General Comments:

I don't comment on typos, minor language things unless they affect the meaning.

I did not read Chapters 3 on the ILC environment or Chapter 6 on Integration this time around, and only had a cursory look at Chapter 9 on costs.

Overall, this document reads very well and has a consistent level of treatment (one caveat (in Chapter 5.2) to this appears below).

Being old with eyesight that is definitely on the decline makes me more susceptible than younger people, but there are many figures that need to have larger fonts, particularly for the legends within the panels. This an endemic problem throughout the document. As a particular example, see Fig. 8.1 on p. 100. Even when I magnify one of those panels on my computer screen I cannot really decipher the legend completely. In many cases, the legends are essential to understanding the message being conveyed. So either enlarge the fonts, or do not pack so many figures to the page.

The text font, though readable, gives me some eyestrain as well.

The physics examples often invoke 500 GeV or 1 TeV running, whereas of course one is now maintaining the posture that the ILC will be a 250 GeV machine. Is this implicit message that we will be back for upgrades politically palatable in Japan? (*Actually I found your discussion of this on p. 109 and that seems fine. You might however want to say something li ke this in Chapter 1 or 2.).*

It occurred to me that you might discuss possibilities for staging in the event that the funding is initially too low. Do you see ways to gracefully stage some elements of the detector but to add them later during moderate shutdowns? Or do you want to hide from that discussion at this time.

Chapter 1, Introduction

You mention that ILC will operate in the range between 90 GeV and 'about' 1 TeV. However, there is no mention of the physics specifications for the program and performance at the Z pole or at the WW threshold, nor the requirements from the physics on the accelerator to make such running useful (luminosity, energy resolution, energy stability/calibration accuracy). You may not want to get too deeply into this, but some words to at least justify your mention of the low energy program would be useful.

Chapter 2: Science with ILC

This chapter is basically fine, but

p. 5, lines 4-8: In Fig. 2.1 (right) and the text you show other Higgs like states with the same ZH coupling that could be observed. But would not such states be sensed and measured at LHC? I guess that if you invoke that such new states only decay to quarks or tau's, the LHC resolution might not be good enough. But it seemed to me that this plot raised red flags that you need not do.

Chapter 4, ILD concept

The justification for ILD-S with the same length in z for the TPC is good. But the discussion left me wondering about other possible modifications to simplify/reduce cost. Some of these (e.g. leave out the FTD initially) could be retrofitted later so are not so much hard choices to make at the outset, but more scope contingency.   But you could consider also such things as increasing the thickness of the ECal absorber and having fewer silicon planes that would not be retrofitted.   The ECal silicon is after all a large cost. It seemed to me that some discussion of why you focused on the option of a reduced size TPC would help, even if you choose not to examine other options in detail for the reduction in performance.

Chapter 5, Detector Layout and Technologies:

Just to reiterate, the text in Fig. 5.6 is another example of the need to enlarge figures.

When I read Section 5.2 on status of detector R&D, my reaction was that this is trying to snapshot a series of programs that are all in flux, so whatever is written here now will become obsolete before too long.   **There** is a fair amount of detail in this section compared to others and yet there are things that are not so easy to follow.   I am not sure what to recommend.   A thought came that you might make this an annexe that you refer to, but that can be updated separately as new understanding of technology and R&D results come in.

p. 41, Fig.   5.29 right: you have two things being varied (with and without an ECal in front, and AHCAL vs. DATA). It is not clear what distinguishes DATA and AHCAL here.

Chapter 7, Modelling:

p. 94, line 9: change (right) to (left)

p. 94, l.13: change (left) to (right)

p. 94, Fig. 7.3 left: most previous experiments have underestimated the amount of material in their tracker volume. How do you know that your estimate is good?   Or, looked at in a different way, do you have some physics simulations to compare the degradation if the amount of material is increased (e.g. the Z -> mu mu)

p. 95, Table 7.2: Are your assumptions about the point resolutions buttressed by any test data? It would be good to indicate why you think these numbers are OK.

I had the same sort of comment about the choice of jet energy resolution, EM energy resolution etc.

p. 95, lines 16-19: I did not understand what you say here about the two stage 'calibrations' For me, this text needs some work.

Chapter 8, Performance:

Fig. 8,2(d): It would be good to insert a comment in the text about this figure, and the immunity of the efficiency on added salt and pepper noise.

p. 102, lines 13 and 19: You should explain how, if "Z decays at rest" you can have jets of "250 GeV" !!

p. 102, Eqn. 8.1: Define rms_90 and mean_90.

p. 102, line 32:   I guess you mean e+ e- - c cbar ???

p. 107, line 6: What Feynman diagram do you have in minds for   e+e- -> 6 q? where all 6 quarks have the same flavor??

p. 107, lines 15-16:   I did not think the sentence “In practice … best performance.” added much; maybe delete?

p. 108, line 8: define PFO

p. 108, line 21: the product of efficiency and purity seems a bit of an odd metric to quote.

p. 108-109, Section 8.2.3: I found this section pretty underwhelming. If ILD cannot do well with Jpsi,it would be pretty amazing! If you keep this section, at least give the dimuon mass resolution that you get for the L and S detectors at the Jpsi mass.

p. 109, Section 8.3: I tried to think if you left out any important physics channels. I came up with (1) anomalous ttZ/ttgamma couplings, (2) top mass at ttbar threshold, (3) ttH, and 4) H self coupling. On thinking a little, I would think that for your purposes the ttH measurement in the case that both tops go to 3 jets and H goes to bb (the largest BRs) would be interesting as a study of complex final states with many jets (bbbbqqqq); how efficient are you, how well do you tag the b-jets. Process (1) is interesting physics but would stress similar things to ttH; Process (2) really depends more on machine energy calibration than detector properties, and (4), though a holy grail for physics again depends on similar things to ttH.

Some of the processes you do consider may not be of first importance for physics, but do stress the performance; e.g. Higgs mass from Z(jj)H(mumu).

So I appreciate that your studies are chosen so as to stress some aspect of the detector/reconstruction rather than as the highest priority physics topics. I suggest however that you stress this point a little more clearly than you do now (p. 109, line 22). You might consider mentioning (or a table) what detector system is being tested with each of the studies you present.

p. 110, lines 6-8: This statement is a bit too Delphic for me to understand.

p. 110, line 12:   Don’t call these decays ‘hadronic’.   They are quark decays, or if you prefer ‘non-leptonic Higgs decays’

p. 111, text: It might be interesting to mention what mass resolution you get for H->bb.

p. 112, Fig. 8.13 left: What is the meaning of LCFiPlus?

p. 111, 25: The placement of the footnote reference (‘2’) makes it look like ‘GeV^2’. Can you move the 2 somewhere else?

p. 112, 20: It would help the reader to have a brief reminder/summary of the ‘approach used for the recoil analysis’ (I read this section a couple of days after the earlier text, so needed a little help.)

p. 112, 27: I expect that in reality, the processes Z(l+ l-) H(mumu) and even Z(jj) H(mumu) will add considerably to the measurement precision for BR(H(mumu)), since you would be able to use the Z mass constraint.   Thus it is artificial to restrict to Z(nunu). I would make a clearer statement that you are artificially trying to get at the mass resolution of the muons, not the ultimate precision on the dimuon BR.

p. 114, 5: ‘high-energetic’ is not a good choice of words

p. 114, 12: you should specify the sqrt s and integrated luminosity corresponding to these 33 events.

p. 115, 8: what is 'wastly'?

p. 115, 12: I think that there is to much stress here on the nunu mumu channel as a testbed for the muon momentum resolution. Since the BR measurement would use all possible Z decay modes, the 40% that you get from the nunu channel alone is not very interesting, and is hard to use to assess the importance of muon resolution. The better process to test muon resolution would seem to me to be the Higgs mass in Z(mumu)H (section 8.3.2)2220 00000. If I am not missing something, you could consider omitting section 8.3.3.

p. 115, Fig. 8.18: define 'sld'. Also, why do you take the positron polarization to be 100%? Does it affect things if you use 30%? Same question on P(e-)=100%.

p. 115, 31: what are 'overlay backgrounds'?

p. 116, 5: define 'BS'

p. 116, 20 and 22. I wonder if your simulation precision is really good enough to claim 1% (or 2%) relative difference between the limits on BR_invisible.

p. 117, Fig. 8.20: what is 'smear'?

p. 117, Fig. 8.20: I like this figure! I had written earlier in this section that I would like to see the derivative d(BR)/d(sigma Ejet), and here it is. It would be nice to show such information for other measurements as it is not usually clear a priori how much the measurement parameter you are testing affects the physics observable.

p. 117, line 8: I had noticed several times before but mention now only – the use of the word 'Thereby' sounds strange to my ears. In this instance, the word could be omitted. A similar case could be made for 'Therein' on line 20 of p. 118.

p. 119: Section 8.3.6 is nice!

p. 121, Fig. 8.25: here is another example where the plot symbols are too small. Using a magnifier I think that the symbols are either squares or circles but I am not sure. Also, when printed out, the difference between cyan and green is very hard to see.

p. 122: Section 8.3.7 is nice!

p. 122, 12: It would be helpful to explain what the four possible combinations are: from the text, I did not get the same impression as from Fig. 8.28.

p. 122, 24: text after 'statistical uncertainty' is garbled

p. 122, 25: 'migration' from/to what ?

p. 124, 6: delete one 'and'

p. 124, Fig. 8.30 and associated text: Are these purities for the all hadronic channel, semileptonic channel, both? What is meant by Vtx + K' (4^{th} entry on abscissa) (or is that K' something else like K*??) What is the Lcut in the last two points?

p. 124: The top pair process of section 8.3.9 and the bbbar process of 8.3.8 both seem to be addressing the ability to tag the quark vs. antiquark character through identification of charged kaons. Why are both processes discussed? (I guess that you did the ttbar process more so as to be able to show Fig. 8.33 and demonstrate the superiority of ILC with polarization over FCC and HL LHC, rather than to demonstrate the ability of K tagging. If that is the case, this is an example where giving the derivative d(anomalous coupling/d(K tag purity) would be useful.)

As noted above, it would be useful to expand a little and describe the various methods for tagging quark vs. antiquark with the two (three) types of information.

p. 126, Section 8.3.10 could benefit from some editorial work.

p. 124, 12: I don't understand that the momentum and angle distributions differ significantly. Of course they do as they are different variables!

p. 124, 12-13: I don't understand the phrase 'also the difference between L and S varies between different samples'

p. 128, 10: is the analysis discussed here and shown in Fig. 8.36 for sqrt s = 500? Should specify.

p. 128, 11: Here you use 'S_0' for the scalar but above you used 'h'. Make if consistent.

p. 128, 14-15: the passage 'results are, however, limits by the avaible SM' is garbled.

p. 128, 17: can you give any information on how badly the limits degenerate as the ISR photon ID probability is reduced?

p. 129, Fig. 8.36: the message would be clearer if you had the sqrt s = 250 result on the same plot. (I am presuming that this plot is for sqrt s = 500.)

p. 129, 11: You cite the higher B field in IDR-S as being the dominant effect, but Fig. 8.37 actually shows the efficiency in IDR-L being larger!

p. 131, 7: insert 'angles' after the first 'different'

p. 132, Fig. 8.40(b) is not such a useful addition to the linear scale plot.

Chapter 9, Costing

Page 134, line 1: not including spares seems odd.

Table 9.1: remove the last digit in the column 'Total'

Table 9.6: it would be worth reminding the reader what the 'classical' solutions are.

p. 143, line 42: I doubt that the existing Ref. 161(Positive Ion Effects) is the intended reference for other physics performance analyses.