March 29, 2022

 1 p.m. Plenaries.

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of the proceedings."

 >> Check, check, check.

 >> Yes, we can hear you well.

 >> Should we start the session?

 >> We do not hear the audio from the room.

 Are the microphones on?

 >> Hello, Alessandro, can you hear now?

 >> Yes.

 >> We're waiting for the panelists to show up

after lunch and since we run a bit late, as you know,

we're just giving a minute or so to people. We're

getting started momentarily.

 >> Thank you.

 >> I guess we can get started with this

afternoon's session.

 So this will be basically a similar session as the

one before in the sense that what we want to do is sort

of start getting opinions on how to put together the

BSM part. How to assemble a little bit of the BSM

part.

 Let me see. What I think is pretty clear from the

presentations you've seen yesterday, that in the BSM

part there is a huge variety of different, if you want,

studies that have been prepared and presented. So

finding a good way to communicate that and to, sort of,

also compress and put them together is not an easy

task.

 And one of the things we should probably try to do

today is focus on the things that are, sort of, maybe

newer or different from the European strategy that was

done a few years ago.

 And that means then -- it's okay -- that means

that sort of, what that basically, what challenges that

brings.

 When I think about some of the European strategy

plots that were already pretty busy before and I think

about the submissions that we got, if one updates those

plot, we will end up with something that is really hard

to digest and make any sense of.

 I think all of these are just examples but we just

wanted to hear what you think. To facilitate that, we

asked a few people to join us as a panel. But we'll be

open to everyone. Here we have Swedana, Sarah,

Michael, Caterina and Stefan. Thank you. As we told

you, if you want to make an initial statement, you're

welcome to, otherwise we will just go around in a

discussion.

 Oh, yes, that's the other thing, sorry. Patrick,

you're online, right? There we go. Sorry. I was

going just in front of me and Patrick is online and

will join us in person tomorrow, I think.

 Yes, if you want to do a very short statement on

this, you're welcome to do it now and then we can I

guess, move to more open.

 Do you think you can grab the microphone so it's

probably the easiest.

 You need the just -- perfect.

 >> You have to do it in order.

 >> No. If someone wants to go.

 >> Perfect.

 >> If I had understood the charge correctly, what

I was supposed to be doing is generating discussion. I

want to say something completely stupid so you can all

stand up and say, come on, Sarah.

 We're in a very interesting place with beyond the

standard model. Right. We all know that there is a

particle out there that we would just love to discover

but we don't have any compelling reason to think it has

any other gravitational interactions.

 We have several interesting hints in B physics and

g-2 that may give us hints on what we should or should

not emphasize when thinking about what searches and how

to optimize searches for the future.

 But on a pessimistic day, you can think it's

statistical fluctuations. It's happened before. A

small amount of cut tuning.

 So we don't really have any way to know what we

should emphasize as the most compelling beyond the

standard model searches.

 Now, this makes you want to think about casting a

broad net. How do you think about, how to quantify how

you cast a broad net over different possible things.

We have traditional ways to do this.

 One thing is the SUSY scan, you scan over a

wide -- you take SUSY as something that generates a lot

of models and scan over a large SUSY space and see what

kinds of machines and what kinds of detectors are

sensitive to the most. And somehow quantify the

broadness of your net that way.

 One thing we used to think about a lot, especially

at the start of LHC was signature-based searches. And

you don't see that discussed that much anymore. What

kinds of signatures, what kind of effective cross

section for a given signature is a given detector or

given experiment sensitive to.

 This would be a challenging thing to do and this

is kind of late in the game. There are an infinite

number of signatures just like an infinite number of

SUSY models. And parameterizing this would be hard.

An easier thing do is take SUSY searches and see which

signatures they're pulling up. But I wonder if there

are creative way, this is what I'm thinking about, to

quantify how broad a program it is. How broad and how

interesting it can be given that right now we know

there is new physics but we don't have a compelling way

to look.

 That is my thought of something we can discuss.

 >> Thank you. Who wants to go next?

 >> I can't wear both of these things at the same

time. So I'd like to make three comments which are

intentionally, they're going to be provocative and very

unhelpful for Simona's goal.

 The first thing I would like to say is I have a

very clear personal approach to what is the most

important question about beyond the standard model

physics. And I think it very much disagrees with the

one that Sarah gave.

 I think it is the question of why is electroweak

symmetry broken? As long as the Higgs is some stupid

scalar field with totally adjustable coupling, we're

never going to make any progress on any of the

important questions of physics.

 We have to understand where the Higgs is coming

from and what physics generates it and generates its

couplings to every particle in the standard model.

Until we understand that, the neutrino physicists are

not going to make progress, the cosmologists are not

going to make progress, no one will make progress until

we answer this question.

 The second point I want to make is this point of

view has driven me increasingly towards exploring the

10 TeV scale is a very important goal for particle

physics. Before the LHC, we always said the 1 TeV

scale is. And I'm one of those theorists who is not

willing to be ashamed of saying that we expected

discoveries immediately after the LHC opened.

 That is what I really thought. But I think that

is also what 90 percent of the theory community

thought, however much they might dis-avow it now.

 There were beautiful models that predicted a

mechanism of electroweak symmetry breaking from SUSY,

from little Higgs, from other kinds of composite models

where the main actors were at the, basically below the

TeV scale.

 We now know all those models are wrong. What we

need to do now is search for models with compelling

physics where that, the really important scale is

elevated and there really are such models.

 Brian Batell in his talk reviewed many of them.

Some that one might add to that is Dirac -- little

Higgs models, the new dynamics is at the 10 TeV scale.

 We got to start thinking about how to get there.

And this motivates 100 TeV proton collider, 10 TeV muon

colliders and 10 TeV e+/e- gamma colliders. We don't

have the technology that gets us there and we have to

think about that and there have to be concrete, beyond

the standard model motivations for that to stimulate

accelerator physics research that we have to start

today.

 The third point is, today even though this is very

important and maybe obvious to everyone in the particle

physics community, the case when we go outside the

particle physics community for a 10 TeV order collider

is weak.

 It's no accident that the proponents of the FCCHH

emphasized the Higgs self-coupling, the search for

neutralinos, things that are parts of what we should be

doing at the next generation of accelerators to

motivate a 10 or 20 TeV scale accelerator.

 What they really wanted to say is we want to find

out what is there to solve the mystery of the Higgs

boson. We can't say that because our knowledge is too

imprecise. Someone will ask why do you want 100 TeV,

why not 200 TeV or 70 TeV is enough. That is a lot

cheaper than 100 TeV. There is no answer to that

question.

 Until we get more knowledge, we're not going to

have an answer to that question. So we better start

working on that now.

 It is true that many models, there are lighter

particles but they may be out of reach of LHC. We have

to figure out a way to go after them.

 Those are three definite statements, I believe

them strongly. The last two are pretty hard problems.

But maybe it's a good place to start a discussion.

That is what I wanted to say.

 >> Thank you.

 We'll go onto take the next.

 >> I don't have much more to add than this but

maybe two points about the specifics of the report is

that we should not under sale the high Lumi LHC for

example. It's not that there aren't a lot of exciting

projects on the table. We need to secure our funding

to fin issue the high Lumi phase. This is 20 years in

the future and it's not guaranteed.

 We still have to make a strong physics case that

the high Lumi has a purpose, we can reach this much

better precision than Higgs coupling. We have

interesting exploration on both the high energy

frontier and exotic processes and rare processes and

rare Higgs decays and lighter particles. There is some

good territory to cover there.

 And the other point is that I love the model

independent approaches, lots of signature based

searches but probably to make a case, we need specific

physics like dark matter and neutrino mass. And maybe

a few of those well-chosen examples are more powerful

than showing a wide breadth of things we can do is try

to have a few, maybe a more limited number of sharp

arguments may work better in our favor. That are

completely physics driven. Rather than, oh, we can do

ten times better on the precision but what does it

mean.

 >> Thanks.

 >> I want to address a few specific things about

near [indiscernible]. In my department I'm talking to

colleagues in AMO or condensed matter. They're super

excited about everything including the dark matter

problem for us. They are excited about light -- dark

matter and axions and whatever it is. We're not able

to convey the same amount of excitement although the

parameter space is basically huge.

 There are many places where many of you have

written models where they're still accessible. Beyond

the accessibility of LHC, or ILC or definitely at muon

collider we talked about several things.

 I think we need to figure out how to convey this

excitement. There is still lots to do in terms of dark

matter and not just giving up on the way a lot of our

friends beyond particle physics look at. Just a

thought.

 Another question.

 >> I have just two more points and very minor, you

already asked great questions. From the discussion we

had before lunch when Michael showed the plots with the

particle physics and the Higgs benchmarks. I think it

would be nice to see at the end of the reports how

based on what you learned at the high lumi LHC and the

Higgs factory which is left to explore. Having as a

function of time, a few benchmarks to show the

parameter space and some models that are left for us to

explore with future machines and the energy scale for

them.

 And another thing, as was wrote up a lot, the next

Higgs -- you will have untriggered. What kind of BSM

scenarios would benefit the most from this experimental

opportunity.

 So just these two points.

 >> Thank you, Patrick, you have waited this long

and now it's your turn.

 >> Thanks, sorry I'm in the there in person. I

will be there tomorrow.

 I agree with pretty much everything that Michael

said. I think one thing though, we do need to be

careful of which I sometimes hear from people in

related fields or you sometimes see on Twitter or the

internet in various forms, it's often viewed what we're

doing is the same thing over and over again and we just

push the scales up higher without explaining why.

People view it as we didn't see something so now we're

just making the models higher and higher.

 I think it's really important that we explain the

state of the field and explain the lessons we learned

from the LHC. One thing that was brought up in Brian's

talk and Michael alluded to as well, if you take the

canonical model of physics, supersymmetry, there are

other potential models, but if we take this beautiful

model and we take what we learn immediately from the

LHC once we found the Higgs at 125 GeV, a lot of

theorists wrote papers before the original Higgs

discovery when we had hints back in December before the

July 4th, that the scale was not the electroweak scale

anymore.

 The scale had moved onto the 10 TeV scale. If we

took the canonical SUSY model and tried to explain what

100 GeV was.

 We need to emphasize the new developments and

theory that have come around since then. Such as

filling out this ideas of neutral naturalness or some

of the new ideas, I think the Higgs program is enough

to motivate any new machine. But what we've learned

about Higgs physics is also changed as a function of

time.

 The canonical two examples I give are learning

about the electroweak phase transition which is

connected to this question of electroweak symmetry

breaking.

 The models back in 2013 don't represent what we

now know about the theoretical possibilities. And it

pushes us to ask for machines that go to this 10 TeV

scale and beyond.

 Another example that was brought up in the morning

discussion and also by Caterina is this idea of going

after flavor. We can say word like dark matter and

naturalness but we shouldn't see flavor just to

dedicated flavor experiments.

 The LHC as well as any future colliders is going

to have a lot to say about flavor and the theory there

in terms of model building and what we can do

technologically is really advanced.

 I think it's a mix of explaining what we learned

from the LHC and also explaining what we learned new

theoretically that we should emphasize so people don't

always think we're asking for bigger for no reason.

 >> Okay. Thank you.

 I think, thanks to all the panelists for making

the opening statements. Now I guess we're moving to

open the floor for comments and additional comments and

questions.

 Those were quite some provocative statements.

Otherwise, we have a few things we want to follow up

with but okay, perfect.

 Just to say, people in Zoom, just feel free to

raise your hand and we'll get to you as well.

 >> Just want to say, it was fascinating to hear

the counter point between Sarah and Michael. Because

thinking about things from a more data science

perspective, visualizing the space that one can explore

and the hierarchies that we have and higher

probabilities and higher energies get more interesting

structures is a fascinating question about how to show

that. And what is a picture of the standard model that

shows the diversity of data that we get. I would love

to figure out a way to do that visualization and

showing the space is that we can explore.

 At the same time, I'm trained as a BSM model

builder and I'm motivated by the dynamics of

electroweak symmetry breaking and how it's connected to

other things. That is a space of theoretical

possibilities that need to be delineated in some way.

 And I get into fascinating tension talking to

people that want to be really, really close to the data

and people that want to be really, really close to the

modeling and both extremes are very valuable but either

by themselves is not. I want to harmonize, we need a

broad search program where those two things are

emphasized at the same time.

 >> Any panelists wanted to address again?

 >> Maybe I can just quickly follow up on this. I

think you raise a very good point and it's going to be

fun to think how to address that in practice, right.

In showing [inaudible]. One thing that probably sort

of comes out -- I see several possible something, how

do I say? Ways to phrase and guide a reader. I'm

trying to put myself on a reader perspective. I'm

opening this hypothetical thing and going through the

BSM part and then I'm trying to think how do we

communicate. Do we make it understandable what are the

breadth of the program, what are the deep physics

meaning of what you're trying to find out at the same

time.

 And this goes to the other point you make which I

think is very important which is how we're going to do

it. And how, if I look at -- as a lot of different

ideas how you can do it.

 And we need, it's not only so what we're after but

how we're after it. And this is another point which I

was trying to allude a little bit at the beginning,

that if I sort of just take the existing approach of

the summary plots and try to create a summary plot with

all the different ways we try to attack that problem, I

don't think I would understand myself the plot. Not

even enough to mention someone from neutrino physics or

someone from condensed matter where they're trying

to -- what we would do.

 That is just another problem I think that is very

practical. Where if anyone has brilliant idea of

definite views of how we should address that, I think

to me at least it would be very valuable.

 So, it's not a question, it's not a comment. It's

just a call for ideas.

 >> Okay.

 >> It would be useful to have some targets here.

This is why it's just showing greater precision maybe

isn't enough. I like that heavy neutrino lepton

example from the electron positron colliders where

there is a phase space of allowed model space bounded

both from above and below from a baryon asymmetry and

from a seesaw.

 So that if you express or explore, that gives you

a target and you can show that you can cover half that

space. That's the kind of argument that is more

convincing than saying there is an open-ended, I can go

to 1 percent, .1 percent, .001 percent and so what? Is

there some kind of physics target. For that example

was quite nice from that viewpoint. It gives you a

finite space.

 >> Yes. I think just a quick comment. I think it

is, I agree with you. It's good to have a target. On

the other hand, we have to bear in mind all these

things are depends on a model. The particular plot you

refer to, the seesaw is not the seesaw we usually think

about. It's quite misleading. Anyways. Okay. We

have 2D cut on Zoom, so go ahead.

 >> Hi, I just wanted to pass on a provocative

comment that I got from a couple of different particle

theorists or rather they are particle theorist by

training and they shall remain nameless.

 They claim that anything we might discover like a

new particle or some of these precision measurements

that we might be doing, they do not fundamentally

change for example quantum field theory. And so they

are not very excited about what our collider searches

are going to give us in the future.

 And so my thoughts are, we have to try to

understand how we can communicate, naturally we talked

about great models and these are not people into model

building. They are playing good old theory. Can we

communicate clearly in our report what we might find is

really going to be a game changer or can do things? I

think this is something we might want to think about.

 When you have particle theorists by training raise

this, it does not look so good for our field.

 >> They probably forget how quantum mechanics --

 >> I would like to directly respond to this point.

You have to remember the history of gauge theories.

Guage theories were invented by Young and Mills as a

mathematical contrivance where isospin was gauged and

it was known to be totally wrong at that time.

 The model was fascinating. This was in the 50s.

Julian Swinger was a big advocate of it and the student

of his, his thesis defense was fascinating for everyone

who was there. They learned all kinds of things about

the possibilities of gauge theories that were not known

outside of Swinger's circle. It took 20 years to find

out how gauge theories are relevant to particle

physics.

 The idea of the Higgs phase and the confinement

phase which was crucial in QCD took 20 years and it was

totally informed by experimental developments and

particle physics. Your friend who are theoretical

theorists must realize they have the benefit of that

now.

 We don't know what kind of theory is out there.

With supersymmetry being maybe more disfavored at LHC,

I have gotten interested in theories of composite

Higgs. I've been interested in that a long time but

those are probably the best candidates for models where

you push the new physics up to the 10 TeV scale.

 In those models we enter the question of what is

the ultimate strong coupling behavior of gauge

theories. What are the new possibilities we haven't

thought of but maybe realized in experiment.

 I think it's possible we're going to learn a lot

about fundamental physics from answering the questions

that we're talking about today. People should be

interested in this. If they're not, they don't have

curiosity. That is my answer to this question.

 >> Thanks, Michael. We should really put it

somewhere front and center in our report just so it's

clear to people. I think that was my emphasis.

 >> I just wanted to say something about the

question of basically how to sell. I don't know if

that's the right way to say it. But how to maximize

the impact.

 I've been struck just now speaking is an old

phobia, I guess, with how little conversation there is

between the current conveners and the last round of

conveners and the last round of P5 people.

 Maybe you guys are better on the energy frontier,

certain frontiers there is a complete reinvention of

the wheel.

 And I think you could learn a lot just by talking

to people who you served on P5 in the past about what

arguments sold and what didn't. I was on P5. It was

probably the worst experience I had on my entire life

on serving on a committee. I'm not interested in

talking about it again.

 There are certain people that shouldn't say

certain things but I still think there is a lot of

wisdom to learn from this. Just as an example, in the

neutrino frontier, they had Ritz (ph.) come and give a

talk just a few weeks ago. And it was fascinating I

think, and I think people should read it.

 He talked about, for example, the arguments that

sell go across frontiers and if you don't have interest

in someone else's frontier why should you expect them

to have interest in your frontier. I encourage you to

talk to the people that have gone through the hard work

of producing a P5 report and learn from their past

mistakes.

 >> May I answer this? Because it was advised to

us, myself, Laura is it okay, you can take -- there was

a discussion between not just the energy frontier

conveners but in our all conveners meeting with some of

the members of the P5 and especially with Steve Ritz.

 And at that time we did hear some of the things

which were discussed about colliders, probably not all

from Steve's point of view. And there was someone

else, I forget.

 And then we have also talked to a few other energy

frontier people, not everyone who were on P5. However,

I think my personal, we are thinking of having a

discussion with the full community with having people

from the previous, I would say Snowmass, I wouldn't say

P5. My hesitation is that at that time in 2013, LHC

for the collider was given. The Higgs was just

discovered. And the money for LHC and R and D and

stuff was already there and how much and what the

profile would be. And it was the endorsement of the

HL-LHC program where the collider community had to be

behind and add to it what we would do in terms of R and

D and behind the future colliders and -- came out and

some, one of the P5 recommendations was involved in a

generic future collider.

 However, right now we have a clean slate. Beyond

the HL-LHC, we have nothing out there which is

guaranteed to just the same way that LHC was in 2013.

People may disagree with my framing of this but this is

my personal framing and this is the way I see it.

 And yes, there are many, many, many ideas there.

And of course, we have allegiances to ideas over time

but there is, what we want to keep in this Snowmass is

a very open discussion until we start discussing the

vision and then bring the people who have participated

in forming our previous vision. First, let the

community speak after all this work is being done by

the community.

 Let's see where everybody wants to be and this is

the focus of this meeting. Is to form, start forming a

vision.

 And that has to come also from the community with

proponents from various ideas before we hear from what

happened in the past. Because it's also politically

driven. Let's first do our physics as the energy

frontier, being neutral and hearing everyone's vision

and then try to come to the table in different groups,

different ways and we are open to suggestions. How to

do it. So I thank Jonathan for you raising this.

We've been discussing all this stuff amongst us in the

background. And we wanted to say this more on Friday.

Our view is let's start from the plain level, level

playing field for everyone and everybody's white paper

and then we figure out how to move ahead and form our

vision.

 >> I don't think we were as contradictory as it

might seem. I'm happy to hear that you did talk with

people already. I completely agree that the purpose of

Snowmass is different than P5.

 Snowmass is dissect the menu and P5 chooses from

the menu. It's a completely different thing. Someone

asked, at some point you do have to make arguments that

will rise up as these excessive conversations by

factors of ten and go on and on, you want the thing

you're interested in to survive. At some point you

have to think in a strategic way.

 >> I think it was summarized well what we have

done in setting up the scene here. Of course, we're

learning from your guys, from all of the people who

have been in this situation before us.

 And from all the community. And I think I just

wanted to add how impressed by the kind of discussion

we have been having since yesterday. Because it's

really showing a lot of incredible work that has been

maturing and now being presented to the community and

I'm, I think -- and the discussions that we have been

having. Some very interesting physics points are

coming out that are a first and main priority of this

exercise.

 Of course, we'll be open and keeping open to all

kinds of suggestions and advice that will be coming to

us in how to present that. The content is very

important, the presentation is also very important.

 So thank you for being here.

 >> Thank you.

 And Patrick is -- please, go ahead.

 >> I mean just echoing on Jonathan's point a

little bit but connecting it to the physics arguments

and how we make the case to the broader community, I

think some of the discussions we've had today and in

the morning about connecting across various frontiers

and communities is really important. For instance, I

mean, this comment that Stefan made about the heavy

neutron leptons and it's a particular implementation of

the seesaws but making these connections across

communities is very important.

 I know for instance the muon collider forum, we've

spent a lot of time talking with the neutrino frontier

as well as the process frontier to build these cases

out a bit more. And I think one of the things that we

can embrace from colliders is emphasizing our breadth

in the sense that one machine can touch all these

frontiers and make lots of progress. But we shouldn't

view it as a competition. We should embrace the fact

that we can go after the physics in so many different

ways.

 We should also think about not just in the context

of making our physics case independent of all the

others as we might get the hints from the other

frontiers. Whether g-2 pans out or flavor anomalies

pan out or there is a wave background that can't be

explained away by astrophysical sources. Oftentimes,

we silo off in the energy frontier. One of our biggest

strengths is with the same machines we can touch this

wide array of physics and maybe emphasizing that more

up front in their work would be helpful.

 >> Patrick, maybe you're saying a bit like with

the dark matter where we have this diagram that we

rotate three ways and show the complementarity of the

ground space, the phase space and the collider, we need

to think of more ways to do that for other things like

the hierarchy problem and other important questions

where we can show there is a complement tarty between

different frontiers so they feel included in part of

it.

 >> Flavor physics is one that we could do a lot

more there. Exactly, that particular example, we can

try to expand in as many directions as possible.

 >> I just like to back up Patrick on this point.

If it turns out that the LHC anomaly of flavor

non-universality is correct, first of all, that's a

definite motivation for physics beyond the standard

model. Secondly, it has to be generated by some new

particles in the TeV regime which are directly relevant

to energy frontier.

 And we ought to be thinking more about how to

explore these anomalies directly by dis-coffering the

particles or manifestations maybe even in Higgs

physics.

 >> Going to a different part that Jonathan was

talking about earlier about previous P5 versus this

Snowmass process.

 When the previous Snowmass started there was a

thing that floated out up front, we have done the

experiment of proposing projects of more than a billion

dollars and failed and we should put something under

our belly. This time there was no such thing that was

put forward at the beginning. Not to say there is a

lot of money there for whatever project but I think

there is a lot more scope for thinking. That is what

we see from the point of view of availability of

interesting ideas to go beyond the HL-LHC, say.

 I think that we should take advantage of that to

bring the community together. I have to be a little

pessimistic that the engagement from the community has

not been as great. Not a lot of people are engaged in

the Snowmass process. We need to get more younger

people engaged. There are lots of new ideas that

people are talking about. It would be nice to get more

people involved. I don't know how we'll do that

between now and July but I think that is what is

needed.

 >> We can go to Zoom, Alessandro.

 >> I wanted to change the discussion to a

different topic but still remaining on the cross

frontier relevance. I will put a dimension of

instrumentation so detector R and D and computing and

accelerator. I think it's very important moving

forward in the next decades to really strengthen the

synergy and the necessity to have strong R and D in all

these sectors to make this field successful.

 Computational progress is mostly driven by the

industry rather than fundamental research. But at

least for detector R&D and accelerator, really the

fundamental research is driving this progress. And of

course, the physics message is to be the key in our

reports. Because what we do is fundamental research.

But we also have to stress these improvements in

instrumentation in all these different fields have

important consequences outside our little niche of

research.

 The BRM did a good job in advertising our needs in

terms of instrumentation. We have to use that as

leverage to pursue investments more and more for the

next decades. I think this is necessary in all these

fields, accelerator, computing and detector R&D.

 Just a remark.

 >> Thank you.

 Anyone else?

 >> I think this is super important especially when

creating excitement about our field. The reality is we

don't know what new physics look like. We have to try

to push all these different areas of technology because

that is the only way that we're going to find what we

don't know what we're searching for.

 At the same time, I don't know the name of the

speaker, but I completely agree that we need to, we

know that from the advancements in fundamental physics,

we see advancements in other areas of our societies.

 One thing that I've been thinking a lot about is

what are the new detector technologies that we can,

that we can form, how do we use machine learning and

some of the advances in material science to try to

develop, you know, new -- like actual new detector

technologies. Not just recycle some of the things

we've been doing for the past 10, 20 years.

 >> Any other comments?

 Is anybody on Zoom?

 Maybe I can push one point a little bit further.

I think, again, as we said at the beginning of this

panel, of this discussion that we're trying to focus

more on what's new in the last few years since the last

European strategy update.

 And I think about it, you know, especially in

terms of going to look into the future and new

facilities and so on, several new facilities have been

newly proposed or pushed with much more enthusiasm than

it used to be. This includes muon collider and C3 and

so on.

 So I think in terms -- I think it's very relevant

in terms of addressing new physics, we wanted to also

think about, we must have some kind of -- in mind.

 I think if you know, you can listen between the

lines that you see sort of the preference in Michael's

comments and in Patrick's comments already. They

didn't, they are not being very explicit.

 Maybe I can push them or anyone else to make a

more, you know, clear statement. How hard are these

things and how does this interplay with the facility

that we have already talked about in detail in the

European strategy updates?

 Can I put you on the spot to do it?

 >> Six months ago or a year ago when we were

beginning, I saw two ends of the spectrum. Some were

interested and thinking only about electron and

positron machines and others were only interested in

muon collider that can get to 30 TeV. I see a lot of

convergence of things and people are able to understand

the time scales involved with the two machines are

different and the Higgs precision could inform us

something about the energy scale that is accessible at

that 30 TeV or 10 TeV.

 I think we have to bring this community a little

more closer together so we go forward to the rest of

the high energy community with a compelling program

where we'll re-endorse perhaps what the strategy group

said but the interest in the Higgs factories as the

immediate next step.

 And also look forward to it, to 10 TeV scale that

we talked about so much. But it doesn't have to be one

project alone. There is a lot of activity and a lot of

interesting ways of going about there. So I think if

the Snowmass process captures the broader ways of

getting to the same physics about the Higgs presence

and 10 TeV but other projects would probably help more

people getting together. Just my thought.

 >> Well, there was something different that I

wanted to say here. But I guess it's connected to the

same theme. So when I talked before, I was very

emphatic that the case for multi-10 TeV colliders was

weak and had to be strengthened by new knowledge about

physics.

 But I think it's exactly the reverse is true for

the case of the Higgs factory. I mean, there, you

know, I give a number of Chloé yum where you have to

explain the Higgs factory to physicists outside of high

energy physics.

 Specifically, always to your condensed matter

experimental colleagues. And there is a point of view

there that goes like this, that the Higgs is the thing

that we don't understand. We have good arguments that

the LHC should not have discovered things about the

Higgs that contradict the standard model. Because they

just haven't gotten, yet, to the level of precision.

 I mean, certainly technically for this audience,

if you take the point of view of Smith, that is almost

obvious. The Higgs coupling deviations are suppressed

by V squared over M squared where M is large scale.

Below that scale, anything is possible.

 So this brings it much more into the realm that

most scientists think about science. You have some

knowledge, you have some system where you suspect that

there is a possibility for learning something. You try

to improve your knowledge of that system. That's

normal scientific exploration. And you hope for a

surprise.

 And that's exactly where we are in the Higgs

program and that's what we want to propose. I think

that that approach is very understandable to scientists

outside our field. Maybe not to the general public but

to scientists, that's the way you do science.

 And that's what we can say about the Higgs boson

exploration today.

 >> Any more comments?

 >> Okay.

 Yes. I see two comments.

 Patrick.

 First Tao and then Patrick.

 >> Tao? Can you go ahead? You're muted.

 >> Am I unmuted now?

 >> Yes. Perfect.

 >> Thank you all for the very nice discussions:

 I wanted to reiterate a statement that was made in

recent years. I think for us it's clear that the Higgs

factory is a -- and Higgs is the particle that we

noticed and all the other particles, we all have

factories, all the way back to pion factory and B

factory and C factory. Every factory has offered

tremendous new knowledge for us and Higgs is no

exception.

 Not only we learn the standard model and all

these, even for the past environments we all learn

something new. In the B factory we learned about early

time in the early 90s and the experiment in Germany.

It gave the existence of a -- and the likely precision

measurement is V factory and W factory.

 We got such a detailed structure of the standard

model laid out and the prediction of a top Quark and

Higgs.

 The reason not to continue on these -- is a must.

Of course, my statement may not be said, may not be

really resonant to everyone if I say Higgs, Higgs is

sufficient for the next machine. So from physics

viewpoint.

 However, if we look at this practically, in the

HL-LHC, it's a Higgs factory. We have -- Higgs there

already. So we have to be sure to make the high

luminosity LHC successful and also what is next,

technologically seems -- for the -- collider.

 It's highly uncertain as we know and we've been

pushing and working on this for years and it's highly

uncertain if we can succeed. That is the only one

technologically closer to reality in the future.

 Given the current situation, we have an argument

for -- and next generation higher energy muon colliders

and possibly replacement for the linear collider, the

international linear collider and -- higher energy.

But I think for our discussions, we probably should not

only make a strong physics case that we are talking

about, we should make very clear statement to set the

tone for our introduction to the next P5 that we should

highly recommend for the very strong R&D for the next

collider or maybe the Higgs factory.

 What I want to summarize is that basically we're

not quite ready for the next collider yet. Linear

colliding might be. But we absolutely have to make a

strong statement that we need a very strong R&D support

for the next generation future colliders.

 Thanks.

 >> Thank you very much for your remark. And next

is Patrick and then we close the session.

 Go ahead, Patrick, you get the last one.

 >> Great. I want to follow up on Michael's

comments where I think you see the breadth of the

community in the sense that I'd say there is much

stronger physics case for 10 TeV in terms of what we've

learned from the LHC if we think in this more model

independent framework.

 But at the same time, I think there is a super

strong case for E minus E plus factories but in the

less model independent way in the sense that Michael

say, below it, the scale M, you really need to think

about the models of what can survive the LHC

constraints and what are the things that a Higgs

factory can do that no other machine can.

 Like David's point this morning, figuring out how

to go from light flavors. An E plus E minus machine is

the only way to go after these things.

 I think it's a question of how do we make the

community come together but I think there's a strong

case for 10 TeV but then the most important thing is

exploiting the Higgs factories to make sure we don't

miss any new physics that is a more model-dependent

thing.

 And once we talk about models, allows us to build

these complement tarry bridges to other communities.

 The exact opposite point to Michael but now you

see the breadth of the theory opinions.

 >> I guess we should close this session. Thanks

everybody, thanks to the panel and thanks to everybody

that gave inputted.