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UK Community Consultation on "Plan B" for the European Strategy for Particle Physics

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Instruction

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Context

FCC remains the UK's priority for the European Strategy for Particle Physics. However, we need to prepare a clear national position on alternative scenarios ('Plan B') should FCC not be feasible. This questionnaire explores community views under three key scenarios.

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Scenarios

Scenario 1: An e^+e^- collider is built elsewhere (e.g. CEPC, ILC in Japan)

Scenario 2: FCC (ee+hh) is unaffordable on a short timescale, but may be possible on a longer timescale

Scenario 3: FCC (ee+hh) is unaffordable

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Scenario 1: e^+e^- collider is built elsewhere (e.g. CEPC)

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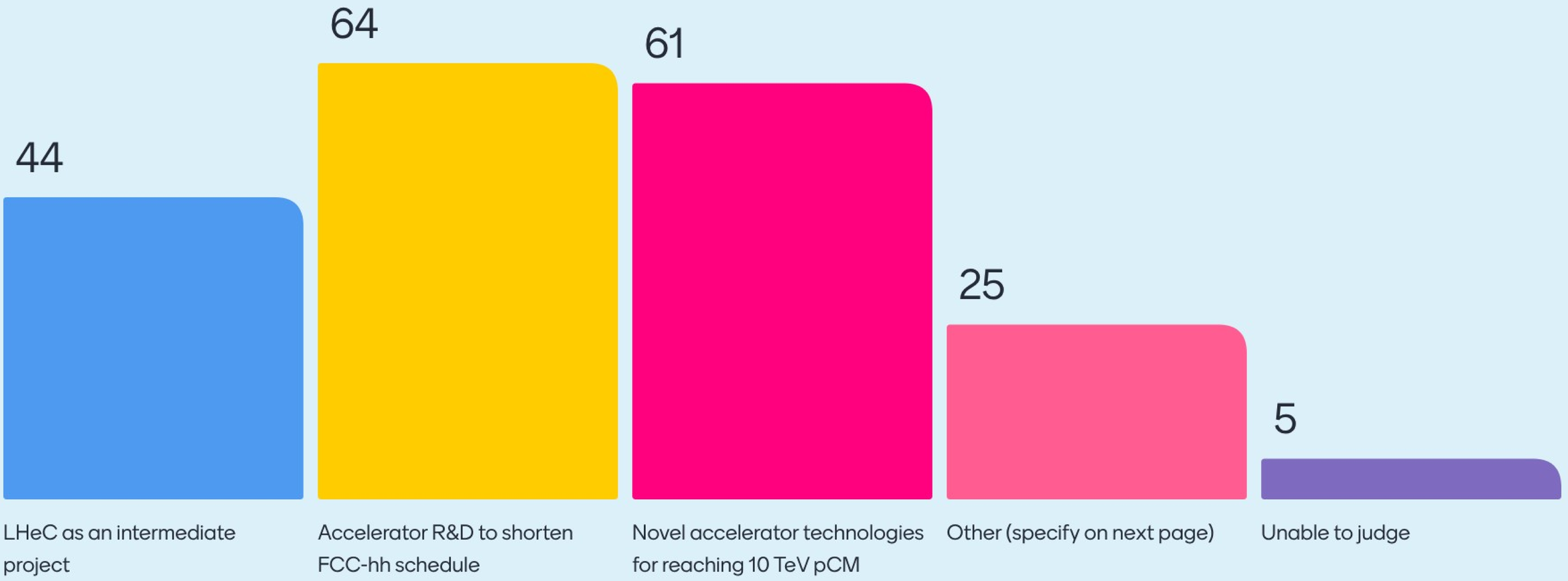
In Scenario 1, what should be CERN's near/medium term focus for after the HL-LHC? Select all that apply.



2 127

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Add any other options here, or leave it blank and press Submit to skip.

I don't think ILC in Japan is a realistic scenario, & China has repeatedly said that if Europe decides to go ahead with FCC, they will not proceed with CepC. Therefore, I find the question misleading

Other new technologies regardless of the situation of FCC-hh, such as plasma wakefield and muon collider

An updated pp collider with whatever extended CoM energy is possible and full exploitation e.g. LHeC, FPF etc

CERN Muon Collider

If feasible, FCC-ee should be CERN's top choice per ESPP2020. If not, LEP3 is the best Plan B, though more work is needed to prepare it for an FCC-ee "go/no-go" decision.

Could consider ghost collider R&D

Muon collider R&D

Lower energy FCC-hh (e.g. 40-60 TeV). It is frustrating that the FCC has lowered the target energy from 100 TeV - 80 TeV, which doesn't really change the maths on the magnets

2 42

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Add any other options here, or leave it blank and press Submit to skip.

Extensive programme of non-collider precision physics to provide the foundations for a future decision on a major collider facility.

I don't see any scenario in which FCC-hh is affordable.

In that case, RnD towards a muon collider with 10+TeV pCM seems to be the best option.

FCC-hh using LHC magnet technology, i.e.91km tunnel with ~10 Tesla magnets reaching 50-60 TeV CoM energy.

Muon Collider

A muon collider

Even with R&D, the straight to FCC-hh is unlikely to be ready before 2060, and the cost is estimated at 30BCHF. So almost twice that of FCC-ee.

I think we should go for FCC-hh anyway.

2 42

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Add any other options here, or leave it blank and press Submit to skip.

Do R&D to make accelerators and detectors consistent with Net Zero carbon emissions, including all aspects of construction, operation and decommissioning.

Do R&D to make accelerator and detectors compatible with Net Zero emissions for all aspects from construction, operation and decommissioning

Developing an electron linac is key. A correct configuration for it could be used for LHeC and /or LCF and/or ERL-based extensions of LCF or LEP3.

N/A

Exploit HL-LHC fully, focus on all types of smaller experiments (e.g. LLP program, beam dump, ...), HE-LHC, ...

CERN Muon Collider

Europe and CERN should lead the field, and not make decisions based on what other countries are doing. If FCC is possible, CERN should do it.

Forward Physics facility, fixed target experiments

2 42

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Add any other options here, or leave it blank and press Submit to skip.

Muon collider

In this scenario: (1) CERN should not pursue e+e- as competitor (2) Has to focus on how to get to 3pm then 10pCM. (3) Muon collider vs staged FCC-hh? (4) Re-use existing LHC tunnel for 27TeV pp

(1) CERN should not pursue e+e- competitor (2) Aim for 3TeV pCM then 10pCM has to be goal (3) Muon collider and HE-LHC+FCC-hh can be goals for that (4) Interim project is critical.

Support multiple smaller projects to test the BSM landscape, while continuing R&D

novel acceleration technologies for this are very important too

differentiation. Made different limited experiments and in case affordable modifications of the beam lines

I think CERN should remain the center for research in Europe as it is the only research center with a constitution which enforces public results and non-military work

A range of smaller projects aligned with keeping the field alive and training people to design and build on shorter timescales than a large project would afford.

2 42

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Add any other options here, or leave it blank and press Submit to skip.

Put PLan B under the scientific aspiration which is surely something everything can agree on, namely probes of the BroutEnglertHiggs-mechanism. For we need time to get the R&D for FCC done.

Novel R&D for accelerators but with a much stronger engagement with Member States Industry to promote the concept of CERN as a global hub of technological innovation

increased emphasis on "small" experiment projects, encouraging novelty and detector technology development, in addition to priority physics targets

flavour & precision physics experiments, Physics Beyond Colliders

-

CERN joins with Japanese / Chinese to develop the e+e- collider as an international project. CERN engineering capability also used to drive build of Einstein Telescope.

Muon Collider & relevant R&D

Lower energy FCC-hh

2

42

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Add any other options here, or leave it blank and press Submit to skip.

Best would be Europe participates strongly in the external Higgs factory, while maximising HL-LHC return, hosting an e-p collider at CERN and accelerating FCC-hh R&D. Imperative to start FCC tunnel

Focus on any e+e- in Europe should be discontinued. Whatever strengthen the plan "keep Europe at the energy frontier via hadrons (HL-LHC → FCC-hh R&D)" should be pursued (eg exascale computing)

Muon collider as a separate option?

I advocated before construction of FCC-hh using LHC magnet technology that can reach say 50TeV CoM energy. The physics case is there, which, in short, is B&B Higgs physics + a shot at BSM physics.

The advantage of this proposal is that it uses established technology in an ~90km tunnel and can be constructed in 10-15 years from decision. This is based on the SSC timeline of site decision in 1988

by the US congress and start of operation planned for the 1996-1999 period. Given the SSC parameters it can be regarded as an Upgraded SSC.

The disadvantage of this proposal is that it is not currently being discussed as an option. I think that it is reasonable as it is not coming from within the experimental particle physics community

I think , though, that the question that should be pondered is: What would be the view of the CERN community if another entity would venture to build such a machine. After leading the field for 40y.

2 42

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Add any other options here, or leave it blank and press Submit to skip.

Would CERN let another entity reap the rewards of discovering the new physics, if it exists? I don't know the answer, but I think that it should at least be pondered.

Muon collider or another technology complementary to developments elsewhere

2 42

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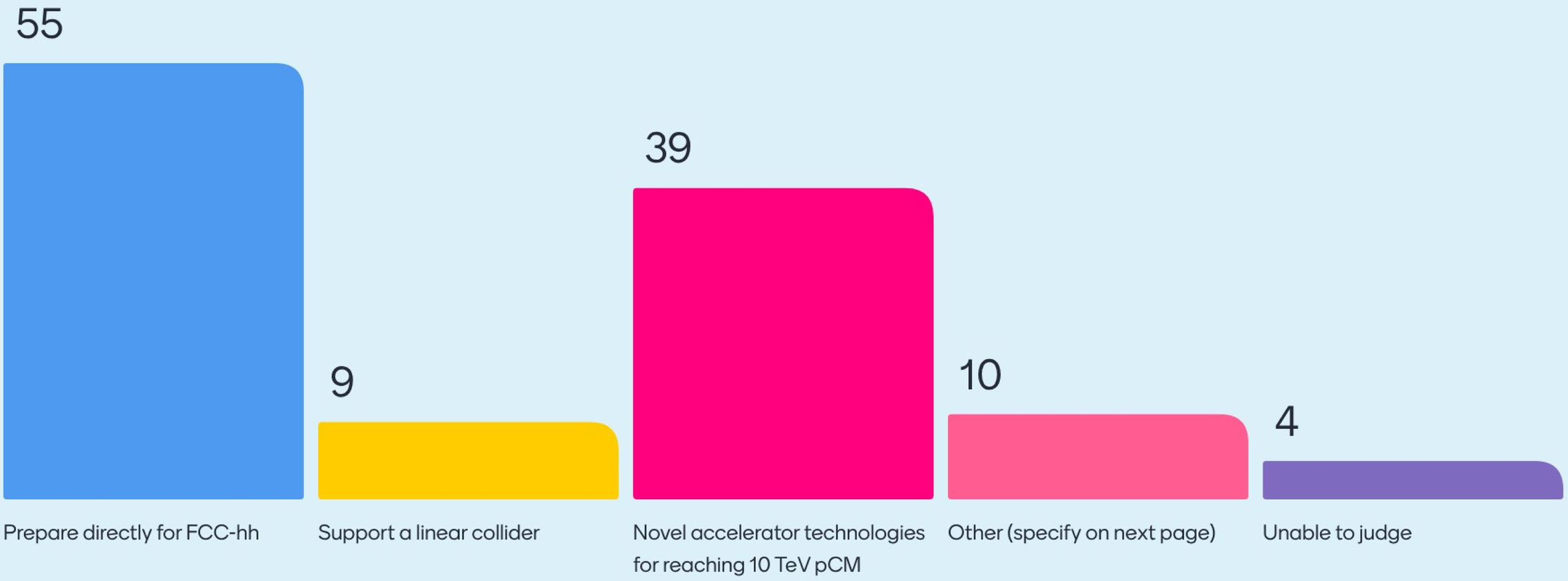
In Scenario 1, what should be the main ambition at CERN in the longer term?



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Add any other options here, or leave it blank and press Submit to skip.

support new technologies not necessary to support the situation of FCC-hh, plasma wakefield, muon collider etc

As previous reply

CERN Muon Collider

Broad programme of non-collider physics.

Collider physics is dead without pushing new technologies. We need to face that head on.

Of the linear collider options, according to the specs, all ILC scenrios are inferior to most circular collider options. CLIC remains a viable option.

Muon Collider

.

3 26

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Add any other options here, or leave it blank and press Submit to skip.

CERN needs to do what is best for Europe. Other projects may fail / be delayed.

N/A

CERN & Europe should lead and not make decision based on what others are doing. If FCC is possible, CERN should do it.

explore muon collider and further develop plasma wakefield accelerators

still do FCCee first

Will also need an interim project to keep field going. No long gaps.

Interim project will be key

think to other physics goals

3 26

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Add any other options here, or leave it blank and press Submit to skip.

As I said before, we need to ensure CERN mantains the leadership in the world as the place where non-militray public research can happen

contribute to a gravitational wave observatory network

Assuming FCC is not built but there are accelerators elsewhere CERN could act as a centre for accelerator and detector R and D. It could also act as a staging post for detector constructrion.

Both Support a Linear collider and R&D

ensuring that there is a strong physics programme, possibly beyond the standard remit of just particle physics, with at the same time a carefully chosen accelerator development activity

Prepare muon collider. CERN (& supporting European institutions) are well-suited to such innovation.

Preparation for FCC-hh requires civil engineering work to start now, and continue 16 T-class dipole R&D and HTS cables/cryogenics. Pave the way to physics at 100 TeV with HL-LHC and LHeC

FCC-ee will do great physics. CERN should build it. Plan B is fictional.

3 26

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Add any other options here, or leave it blank and press Submit to skip.

LEP-3

this questionnaire does not seem to work

3 26

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Scenario 2: FCC feasible only on longer timescales

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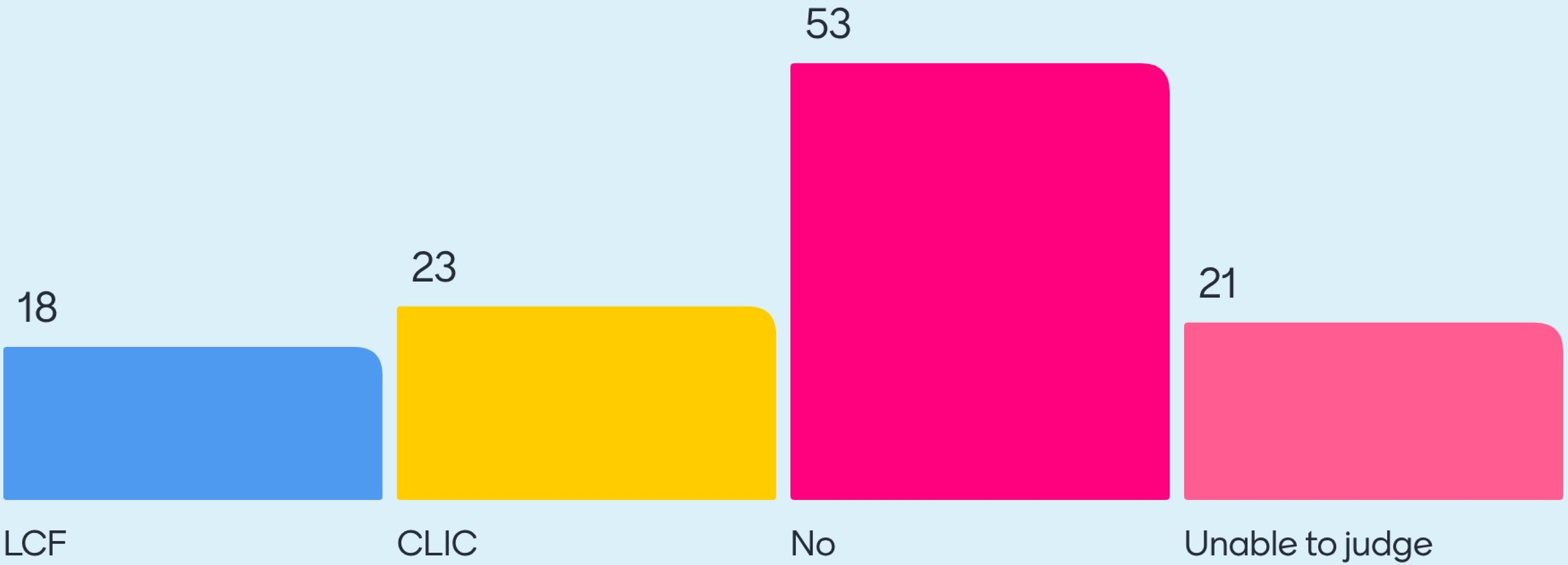
Should CERN proceed with a linear e+e- collider in Scenario 2?



115

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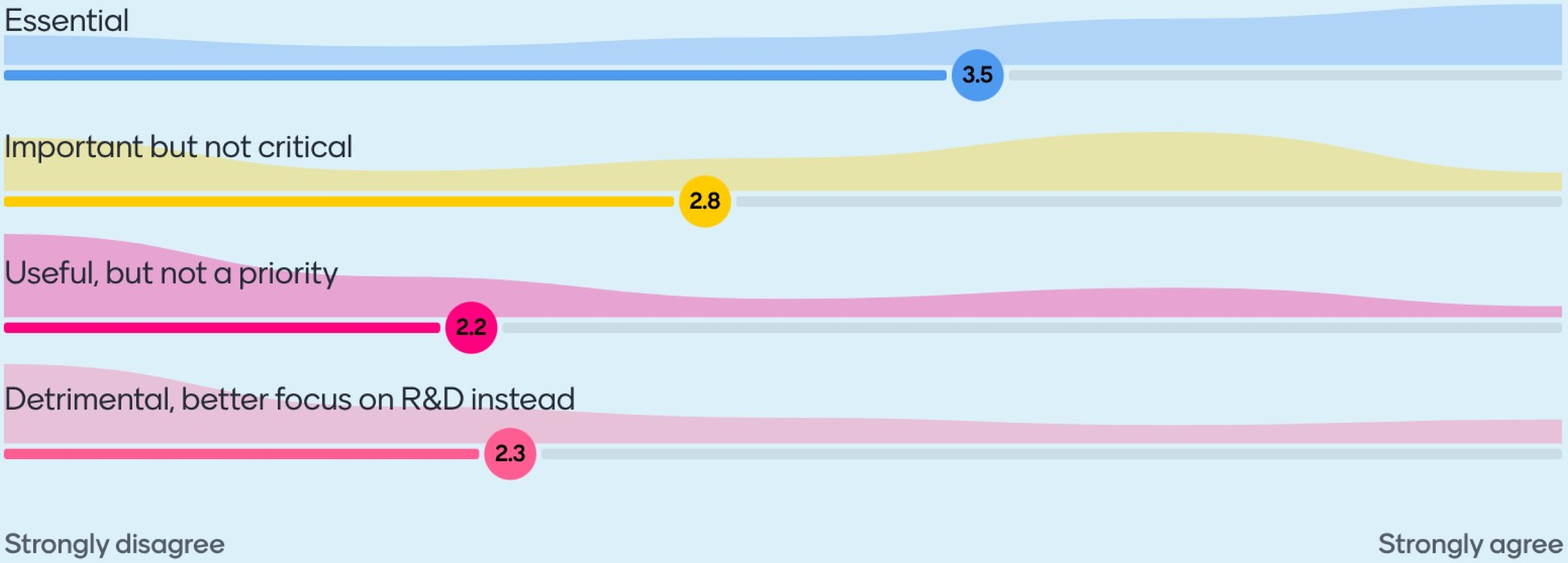
How important is it to have an intermediate project at CERN (e.g. LHeC, LEP3) in Scenario 2? Press skip if unsure.



107

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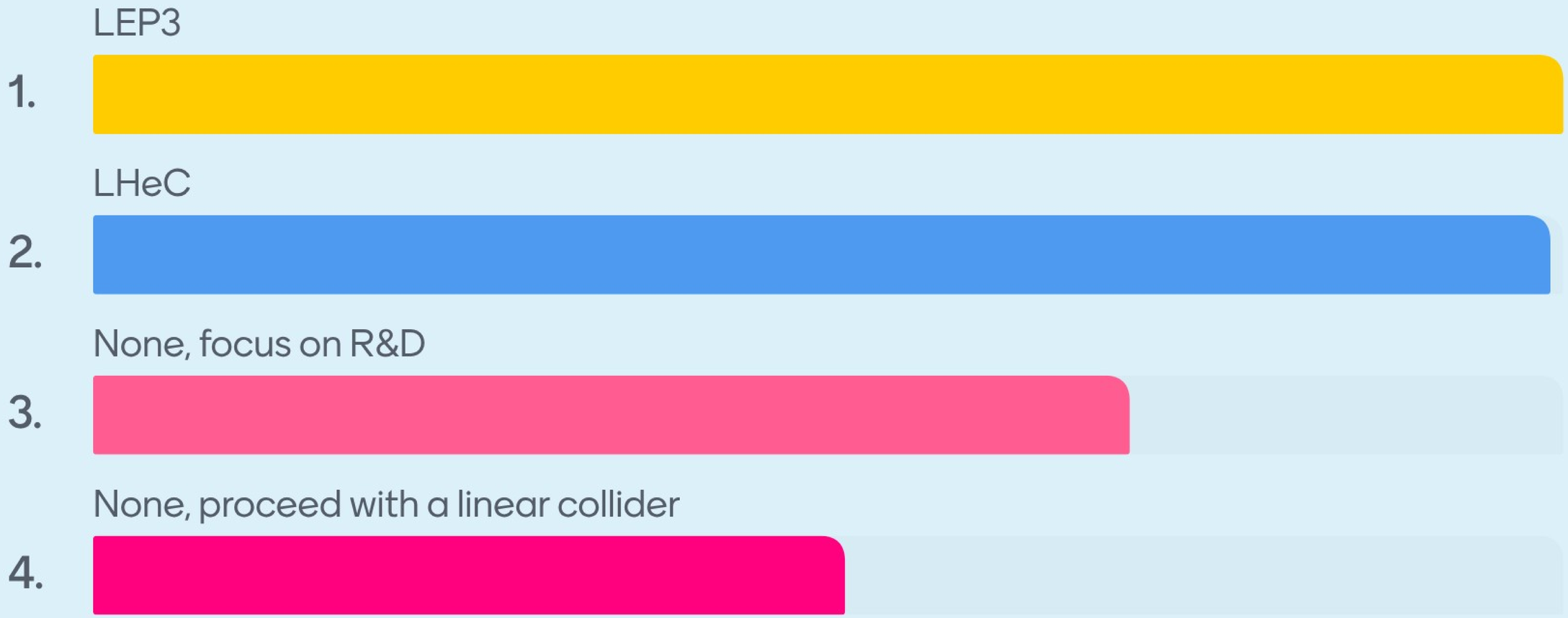
Which intermediate project should be prioritised? To skip press **Submit** without selecting.



98

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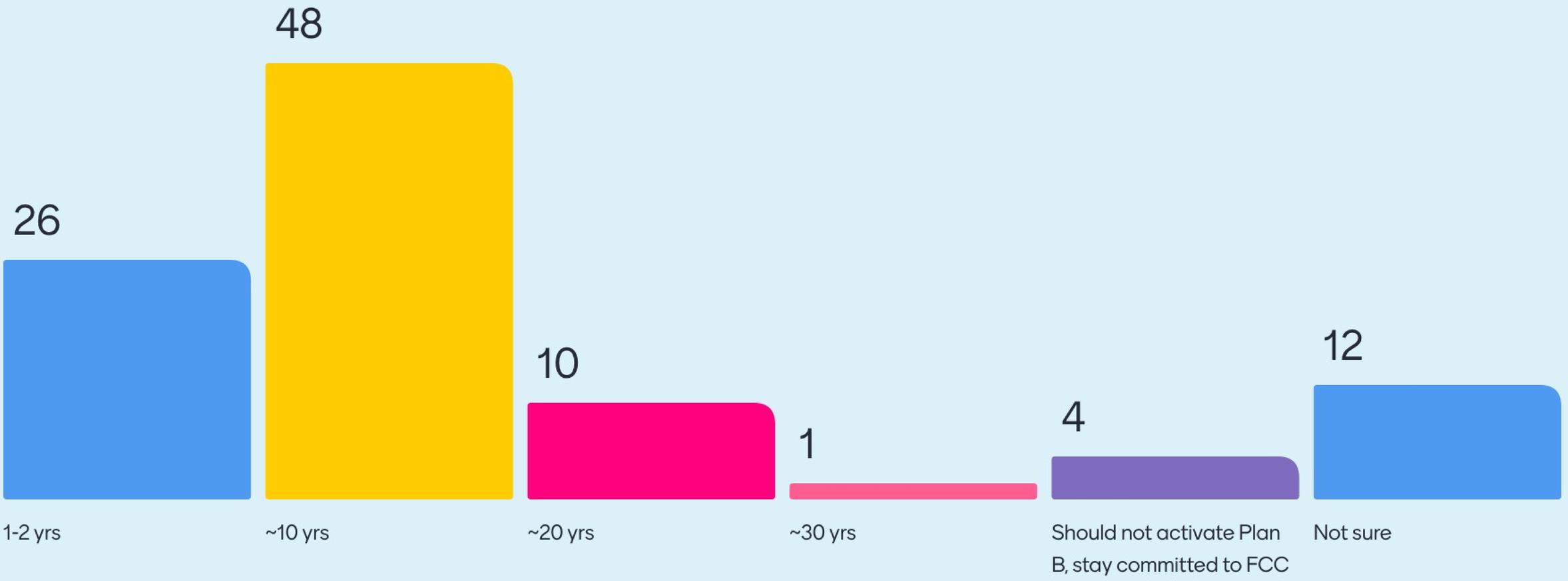
In Scenario 2, what is the maximum acceptable gap between the end of HL-LHC and the start of FCC before activating Plan B?



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Scenario 3: FCC not affordable at all

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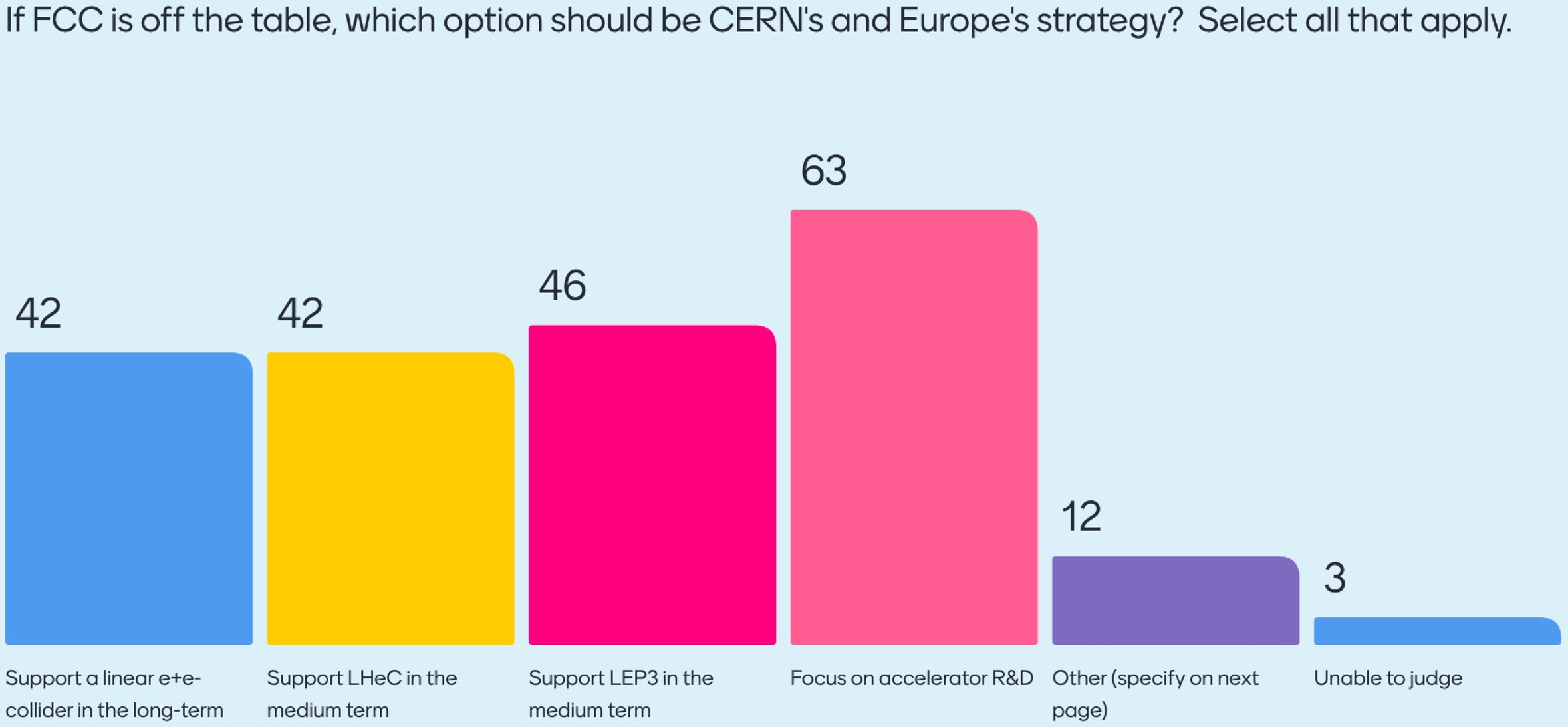

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Add any other options here, or leave it blank and press Submit to skip.

CERN should focus on new technologies not necessary to support FCC-hh

Highest possible energy HE-LHC in the current tunnel

CERN Muon Collider

Any Plan-B alternative to FCC must (1) cost significantly less than FCC, (2) be affordable within the available portion of the CERN budget and (3) deliver physics within 10 years of end of LHC.

On this timescale other projects may become viable, e.g. ghost collider. This possibility should be left open.

Although I understand why this question is being asked, we should make clear at a political level that this would be a catastrophic scenario for European science and more funding is the only recourse

Non-collider physics programme

Europe should pursue an affordable collider and push accelerator R&D.

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Add any other options here, or leave it blank and press Submit to skip.

A high pCM linear collider should be priority, ie. CLIC or Muon Collider.

Muon Collider, explicitely

Muon collider is the option I would like to see.

.

Develop an electron linac that can be utilised flexibly: LHeC and/or LCF and/or ERL-based extensions of LCF or LEP3.

N/A

LHeC, and accelerator R&D for e,g, muons.

Support LLP experiments exploiting SPS/LHC beams.

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Add any other options here, or leave it blank and press Submit to skip.

Support diversity of research program with smaller experiments looking in corners not accessible by colliders (high intensity frontier)

CERN Muon Collider

CERN needs a flagship project. In this scenario it should do the simplest one (LEP3) and put as much resources as possible into accelerator R&D or possibly non-accelerator physics.

LEP3 + Muon Collider [R&D]
LEP3 + HE-LHC [R&D]
Depends on ambition.....

differentiate with different projects/experiments

CERN needs to find a new identiy to stay relevant or wind up

A real project like LHeC is mandatory to keep the accelerator and physics expertise alive at CERN. It also actively prepares the ground for further break thru developments e.g. in RF for FCC.

It is ESSENTIAL to have cutting egde physics always being done at CERN.
LEP3/LHeCsecure this, but despite having been a protagonist for LHeC a decade ago, LEP3 prvides the best Plan B (BEH physics).

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Add any other options here, or leave it blank and press Submit to skip.

Consider relationship with ALL international states to consolidate efforts. China already dominates UK manufacturing....we need to consider strategic alliances more openly.

focus on physics beyond colliders, precision & flavour physics

It is actually important that CERN and the particle physics community in general embrace non-accelerator physics.

Muon Collider -- Lepton collisions at TeV CoM energy, requiring much academic work.

Anchor Europe's frontier at CERN with an e-p collider and a world-class HL-LHC-centred programme; keep targeted FCC-hh R&D to preserve long-term option value. Expand also intensity-frontier.

Expand intensity-frontier and astroparticle synergies (EDM storage rings, fixed-target dark sector, neutrino cross-sections with European leadership).

The highest priority for CERN should either be an affordable alternative for continuing at the energy frontier or moving to more broadly useful applications of accelerator technologies.

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Cross-Cutting Themes

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

Non-collider physics should not be reduced at all, or should be increased in coming years

Non-collider physics is essential of particle physics and CERN should not reduce it at all

We need to ensure we have a broad program and should not cut this to the bone for one expensive option

Away from *energy-frontier* collider, a European programme to probe QCD rather than entirely relying on EIC/US seems sensible. On true non-collider, direct dark-matter / neutrino complemenarity?

The priority should be a collider / collider R&D, however if no collider project is affordable then non-collider physics must be the priority.

Yes and no more than necessary

The primary purpose of CERN is to advance our understanding of the fundamental particles and fields from which our Universe is woven, not to build particle accelerators for their own sake.

Many of the most relevant questions in our field are in non-collider physics, e.g. neutrino sector and dark matter. None of the current activities should be reduced, in fact DM should join the list!

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

CERN's involvement in neutrino programmes should be sacrificed if the situation is really that dire, as well as non-collider R&D. But I think this would be a worst of worst case scenarios.

No comment

Sacrificing the breadth of the field in pursuit of ambitious colliders with speculative physics outcomes would be unwise. The field may not survive an 10+ year gap in new physics results coming out.

Strong support for non-collider physics should at the heart of any plan-B option (and should feature strongly in any plan A). It should not be reduced.

Should not be reduced substantially, but should ideally align with collider programme and not be entirely separate.

Cut it completely. CERN priority is FCC-hh

Collider physics should be the focus at CERN, non-colloder phsyics hould be reduced

Robust support

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

It is essential that during preparation of any further large project, CERN has a portfolio of projects extracting real particle physics results.

It should not be reduced to prioritise colliders - else CERN will not produce physics results for 20+ years and will lose the people with this expertise while considering compromises on detector/accel

Major contribution to non-collider physics inc. astroparticle physics. European leadership is possible regardless of feasibility of FCC, e.g. next generation direct detection dark matter experiments.

Colliders must be prioritized at CERN, but CERN should support R&D efforts to assist other experiments as long as it doesn't defer from the collider programme

It should not be reduced much more to still contribute to global experiments

CERN should continue to support future programmes at the same level at least to now in neutrinos, nuclear physics, astrophysics and medical. It should also consider future "precision" K, mu, EDM exp.

Go for colliders.

CERN must focus on collider physics.

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

not qualified to respond

Non-collider physics is an impactful and complementary part of European research, and it should be a priority to maintain, at the very least, the current level of support.

Utilising CERN's resources in the most optimal way includes using the complex for non-collider physics. This is both from a technical and personell (training) point-of-view.

not sure

"Low background physics" generically, including direct DM, neutrinos and neutrinoless double-beta decay, should be a priority. Symbiotic FIMP-type experiments in the forward region of colliders also.

As diverse as possible and no reduction.

CERN is accelerator lab and unless this changes it must prioritise accelerator-based particle physics (not necessarily colliders). But it should support non-collider physics at CERN and elsewhere.

use the existing accelerator facilities as shown by many PBC studies: fixed target 9also with the LHC), medical physics etc.

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

medical physics, radiation studies, precision physics with high intensities

Prioritise all non-collider options with the highest priority to keep the breadth of the programme. It should not be reduced to prioritise colliders at all.

Drop it

I fully support non-collider physics as an activity, but CERN has always been an accelerator lab and should continue to focus on that.

I think that non-collider physics is as important as collider physics and therefore there should be no economic impact for non-collider physics experiments.

Support for non-collider should be maintained 10TeV pCM should be the goal. R&D to that should outweigh new non-collider initiatives

Diversification and support (not only) for blue skies R&D but also for technology deployment into prototypes/demonstrators and small-scale tabletop style detectors to probe fundamental questions

Collider physics should be the priority and non-collider cut if required

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

none	Gravitational waves	Full program in North Area, neutrino platform,	Important that CERN supports testbeam, neutrino and fixed target as well as nuclear
Need to maintain diversity if in field - neutrinos and dark matter	Non-collider physics is important and should be supported. So should detector R and D.	It should not be reduced at all.	If no main collider project planned or likely at CERN, then have to think whether CERN is the right way to go for non-collider physics. My view is non-collider physics should be ~25% of PP effort

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

I don't think non-collider physics/experiments should be reduced at all. If anything the strategy needs to be revamped to support them more.

Europe should keep supporting non-collider physics at CERN and not reduce its support at all.

Research diversity is crucial for the broader development of the field.

Any well-justified and reviewed non-collider program should not be reduced.

Continue supporting existing and currently agreed to future European experiments and international collaborations.

Measure experimentally wherever possible with experiments which produce important numbers of lasting value. For example a good set of flavour physics experiments,

So revisit the appalling decision to reject HIKE which was forced through by the directorate at CERN ussing questionable and unprecedented methods.

This is a matter of judgement nearer the time.

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

CERN's focus should very much remain on colliders, or at least experiments using its accelerator complex. It could have a role in non-collder physics, using its wider skills base.

none

none

non-collider physics is essential even if a new collider is build. Physics Beyond Colliders is order of magnitude cheaper for comparable-ish physics and the cost must not be cut further

There should be a strategy, but I don"t see how reducing the activity would materially benefit colliders

Diverse physics programme is important I don't believe it should be reduced to prioritise colliders

Do not reduce non-collider physics at CERN; increase it, and as I said before, accept that accelerator physics is becoming affordable, and less productive.

It is essential to maintain breadth in the programme. The Neutrino Platform is vital. And it is essential that European funding remains available for R&D, dark matter, $0\nu\beta\beta$ and neutrino mass.

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

Outside colliders, CERN should split about half of its total focus between: 1. Nuclear fusion power experiments using accelerators as possible drivers 2. Medical applications of particle accelerators

CERN should fully focus on the next generation collider experiments. If there is any conflict in achieving this, non-collider physics should be reduced til the conflict is removed.

Run a tightly-focused intensity/precision frontier that (1) delivers unique physics Europe can lead, and (2) de-risks tech for future colliders (HL-LHC, e-p, FCC-hh).

Themes: Dark sector & rare processes (SPS fixed-target/NA area); Antimatter & fundamental symmetries (ie ELENA); Neutrino platform; Acc. & detector tech platforms (high-field/HTS magnets, ERL etc)

If collider projects must be prioritised anyway, as I think, CERN can preserve a minimum viable backbone for non-coll projects (10-15%) protecting unique capabilities

There should be a coherent strategy for non-collider physics but it does not necessarily need to be hosted at CERN.

CERN should focus on colliders

A higher priority should be given to developing technologies for experiments worldwide, for colliders, astroparticle, and non-physics applications, if FCC unaffordable, do not prioritise colliders

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What should Europe's unified strategy be for non-collider physics at CERN, and how much of it could be reduced to prioritise colliders?

non-collider high-energy physics should continue to be supported and NOT reduced

Exploit unique potential of collider location - FPF, SHiP, FASER2. Support complementary physics - e.g. liquid noble, protoDune. If critical to CERN future as collider laboratory, strong reduction ok

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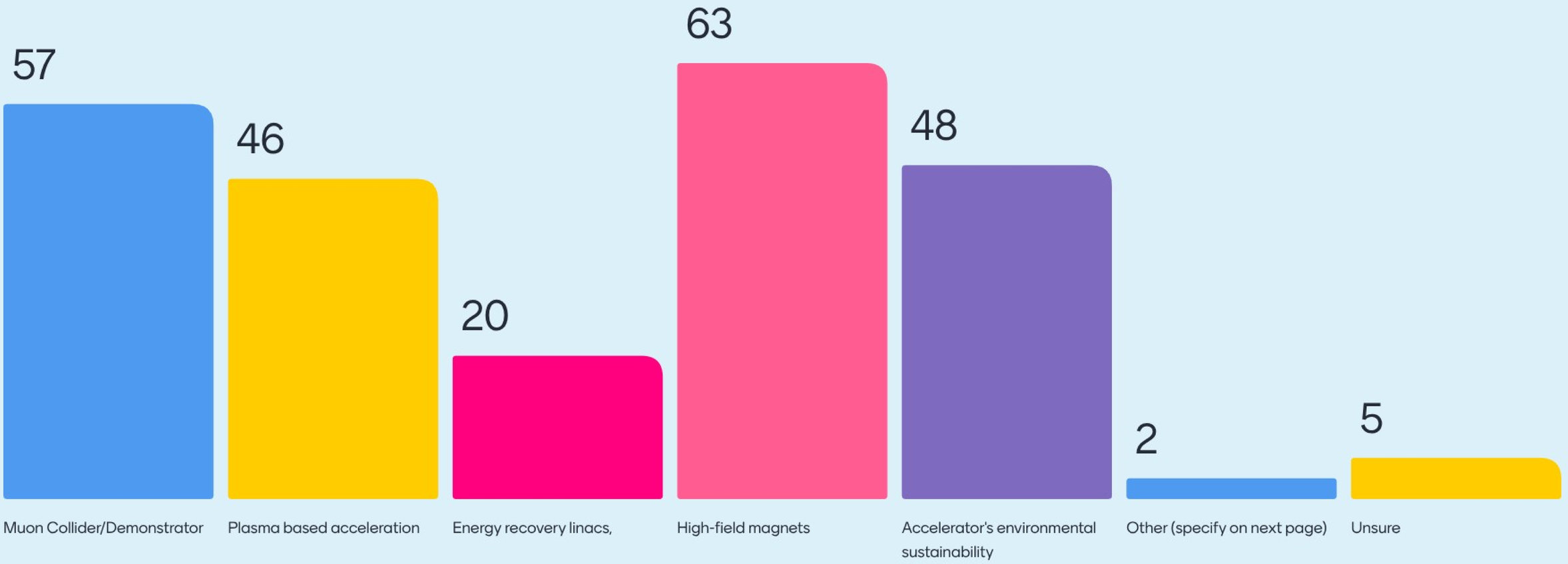
What acelerator R&D area should be the priority regardless of scenario? Select all that apply.



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Add any other options here, or leave it blank and press Submit to skip.

No comment

High field superconducting RF with novel materials

.

N/A

not sure

not sure

Join in the big themes of our century: long-term energy sources, radioactive waste retreatment, ...

would like to see 125GeV muon collider as a target (or demonstrator). Otherwise just aiming at 3-10TeV CoM is too risky.

1 10

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Add any other options here, or leave it blank and press Submit to skip.

Superconducting RF

Superconducting and quantum electronics.

Prioritised for Plan A. Not clear a prioritisation independent of long-term plan for field is meaningful.

1 10

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Any further comments on Plan B strategy that you feel must be reflected in the UK input?

I think it must be recognised that if Europe decides to go ahead with FCC, China will not proceed with CepC. Therefore, the decision is upon Europe, not China.

Non-collider physics should be one of valid options for the future of particle physics. IT's the most sustainable option (cf astronomy)

Factionalism needs to be taken out. Everyone wants *their* project, but collectively what is the best opportunity/USP for UK strengths including theory/pheno complementarity to experiment.

LEP3 should be kept alive until a FCC "go-no go" decision. CERN must invest sufficient resources to enable this.

Again, the lower energy FCC-hh is not being given enough attention on feasibility and physics reach (although, it probably doesn't help much with costs).

I would like to see an expanded LHeC discussion. We have an e-p interaction point, but 3 other spots to put experiments on the ring. What else can we do with the proton beam?

No comment

CERN is too big and expensive to be seen to be pursuing a less favoured (plan B) option. A sensible approach is to strengthen support for programmes already deemed high priority.

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none.

N/A

Without a clear, unified vision, a lot of young researchers will be "lost" to other fields/industry.

The key detail is the timing. If the delay is less than 10 years then it might well be we can run the LHC longer and still do excellent physics.

We need to be congnisant of the high impact that advances in software and computing (ML etc, possibly Quantum Computing) are going to have on what we can learn from our experiments.

I think Plan B will quickly become Plan A once we realise that the FCC is not feasible

Plan B will need an interim project (LEP3 or LHeC) and R&D towards the long term goal (10pCM). The interim project cannot be linear collider because that is too expensive and removes the R&D aspect.

I would suggest to focus on muon physics

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Plan B is actually what will and what should happen. To start FCC-ee a couple of years after the end of HL-LHC is preposterous

I do not think the UK should support FCCee as Plan A

Plan B as driven by the science case of probing the BEH mechanisms at the Fermi scale and beyond must be at the forefront. The quickest way to this is LEP3 which seems to be a better than expected ..

... proposition than originally thought. Further, though not strictly driven by eneregy recovery there is even a novel posssibility of upgrade to LEP3, the Ghost collider (Andrew Hutton @ JLab) which

... may make LEP3 go to higher energy. The desin work on the idea needs pursuing.

Any viable plan B has to be considerably cheaper than plan A - as well as having a clear physics programme.

Plan B will only be funded by major contributors if there is a much stronger commercial / economic / political case. This will mean CERN seriously diverting resource.

Can Plan B consider if anything useful can be built, to the benefit of both PP and MS industry, from existing or 'near term' technologies. Accept a stop gap facility to demonstrate innovation value.

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Physics beyond colliders, precision physics, flavour physics are very very important and are not properly represented in the UK physics programme.

I think there needs to be realism about the future of high-cost accelerator physics research. Other science is being undercut in the name of physics having no new results in 10 years.

Europe has the resources to innovate to a muon collider. For an FCC-like project I would be concerned about the large-scale hiring/firing required for an "effectively same-but-bigger" collider.

Plan B should not divert Europe into LC or LEP3 paths. Both high-cost, low-option-value choices: a LC duplicates a Higgs factory likely hosted elsewhere, LEP3 consumes LHC tunnel for a decade or more

The UK's input should stress a Plan B that (i) maximises HL-LHC, (ii) delivers a CERN-hosted e-p programme (unique physics and clear UK strengths), and (iii) accelerates enabling R&D for FCC-hh.

UK should support a staged low-E FCChh, provided the 100 km tunnel is built. The tunnel is the key long-term asset, securing Europe's EF future. Unique physics can be done from 60 TeV on!

No future collider plan is credible without equally ambitious strategies for computing, data management, and sustainable, energy-efficient operations from the start.

LEP3 and LHeC are "dead-end" projects that do not have sufficient scientific potential.

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A linear collider provides flexible options for a longer-term future in case FCC-hh continues to be unrealisable (cost/ magnet technology)

It is not clear from what has been presented how the other uses of accelerator technologies in the UK that have benefitted from previous involvement in CERN have been factored into the considerations.

The complementarity of CERN's experiments to other HEP areas should be maintained - examples include: i) DM between (in)direct detetion, accelerator-based/beamdump experiments, ii) collider neutrinos