

Physics motivation for next-generation colliders

(A personal, incomplete perspective)

Tevong You

Contents

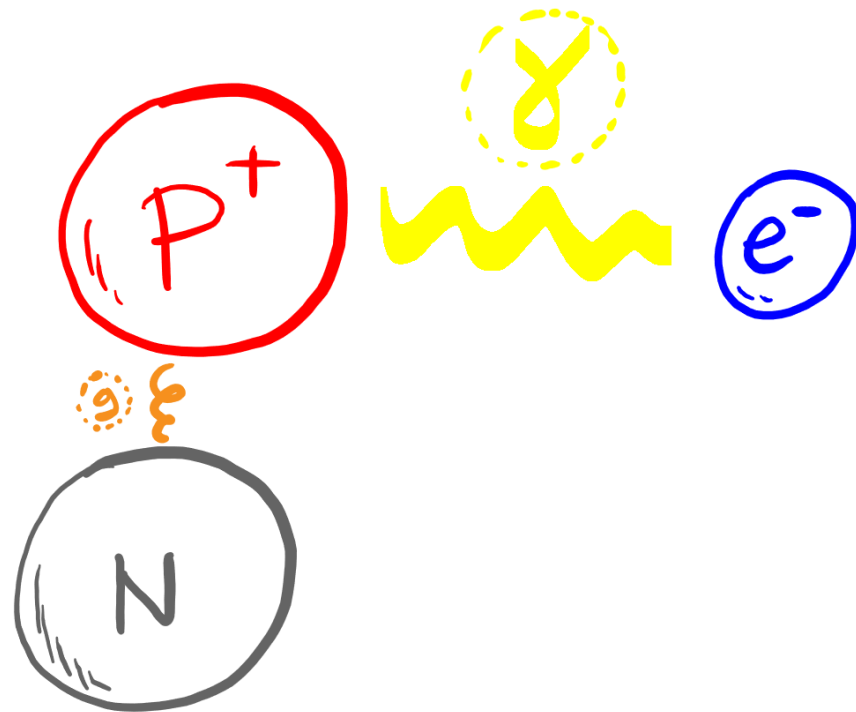
- Historical introduction
- The naturalness problem
- Colliders as general purpose particle observatories
- Exploring our fundamental origins through high energy and precision
- No BSM is still a success
- Radical BSM outcomes is still a possibility

Introduction

- **3 lessons** from history

Introduction

- *1930s: everything* is made of **protons**, **neutrons**, and **electrons**

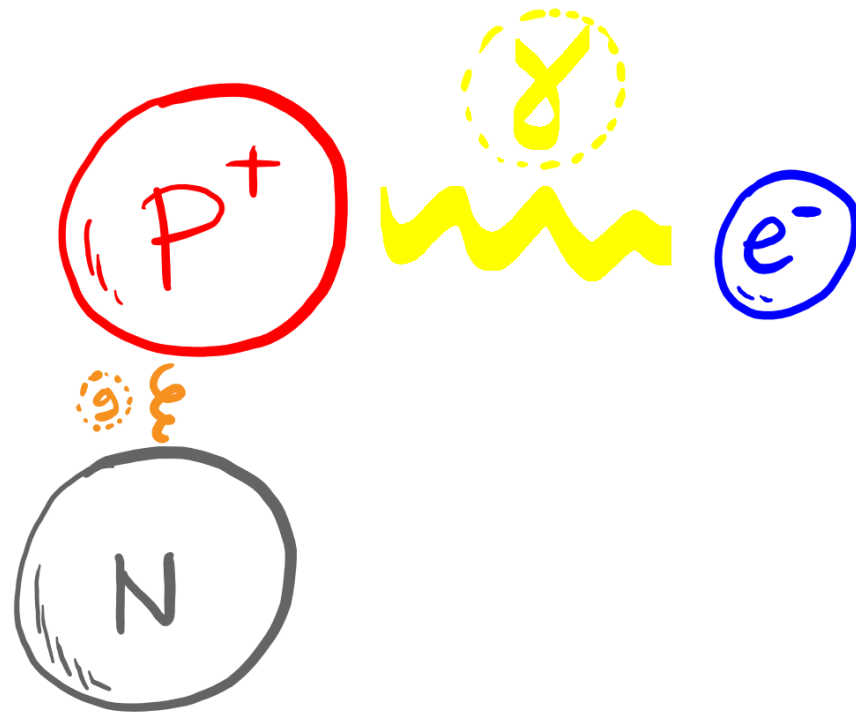


Minimal, economical theory!
However...

- Held together by **electromagnetism** and the **strong force**

Introduction

- *1930s: everything* is made of **protons**, **neutrons**, and **electrons**



"If we consider protons and neutrons as elementary particles, we would have three kinds of elementary particles [p,n,e].... This number may seem large but, from that point of view, two is already a large number."

Paul Dirac 1933 Solvay Conference

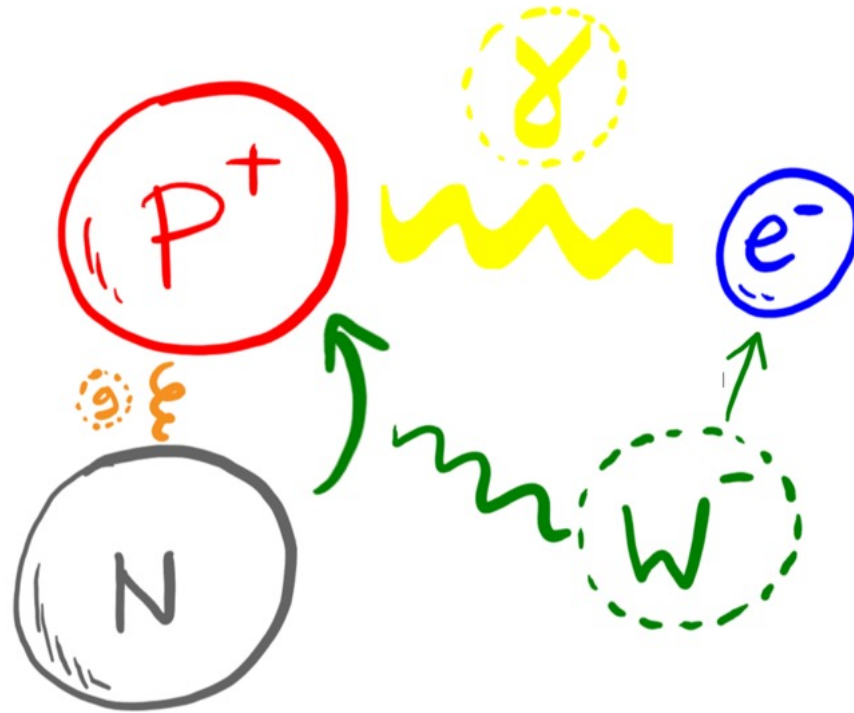
From D. Tong slide

Lesson 1: Beauty in fundamental physics is not an economy of particle multiplicities, it's an *economy of theoretical principles*

- Held together by **electromagnetism** and the **strong force**

Introduction

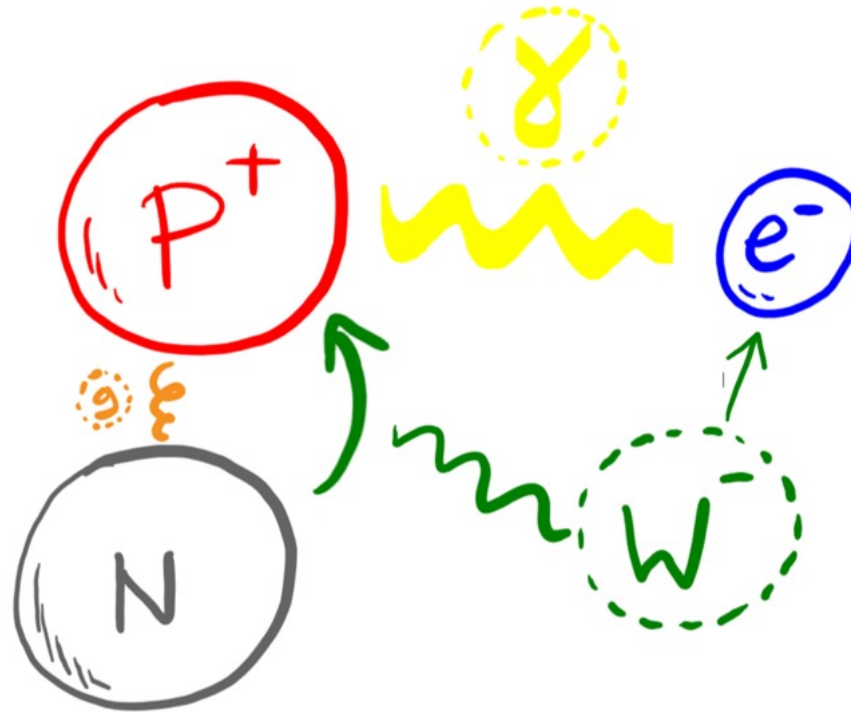
- **Weak force** explains *radioactivity*



- **Neutron** can change into **proton**, emitting **electron**

Introduction

- **Weak force** explains *radioactivity*

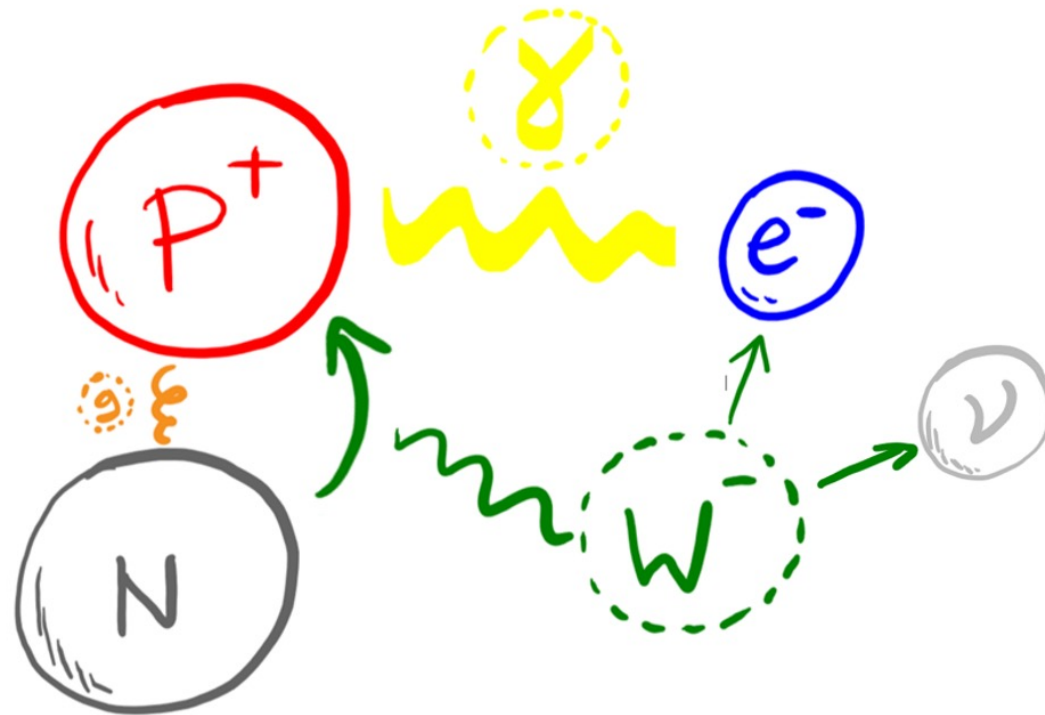


Missing energy? Pauli postulates “a desperate remedy”

- **Neutron** can change into **proton**, emitting **electron**

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- **Weak force** explains *radioactivity*

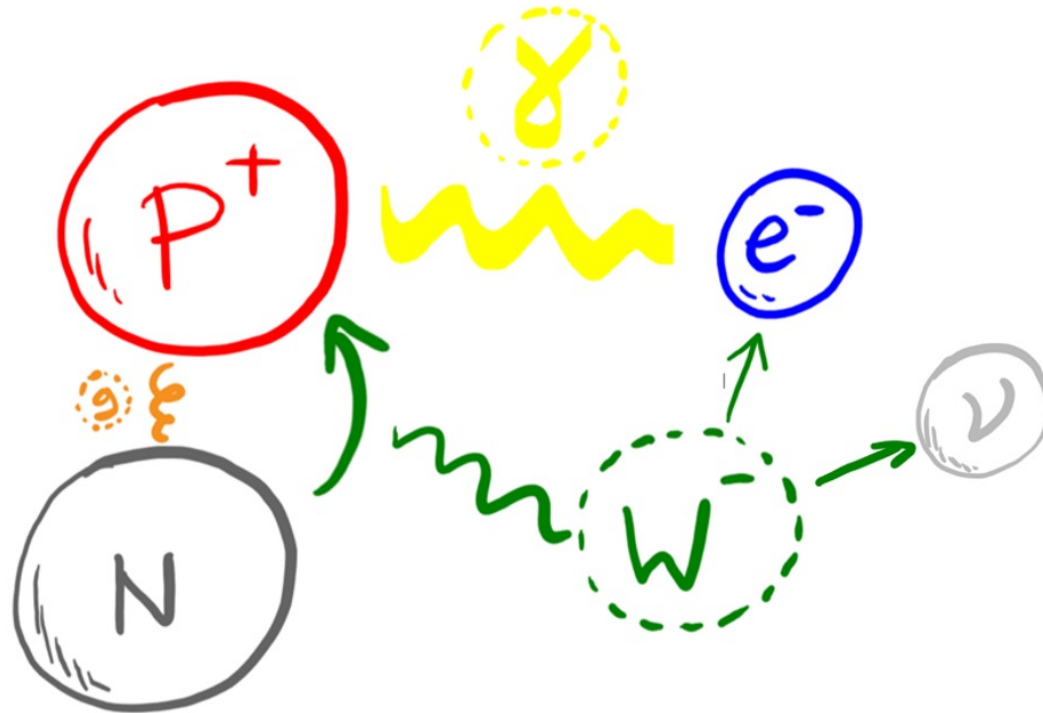


Missing energy? Pauli postulates “a desperate remedy”

- **Neutron** can change into **proton**, emitting **electron** and *elusive neutrino*

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- **Weak force** explains *radioactivity*



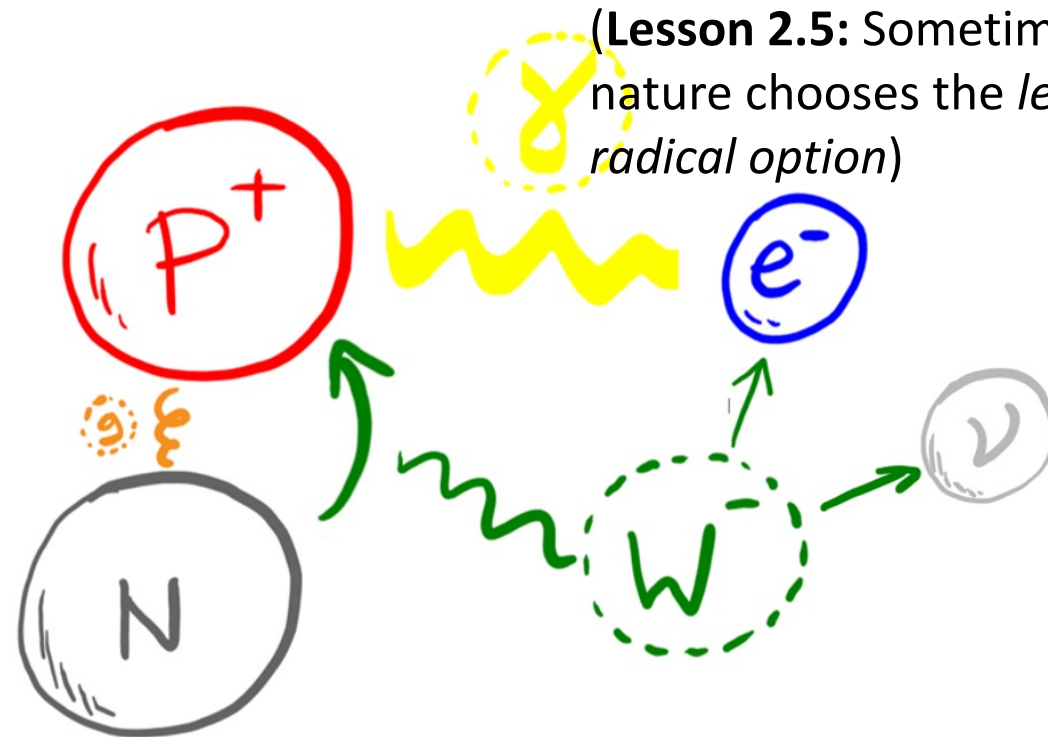
Missing energy? Pauli postulates “*a desperate remedy*”

Lesson 2: *perceived* prospects of experimental confirmation is *not a useful scientific criteria* for establishing **what nature actually does**

- **Neutron** can change into **proton**, emitting **electron** and *elusive neutrino*

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- **Weak force** explains *radioactivity*



Missing energy? Pauli postulates “*a desperate remedy*”

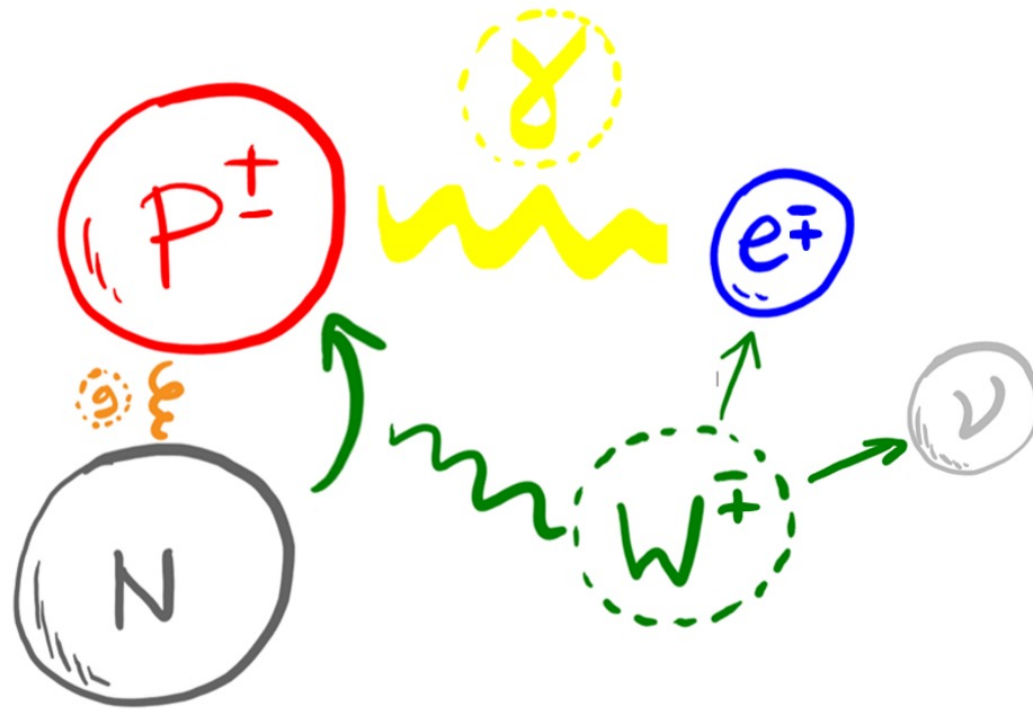
(Bohr suggests fundamental violation of energy conservation principle)

Lesson 2: *perceived* prospects of experimental confirmation is *not a useful scientific criteria* for establishing **what nature actually does**

- **Neutron** can change into **proton**, emitting **electron** and *elusive neutrino*

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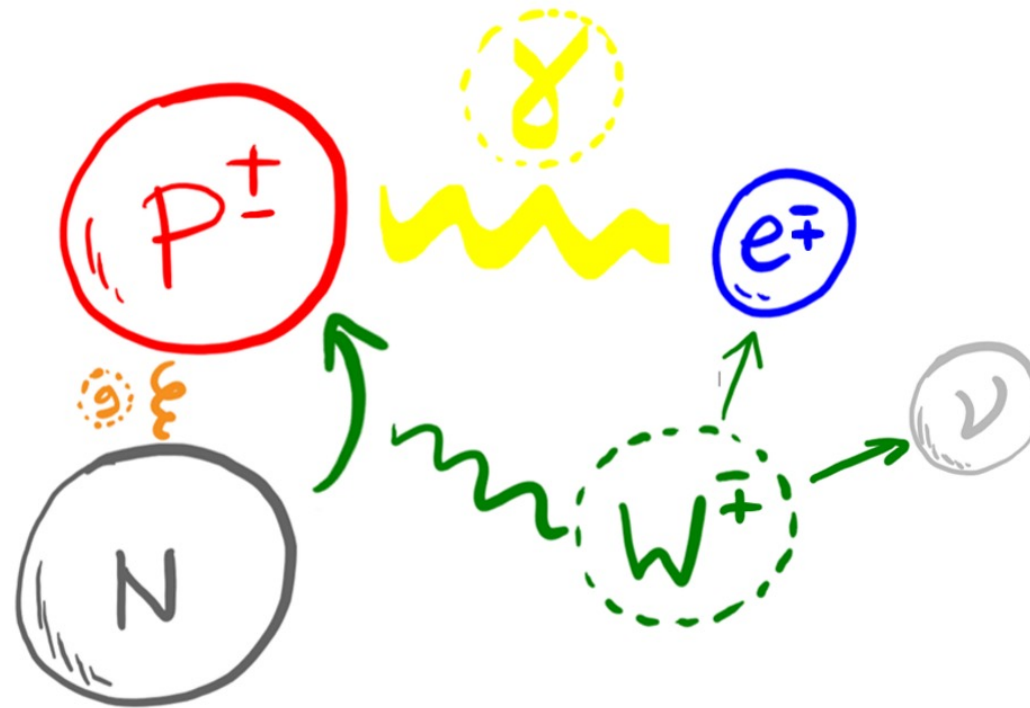
- Dirac: Einstein's **relativity** + **quantum mechanics** = **antiparticles**



- *Every particle has an oppositely charged antiparticle partner*

Introduction

- Dirac: Einstein's **relativity** + **quantum mechanics** = **antiparticles**

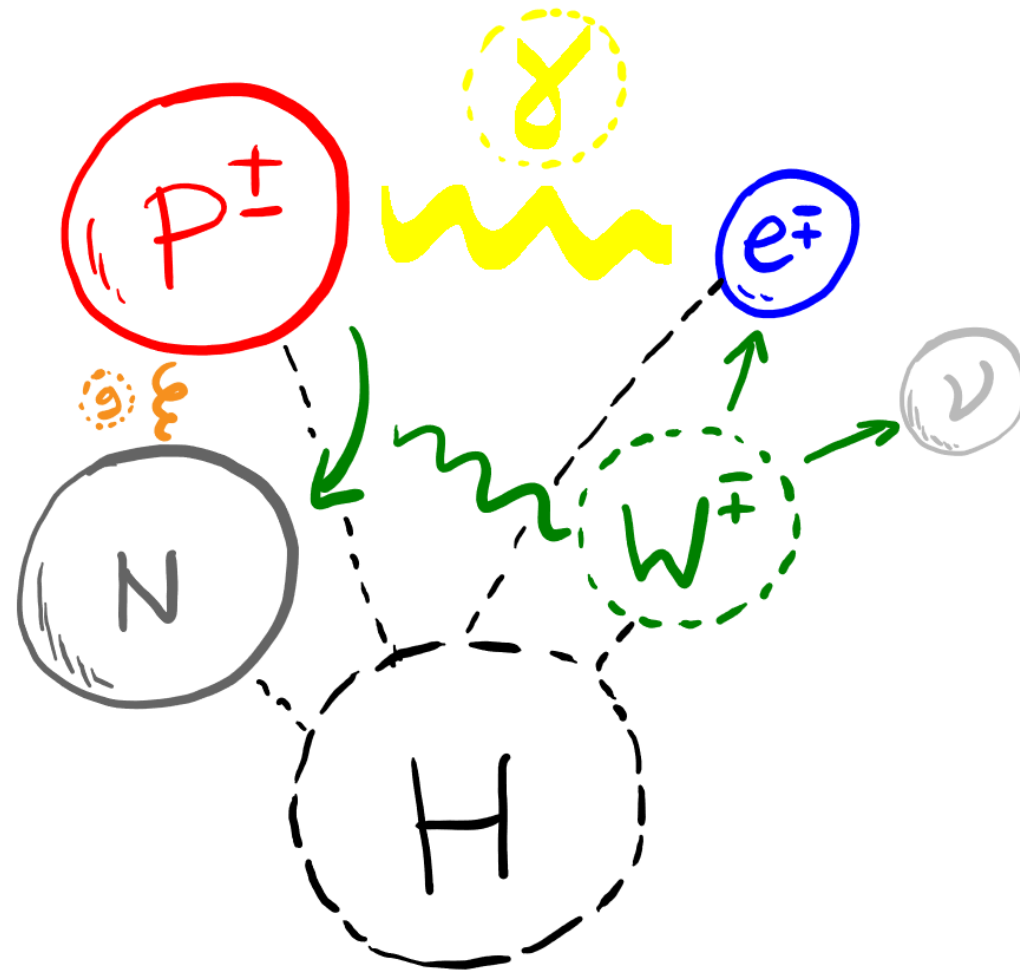


c.f. Lesson 1: antiparticles double the particle spectrum. Nevertheless, the theory is much tighter, less arbitrary, and more elegant

- *Every particle has an oppositely charged antiparticle partner*

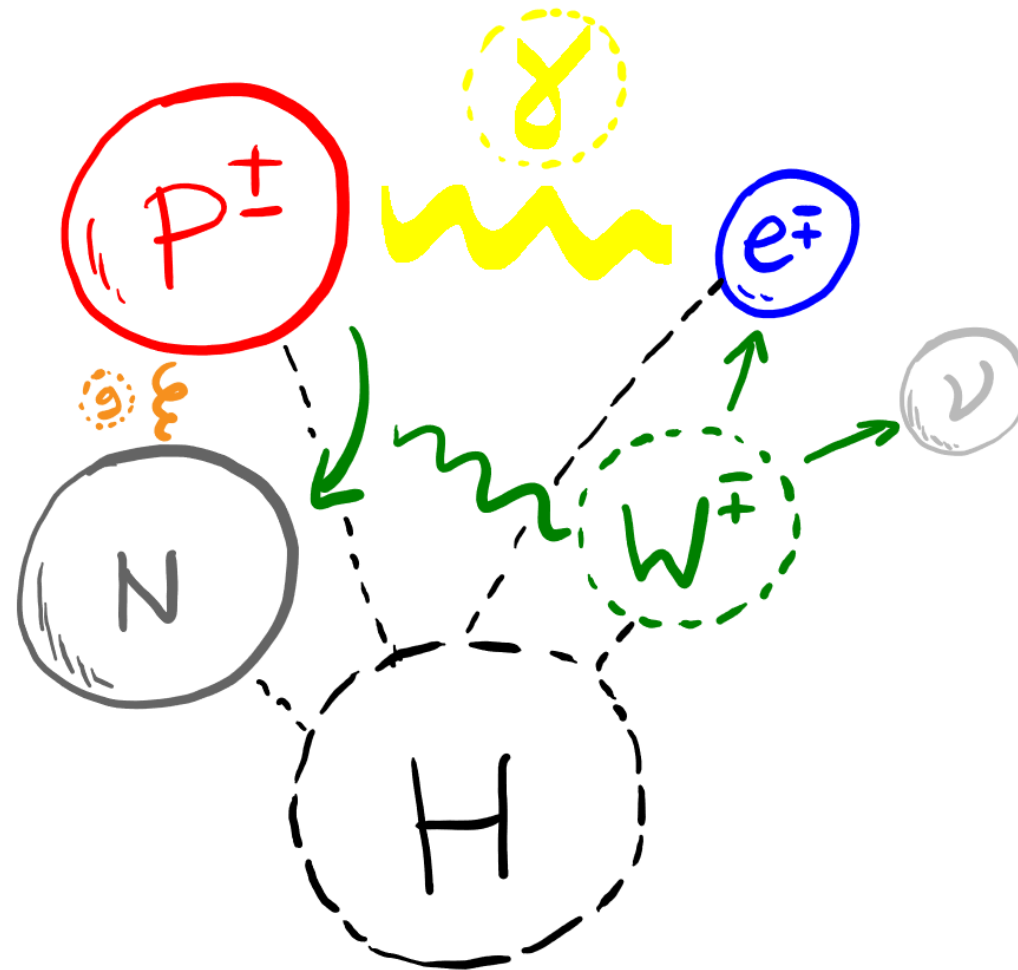
Introduction

- *Higgs(+Brout+Englert)*: **particle masses** require a new **scalar boson H**



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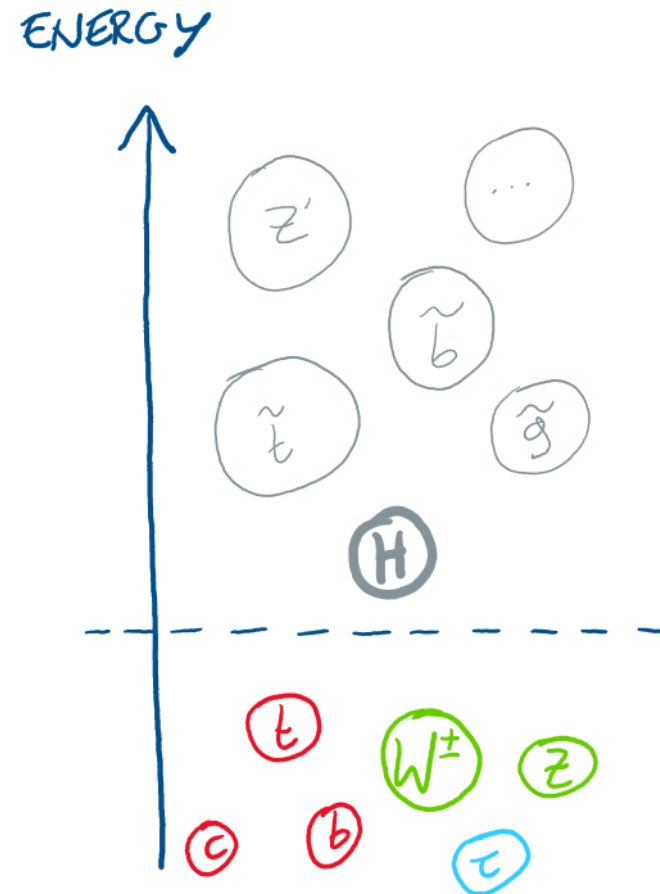
Lesson 3: Keep an open mind. Ideas initially dismissed as **unrealistic** (e.g. non-abelian gauge theories and spontaneous symmetry breaking, because they predicted **unobserved massless bosons**) can *click together suddenly and make sense*

Introduction

- 1930-40s: Success of QED. **QFT** emerges as the *new fundamental description of Nature*.
- 1960s: QFT is **unfashionable**, non-Abelian theory dismissed as an **unrealistic generalisation** of local symmetry-based forces. Widely believed a **radically new framework** will be required *e.g. to understand the strong force*.
- 1970s: **QFT triumphs** following Yang-Mills+Higgs+asymptotic freedom+renormalisation. Nature is **radically conservative**, *but more unified than ever*.
- 1980s: Success of SM. QFT understood as **most general EFT consistent with symmetry**. *Higgs* (and cosmological constant) *violates symmetry expectation*.
- **Tremendous progress** since, *despite lack of BSM*

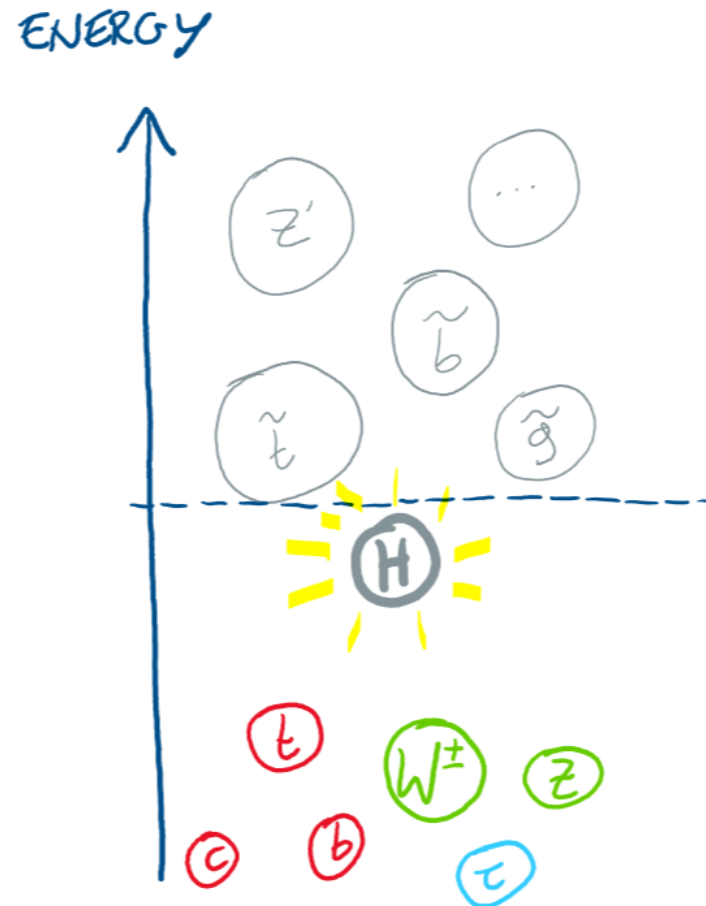
Motivation

- Until now, there had been a **clear roadmap**



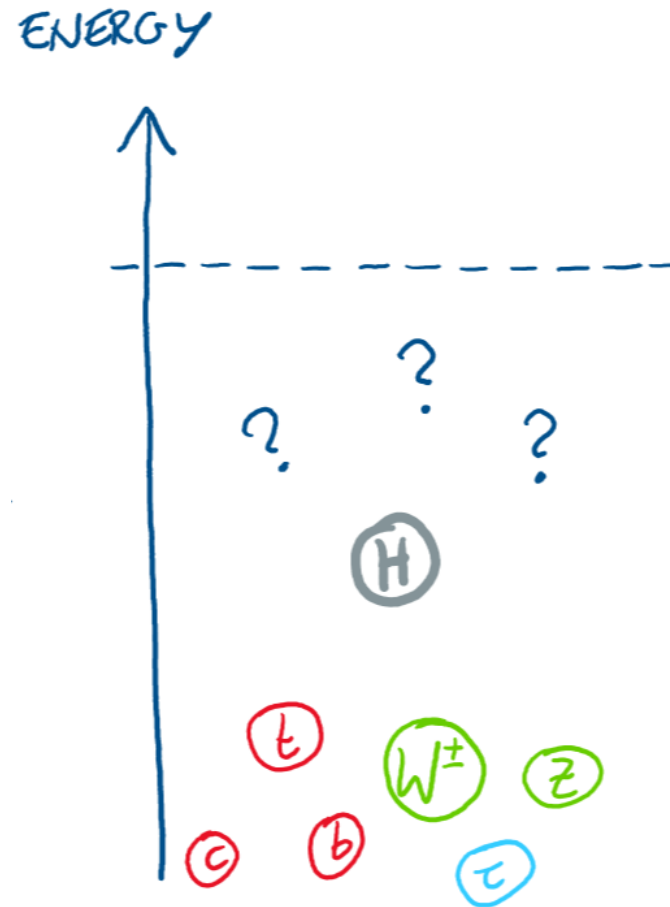
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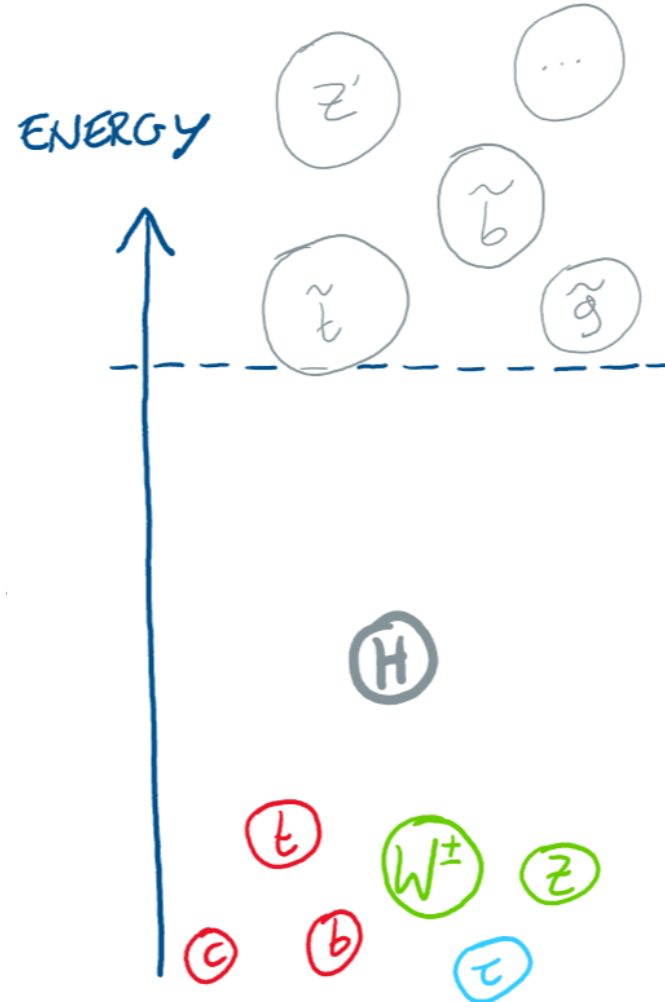
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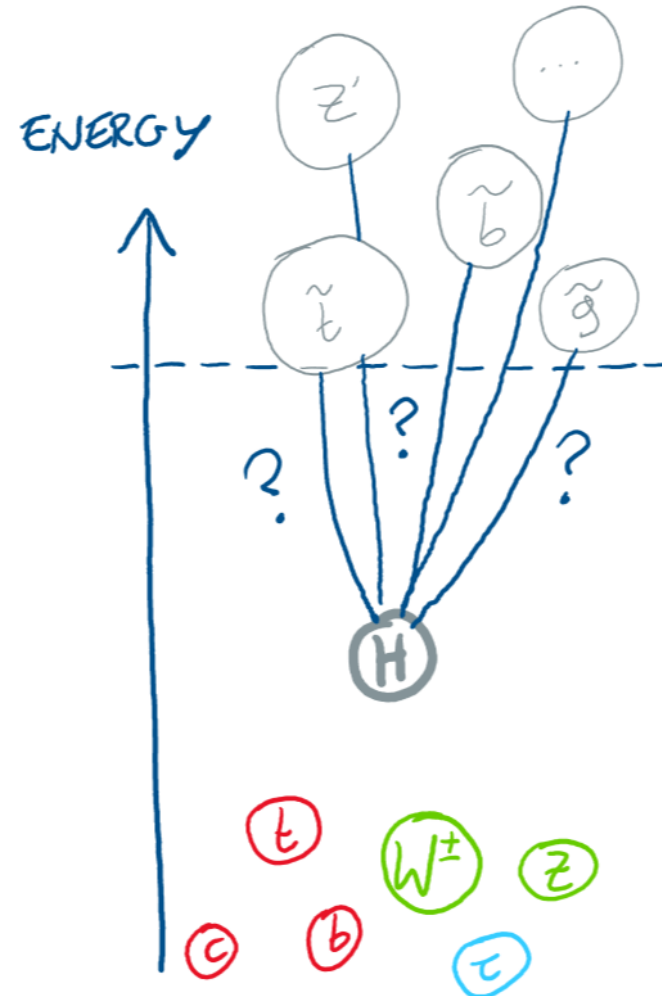
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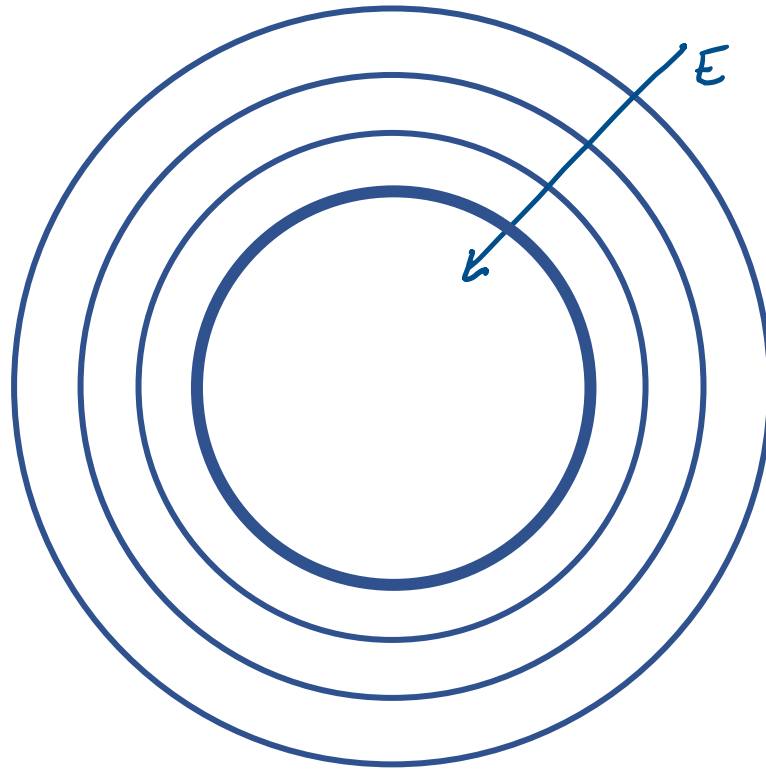
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Naturalness is still a fundamental problem

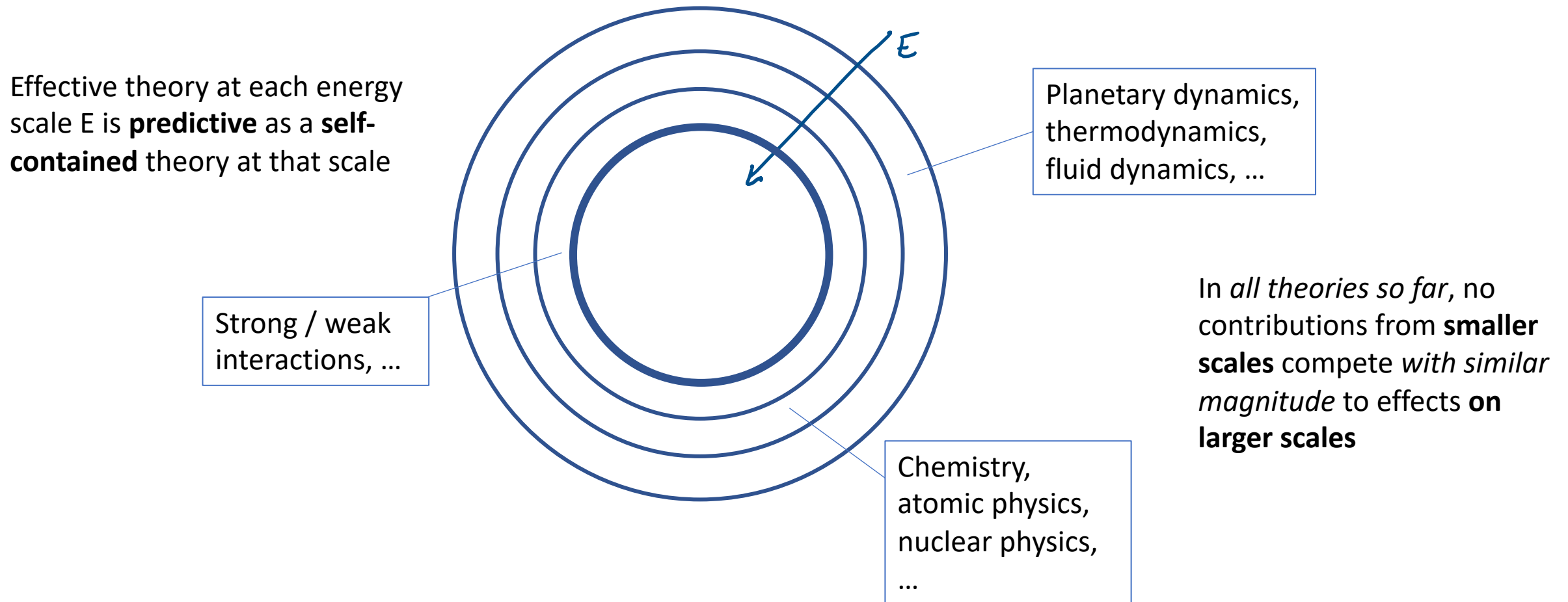
- *Why is unnatural fine-tuning such a big deal?*

Effective theory at each energy scale E is **predictive** as a **self-contained** theory at that scale



Naturalness is still a fundamental problem

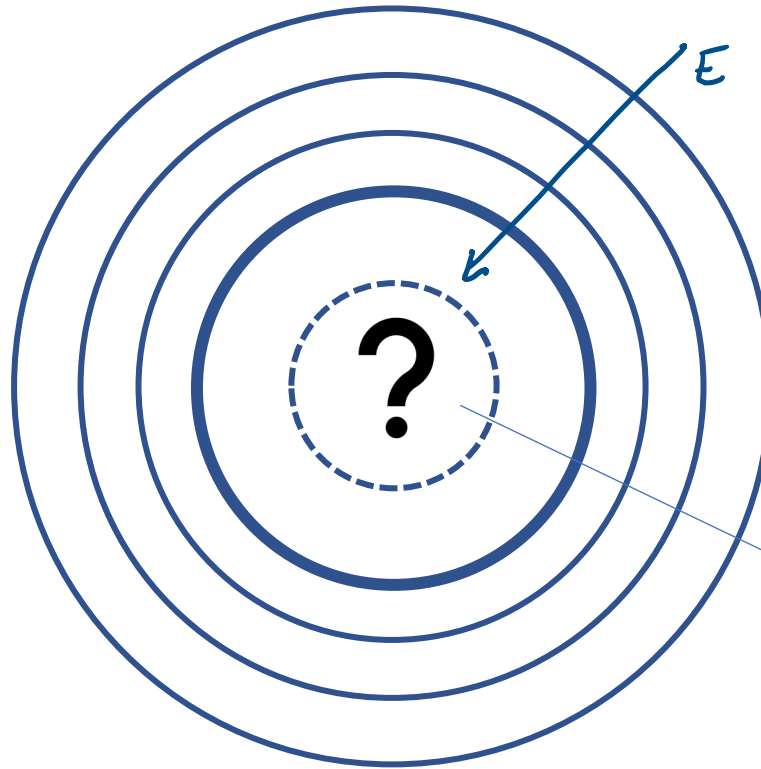
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Naturalness is still a fundamental problem

- *Why is unnatural fine-tuning such a big deal?*
- Indicates *an unprecedented breakdown* of the **effective theory** structure of nature

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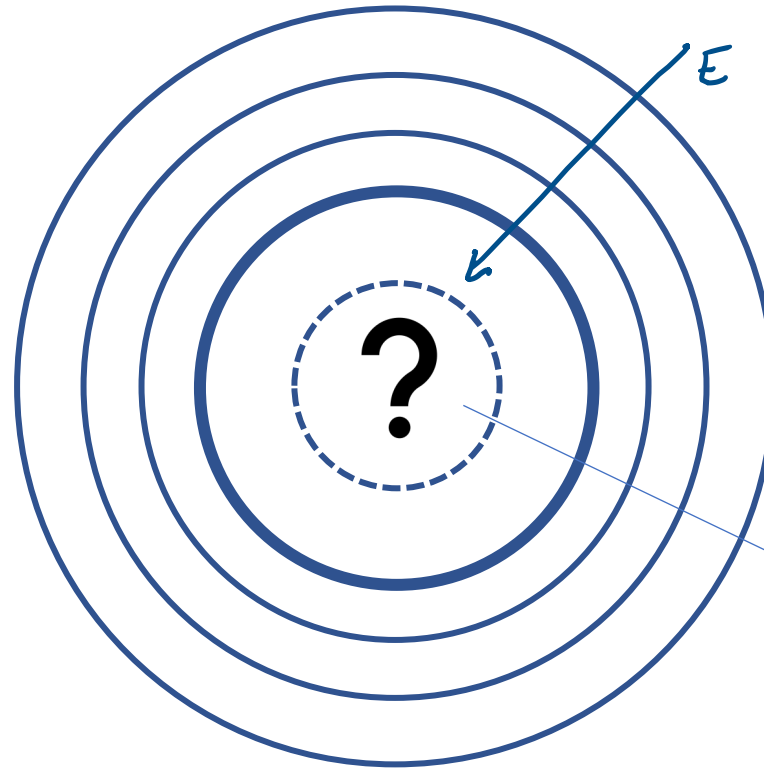


Unnatural Higgs means the next layer *is no longer predictive* without including contributions *from much smaller scales*

Naturalness is still a fundamental problem

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Unnatural Higgs means the next layer *is no longer predictive* without including contributions *from much smaller scales*

- Are we missing a **fundamentally new** “*post-naturalness*” principle? c.f. null results in search for aether



- Telescopes are observatories of the very large
- Colliders observe the very small
- We need *all eyes open on all scales* in our universe



“Discovery prospects” → “Exploring origins”

- What is the **purpose** of a **next-generation particle observatory**?

To explore the fundamental origins of our universe and its laws

- **Exploring**, not searching
 - “*Exploring the origins of our universe*” is a more accurate **mission statement**, unlike e.g. “*searching for supersymmetry and dark matter*”
 - “*Exploring the origin of the Higgs*” simpler to convey than **naturalness**
- “*Discovery stories*” risks putting the focus on *promising* to **find new physics**
- “*Exploring origins*” puts the focus on **open BSM questions** to be answered
 - Emphasises colliders as a **general purpose particle observatory** with a *wide-ranging physics programme*, rather than just e.g. a search for supersymmetry

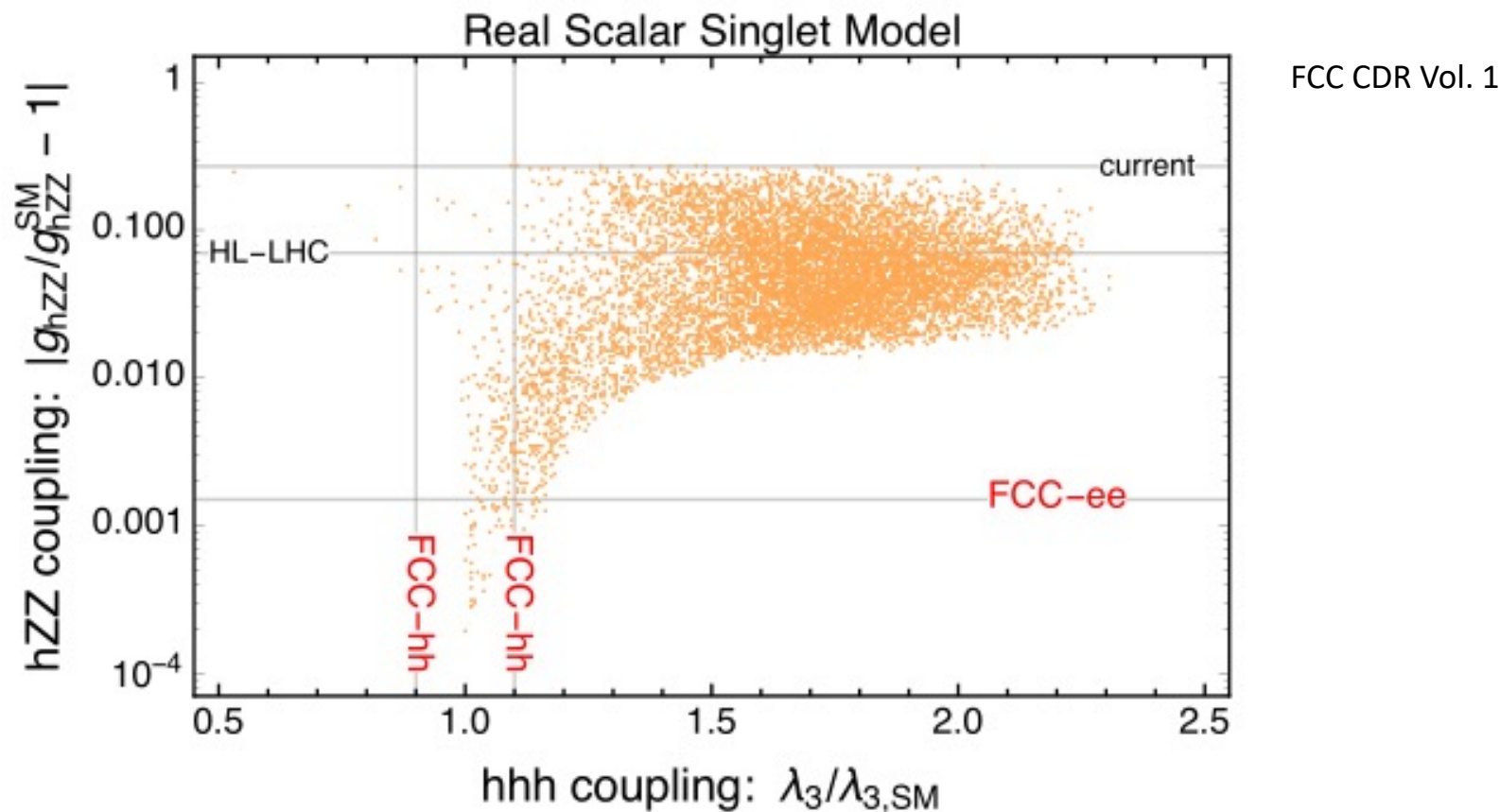
(My proposal: rename FCC to the **International Particle Observatory**)

FCC as an origins explorer

- **Origin of matter**
 - EW phase transition, CP violation, baryogenesis, etc.
 - **Origin of the Higgs**
 - BSM in post-naturalness era, supersymmetry, compositeness, etc.
 - **Origin of flavour**
 - BSM flavour models
 - **Origin of dark matter**
 - Including dark sectors more generally
 - **Origin of neutrinos**
 - BSM neutrino models, neutrino portal, etc.
 - **Origin of the Standard Model**
 - SM is ultimately an EFT of an underlying UV theory that it originates from
- etc.

Origin of matter

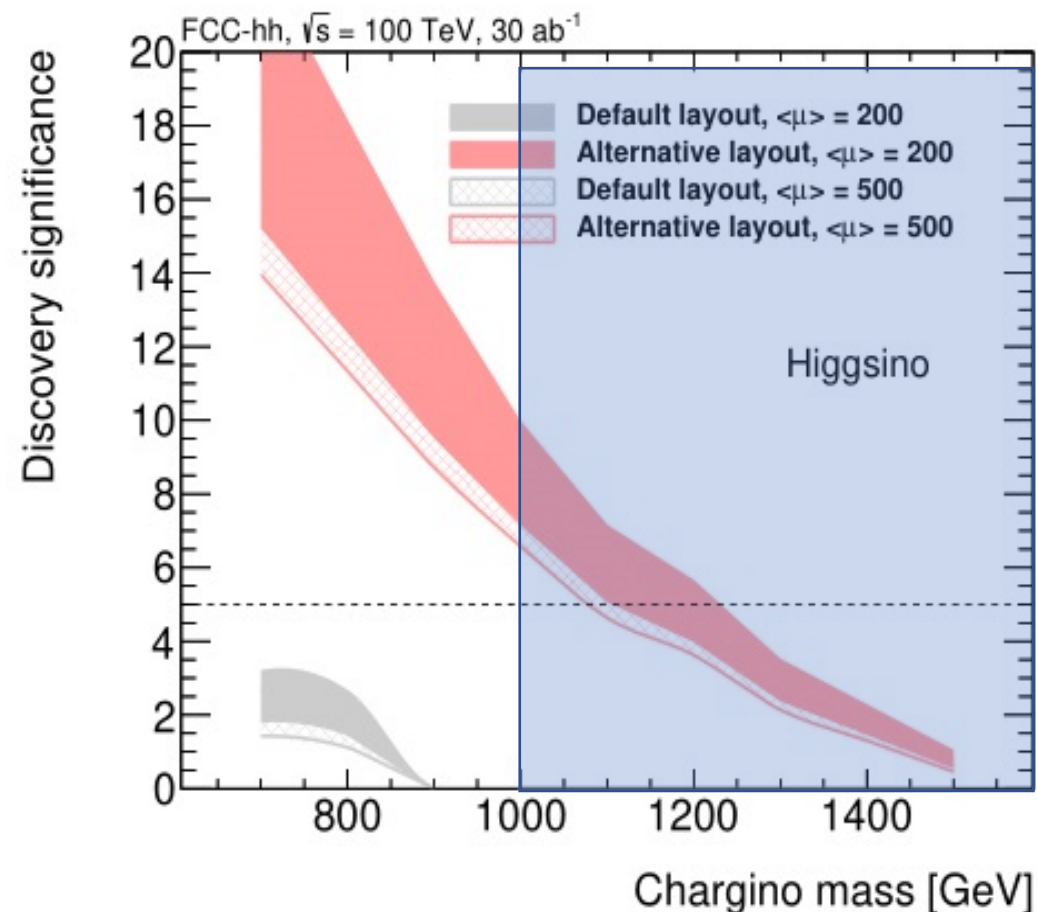
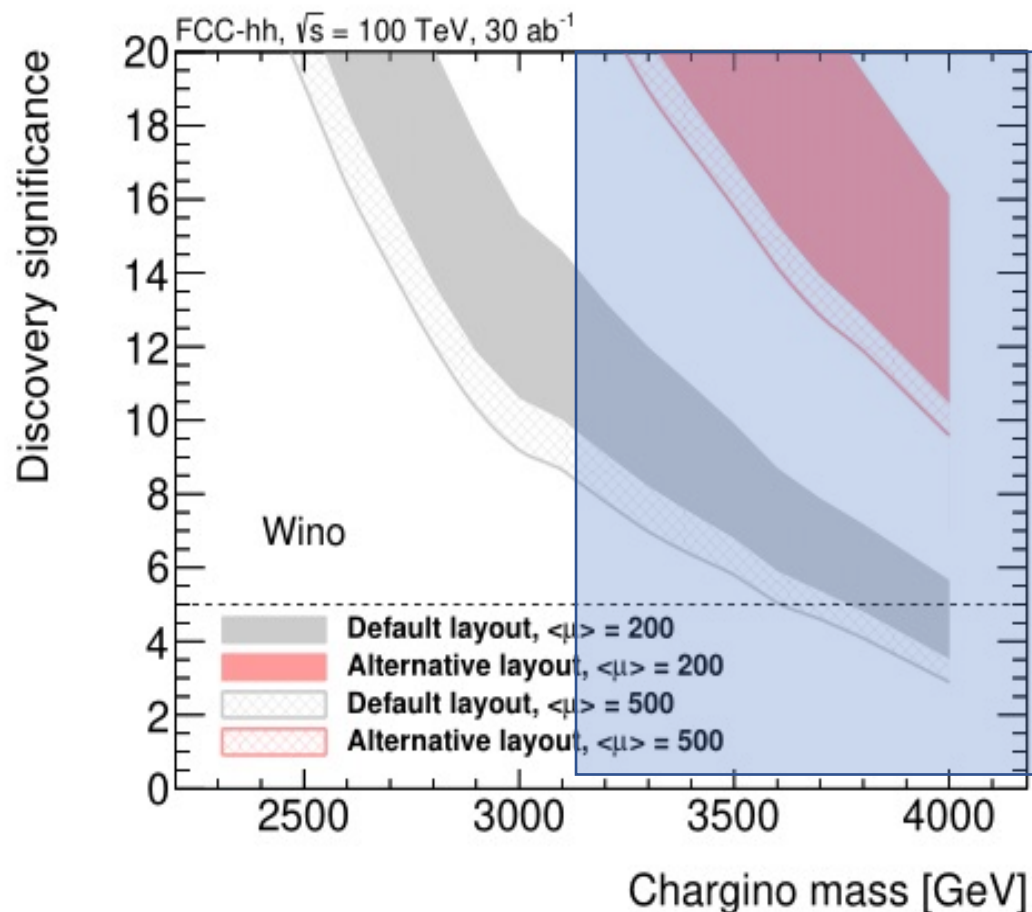
- Nature of the **electroweak phase transition**: *first or second order?*



- *Potential corroboration* with **gravitational wave signal** at LISA

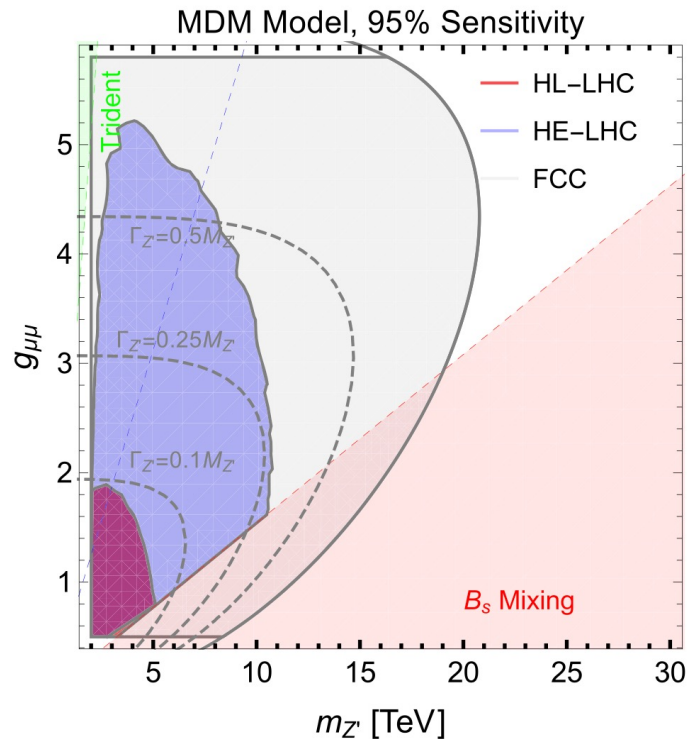
Origin of dark matter

- Coverage of *entire* **doublet** and **triplet thermal WIMP** mass range

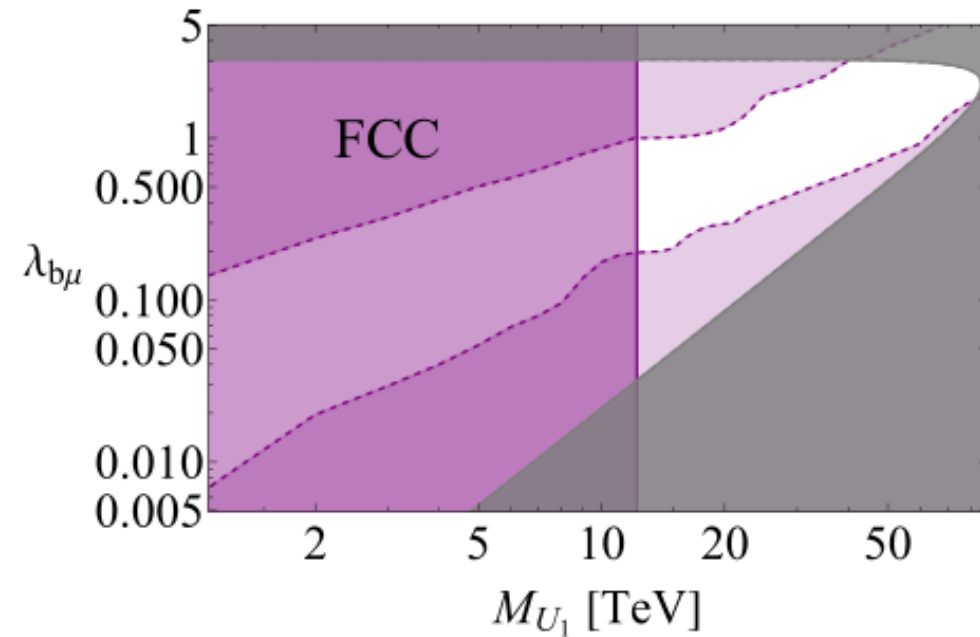


Origin of Flavour

- B anomalies may be going away, but flavour still one of **the most sensitive probes of new physics** (*which may or may not be related to the origin of flavour*)



Allanach, Corbett, Dolan, You [1810.02166]



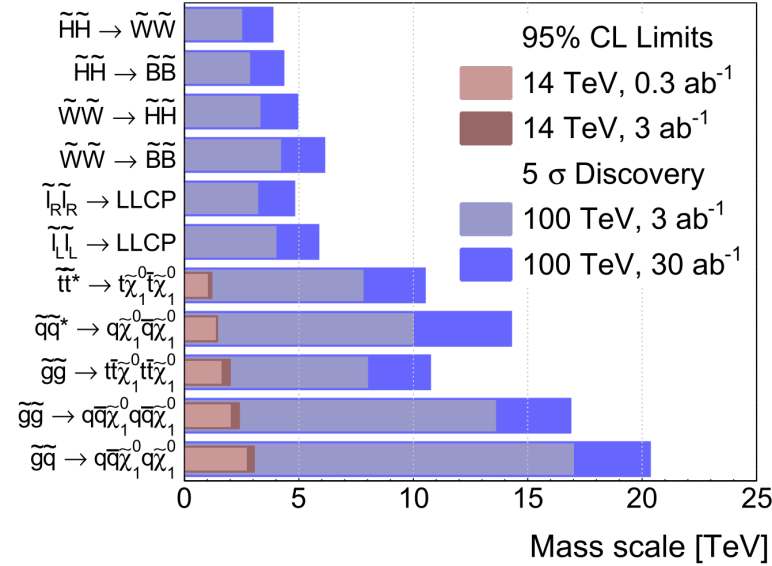
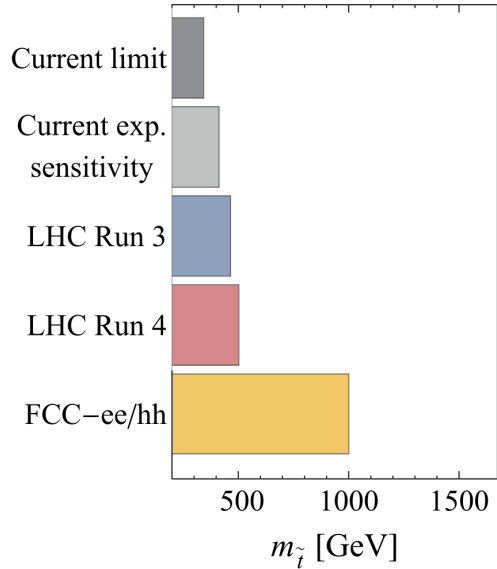
Azatov et al [2205.13552]

- Shows FCC-hh can probe **directly** most of the range that flavour physics is indirectly sensitive to

Origin of the Higgs

FCC CDR Vol. 1

Note: naturalness aside, still motivation in exploring origin of Higgs in models from which it emerges, where its mass is *calculable*

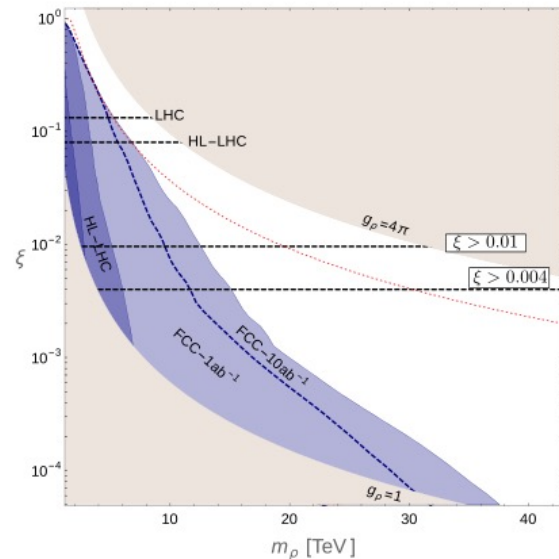
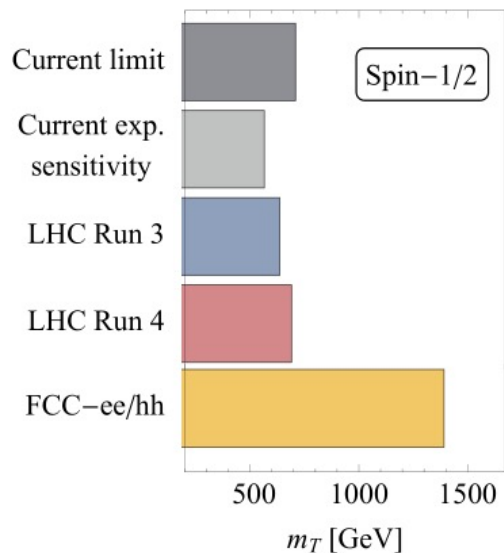


• Supersymmetry

- Massless spins 0, $\frac{1}{2}$, 1, $\frac{3}{2}$, 2 *only*
- Spin $\frac{3}{2}$ *must* be supersymmetric
- (Ir)relevant for solving **naturalness**?

• Composite Higgs / extra dimensions

- Is the Higgs **elementary** or **composite**?
- Are there *accessible* extra dimensions?



Understanding the origin of EWSB

- The SM has many *arbitrary* features put in by hand which hint at **underlying structure**
 - *Pattern of Yukawa couplings, CKM*
 - *QCD Theta term*
 - *Neutrino mass*
 - *Higgs potential*
 - ...
- Maybe it just is what it is $_ _ (_ _) _ / _$
- but we would like a **deeper understanding** i.e. an *explanation* for why things are the way they are
 - *e.g. PQ axion for Theta term, see-saw for neutrino mass, Froggatt-Nielsen for Yukawas...*
- In SM, **no understanding** of Higgs sector: Higgs potential and couplings *put in by hand and unexplained*
- We feel there must be some underlying system that **explains the origin of EWSB**
- In any such theory *in which the Higgs potential is calculable*, there is a **UV sensitivity** to the Higgs mass (*that is no longer a free parameter*) which requires fine-tuned cancellations
- Unlike solutions to other arbitrary features, this one points to **weak-scale new physics**

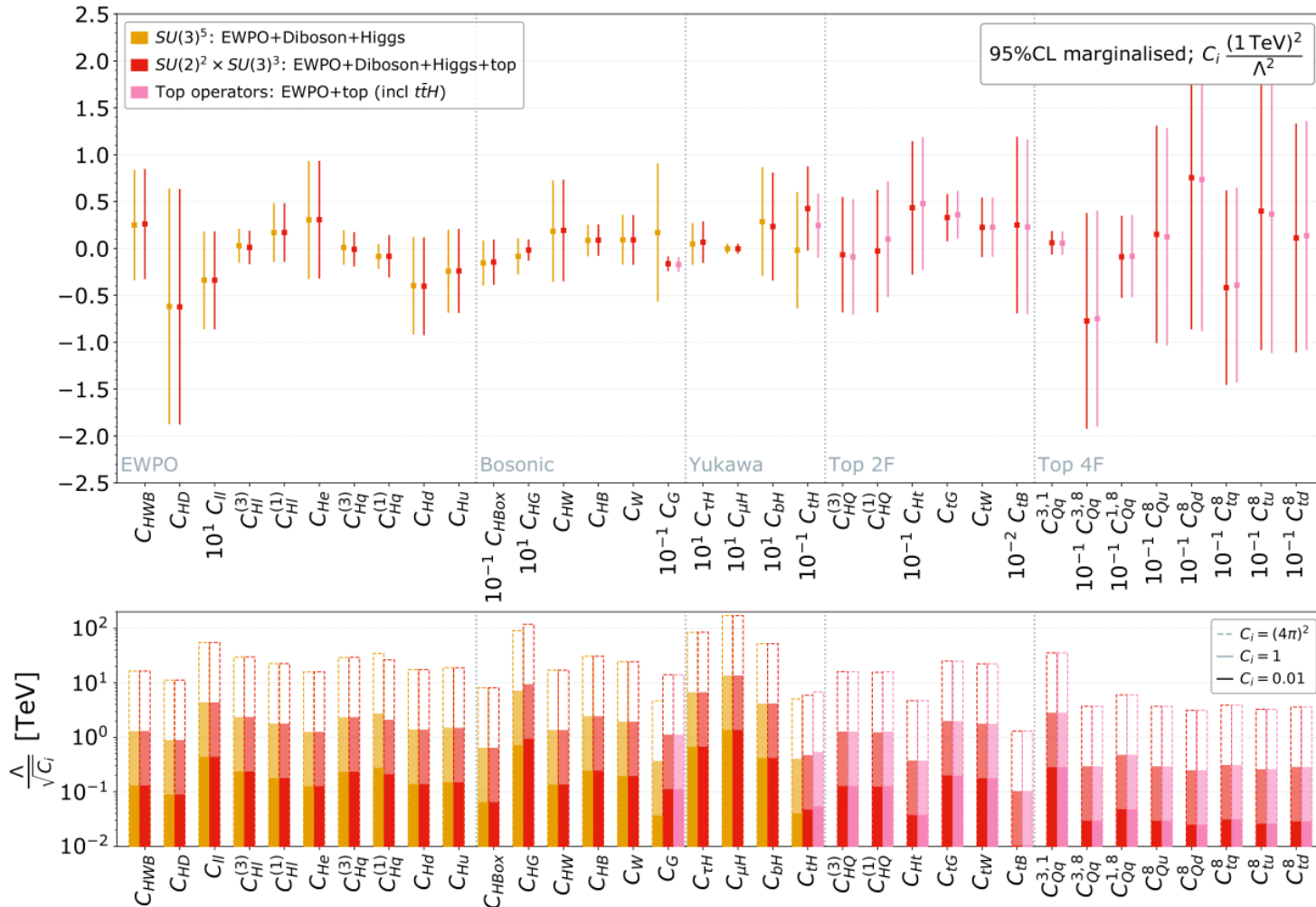
Origin of the Standard Model

- The SM *is* an **Effective Field Theory** (EFT)
- **Indirect hints** typically *precede* direct discovery
- **Global SMEFT fits** will play a crucial role
- Higher-dimensional operator coefficients **encodes BSM information**

$$\mathcal{L}_{SM} = \mathcal{L}_m + \mathcal{L}_g + \mathcal{L}_h + \mathcal{L}_y + \frac{c_5}{\Lambda} \mathcal{O}^{(5)} + \frac{c_6}{\Lambda^2} \mathcal{O}^{(6)} + \frac{c_7}{\Lambda^3} \mathcal{O}^{(7)} + \frac{c_8}{\Lambda^4} \mathcal{O}^{(8)} + \dots$$

Origin of the Standard Model

- **SMEFT** phenomenological framework is the *Fermi theory of the 21st century*



- What are the experimental constraints on the **energy scale** of new physics, Λ ?
- What are the experimental constraints on their **interaction strengths**, c_i ?

J. Ellis, Madigan, Mimasu, Sanz, TY [2012.02779]

- e.g. Combined global fit to **Top, Higgs, diboson, and electroweak** experimental data

Origin of the Standard Model

- FCC-ee is **highly motivated** as an ultimate *precision particle observatory*:
 - Quantum leap in Higgs+electroweak physics
 - Huge increase in indirect sensitivity to high energy scales
 - Exploration of dark sectors
 - Also a flavour factory

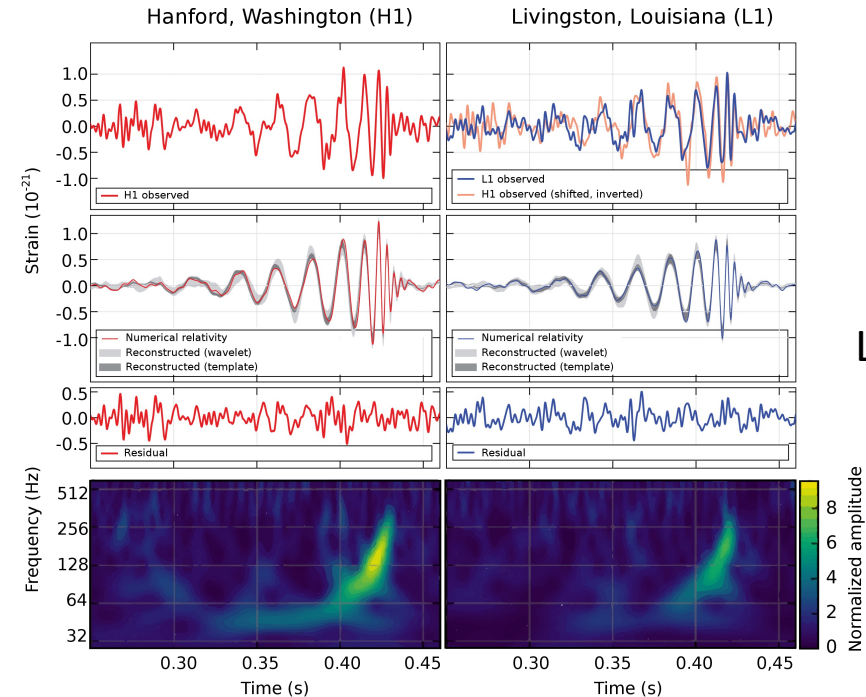
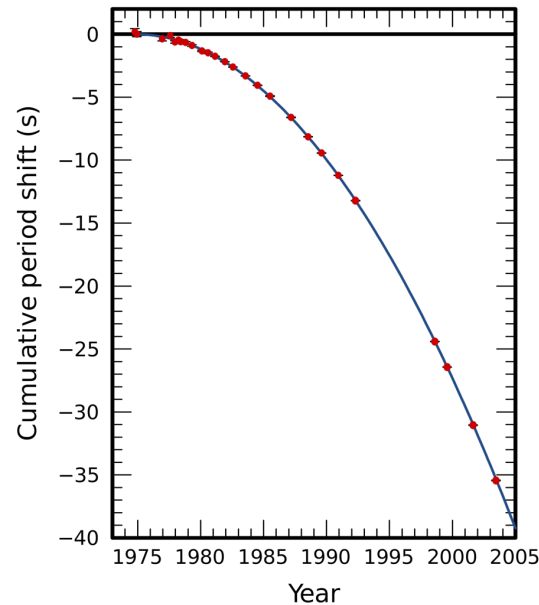
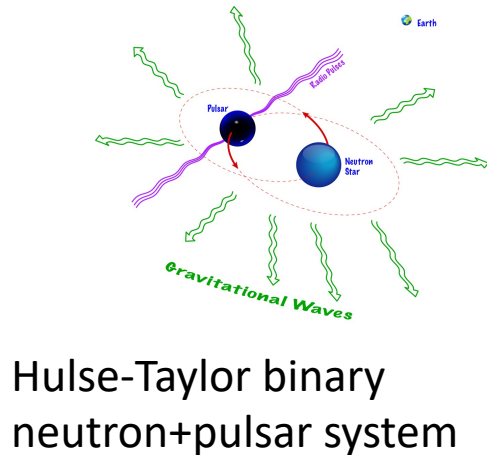
Table S.4: Expected production yields for b-flavoured particles at FCC-ee at the Z run, and at Belle II (50 ab^{-1}) for comparison.

| particle production (10^9) | B^0/\bar{B}^0 | B^+/B^- | B_s^0/\bar{B}_s^0 | $\Lambda_b/\bar{\Lambda}_b$ | $c\bar{c}$ | $\tau^+\tau^-$ |
|--------------------------------|-----------------|-----------|---------------------|-----------------------------|------------|----------------|
| Belle II | 27.5 | 27.5 | n/a | n/a | 65 | 45 |
| FCC-ee | 1000 | 1000 | 250 | 250 | 550 | 170 |

- FCC-ee **ensures next-next-generation FCC-hh!**
- **Physics case is *complementary***

Why high energy after high precision?

- Follow up **indirect observations** with **direct exploration**

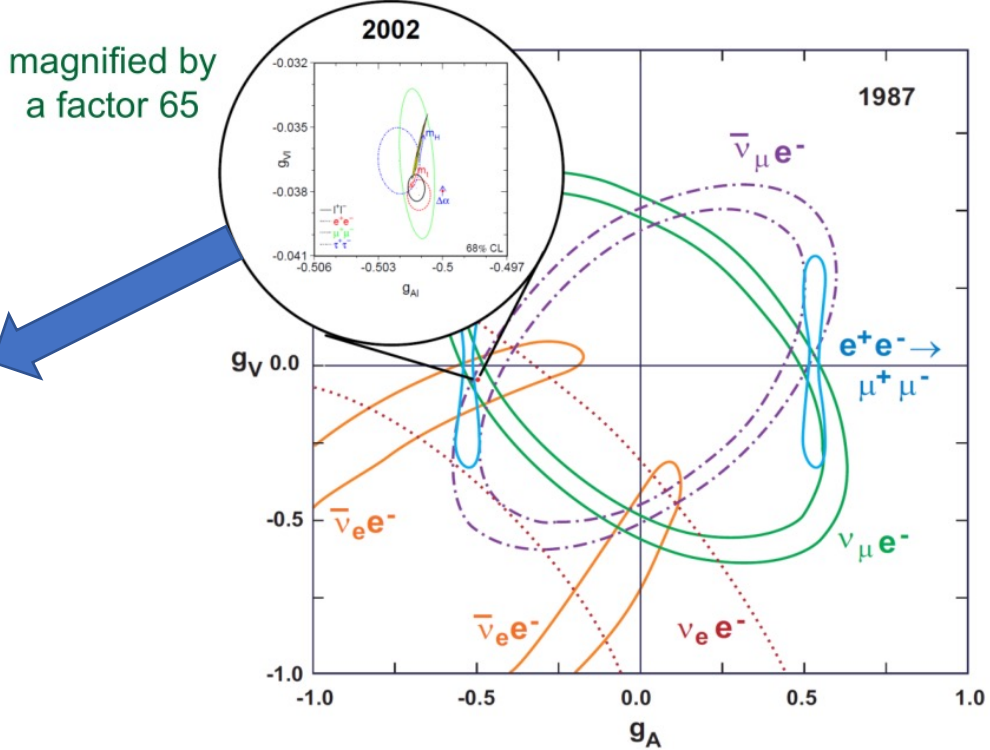
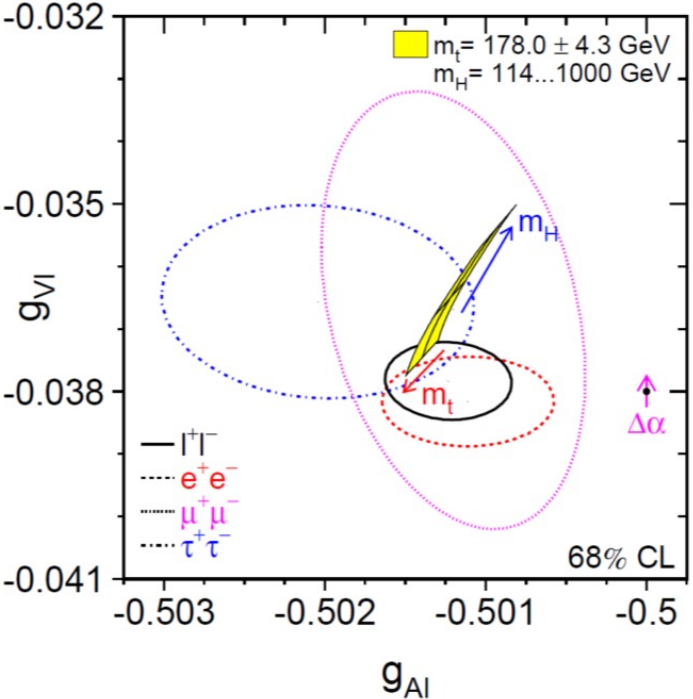


LIGO+VIRGO

- **Note:** in astro/cosmo, observing *known* objects and processes **in new regimes** or to **better accuracy** is reason enough to keep making progress!

No BSM or new discoveries at LEP

- 1980-1990s: LEP physics programme a **resounding success**
- Improved our fundamental picture of nature *by orders of magnitude*

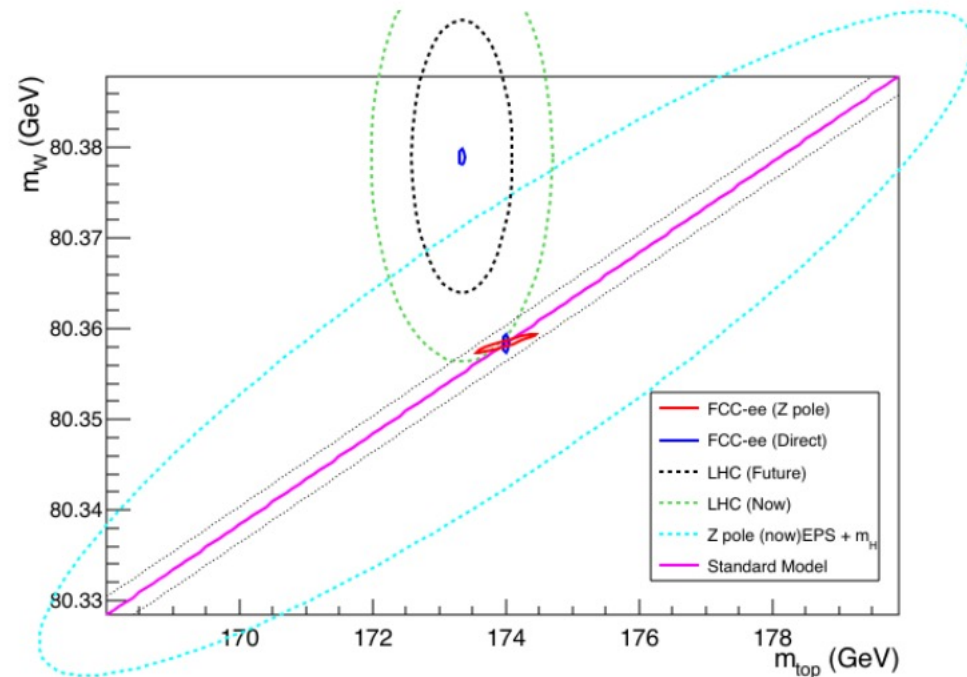


Guy Wilkinson slide

- *Indirect precision probe* of physics at **higher energies**

No BSM or new discoveries at FCC-ee?

- Further **zooming in** on our fundamental picture of nature



- **Rich physics programme** covering Higgs, top, electroweak, multi-bosons, flavour, rare decays, neutrinos, QCD, heavy ions *and more*.

No guarantee of new discoveries at FCC-hh?

- **Note:** *GAIA, JWST or LIGO did not promise to discover exotic new physics or break GR*
- **No guarantee of discovery** at Tevatron either. Hadron collisions thought by some to be too messy to do physics.
- **Value in pushing frontiers:** *we learn something regardless of outcome*
- **Definite questions** are answered, *even if in the negative*
- Science is about *continually refining existing knowledge and exploring the unknown*
- **A new generation** of data management, analysis techniques, improved measurements, theoretical calculational tools, hardware development, cutting-edge engineering, large international collaboration, popular culture inspiration, and spirit of fundamental exploration, **can only benefit humanity** regardless of our own short-sighted disappointment at lack of BSM. *Doing good science is its own reward.*

Potential BSM outcomes for naturalness

- **Radically conservative:** naturalness restored just around the corner
 - Natural supersymmetry
 - Composite Higgs/extra dimensions
- **Creatively conservative**
 - Twin Higgs
 - Stealth supersymmetry
- **Post-naturalness BSM**
 - Split supersymmetry
 - Vector-like fermions only
 - Lowered vacuum instability scale
 - Weak-scale new physics for cosmological dynamics
- **Radically new?**
 - Hard to imagine what form this might take, by definition
 - How might this show up?

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“Radically conservative” historical precedent

- 1930-40s: Success of QED. **QFT** emerges as the *new fundamental description of Nature*.
- 1960s: QFT is **unfashionable**, non-Abelian theory dismissed as an **unrealistic generalisation** of local symmetry-based forces. Widely believed a **radically new framework** will be required *e.g. to understand the strong force*.
- 1970s: **QFT triumphs** following Yang-Mills+Higgs+asymptotic freedom+renormalisation. Nature is **radically conservative**, *but more unified than ever*.
- 1980s: Success of SM. QFT understood as **most general EFT consistent with symmetry**. Higgs and cosmological constant *violate this symmetry principle*.

“Radically conservative” naturalness solution at FCC?

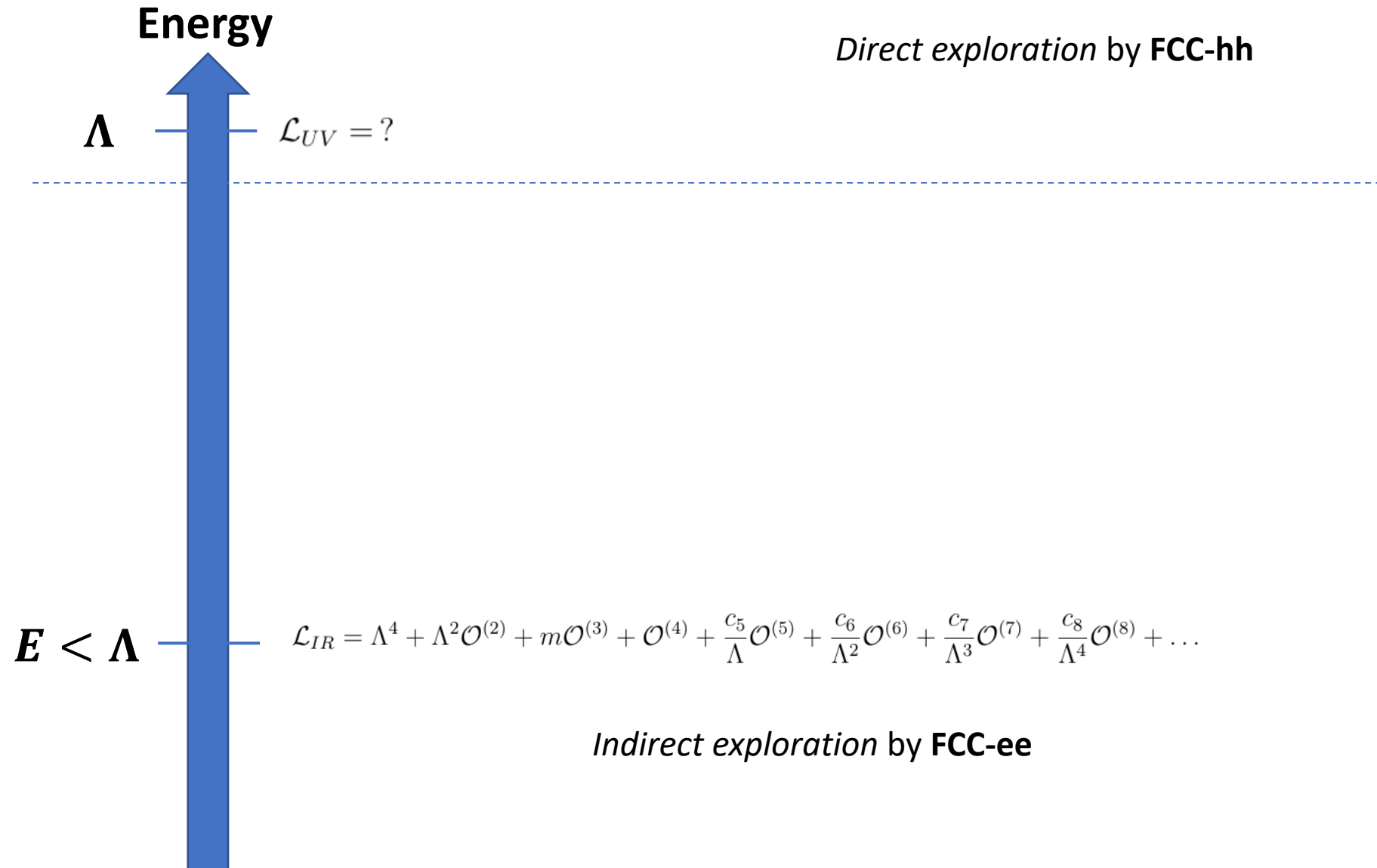
- 1980-2020s: Success of SM, established as the *fundamental description of Nature up to TeV scale*.
- 2040s: QFT is **unfashionable**, supersymmetry theory dismissed as an **unrealistic generalisation** of symmetry principles. Widely believed a **radically new framework** will be required *e.g. to understand naturalness*.
- 2060s: **QFT triumphs** following Yang-Mills+Higgs+asymptotic freedom+renormalisation+**supersymmetry**. Nature is **radically conservative**, *but more unified than ever*.
- 2080s: Success of MSSM

(This slightly facetious example is nevertheless one possible scenario)

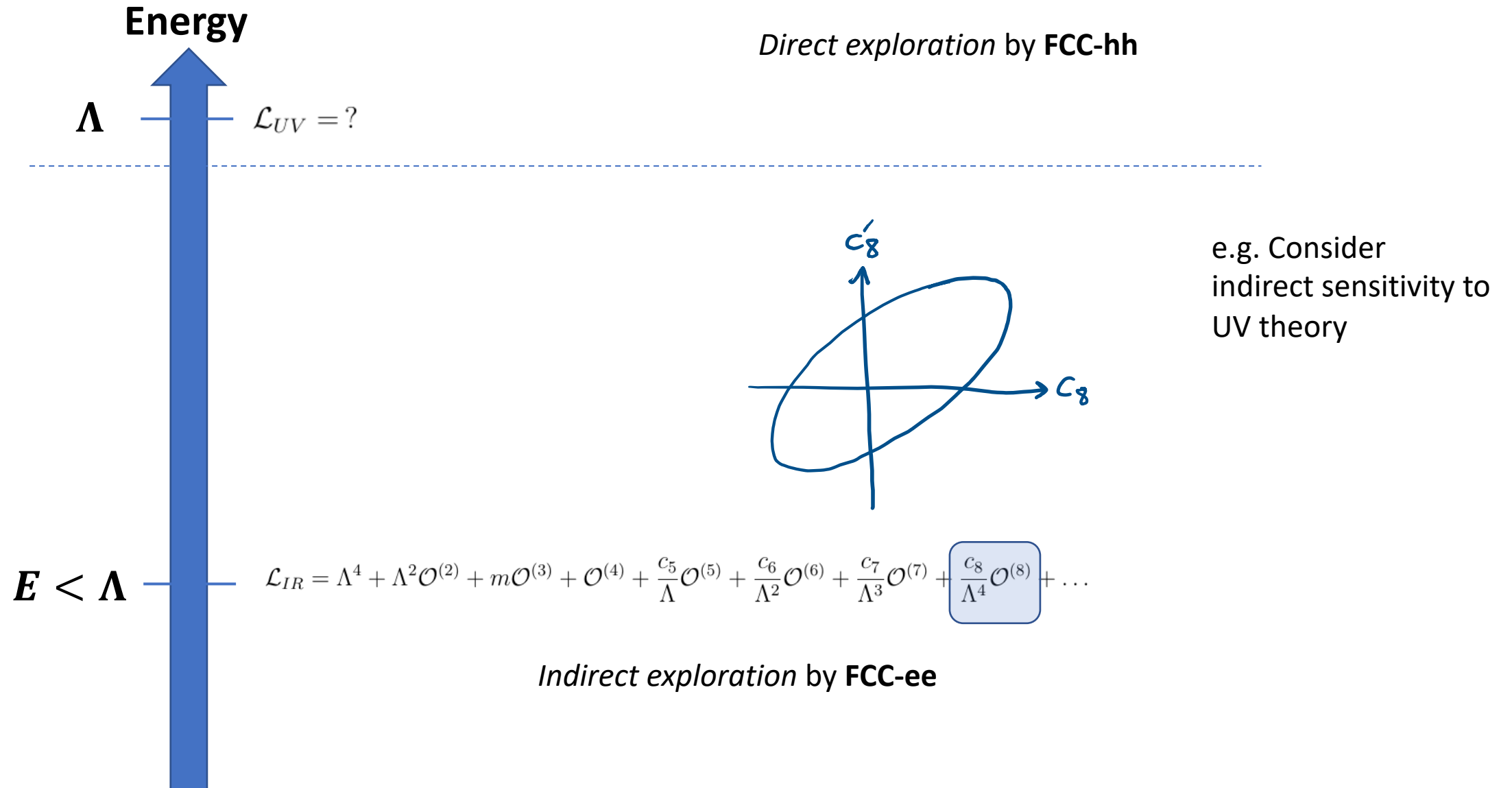
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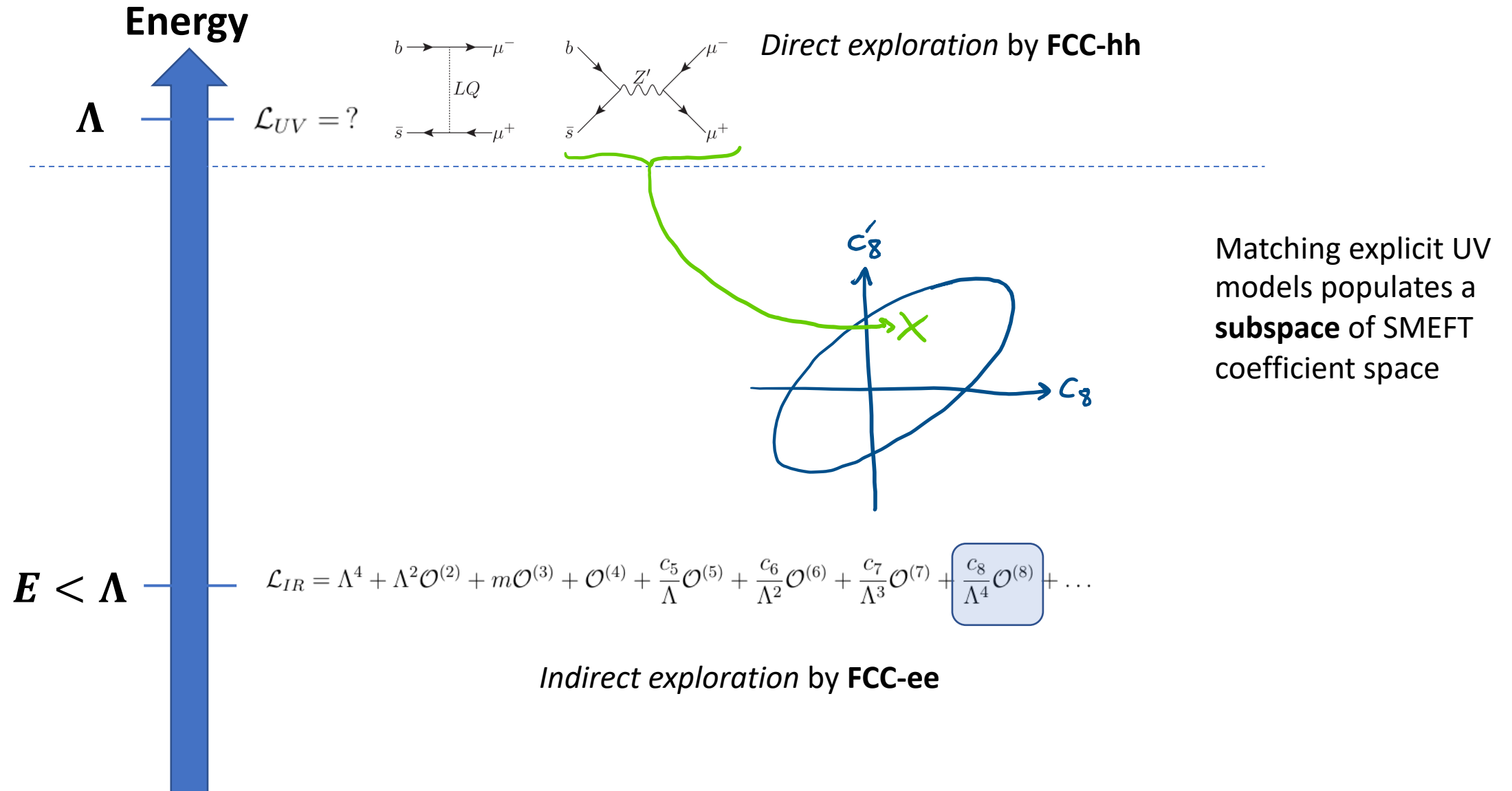
Radically new BSM?



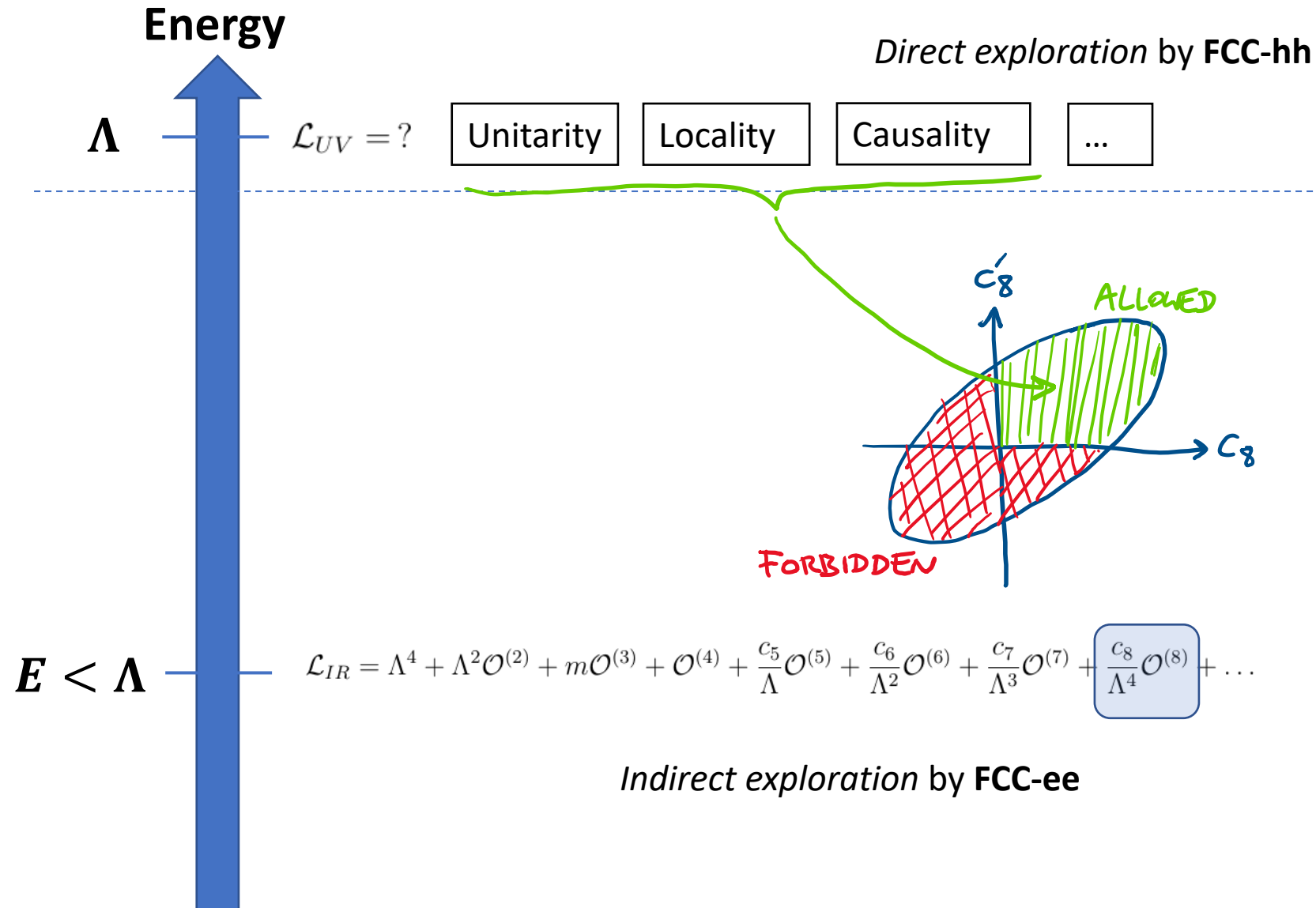
Radically new BSM?



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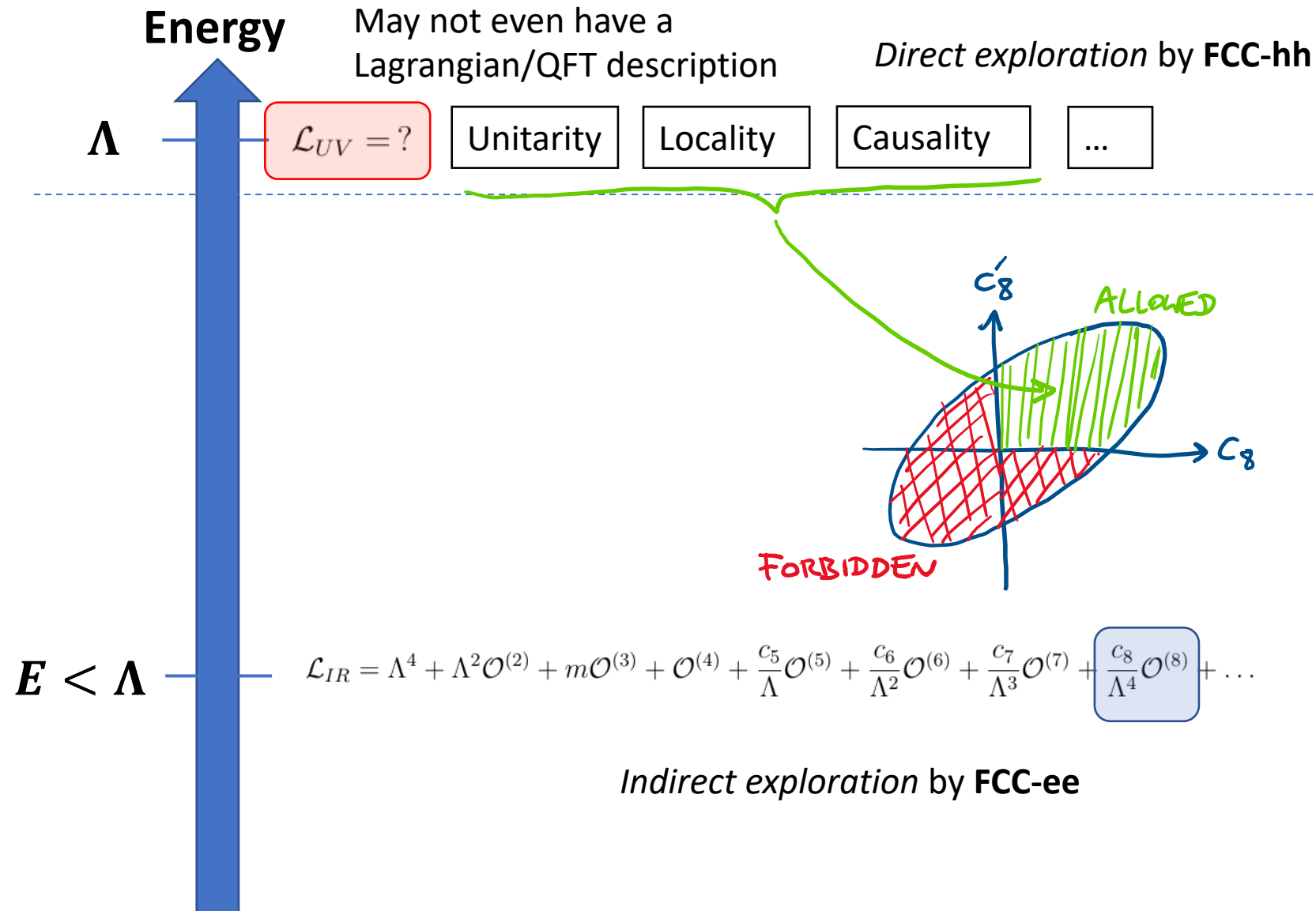
Radically new BSM?



Positivity bounds forbid **negative signs** of SMEFT coefficients assuming only general fundamental principles in the UV

Measuring the “*wrong*” sign experimentally would have **truly revolutionary** consequences for the underlying theory!

Radically new BSM?



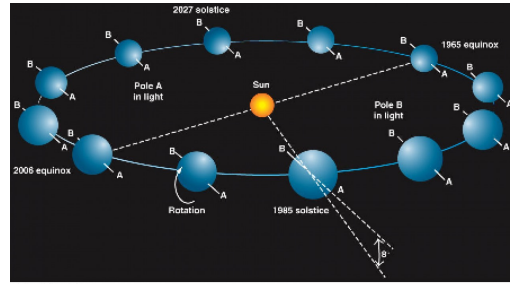
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Radically new BSM?

- Sometimes an anomaly in **indirect precision** measurement = *something missing*

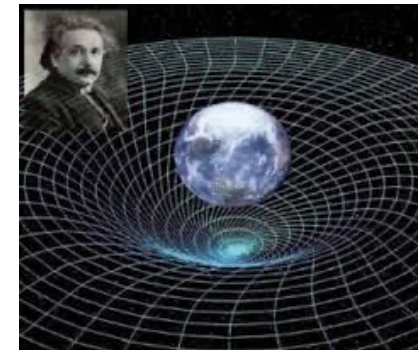
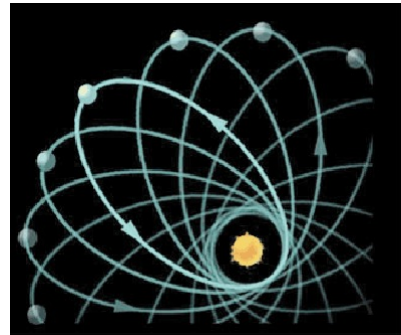
Anomaly in orbit of Uranus



Discovery of Neptune

- Sometimes its implications are *far more radical*

Anomaly in orbit of Mercury



Explained by General Relativity

Conclusion

- 1900: Almost all data agree spectacularly with the fundamental framework of the time, *no reason to doubt its universal applicability or completeness.*
- 1920s: A combination of **precision measurements** (Mercury), **aesthetic arguments** (relativity) supported by **null experimental results** (Michelson-Morley), and **theoretical inconsistencies** (Rayleigh-Jeans UV catastrophe) lead to an overhaul of the fundamental picture at **smaller scales** and **higher energies** after *pushing the frontiers of technology and theory into new regimes.*

Conclusion

- 2020: Almost all data agree spectacularly with the fundamental framework of the time, *no reason to doubt its universal applicability or completeness.*
- 2050s: A combination of **precision measurements** (MW, Hubble), **aesthetic arguments** (naturalness) supported by **null experimental results** (LHC), and **theoretical inconsistencies** (black hole information paradox) lead to an overhaul of the fundamental picture at **smaller scales** and **higher energies** after *pushing the frontiers of technology and theory into new regimes.*

Conclusion

- Keep the spirit of **fundamental science and exploration** alive for the next century and *pass the baton on to future generations*
- Going to *even smaller scales* is one of **the most exciting journeys into the unknown** we can make as a species
- We can **only win!**
 - Improve by orders of magnitude our understanding of the universe
 - Give back more than we take to society
 - Stimulates the economy
 - Stimulates popular imagination
 - Stimulates international cooperation
 - Stimulates technology and data science

Backup

Is it too ambitious?

- FCC less ambitious than LEP and LHC was for their time

Is it too expensive?

- No, not relative to other taxpayer-funded big projects
- Olympic games costs \$10-20 billion to a single nation for a summer's entertainment
- FCC-ee+hh costs \$20 billion shared between dozens of countries over decades for improving our fundamental knowledge of the universe

Is it worth it?

- See talk

When do we stop?

- When we lose our spirit of exploration and curiosity
- When we don't learn anything or gains become marginal
- Far from being marginal, the gains are huge
- We just washed ashore upon *terra incognita* and have barely left the beach
- LHC enters threshold of TeV-scale physics that FCC can explore fully

What about climate change?

- 90% of CERN's energy is from non-warming sources
- All activity contributes to climate change; this question implies particle physics is not worth continuing
- Expanding our fundamental knowledge of the smallest scales is as important as many other human endeavours we would not want cancelled completely
- Of course, we should make particle physics as efficient as possible

I won't be alive to see it

- Ensuring particle physics thrives for the rest of the century is more important
- These ambitious multi-generational projects are the cathedrals of our era

Why not skip FCC-ee and do FCC-hh first?

- We can't – technology and cost won't be feasible on that timescale
- FCC-ee is just as exciting and worth doing in its own right

Conclusion



Why should CERN build the Future Circular Collider?




There are several reasons why CERN and the international scientific community should consider building the Future Circular Collider (FCC):

1. The FCC would allow scientists to study particle interactions at much higher energies and with greater precision than is currently possible with the Large Hadron Collider (LHC), which would enable new discoveries and a deeper understanding of the universe.
2. The FCC would allow scientists to study the properties of the Higgs boson and other known particles in more detail, and to search for new particles and phenomena that have not yet been observed.
3. The FCC would also be a major driver of technological innovation, leading to the development of new technologies and techniques in areas such as high-energy physics, superconducting magnets, high-performance computing, and data analysis.
4. Building a Future Circular Collider would keep CERN and Europe at the forefront of high-energy physics research, which is an important area of study that can lead to a deeper understanding of the universe and the nature of matter.
5. Finally, The FCC would also bring significant economic benefits to the host region, through the creation of jobs, the development of new technologies, and the spin-off of these technologies to other areas of industry.

It's worth noting that the decision to build the FCC would depend on a variety of factors such as the potential scientific benefits, the cost and feasibility of the project, and the priorities of the international scientific community and funding agencies.

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The physics motivation for building the Future Circular Collider (FCC) at CERN is to study particle interactions at higher energies and with greater precision than is currently possible with the Large Hadron Collider (LHC), in order to make new discoveries and gain a deeper understanding of the universe. 

1. **The Higgs boson:** The FCC would allow scientists to study the properties of the Higgs boson, a particle that was discovered at the LHC and is crucial to our understanding of how other particles acquire mass. The FCC would be able to produce many more Higgs bosons than the LHC, allowing scientists to study its properties in more detail.
2. **New particles and phenomena:** The FCC would have the capability to search for new particles and phenomena that have not yet been observed, such as dark matter candidates and new physics beyond the standard model.
3. **Heavy Ion Physics:** The FCC would have the capability to produce and study heavy ions, which are atomic nuclei with a large number of protons and neutrons. This would allow scientists to study the properties of nuclear matter under extreme conditions, such as those that existed in the early universe.
4. **Precision measurements:** The FCC would also be able to perform precision measurements on known particles, such as the W and Z bosons, in order to test the standard model of particle physics and search for deviations that could indicate new physics.
5. **High luminosity:** The FCC would have a much higher luminosity than the LHC, which would allow for more precise measurements of rare processes and a greater chance of observing new phenomena.

Overall, the physics motivation for building the FCC is to study particle interactions at higher energies and with greater precision than is currently possible with the LHC, in order to make new discoveries and gain a deeper understanding of the universe.



Should CERN build a future collider?



Whether CERN should build a future collider like the FCC is a matter of ongoing debate within the scientific community.



Proponents argue that the FCC would allow scientists to study particle interactions at much higher energies and with greater precision than is currently possible with the LHC, which would enable new discoveries and a deeper understanding of the universe. It would also keep CERN, and Europe, at the forefront of high-energy physics research.

Opponents, however, argue that the cost of building and operating the FCC would be extremely high, and the benefits of the research may not justify the cost. There are also concerns about the environmental impact of building such a large collider.

Ultimately, whether or not CERN should build the FCC will depend on a variety of factors, including the potential scientific benefits, the cost and feasibility of the project, and the priorities of the international scientific community and funding agencies.

TE

What is the cost of CERN building a future collider?



The cost of building the Future Circular Collider (FCC) at CERN is currently estimated to be around \$20-30 billion. However, it is important to note that this is a preliminary estimate and the final cost could be higher or lower depending on various factors such as the final design of the collider, construction costs and schedule, and any unforeseen technical challenges.

It's worth noting that this is a large investment and a multi-decade project, and it would require significant financial commitments from multiple countries and organizations. As of now, there is no formal funding plan for the FCC and the construction has not been officially approved by CERN's member states.

It's also important to consider that, as with any scientific project, the benefits and costs of building the FCC need to be weighed against each other. While the cost is significant, the potential scientific benefits of the research that would be possible with the FCC could be even greater, and could lead to new discoveries and a deeper understanding of the universe.

TE

What is the cost of the JWST space telescope?



The James Webb Space Telescope (JWST) has been one of the most ambitious and complex space telescopes ever built. The total cost of the JWST project is estimated to be around \$10 billion.

It's worth noting that the cost of the JWST has increased significantly over the course of its development, due to various factors such as technical challenges, design changes, and launch delays. The original cost estimate for the project was around \$1 billion, but it has grown over time due to cost overruns and budget increases.

It's also important to note that the JWST is a collaboration between NASA, the European Space Agency (ESA), and the Canadian Space Agency (CSA), and the cost has been shared among these agencies. NASA's share of the total cost is around \$8.8 billion, and the remaining share has been contributed by the European Space Agency and the Canadian Space Agency.



What is the cost of a typical summer olympic games?



The cost of hosting a typical Summer Olympic Games can vary significantly depending on a variety of factors such as the location of the games, the size and scale of the facilities and infrastructure required, and the specific requirements of the International Olympic Committee (IOC). However, on average, the cost of hosting a Summer Olympics can range from \$10 billion to \$20 billion or more.

TE

Given the cost of a typical summer olympic game, is the cost of CERN building a future collider expensive?



The cost of building a future collider like the Future Circular Collider (FCC) at CERN, which is estimated to be around \$20-30 billion, is significantly higher than the cost of hosting a typical Summer Olympic Games, which can range from \$10 billion to \$20 billion or more.

It's important to note that the cost of major scientific projects such as the construction of a new particle accelerator can be much higher than the cost of hosting a sporting event.

This is because building a particle accelerator requires the construction of large and complex infrastructure, as well as the development of advanced technologies and techniques. Additionally, the cost of operation and maintenance of the facility over a period of several decades also needs to be taken into account.

In comparison, hosting the Olympic Games also require significant investments in infrastructure and facilities, but those are usually temporary and limited to the duration of the event.

However, it's also worth considering that the benefits of building a future collider like the FCC could be much greater than the benefits of hosting an Olympic Games. While the cost is high, a future collider could lead to new discoveries and advancements in our understanding of the universe, and could also drive technological innovation in a wide range of fields.

TE

Given the cost and duration of a typical summer olympic game hosted and paid for by a single country, is the cost of CERN building a future collider expensive, taking into account the fact that the cost of a future collider is shared between many countries and would provide several decades of fundamental physics research?

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When considering the cost of building a future collider like the Future Circular Collider (FCC) at CERN, it is important to take into account that the cost would be shared between multiple countries and organizations, and that the benefits of the research that would be possible with the FCC would extend over several decades.

In comparison, the cost of hosting a Summer Olympic Games that is paid for by a single country, which can range from \$10 billion to \$20 billion or more, is a one-time expense that is limited to the duration of the event. While the Olympic Games can also have a long-term impact on the host city, it may not be as significant as the long-term impact of a future collider in terms of scientific discoveries and technological advancements.

In this sense, it can be argued that the cost of the FCC, shared among many countries and organizations, is not as expensive as it might seem at first glance, especially when taking into account the long-term scientific benefits and technological advancements that would result from the research conducted at the FCC.

It's also worth noting that the cost of a future collider is not a direct comparison with the cost of hosting an Olympic Games, as the latter includes costs that are not related to the scientific research, such as security, transportation, accommodation and entertainment.

Overall, the cost of building a future collider like the FCC should be evaluated in the context of the long-term scientific benefits and technological advancements that it would bring, and the shared cost among several countries. The decision to build the FCC should consider all factors such as the potential scientific benefits, the cost and feasibility of the project, and the priorities of the international scientific community and funding agencies.