimental research direction that can guarantee a frontier scientific program (even without guaranteeing any discovery)
- **The (HL)-LHC legacy may be given by "BSM measurements"** which extend the concepts of SM measurement, NP search, and global fit
- There are several issues to be addressed on top of building the next collider (precise theory predictions, combination of experimental analyses, definition of observables, large parameter space and signal generation, EFT in backgrounds, treatment of TH uncertainties, etc.) but **the path is clear**
- Optimized schedule would suggest CepC+ILC+FCC-hh (or CepC+ILC+muon-collider) but too many other considerations are in place (political/economical/sociological)

Thank you for your attention!