

Photons First !

a staged approach to ILC
with a first-stage
photon collider

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Last fall, Hirotaka Sugawara proposed to the ILCSC that the ILC formally adopt a new policy in which the ILC would be constructed in a stage approach, with the first stage being a photon collider.

Sakue Yamada asked the new LOI Physics Panel to investigate the physics case for the photon collider stage. Tim Barklow volunteered to prepare a draft report for that group.

In parallel, the GDE asked Andrei Seryi to create some machine designs for ILC stages that would come before the full ILC RDR machine in this plan.

Jeff Gronberg (LLNL), our local photon collider expert, agreed to help us understand the laser technology.

The goal was to prepare a briefing paper for the GDE to discuss with the ILCSC.

At a certain point, this study went rogue.

I felt strongly that the overall program of the ILC needed to be shaken up, and that, in this context, someone should advocate positively for Sugawara's plan.

Barklow, Gronberg, Seryi, and I wrote such an advocacy report. It was presented to the LOI physics panel, the GDE, and the ILCSC. **Everyone rejected it.** So this report is our responsibility only.

Nevertheless, I will now present the arguments and plans to you.

The report is not complete; more studies are needed. You can obtain a copy of the current version from Tim Barklow.

What is the problem to which we need a solution ?

There is no path from where we are now to the realization of the ILC, even in the best case in which the LHC makes major discoveries.

Because of the cost of the ILC RDR machine, it will be very risky to propose this machine **even if the world is in a state of excitement over the discovery of new particles ('SUSY') at the LHC.**

In the current plan, we will need to pour money down a hole for **10-15 years**, with **no physics output**, and with constant vulnerability to cancellation of the project.

Straw-man schedule presented by Mike Harrison to P5 (2007)

US costing; FY 2007 dollars

Year	Funding Type	CD's	Funding \$FY07	Funding at year	Inflator	Host 50%	non-Host 20%
FY11	Program	CD0	150	172	1.148	86	34
FY12	Program		250	297	1.188	148	59
FY13	PED	CD1	420	516	1.229	258	103
FY14	PED	CD2	795	1011	1.272	506	202
FY15	PED		1074	1414	1.317	707	283
FY16	Project	CD3	1492	2033	1.363	1017	407
FY17	Project		1900	2680	1.411	1340	536
FY18	Project		2174	3174	1.460	1587	635
FY19	Project		2300	3475	1.511	1738	695
FY20	Project		2200	3441	1.564	1720	688
FY21	Project		1700	2752	1.619	1376	550
FY22	Project		845	1416	1.675	708	283
FY23	Ops			0	1.734	0	0
Totals (\$M)			14900	21913		10957	4383
			Inflation	3.5%			

The host nation contributes \$1B/yr in constant dollars.

For the rest of this talk, I assume that we are not giving up on ILC cold technology.

The only course, then, is to invent a program that gives **relevant physics results** at an intermediate stage of the project, with the expenditure of a smaller amount of money.

Over time, we will complete the ILC RDR machine and do the full physics program.

The sooner we can start doing physics, the better. **However, the LHC will already be studying 'SUSY'**. We should be in the game; otherwise, we are irrelevant.

How could it be possible to do this ?

Most current models of new physics contain a very light Higgs boson. The LHC will presumably discover this particle. Then the first stage of the ILC can be aimed at a deeper study of the Higgs.

There are two obvious options:

PLC - photon collider studying $\gamma\gamma \rightarrow h^0$

HLC - e⁺e⁻ collider studying $e^+e^- \rightarrow Z^0h^0$ near threshold.

The HLC has a better physics program.

However, for **m(h) = 120 GeV** (assumed from here forward), the PLC needs only an e-e- collider at 160 GeV; the HLC needs an e⁺e⁻ collider at 230 GeV.

The LHC may discover the Higgs, but we will need another machine to make the Higgs real. Let's recall why.

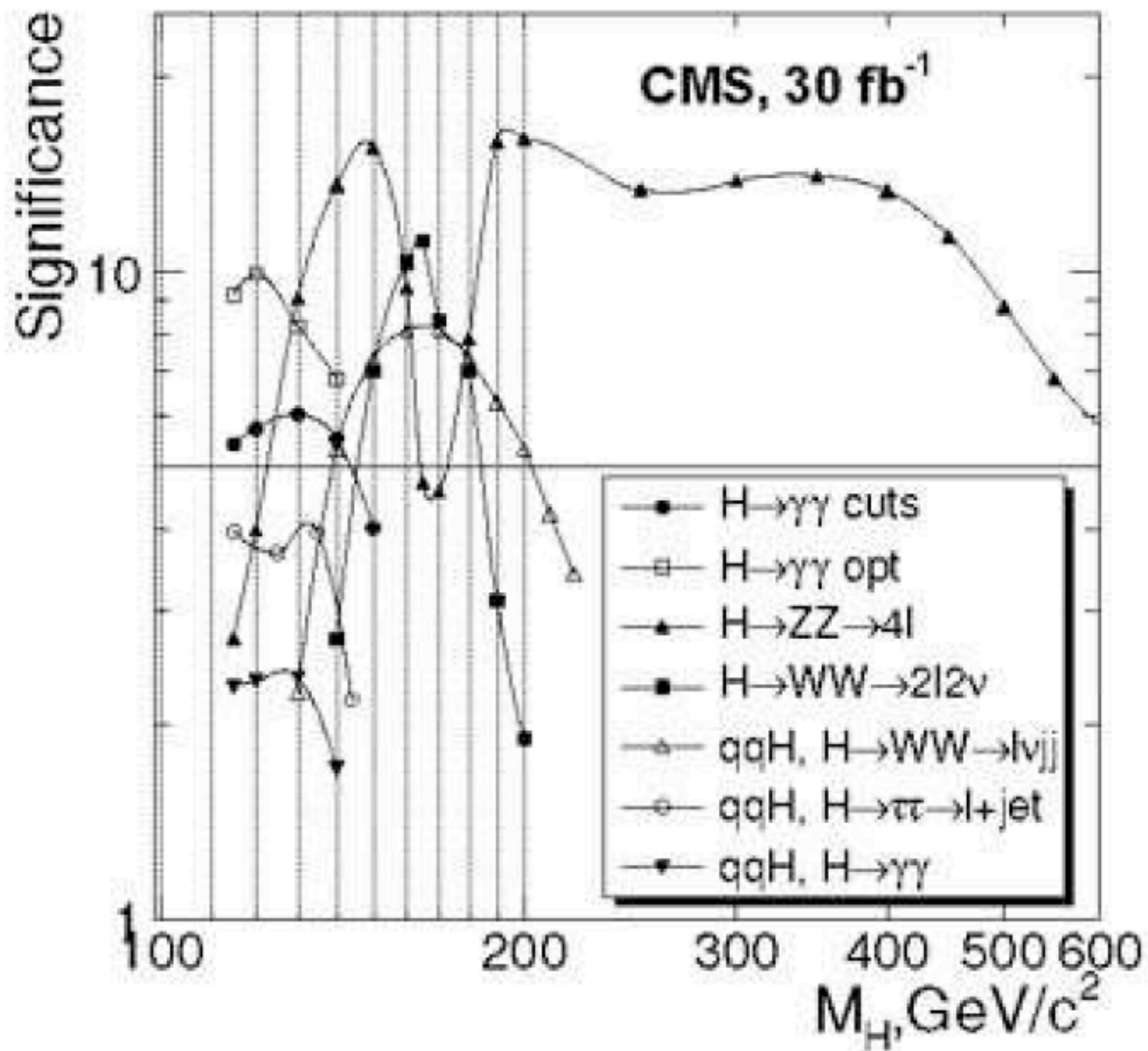
For $m(h) = 120$ GeV:

$BR(b\bar{b})$: 65.74%	$BR(c\bar{c})$: 3.60%
$BR(WW^*)$: 15.00%	$BR(ZZ^*)$: 1.72%
$BR(\tau^+\tau^-)$: 7.96%	$BR(\gamma\gamma)$: 0.29%
$BR(gg)$: 5.50%	$BR(\gamma Z)$: 0.13%

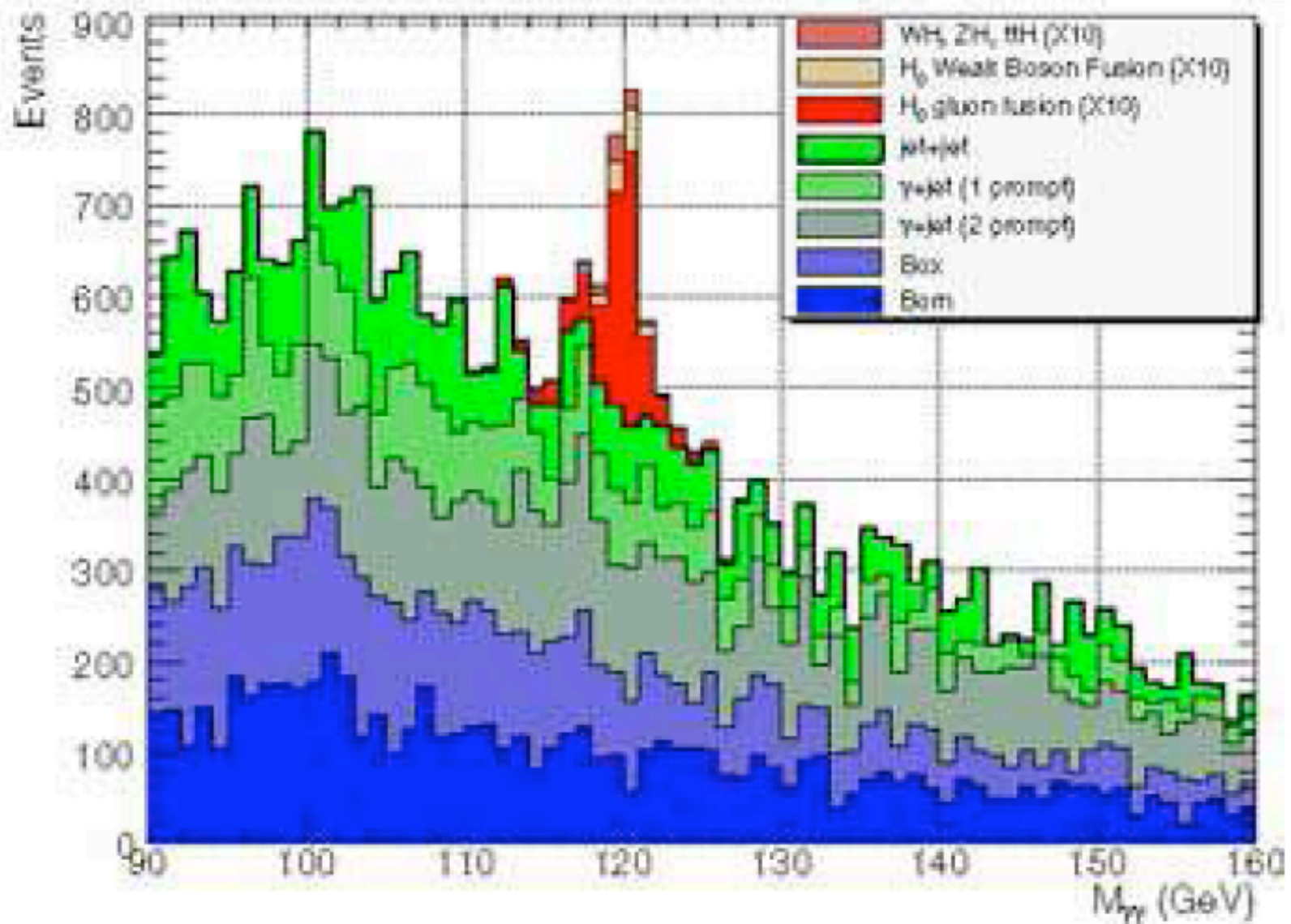
Of these, the LHC can see:

inclusive h: $\gamma\gamma$, ZZ^* **only**

WW fusion: $\tau^+\tau^-$, WW^* **also**



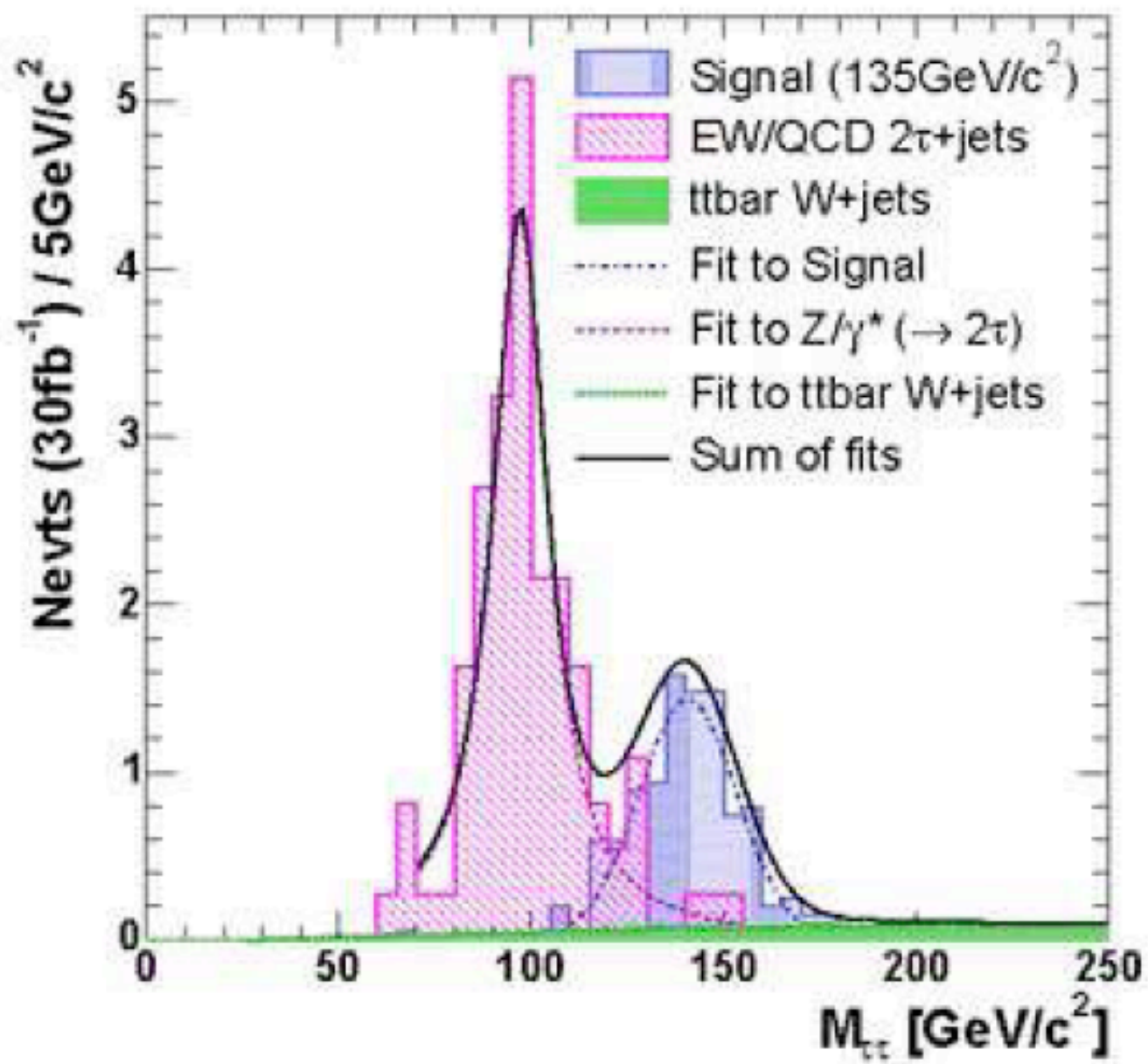
CMS $h^0 \rightarrow \gamma\gamma$ 7.7 fb^{-1}



signal x 10 for visibility

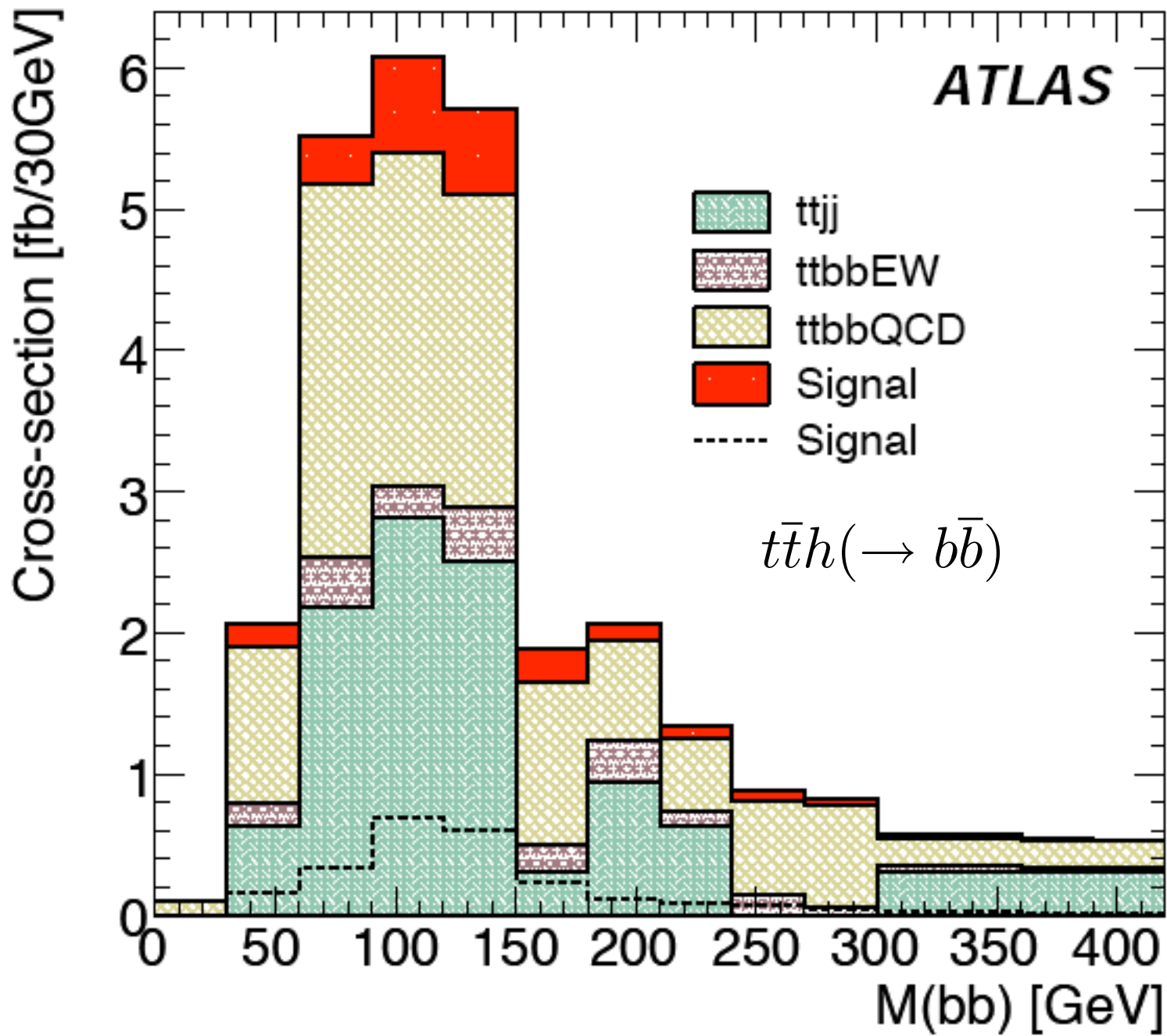
CMS

$$h^0 \rightarrow \tau^+ \tau^- \quad 30 \text{ fb}^{-1} \quad m(h) = 135 \text{ GeV}$$



The LHC really cannot see $h^0 \rightarrow b\bar{b}$?

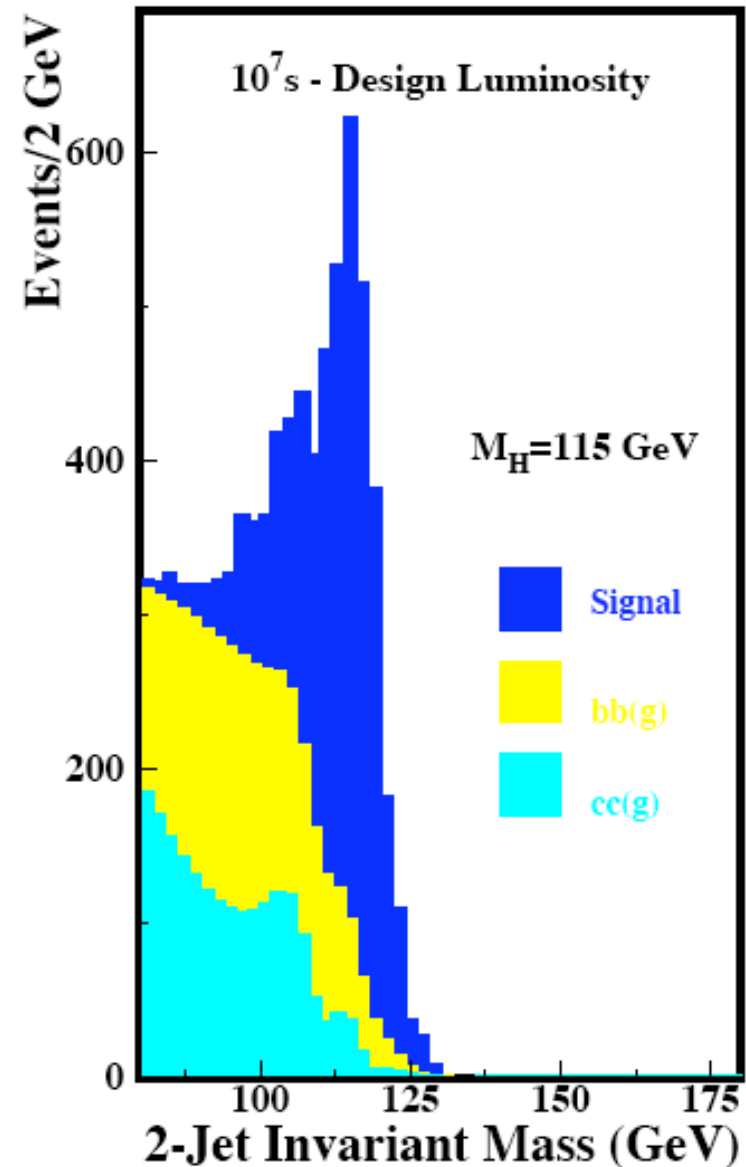
The best hope is to look for h^0 in association with top quarks.



The main goal of the PLC would be to observe the Higgs in its dominant decay mode $h^0 \rightarrow b\bar{b}$.

CLICHE study: 200 fb⁻¹

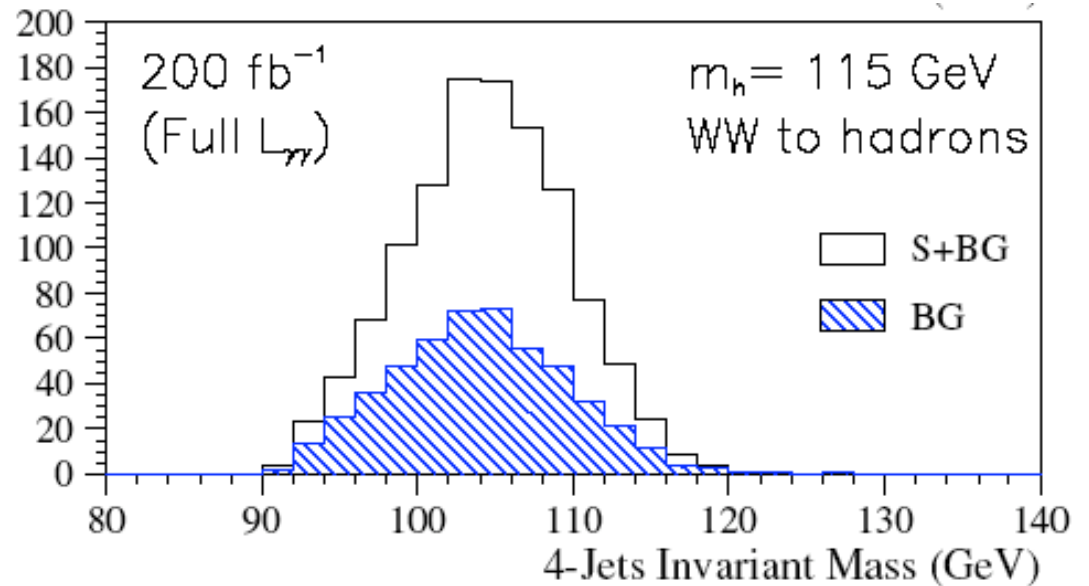
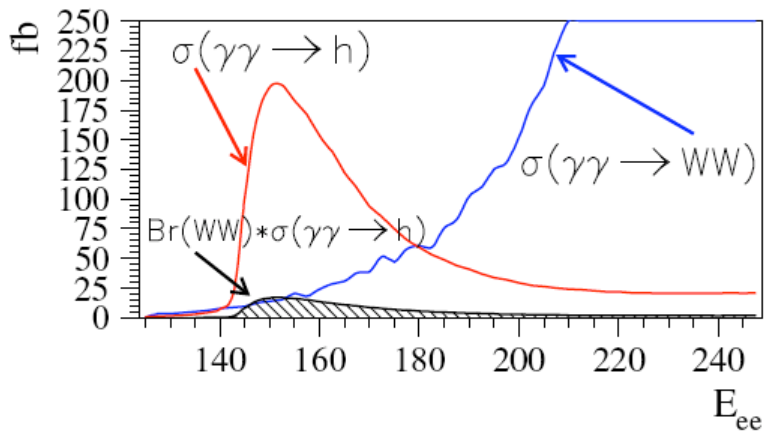
similar results from TESLA,
ACFA studies



It is not possible to study the Higgs decay modes comprehensively at the PLC.

available: $b\bar{b}$, WW^* , ZZ^* , $\gamma\gamma$

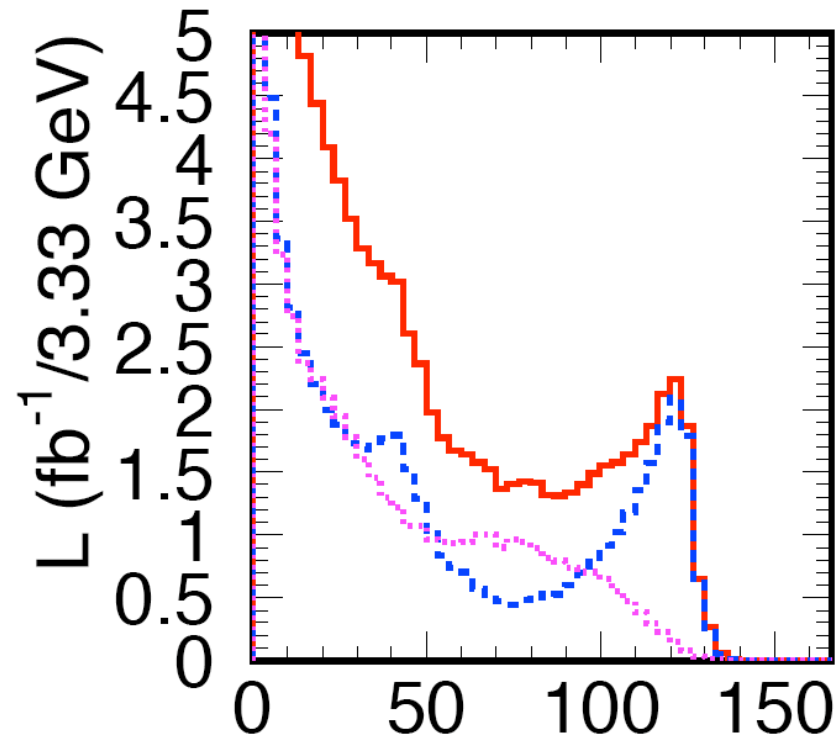
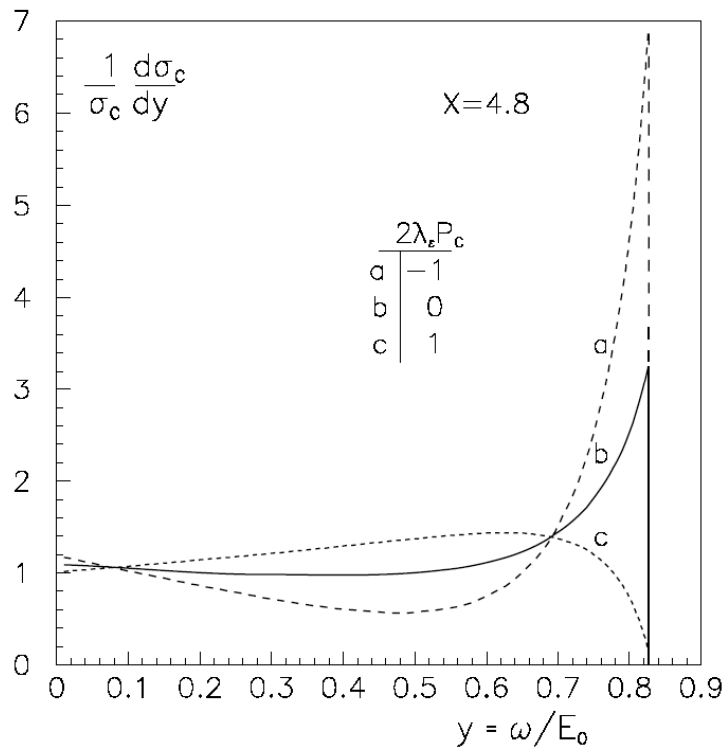
not available: $c\bar{c}$, $\tau^+\tau^-$, gg , invisible



Beam polarization is crucially needed for these measurements.

to give a photon spectrum peaked at the resonance

to suppress $\gamma\gamma \rightarrow b\bar{b}, c\bar{c}$ SM background



Unfortunately (from a cost viewpoint),
we cannot do without the e- damping ring.

As you know better than I, the HLC can measure absolute Higgs branching ratios, gg channel, invisible Higgs decays, ...

How can we compare LHC, PLC, HLC ?

Assume that h^0 is a mixture of SU(2) singlets and doublets only.

From this mild assumption, it follows that

$$g(hWW)/g(hZZ) = \cos^2 \theta_w$$

$$\Gamma(h \rightarrow WW) \leq \Gamma(h \rightarrow WW)|_{SM}$$

We measure $\sigma \times BR$

This is proportional to $\frac{\Gamma(h \rightarrow A)\Gamma(h \rightarrow B)}{\Gamma_{tot}}$

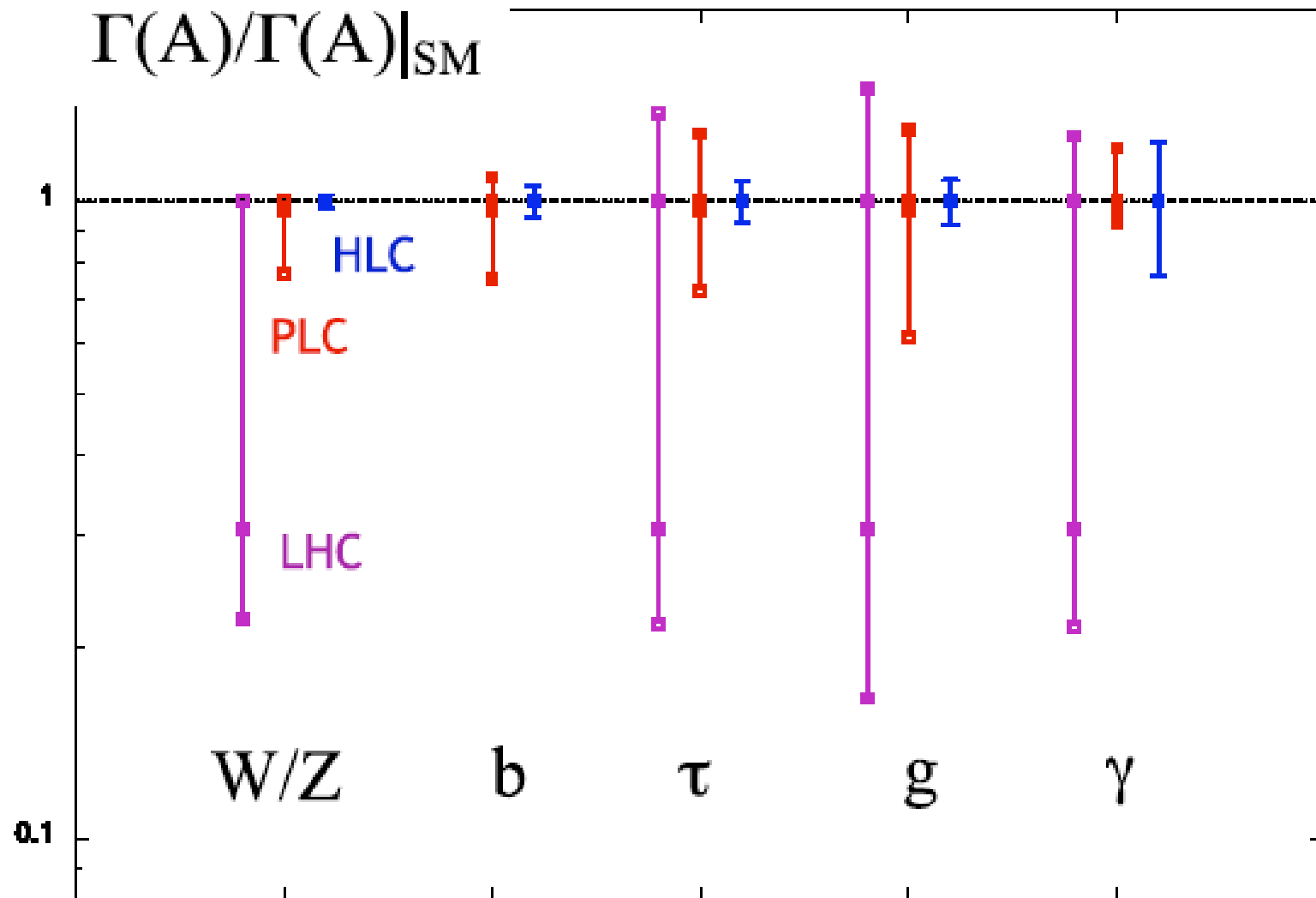
But, even if there are unobserved Higgs modes,

we get an **upper bound** on $\Gamma(h \rightarrow WW)$ from the previous slide.

we get a **lower bound** on $\Gamma(h \rightarrow WW)$ by assuming that unobserved Higgs modes do not exist.

Then we can compare **LHC, PLC, HLC** in their ability to give model-independent values of the Higgs couplings (up to our mild assumption).

For realistic integrated luminosities, here is the picture:

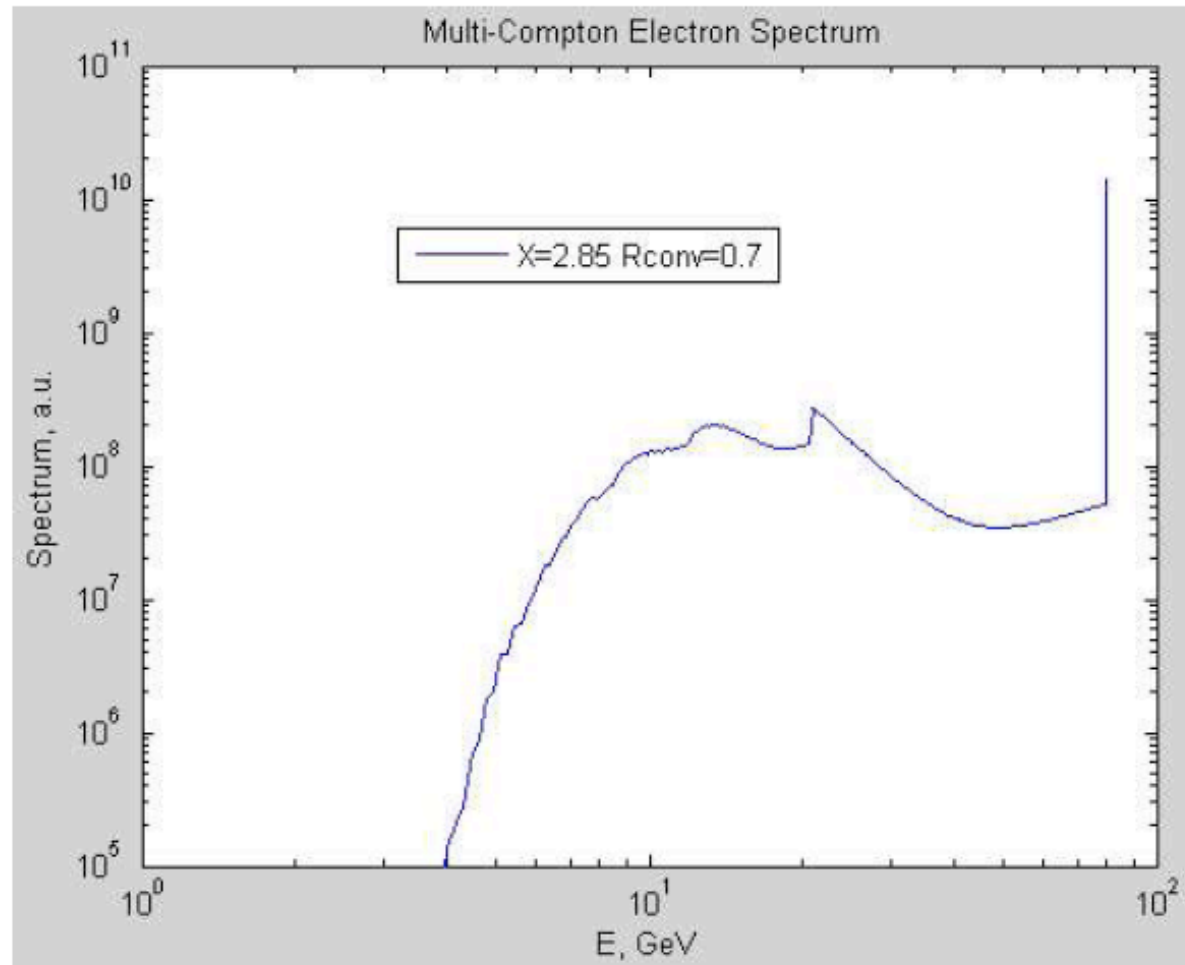


Now discuss the realization. **Andrei Seryi's** staged plan:

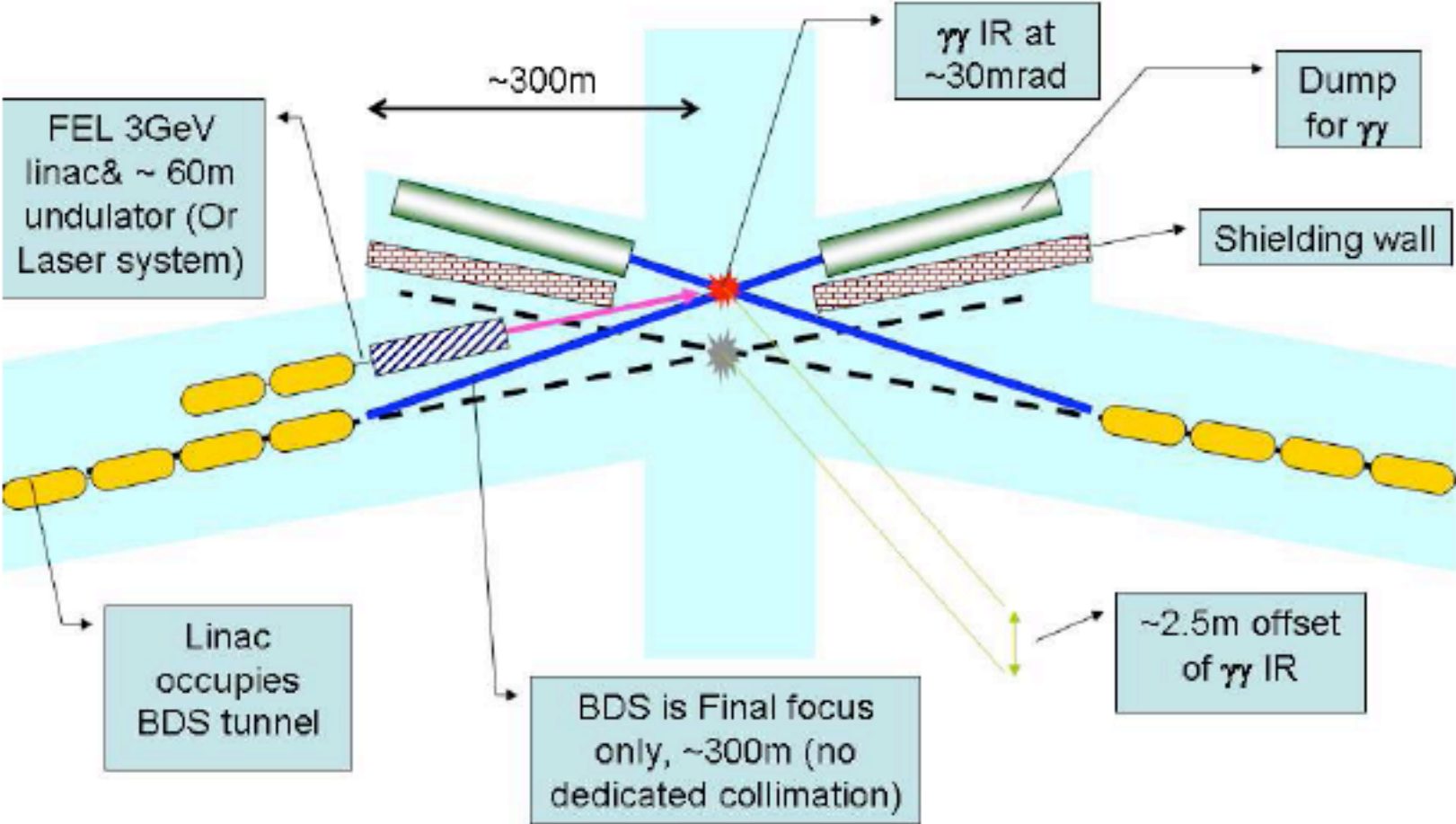
Stage 1	180 GeV	e-e-	2 yrs	Single DR
Stage 2/3	180 GeV	e-e-	2 yrs	Faster kicker or two DRs
Stage 4	230 GeV	e+e-	3 yrs	lengthen BDS
Stage 5	500 GeV	e+e-	5 yrs	ILC RDR machine
Stage 6	500 GeV	e-e-	2 yrs	final photon collider stage

Why 180 GeV, not 160 GeV ?

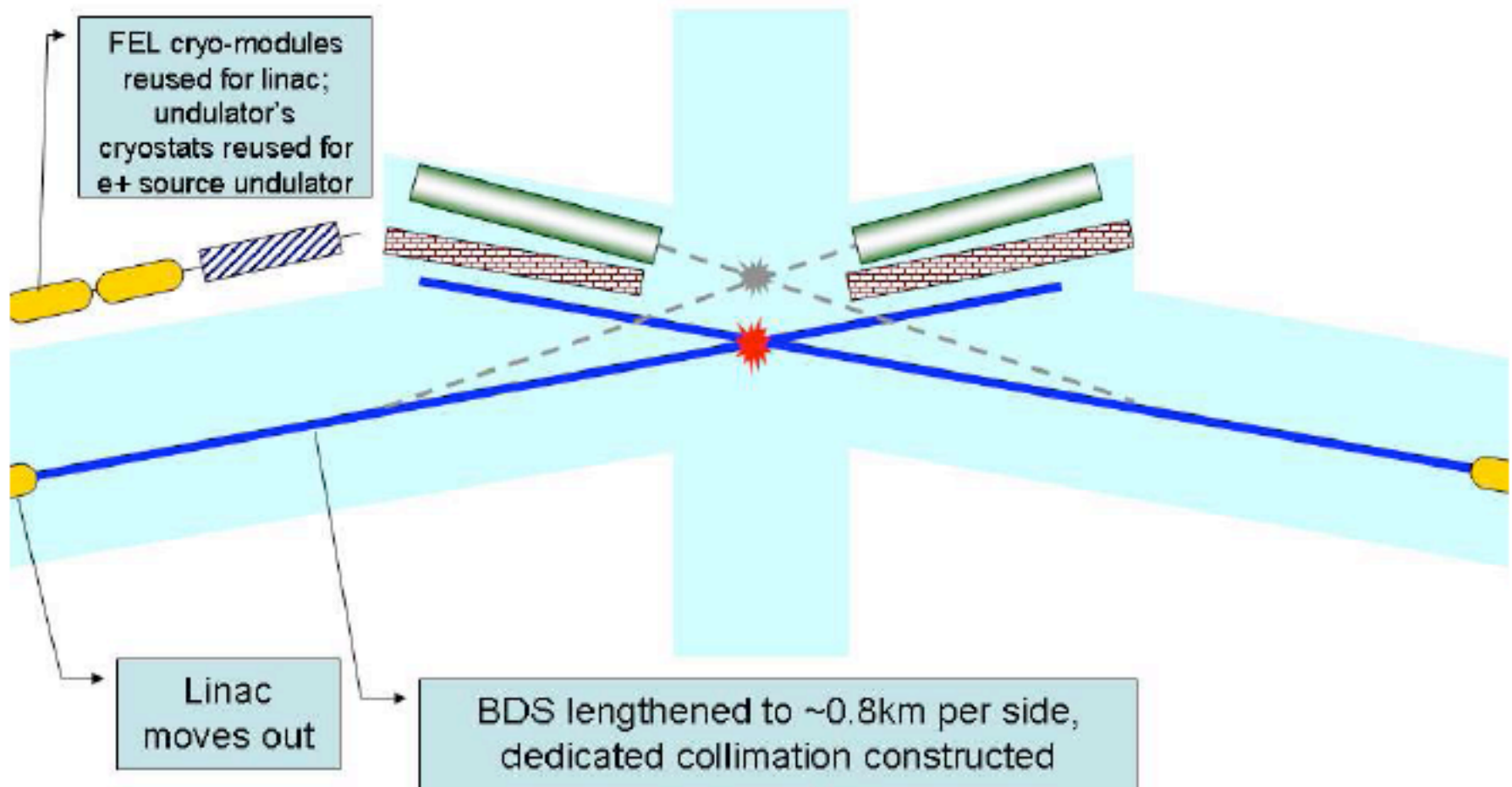
We were conservative. Nonlinear Compton processes give many low-energy electrons. These may not fit through the beam exit hole and might instead melt the final quads. This problem needs more study.



Here are some sketches of the linac evolution:

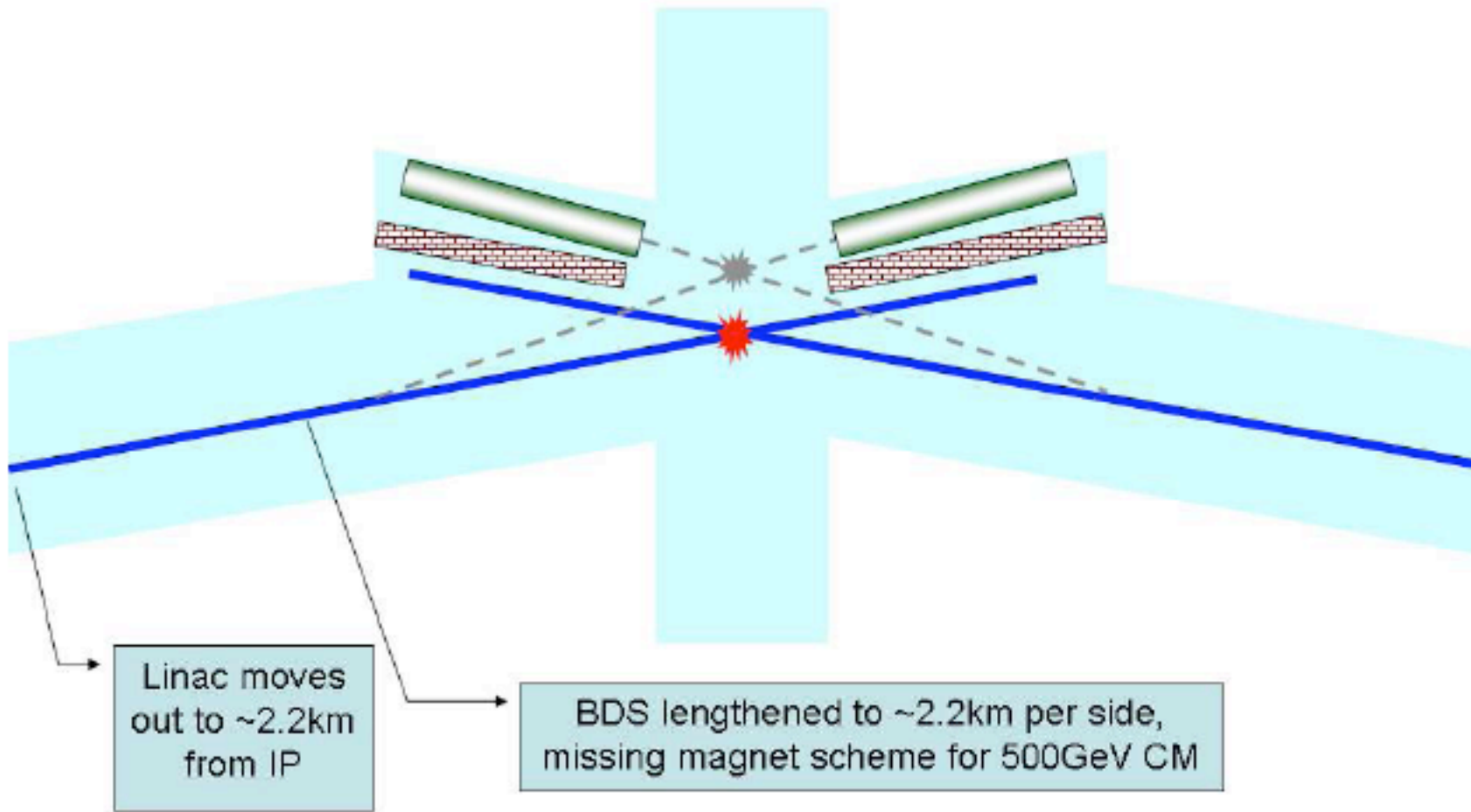


stage 1 at 180 GeV



180 GeV to 230 GeV conversion

Are these changes possible in a 1-year shutdown? Last year, there was similar skepticism about push-pull.



230 GeV to 500 GeV (ILC RDR) conversion

Some more reality:

Peter Garbincius costed these schemes in the ILC costing framework.

PLC costs **52%** of the ILC RDR machine.

HLC costs **67%** of the ILC RDR machine.

So we are not yet realizing the goal of a cheap and easily constructed intermediate stage.

Some key technologies are needed to make this work.

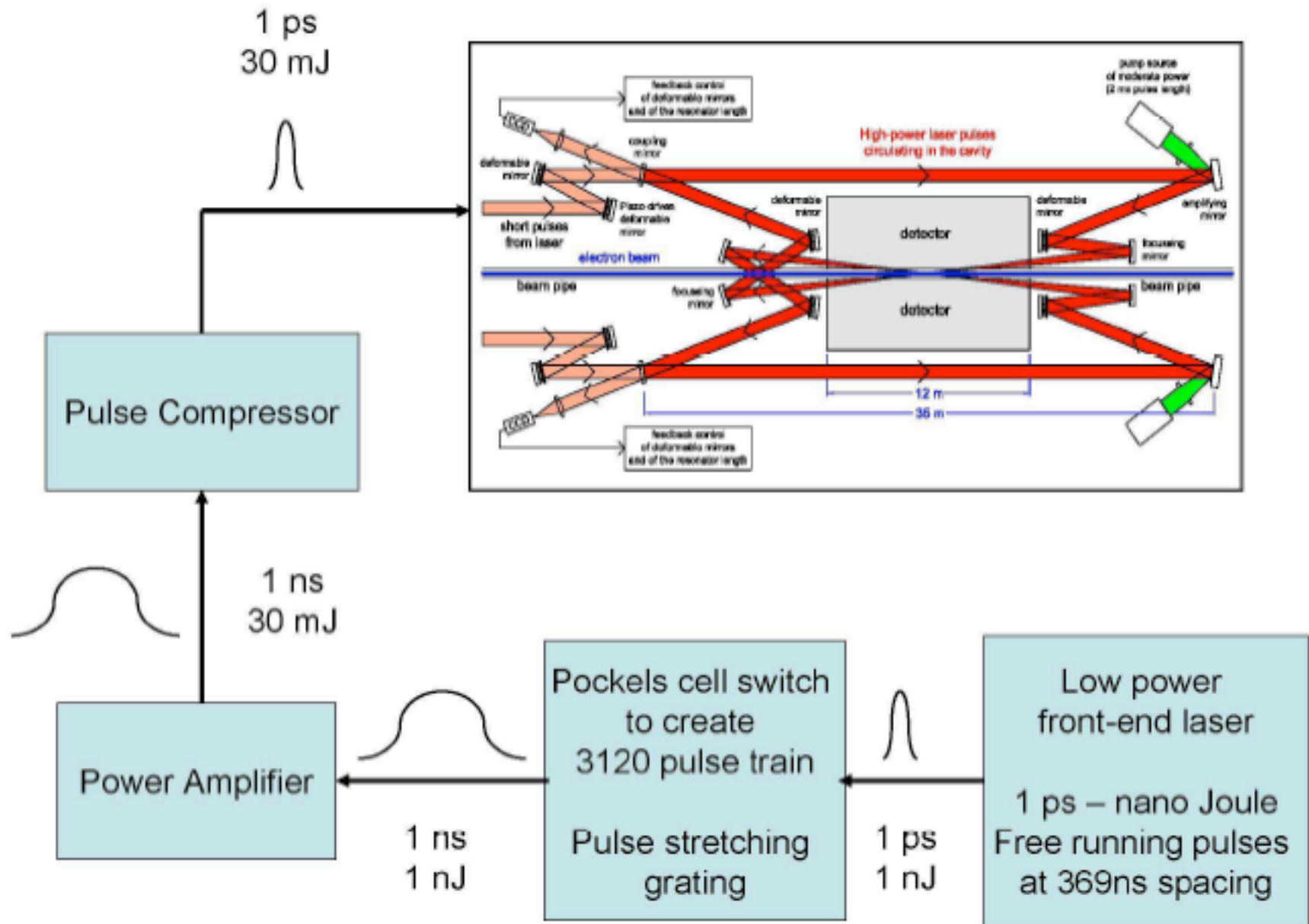
For the photon collider, we need to intersect photon bunches of

5 J, compressed to 1 ps

With the ILC time structure, we can imagine an optical cavity whose round trip time is the bunch spacing (480 ns). With $Q \sim 125$, this requires

40 mJ/shot, 5 X 3820 shots/sec

Note that the cold machine time structure makes the problem of the laser easier. The MERCURY laser at LLNL is close to the required kW output. An FEL laser solution is also possible.



DESY-Zeuthen / Max Born Institute optical cavity design

This brings up the question of orphaned technologies.

We could develop this optical cavity technology, but only if someone will stop working on 'ILC minimal machine' and build a prototype. IMHO, the arguments that this technology is more difficult to develop than other ILC paths has a strong flavor of 'not invented here'.

There is an even more important orphaned technology. This is **polarized e- RF gun** .

Current, we have guns that produce polarized e-, and RF guns with emittance comparable to the ILC specification. However, these two features do not yet coexist.

If we could develop a polarized RF gun, we could eliminate the damping ring and its conventional construction. This would save the first-stage program \$ 1 B!

Finally, we come to the really difficult question:

Doesn't a staged program with the same eventual goal of studying 'SUSY' with precision e^+e^- experiments just increase the total cost of the project ?

Of course, the answer is yes.

A staged ILC must be proposed in a different way than a project. It must be proposed as a Laboratory that will exist into the future. We will seek incremental funding for each stage.

This is the right way to think about funding of an accelerator. However, this language does not mesh easily with that of (e.g.) the US DOE.

Because of its unique constitution and structure, CERN can plan a linear collider program in this way.

If we would like a site other than CERN, we need to be creative in finding the right way to approach the host government. I do not have a good idea of how to do this.

Conclusion:

I believe that the idea of staging the ILC is an important one that needs intensive thought and planning.

The **photon collider** is a potentially interesting approach. However, we need to develop the technologies that will make this machine straightforward to build.

This is part of a plan. I don't have the rest of it.

Your creative ideas are needed.