ILC:
What did we learn at Snowmass ? and, what comes next ?

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At previous SiD workshops, I have spoken as one of the conveners of the Energy Frontier study for Snowmass. This required balance and respect for consensus.

The Energy Frontier report, of which I am an author, is still in preparation. It will be released by the end of October. However, the Snowmass Executive Summary has been released to HEPAP, and I will quote it here.

In this talk, I will give my own personal opinions. Those might be different from the opinions of the Snowmass convener, but, I hope, not too much different.

It is important to begin with what the Snowmass process did and did not seek to accomplish.

Snowmass 2013 was the first major survey of the possibilities for the future program of US particle physics since Snowmass 2001. That Snowmass was followed by a HEPAP subpanel, chaired by Jon Bagger and Barry Barish, that made decisions about priorities.

Similarly, this Snowmass study is about enumerating physics opportunities. Formally, projects were not in competition.

There is a new subpanel (P5), chaired by Steven Ritz, which must now decide on priorities.

However, even with no priority setting, the Snowmass process was challenging.

Our community has splintered:

The DOE regulates funding according to the three frontiers, which are then put in competition.

Major experiments requires decades of commitment. Even young people do not move freely between experiments in different frontiers.

Energy Frontier research is taking place only at offshore accelerators.   Under Pier Oddone, Fermilab aligned itself with the Intensity Frontier.

and, our budget has decreased significantly, even as our goals have become more ambitious.

In my opinion, Snowmass was very successful at overcoming these obstacles.

We talked, and we listened. Many events encouraged evaluation of the major goals of each frontier by the whole community. The spirit of interchange was positive.

This positive attitude may not continue in the prioritization phase, but it should. It is important that he whole community accept the P5 results.

An important driver was the downsizing of LBNE by the DOE. Now LBNE cannot achieve its goals without international collaboration. This forced the neutrino community to have a change of attitude toward globalization.

This led to the most important overall conclusion of the Executive Summary:

The experiments that address these questions are ambitious, large-scale projects. Mounting them requires long-term vision. We are fortunate that our priorities are shared by physicists in other regions of the world, so that these experiments can be realized as global partnerships. The U.S. brings crucial leadership, design talent, technology, and resources that will be essential to these experiments wherever they are located.

Now, let’s talk about ILC.

ILC received three important boosts in the past year:

The completion of the ILC TDR and its acceptance by the global accelerator physics community.

This discovery of the Higgs boson at the LHC, at a mass at which the ILC gives a perfect setting for the measurement of its properties.

The encouragement of ILC by the Japanese government, and the hope for its inclusion in the Abe government’s stimulus plan.

meeting of Lyn Evans and Prime Minister Abe, March 27, 2013

These developments, and especially the last, changed the debate on ILC in a crucial way.

They set up a situation in which the Japanese government could inject new and very large resources into particle physics.

These resources would support a project -- the study of the Higgs boson -- that is universally believed to be of high importance for particle physics.

The community began to understand that it would be foolish not to encourage this.

Here is the statement from the European Strategy Report:

There is a strong scientific case for an electron- positron collider, complementary to the LHC, that can study the properties of the Higgs boson and other particles with unprecedented precision and whose energy can be upgraded. The Technical Design Report of the International Linear Collider (ILC) has been completed, with large European participation. The initiative from the Japanese particle physics community to host the ILC in Japan is most welcome, and European groups are eager to participate. Europe looks forward to a proposal from Japan to discuss a possible participation.

The acceptance of the TDR by the Snowmass Accelerator Capabilities group is an important further step. Here is the language from the Snowmass Executive Summary:

The ILC, as described in its Technical Design Report, is ready to proceed to construction. Its design incorporates U.S. contributions in accelerator theory, damping ring design, superconducting accelerator technology, and beam control and delivery.

Still, three hard questions needed to be answered in the Snowmass Energy Frontier study:

1. Do we really need 1% accuracy in Higgs coupling measurements ?

2. Isn't what the LHC will do good enough ?

3. Is what the ILC will do good enough, or do we have to go to a new technology that can do better ?

1. Do we really need 1% accuracy in Higgs coupling measurements ?

The answer from Snowmass is a clear yes. The reason invokes the Decoupling Theorem: The Higgs sector can be as complicated as you wish, but if all particles except the lightest (h) are at mass M, the deviations from the Standard Model predictions for h are of order

$$m_h^2/M^2$$

This is a tough criterion. It is tougher when you realize that even an observation should be at 3σ, and with many couplings, there is a substantial look-elsewhere effect.

Here is the table of estimates for Higgs coupling deviations in BSM models compiled by the Higgs working group:

Model	κ_V	κ_b	κ_γ
Singlet Mixing	$\sim 6\%$	$\sim 6\%$	$\sim 6\%$
2HDM	$\sim 1\%$	$\sim 10\%$	$\sim 1\%$
Decoupling MSSM	$\sim -0.0013\%$	$\sim 1.6\%$	$< 1.5\%$
Composite	$\sim -3\%$	$\sim -(3-9)\%$	$\sim -9\%$
Top Partner	$\sim -2\%$	$\sim -2\%$	$\sim +1\%$

for mass of new particles = 1 TeV

This actually calls for sub-1% precision in many couplings.

2. Isn’t what the LHC will do good enough ?

In view of the above, obviously not.

LHC will measure Higgs couplings at the ~ 5% level.

$3\sigma \times 5\%$ = not yet in the game.

However, the detector upgrades for HL-LHC are expensive and must be justified, so there was great pressure to make the LHC capabilities look as impressive as possible.

Strategy of CMS: try to estimate how well one could possibly do in measuring Higgs couplings at the LHC in the High-Luminosity era.

Scenario 1: current systematic and theory errors
Scenario 2: divide current theory errors by 2
divide current exp. systematics by $(\int dt\mathcal{L})^{1/2}$

Scenario 2 is maximally optimistic. But experimenters are clever. This might in fact reflect reality in 2030.

This method of estimation for LHC was generally accepted by the Snowmass Energy Frontier study.

300 fb^{-1} :

Observable	ATLAS	CMS-1	CMS-2
$\sigma(gg) \cdot BR(\gamma\gamma)$	$9 \oplus 12$	$6 \oplus 12.3$	$3 \oplus 6.2$
$\sigma(WW) \cdot BR(\gamma\gamma)$	$31 \oplus 11$	$20 \oplus 2.4$	$14 \oplus 1.2$
$\sigma(gg) \cdot BR(WW)$	$25 \oplus 12$	$6 \oplus 12.3$	$5 \oplus 6.2$
$\sigma(WW) \cdot BR(WW)$	$66 \oplus 11$	$35 \oplus 2.4$	$28 \oplus 1.2$
$\sigma(gg) \cdot BR(ZZ)$	$10 \oplus 12$	$7 \oplus 12.3$	$5 \oplus 6.2$
$\sigma(WW) \cdot BR(ZZ)$	—	$12 \oplus 2.4$	$10 \oplus 1.2$
$\sigma(gg) \cdot BR(\tau\tau)$	—	$13 \oplus 12.3$	$6 \oplus 6.2$
$\sigma(WW) \cdot BR(\tau\tau)$	$19 \oplus 11$	$16 \oplus 2.4$	$9 \oplus 1.2$
$\sigma(Wh) \cdot BR(b\bar{b})$	—	$17 \oplus 3.8$	$14 \oplus 1.7$
$\sigma(t\bar{t}h) \cdot BR(b\bar{b})$	—	$60 \oplus 11.7$	$50 \oplus 5.9$
$\sigma(t\bar{t}h) \cdot BR(\gamma\gamma)$	$54 \oplus 9$	$40 \oplus 11.7$	$38 \oplus 5.9$
$\sigma(Zh) \cdot BR(invis)$	—	$16 \oplus 4.3$	$11 \oplus 2.2$

3000 fb^{-1} :

Observable	ATLAS-HL	CMS-HL-1	CMS-HL-2
$\sigma(gg) \cdot BR(\gamma\gamma)$	$4 \oplus 12$	$4 \oplus 12.3$	$0.9 \oplus 6.2$
$\sigma(WW) \cdot BR(\gamma\gamma)$	$11 \oplus 11$	$10 \oplus 2.4$	$4.4 \oplus 1.2$
$\sigma(gg) \cdot BR(WW)$	$25 \oplus 12$	$6 \oplus 12.3$	$1.6 \oplus 6.2$
$\sigma(WW) \cdot BR(WW)$	$57 \oplus 11$	$24 \oplus 2.4$	$8.9 \oplus 1.2$
$\sigma(gg) \cdot BR(ZZ)$	$5 \oplus 12$	$4 \oplus 12.3$	$1.6 \oplus 6.2$
$\sigma(WW) \cdot BR(ZZ)$	—	$8 \oplus 2.4$	$3.2 \oplus 1.2$
$\sigma(gg) \cdot BR(\tau\tau)$	—	$7 \oplus 12.3$	$1.9 \oplus 6.2$
$\sigma(WW) \cdot BR(\tau\tau)$	$17 \oplus 11$	$8 \oplus 2.4$	$2.8 \oplus 1.2$
$\sigma(Wh) \cdot BR(b\bar{b})$	—	$8 \oplus 3.8$	$4.4 \oplus 1.7$
$\sigma(t\bar{t}h) \cdot BR(b\bar{b})$	—	$35 \oplus 11.7$	$16 \oplus 5.9$
$\sigma(t\bar{t}h) \cdot BR(\gamma\gamma)$	$18 \oplus 9$	$28 \oplus 11.7$	$12 \oplus 5.9$
$\sigma(Zh) \cdot BR(invis)$	—	$10 \oplus 4.3$	$3.5 \oplus 2.2$

notes:

uncertainties for ATLAS are from the ATLAS Snowmass report. Just-released ATLAS results for ECFA are not included.

uncertainties for CMS are estimated by MEP from the results of their parametric fit to couplings. They are not "official" CMS numbers.

This method of estimation brought the LHC uncertainties close to those presented in the ILC TDR.

I accept responsibility for the conservative nature of the TDR estimates.

These were results we actually obtained from full simulation using the GDE's conservative estimates of delivered luminosity for 250, 500, 1000 GeV. This approach is appropriate to a formal proposal, which the TDR was.

The TDR results do not meet the Snowmass criterion for the discovery of new physics in Higgs couplings.

3. Is what the ILC will do good enough, or do we have to go to a new technology that can do better ?

This stimulated a challenge from the other side. A small but vocal group at CERN proposed a circular e+e- Higgs factory in a 100 km tunnel based on super-B-factory technology, TLEP.

The TLEP proponents argued that they could deliver

$$2.5 \text{ ab}^{-1} \times \ 4 \text{ detectors}$$

at 250 GeV, with a construction start at CERN just after the HL-LHC begins operation in 2023.

Higgs coupling measurements in e+e- might still be statistics limited at such high values of integrated luminosity.

There are obvious objections:

No full accelerator design, not even a lattice or interaction region.

New regime of synchrotron operation with very short beam lifetime and constant top-off injection.

No political traction at CERN, but the most optimistic possible timescale.

Early claims that cost is << ILC rejected even by the proponents (e.g. F. Zimmermann) after study.

The most disturbing argument concerning TLEP:

Eventually, proponents of TLEP say,

The best argument for TLEP is that is the first stage of a very large hadron collider at 100 TeV.

To which one might ask,

If what you want is a very large hadron collider, why spend 20 years for TLEP construction and operation ?

In fact, the VLHC at 100 TeV attracted widespread interest at Snowmass. We cannot make the physics case now, but we can start preparing studies for a case based on LHC results from the coming decade.

Response of the ILC community to these challenges:

(with much credit to Tim Barklow, Keisuke Fujii, Tomohiko Tanabe, Jianping Tian, and other authors of the ILC Higgs White Paper)

The TDR is the proposal on the table now, but this is just the beginning. Like LHC, ILC will run for 20 years. Many possibilities for luminosity upgrade are validated in the TDR. ILC will eventually acquire many ab^{-1}.

An anomaly observed in early ILC running can be validated at the ILC with much higher statistics.

So, what is the problem with starting now ???

“Higgs factories” of the Snowmass study: from the public draft of the Higgs WG report

Facility	HL-LHC	ILC	ILC(LumiUp)	CLIC	TLEP (4 IPs)
$\sqrt{s}$ (GeV)	14,000	250/500/1000	250/500/1000	350/1400/3000	240/350
$\int \mathcal{L}dt$ (fb^{-1})	3000/expt	250+500+1000	1150+1600+2500	500+1500+2000	10,000+2600
$\int dt$ (10^7s)	6	3+3+3	(ILC 3+3+3) + 3+3+3	3.1+4+3.3	5+5

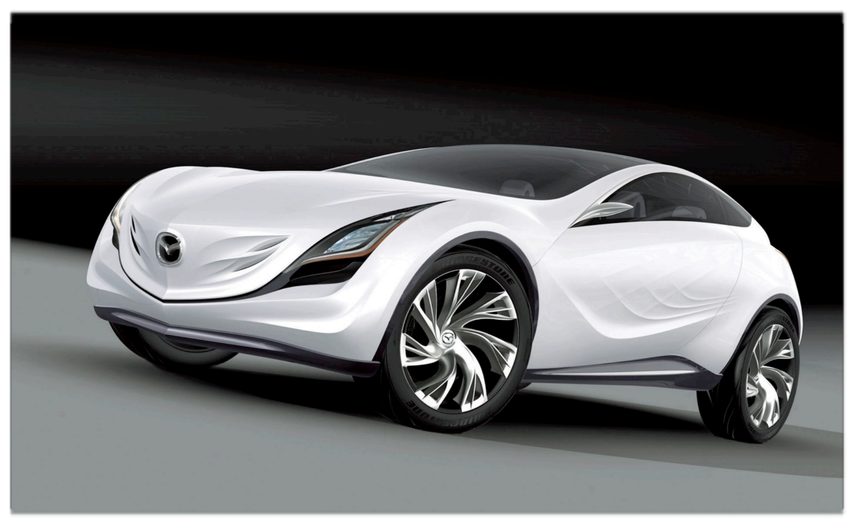
The impressive CLIC report had its main impact on issues at high energy: Higgs self-coupling, electroweakino searches.

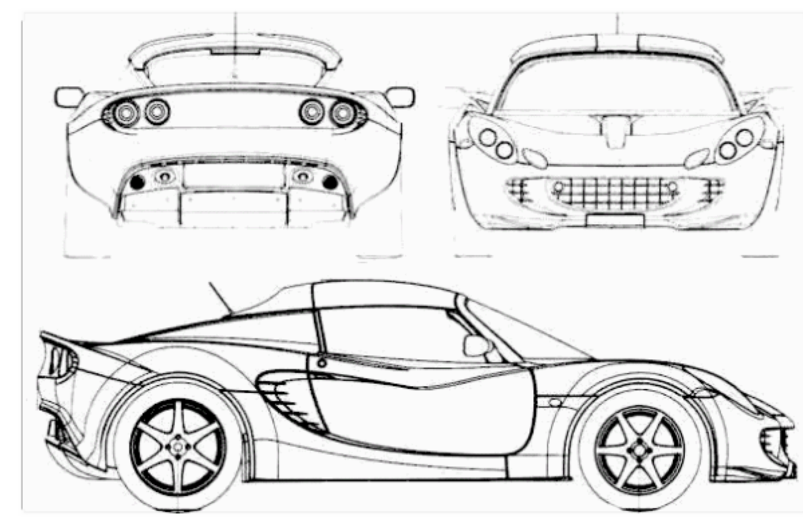
The Muon Collider had minimal impact at Snowmass. Some new results were shown, but still the EF conveners complained that the MC physics community did not show up.

Here are the conclusions of the Energy Frontier and broader Snowmass conveners:

the Proposal Frontier

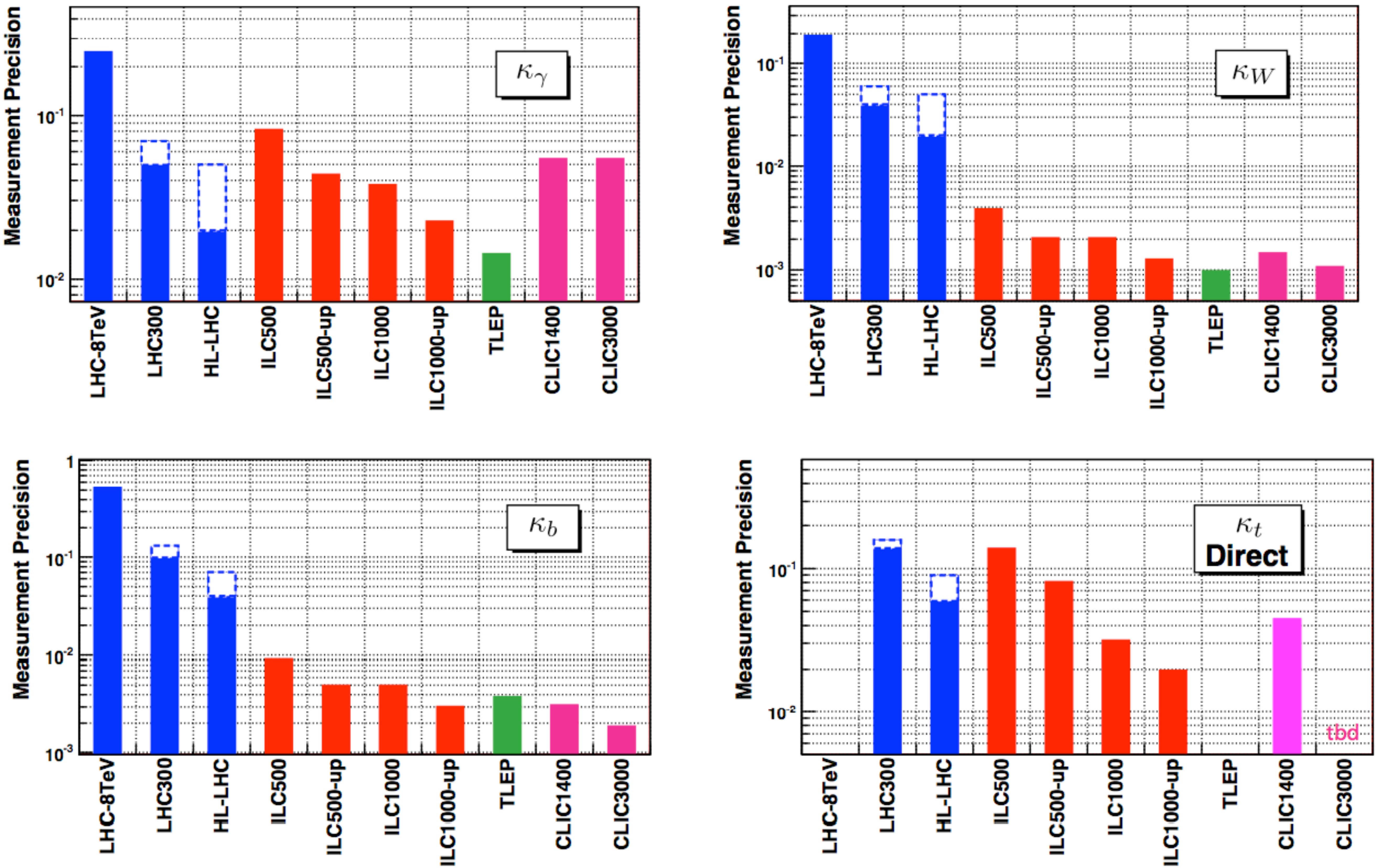
LHC 100/fb	LHC 300/fb	LHC 3/ab	ILC 250-500GeV	ILC 1TeV	CLIC >1TeV	MC	TLEP	VLHC
years beyond TDR	TDR	LOI	TDR	TDR	CDR			





Chip Brock: “the cars” slide

Higgs working group summary of coupling measurements



The Snowmass Executive Summary highlights 3 future Energy Frontier accelerators:

High-Luminosity LHC,   ILC,   VLHC

The HL-LHC statement is a strong endorsement.

The ILC statement is given on the next slide.

The VLHC statement says:
The Snowmass study identified, in particular, the promise of a 100 TeV-class hadron collider (VLHC), which would provide a large step in energy with great potential for new insights into electroweak symmetry breaking and dark matter. The feasibility of such a machine should be clarified through renewed accelerator R&D and physics studies over the next decade.

Compelling science motivates continuing this program with experiments at lepton colliders. Experiments at such colliders can reach sub-percent precision in Higgs boson properties in a unique, model-independent way, enabling discovery of percent-level deviations from the Standard Model predicted in many theories. They can improve the precision of our knowledge of the W, Z, and top quark well enough to allow the discovery of predicted new-physics effects. They search for new particles in a manner complementing new particle searches at the LHC. A global effort has completed the technical design of the International Linear Collider (ILC) accelerator and detectors that will provide these capabilities in the latter part of the next decade. The Japanese particle physics community has declared this facility as its first priority for new initiatives.

Please note that the ILC White Papers on the importance of high precision studies of W, Z, and top were read and recognized by the respective EF working groups and were reflected in the final conclusions. Even the dark matter capabilities of ILC get a nod.

Thanks to all involved !

What lessons should be draw from these results ?

What are the important points to be made to P5 ?

1. The statement that the ILC physics case is very strong has been accepted by the Snowmass study. P5 should not back away from this.

2. ILC must find a way to peaceful coexist with collaborators in ATLAS and CMS whose first priority is the upgrades for HL-LHC.

After all, they are us. We not only have the same physics goals, we are the same people wearing different hats. I consider it a conclusion of Snowmass that ILC is a natural follow-on to the HL-LHC.

P5 should envision a smooth transition of effort from ATLAS and CMS to ILC, with detector designers moving in the late 2010's and physics analysis experts in the mid-2020's.

3. ILC must find a way to peacefully coexist with physicists interested in the VLHC.

VLHC is not happening now. What the community said about ILC in the 90’s is true now for VLHC: The proposal needs to know what the LHC data will say. Much physics study and technology development is needed.

VLHC is a project for the 2040’s. ILC can be the bridge for our community between LHC and VHLC.

4. Electron folks need a vision for what comes after ILC.

This is somehow opposed to the conclusion of the previous slide, but it also speaks to the same notion that the truth will be found at higher energies.

The idea that plasma wakefield or some other technology in the ILC tunnel can reach 10 TeV in the CM (comparable to the VLHC) did not catch on at Snowmass.

But, such a vision is necessary for the new ILC lab to have a 50 year future.

More generally,

We Energy Frontier ladies and gentlemen must stick together.

We are united by a common idea, the idea that we learn more about the fundamental interactions by going to higher energies, producing new particles, and studying them in detail.

This idea is out of favor in Washington, and there is a push against it by many particle physicists.

But, it is a correct idea. Correct ideas, effectively argued, must eventually come to the top.