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Date:	7 May 2008	Refer to:	L080051-01
Subject:	Response to committee comments and questions on Systems Design, T010075-01		
To:	Advanced LIGO Systems PDR committee		
From:	Peter Fritschel, Dennis Coyne		

1. General Comments: There was little discussion of Input-Output Optics including the Input Mode Cleaner, Phase Modulator, Input Faraday Isolator or the Output Faraday Isolator. (EG)

You mean in Section 3? There’s nothing on the laser here, either. We could add a subsection here, if that would help.

2. (1)Last sentence first paragraph. “..all three interferometers could in principle be operated with the same sensitivity”. Question: Does this mean that they might not be? Comment: A justification for why this major change to three identical interferometers should be given. (EG)

Yes, they might be operated in different, e.g. from the set shown in Fig. 2. Regarding increasing H2’s length to 4 km: will add some text to the document; justification is of course better sensitivity with 4 km.

3. (2.1)“The goal sensitivity for Advanced LIGO is defined in refs. [1] and [2] as: ... strain noise of 10^{-22} RMS...” Comment: Table 1 of reference [2] Advanced LIGO Reference Design (LIGO-M060056-10-M) has the number as 4×10^{-23} . (EG)

Yes, that’s the ‘anticipated minimum strain noise’. The requirement is given in the first paragraph of section 1, as $1e-22$ rms.

4. (2.2) Paragraph 1: “Lessons learned from initial LIGO about potential sources of glitches must be taken into account.” Comment: Please list these lessons and explain how they are being taken into account. (EG). It would be useful to make a particular statement about limiting correlated accidentals in H1 and H2, in addition to some quantitative singles rate criterion for all three interferometers. Granted, the influence on subsystem design is admittedly tenuous; however this manifesto (like the fabled SRD) will presumably live on to guide commissioning tradeoffs. (MZ)

5. “The detector availability goals for initial LIGO are adopted for Advanced LIGO; these are”
Question: Why were these goals adopted? (EG)

Seemed to be the best choice out of the following options: say nothing about availability goals; set higher goals (after all everything else in AdLIGO is better); set lower goals.

6. (2.4) Many iL PEM systems have fallen into disuse, evolved, diverged markedly between the sites, or were never completed (e.g., gamma-ray detectors). Rather than “no additional functions required”, please list the *required* functionality so unneeded systems can be decommissioned and those with mission priority can receive necessary attention. (MZ)

This sounds like a good suggestion. Propose we ask the Detector Characterization group to come up with the ‘required functionality’ list, and go from there. (SYS PDR committee input welcome too, of course.)

7. (2.5) “... strain amplitude calibration accuracy, +/- 2%;...”. In reference [4] [Strain Calibration in LIGO](#) (LIGO-T970101-B-D) the Introduction says “This document discusses the gravitational wave strain calibration in LIGO. Section 2 shows that requirements of ~2% in amplitude ... will be of sufficient accuracy for most of the physics derived from early detections.” Question: Will this calibration accuracy also be of sufficient accuracy for all of the physics not only for early detections but for the regular detections expected in Advanced LIGO? (EG)

For distance measurement, from T970101: “Requiring a 2% calibration accuracy guarantees that the statistical uncertainty dominates the error budget for signal-to-noise ratios at and below ~30.”

Paragraph 2: “The gravitational-wave data channels will be recorded at 16384 samples/sec with a bandwidth of 7400...”

Question1: How was this data rate determined?

Question2: If it is necessary to damp the internal test mass modes at frequencies higher than 7400 Hz will there be separate high frequency channels available for this purpose? (EG)

The rate was chosen for initial LIGO, in order to have a signal bandwidth of ~10 kHz. I think there were some advantages given to it being a power of 2. At this point, a large part of our software assumes the data rate is a factor of 2, so we should not deviate from this. There doesn't seem to be any point in going to 32 kHz. Answer to Q2 is yes (we currently also sample the AS port channels at 248 kHz).

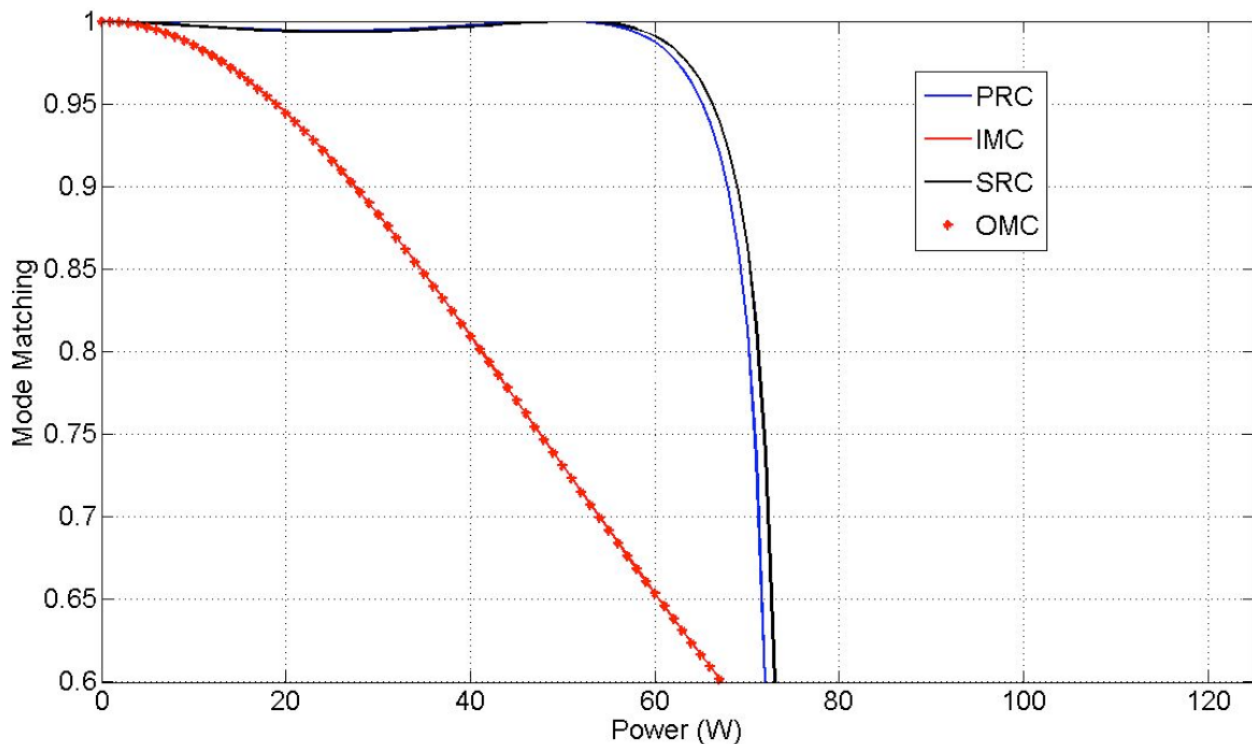
8. (3.1) Modes of Operation. “We propose to capitalize on these hardware improvements as rapidly as possible by taking a staged approach to the interferometry.”

Question: What argues for four modes of operation. For example, why not two modes - modes 0 and then a combination of mode 1-3 with a variable signal recycling mirror as discussed in Section 3.2. (EG)

Four modes is just the result of taking: something really simple (no SRM); something relatively simple but better broadband sensitivity (zero detune); two cases of optimizing for two specific sources. Note that modes 1-3 all use the same signal recycling mirror.

“The approach that currently appears most attractive is to begin operating the interferometer without signal recycling and at a modest power level.” Question: Will this modest power level be sufficiently low that the Thermal Compensation System will not be needed? (EG)

That is the hope. There hasn't been a lot of analysis of this yet, but here is a recent calculation of the effect of the ITM thermal lens, assuming 0.5 ppm of surface absorption (from Muzammil Arain):



9. Signal Recycling. “A later upgrade could implement a variable-transmission SRM.”

Question: Are the authors proposing this as a fifth mode of operation.”

No, that is not for ‘initial-Advanced LIGO’. In general I thought it useful to point out in the document improvements that might be made later.

10. (3.4) Beam size “... ETMs are more important for reducing thermal noise because their coatings are thicker (2-3 times as thick as the ITMs).”

Comment: This difference in coatings thicknesses would seem to argue that a much larger difference in spot sizes would be warranted especially given that the diffraction losses are so small. Paragraph 3: Comment: Paragraph 3 answers the previous comment but raises the question of how $g_1 g_2 = 0.845$ was selected.

Certainly a good question. One could imagine a ~10% increase of ETM beam size, which would have a diffraction loss of about 20 ppm (ideally though, without accounting for flats or miscentering); 20% increase gives about 120 ppm loss, which seems too high. This would increase the g parameter from 0.845 to 0.87, and reduce the one-way Gouy phase from 0.405 rad to 0.37 rad. Compare to the arm linewidth of $\pi/\text{finesse} = 7$ mrad, so it's still many linewidths. Running Bench with a 6.8 cm ETM spot only gives a NS/NS range increase of 3% though ...

11. (3.7) Useful to state the presumed allocation of coating absorption losses; maybe an optics loss table? (MZ)

Optical loss allocations are given in section 3.9, though the absorption target isn't specifically called out; will add it to this section.

The ITM substrate absorption is given (pg 8 of T010075) as < 0.5 ppm. Is this correct, or should it be 0.5 ppm/cm? (FR)

Well, the OH spec on the material corresponds to an absorption of 0.02 ppm/cm, so 0.4 ppm in the ITM. Virgo made a measurement of a sample, but it came out at their sensitivity level of 0.2 ppm/cm. So might be better to say < 0.2 ppm/cm.

12. The phrase "...it is too late in the design process to increase the mass..." begs questions of how the trade study was managed, whether timely decisions were taken, to what degree further optimization could have offered improved scientific capability, what magnitude of redesign would have been required and what it might have cost in time or \$, etc. Having pressed the question with Peter, I mostly think the words are just poorly chosen, but as written the tone indicates we settled a key parameter "by default" without due circumspection. Please outline the true decision process. (MZ)

The switch from sapphire to fused silica was made right at the end of 2004. At that time the quad suspension prototype design was nearly complete, and most parts were in the shop or already delivered. We judged it too late to investigate increasing the mass. The possible benefit of course, is a reduction in low frequency noise when operating at high power. Low frequency noise can also be reduced by lowering the power. And then there's the promise of squeezed light, which could reduce high frequency noise while at low power.

13. (3.11) There should be a constraint on relative velocities between HEPI platforms and between HEPI and non-HEPI optics (PSL) within and between stations. This involves AOS, ISC, SEI and potentially SUS so it seems to be a Systems level requirement. (MZ)

Let's impose a maximum fringe upconversion cut-off frequency of 2 Hz. Then $v_{\text{max}} = f_c * \lambda / 2 = 1$ micron/sec. If this sounds OK, are you suggesting this gets added to the SYS design document, or the SEI requirements? (Note that the SEI displacement requirements aren't in the SYS design either.)

14. Table 3 of T010075 says there are 3 fold mirrors; right now H2 has only 2 FMs; typo or am i missing something?

The table gives the number of stages for each suspension type.

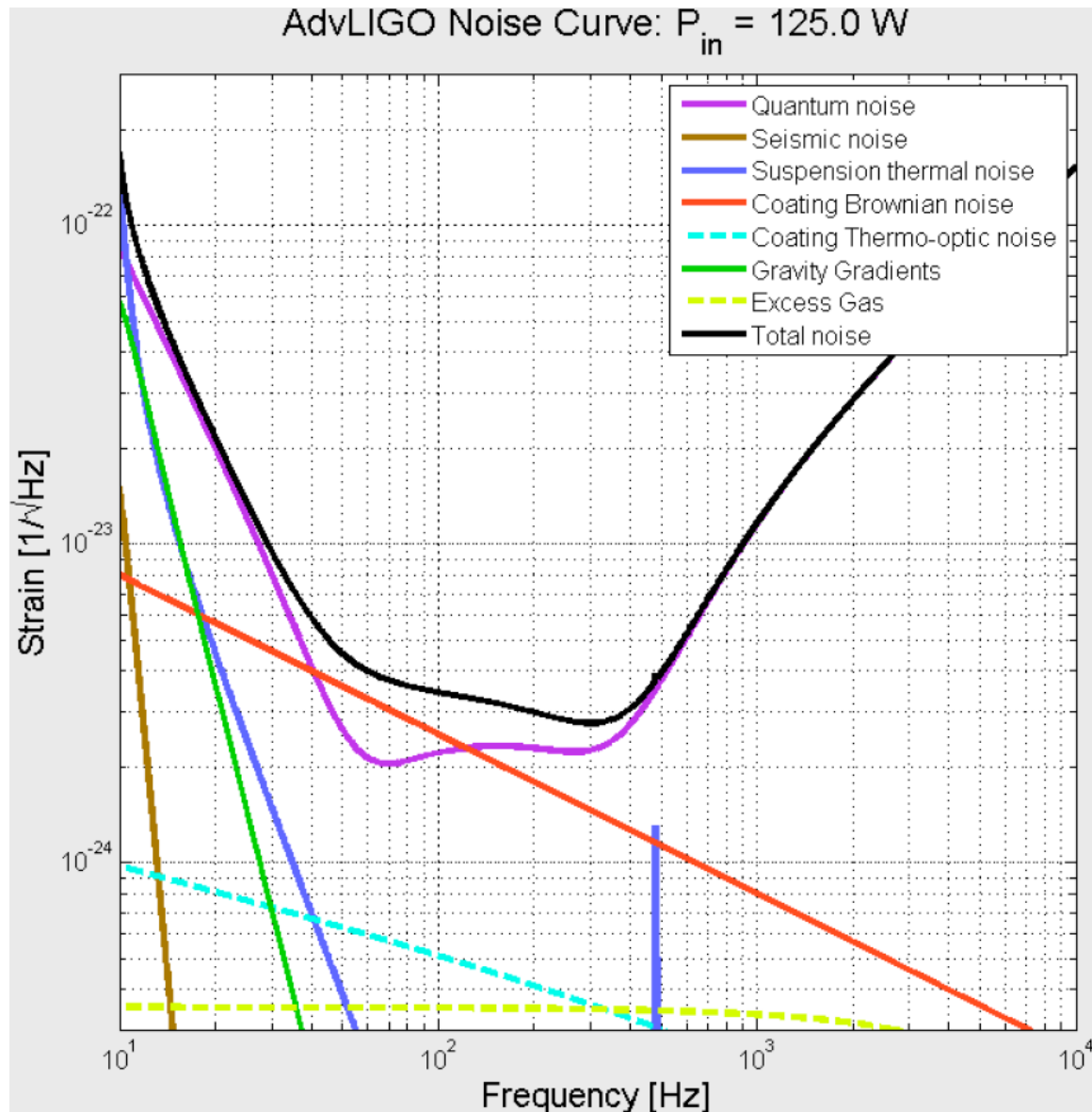
15. (3.13) There's no top-level discussion of how the distortions are to be sensed; the Hartmann sensors shown in the layout document aren't motivated. (MZ)

The existence of thermal distortion (Hartmann) sensors will be included in the document.

Questions about parametric instabilities (3.14) answered later.

Noise budget and noise sources.

The omission of a fundamental noise source plot was just an oversight, and will of course be added. It'll look like this:



16. (4.1.2) Can you clarify what it means to combined thermoelastic and thermorefractive noise terms “coherently”? It seems they should have intrinsically different correlation scales (tr presumably only depends on local temperature fluctuation, whereas te also depends on global stress distribution and boundary relaxation...??) (MZ)

Coating Brownian Noise. “This is calculated according to the formulation in Ref. [28]. The main caveat is that the calculation does not account for the finite size of the test mass.”

Question: Is it not possible to estimate the size of this effect or calculate it numerically?

Paragraph 2: “An alternative design is being pursued, where the same materials are used, but the layer thicknesses are altered to minimize thermal noise.”

Comment: The change in the coating design away from a quarter ($1/4$ - $1/4$) wave stack may increase the sensitivity to the coherent sum of the thermo refractive and thermo elastic noise by changing the magnitude of $d\phi/dT$. Question: Can this be estimated for the $1/8$ - $3/8$ stack? If not is it worth taking a risk for only a 10-15% improvement in noise.

Comment: The overall coating stack will have to be increased to obtain the same reflectivity.

Question: Is this likely to increase the coating scatter. (EG)

Coating thermo-optic noise. The formulation we have been using over the last year or two (and in v01 of T010075) was found recently by Matt Evans to be just plain wrong. He has a new formulation that is being written up as a paper. There is also more clarity on the thermo-optic parameter values. The bottom line appears to be that thermo-optic noise is just not significant compared to Brownian (whereas old model had TO crossing Brownian at 500Hz).

It’s worth pointing out that with lower mirror thermal noise, there is more of an advantage to the NS/NS tuned mode, compared to the non-detuned mode; there is about a 10% range increase with the former.

Table 4, First Line: Question: Shouldn’t the frequency dependence of the fused silica bulk dissipation go as f rather than $f^{0.77}$? (EG)

The value shown in the table is the result for Suprasil 312 found by Penn et al in Phys Lett A, 352, p3-6.

17. (4.2.2) I was surprised that the residual gas noise curve does not include H₂O. Is it really negligible? (FR)

The postbake measurement of module X1 at LHO gave an upper limit to the H₂O pressure of 2.3×10^{-12} torr; according to Rai this is still valid, as we have not filled up all the surface adsorption sites. The H₂ pressure is 3×10^{-9} torr, and H₂O gives only 3 times higher phase noise for the same pressure, so H₂O’s phase noise contribution is a couple orders of magnitude down.

18. (4.3) Figure 8 Comment: I don’t understand why 10% of the thermal noise is plotted on this graph.

The idea was – we’re nominally limited by mirror thermal noise around 70 Hz, but potentially at other frequencies too for low-power operation, or high-frequency narrow-band operation. So, an aggressive goal for technical noise could be set at 10% of thermal noise. It’s probably confusing at present; maybe better to just plot thermal noise, or leave out all together.

19. (5.7) Have we established that photon calibrators are necessary/useful? (MZ)

It’s not clear how easy it will be to calibrate AdLIGO without them. I doubt we’ll be able to do what we do now with the test mass EM actuators in AdLIGO with the ESD actuators, given their much smaller range. Seems like a good idea to keep them in.

Open design issues.

20. Test mass suspension fiber geometry. The SUS group proposed changing the baseline to a stepped circular geometry at the 16 April 2008 systems meeting, and we approved the change. The technical considerations are written up in T080091-00. The LASTI glass quad prototype will be made with such fibers.

21. Test mass dielectric coating design (i.e., something other than $\frac{1}{4}$ wave design). We need to finalize the coating design by January 2009. Matt Evans’ new coating model shows a fairly small difference in thermal noise between $\frac{1}{4}$ -wave design and optimized thickness – about a 5% reduction in displacement noise for the latter. How much improvement is necessary to be worthwhile? Input from committee on this question is welcome. There will probably also be some loose design requirements for transmission at another wavelength, for the arm-length stabilizer.

22. Gold coating on the test mass barrel. Need to decide by January 2009. We will form a technical review board to review this issue in May, to recommend a course of action in June.

23. Mitigation for parametric instabilities. Paragraph 1:”Opto-mechanical parametric couplings in the arm cavities have the potential to lead to unstable build-up of coupled, higher-order modes. Unchecked, the mechanism would impose a limit on the stored in power the arms.” Comment: As described this would seem a serious problem. The reviewer notes that both references 22 and 23 were made before the cavity parameters were changed and these analyses should be redone as quickly as possible to see if the problem has been largely mitigated or not. If the latter the authors should provide more detail on what is to be done. (EG)

When will the parametric instability calculations be completed using the planned finesse? Seems this is needed for a PDR. (FR)

Indicate the likely effect of the revised arm cavity finesse on the optomechanical parametric instabilities (i.e., is it possible that no potentially unstable modes remain? Should this be a factor in further reconsideration of the arm finesse?) (MZ)

Here is what we currently have planned for PI investigations:

Modeling:

- Redo single arm cavity FFT simulations (Bantilan's calculations) for the new arm parameters. Still looking for a solid commitment to getting this done (ie, Kells). Presumably UWA folk will redo their analyses. Likely effect is to reduce the R factors by a factor of 2.8. Bantilan's analysis would then result in one mode with $R > 1$ (for a single arm cavity, one test mass). Look at effect of reducing optical aperture (probably just on ITM) to increase higher-order mode diffraction loss.
- Calculate effectiveness of ESD actuators in damping test mass modes (all modes below 90 kHz). I.e., how much force is needed to damp each mode to a given Q, with thermal excitation? Can also look at breaking up ESD pattern to increase effectiveness. This is being done now by a visiting student at MIT; results in the next month or two.
- Look at other mechanical damping techniques. For example, a new idea is to make a piezo-based damper, on the back face of the TM (equivalent to velocity damping, or faster with frequency).

Measurement:

- Measure model Q's of the LASTI test mass, once it is installed. Should start late summer 2008.
- Investigate ESD damping of internal modes at LASTI.
- Gingin also has an experiment planned to create a PI and damp it.

24. Electro-static charge mitigation. [Outline the program to determine if charging noise is a problem for advanced LIGO \(RW\)](#).

This is a work in progress. Gregg Harry is currently assessing the whole charging issue for Advanced LIGO. He will be circulating a technical note in the near future (and giving a talk to the MIT group this Friday).

25. Arm length stabilization scheme. [\(3.15\) Describe the three proposed technical schemes for SPI/LAI, their respective design impacts on SEI, SUS, ISC, AOS and other systems, and the criteria to be used for downselection. \(MZ\)](#)

[What are the steps needed to decide on the acquisition strategy and is there any harm in simply assuming that there will be an auxiliary interferometer to reduce the relative longitudinal velocities. \(RW\)](#)

[I don't understand how one can have a valid system design without a scheme \(arm-length stabilizer\) for locking. Can anyone clarify? I would say without this SYS is not yet at the preliminary design phase. \(FR\)](#)

I think the second two comments are actually addressed in the document: i.e., we are assuming there will be an auxiliary interferometer, and we have three schemes that are viable. We just haven't yet chosen between the three. They are:

- SPI. Attach additional mirror suspensions – single stage, relatively small mirrors – to the ISI platforms, so that the mirrors hang in front of the TM suspensions (in the space between the TM and penultimate mass). Make a low finesse cavity, and sense with PDH; feedback to ISI to stabilize length. Modeling suggests this could get to ~1 nm rms for the test mass separation

- Digital interferometry. Sense the test mass relative displacement directly, using a digital pseudo-random noise phase-modulated light source. Feedback to quad suspension actuators.
- Frequency shifted PDH. Sense the test mass relative displacement directly, using PDH with a different wavelength. Feedback to quad suspension actuators.

Plan is outlined in the document: there is a technical document on the different techniques, written by ANU group (with input from LIGO). Decide on approach to follow summer 2008 (end of June). Layout implications, for beam injection and extraction, can then be worked through.

26. Particulate cleanliness. Mike Zucker is looking at the overall requirements for particulate cleanliness for all subsystems at all process stages. From this, specific subsystem solutions, tooling, etc., will be worked out. Time frame – expect a review this summer.

Other comments.

27. *Scattered light.*

The scattering with the DC readout is a deeper worry than in the initial detector not only is the system more sensitive but also the scattering noise is bound to increase at low frequencies. I am not convinced this has been analysed well enough. The beam tube and the baffling has been studied adequately. The scattering from almost specular surfaces near the beam has proved troublesome in initial LIGO and needs to be evaluated more critically for advanced LIGO - the effect of the hot spots we now know about and the proper baffling. (RW)

Scattering paths will need to be 100's of times weaker than in Initial LIGO at frequencies above 40 Hz and we have had a free pass on scattering noise in the 10-40Hz region so far. How are the scattering requirements flowed down? They ought not to flow up from AOS but flow down from SYS. (FR)

How is scattering between H1 and H2 handled? Spec's? Special precautions or techniques? (FR)
How come the no-wedge CPs don't create etaloning problems? (FR) Same question from Rai.
I did not catch the analysis of which order ghost beams need dumping. Where is that? (FR)

Scattering is handled by AOS (ghost beams and diffuse scattering from core optics), IO (ghost beams and diffuse scattering in IO section) and ISC (detection beam lines). Here's an overview of what is being done to handle scattered light:

- For each AOS and ISC, total scattered light noise is limited at 1/15-the of strain noise floor. Actual estimate is expected to be smaller than this: more margin is designed in wherever it's relatively easy to do.
- For scattering paths that hit vacuum enclosure, actual vibration measurements of the vacuum walls/flanges are used (levels are much higher than floor vibration in the band)
- AOS components: better baffle/dump material is used in many cases, even if calculations show it isn't necessary (i.e., black porcelain-ized steel instead of oxidized steel).
- Wide angle scatter from test masses is not assumed to be any smaller than in iLIGO.

- Lots (all?) of AOS baffles and dumps are either mounted on ISI platforms, or suspended from ISI platforms
- ISC ports. All beams used for ifo control in low-noise mode are detected on isolated (ISI) platforms. Beam sample paths that come out of the vacuum will be equipped with shutters in vacuum.
- iLIGO hot spots: most are covered by the AL arm cavity baffles. MC tube reducer flange: MC tubes being replaced with larger diameter ones; new flat surfaces will have to be baffled.
- Scattering between H1 and H2. There are arm cavity baffles that should block scattered light from one ifo arm getting into the recycling cavity area of the other. However this will not block scattered light from one ifo arm getting into the other ifo arm (angles are so small). Any ideas how to do this?
- CP wedge. The CPs have their faces parallel to the wedged AR face of the ITM, so that reflections are not in line with the cavity. The advantage is that the ghost beams from these three surfaces (ITM HR, CP front and back) come out at the same angle, and can be dumped together.
- Ghost beam dumping (which order): This is found in the Stray Light Control Conceptual Design, T070062-02. Answer: first order ghost beams. The estimated noise from the undumped second order beams is shown there to be 2 orders of magnitude below the scattered light requirement (ie, 1000x below the strain noise). Also, the AR reflectivity used in these calculations was 300 ppm, where in fact it should be less than 50 ppm.

28. *Lessons from Initial LIGO*

2.2 Non-Gaussian Noise: “Lessons learned from initial LIGO about potential sources of glitches must be taken into account.”

Comment: Please list these lessons and explain how they are being taken into account (EG)

Here’s a list of glitch/upconversion mechanisms we saw (with input from Keith Riles), and how they apply to AdLIGO:

* *Glitches from dust falling through ISCT beam paths.*

Avoid non-essential beam waists and provide clean air flow.

* *Retuning of H1 ETX suspensions filters in September 2006 to reduce susceptibility to stack resonance excitations from trucks on 240.*

Doesn’t really apply.

* *Changing out of magnets to address Barkhausen effect*

Use SmCo magnets in AL.

* *Moving earthquakes stops farther from mirrors and changing tips to reduce charging effects*

Use glass-tipped stops; have good mechanisms to adjust stop position.

* *Scattering of light from H1 off H2 mirrors and back again (and vice-versa). Although we have had to live with it in S5 and just veto epochs, mitigating this for AdLIGO seems worth looking at.*

As mentioned above, don't really know how to fix scattering between arm cavities.

** AWG malfunctions (line dropouts and channel hopping) were identified early in S5 and fixed with new AWG code. This is pure software and therefore 'trivial', but we should remember it for AdLIGO. We also had an incident with unintended blind injections.*

** We had trouble early in the run with OSEM glitches on H2 because of improper filters in local damping. Again, 'trivial', but we didn't catch it immediately.*

** TCS laser glitches have been a problem from time to time. Well behaved lasers are obviously desirable, along with rapid diagnostics in place - planned for S6.*

Pay attention to mechanical vibrations of TCS table components, monitor the beam power, and most importantly – don't shine the TCS laser on the HR surface of the ITM!

** Glitches in PSL servos (FSS and ISS) can be especially nasty. Same comments as just above.*

** This issue is more on the upconversion end of the spectrum, but the HVAC plenum design required the cooling to be turned down. My impression is that a plenum redesign to reduce turbulence could allow the cooling to be turned higher.*

** The whole acoustic mitigation program: larger optics to reduce coupling; reduce sources of acoustic noise where possible; implement acoustic isolation.*

** Floating of optical tables mitigated glitches (and occasional unintended grounding of tables brought them back)*

** Thermally insulating dewars at LHO greatly lowered the rate of (but did not eliminate) glitches from their creaks.*

Spectral line contamination:

** Quite a number of motors, chillers, etc have frequencies that are slightly below 60 Hz and have frequencies that drift around a bit, landing occasionally on the Crab frequency.*

** Power pulsing of heating coils led to 3 Hz sidebands on 60 Hz in DARM_ERR. Stopped using the pulsed heating mode.*

** Seismically noisy machinery, e.g., chillers, air handlers, were placed on damping supports, reducing some spectral lines.*

** Power supply ripple led to some pulsar outliers*

[Fred's list of initial LIGO lessons:](#)

How have lessons learned from initial LIGO been incorporated into the system design? I think a document addressing how lessons learned have been addressed should be part of a PDR set. For example:

- a. We have very different EMI implementations at the two observatories; each was done according to our best understanding of what was needed at various times when the work was done; are we convinced we settled on the most cost effective solution that gets us where we want to go? What was the process?

A decision on the EMI implementation will be made with the DAQ group (which oversees the CDS infrastructure) at the DAQ PDR this summer.

- b. We spent 4 science runs learning not to close the doors on electronics racks to meet acoustic and shielding requirements, because electronics could not operate in the design enclosures due to thermal issues; also, we see cooling fans in racks showing up in DARM_ERR; how will such issues be handled better? Is it now possible to handle acoustic couplings and thermal issues independently?

We plan to keep all electronics racks and most electronics out of the LVEA. In addition the sensitive detectors are placed in the vacuum system so coupling from fan noise to detectors is eliminated. Adequate rack cooling will be verified with rack testing of fully integrated assemblies; if problems are found then the cooling will be improved or the heat load reduced by redistributing the electronics among the racks.

Robert Schofield will also perform noise coupling measurements on the electronics.

- c. How will we handle obsolescence of computing and electronics systems from now until 2015, when science runs might begin? How will we handle it during operations?

Computing systems will be purchased as late as possible on the project to minimize early obsolescence. We do not anticipate significant operations costs for replacement/refurbishment during the commissioning phase of AdL. Once systems are delivered, installed, tested and accepted the maintenance, repair and replacement/refurbishment becomes an operations effort and cost (not AdL).

- d. How has SYS addressed the major causes of downtime from science mode? I think the major elements in the duty factor pies should be addressed.

Here are the major causes of downtime:

L1: summer 2007	H1: through May 7, 07
7.1%: Maint/commissioning	8.3%, Other: over half on commissioning; remaining anomalous issues which were single events/mysteries
4%: earthquakes	2.2%: Tues maintenance
3.4%: hardware & software failures	2.2%: Seismic
2.6%: logging	2%: lots – wind; scripts; software; hardware

- e. How come the documentation will be good this time around? Previously, deficiencies in the DCC were cited as a major factor leading to documentation

problems, especially as commissioning proceeded. On what date will the sufficiently good DCC be running?

Please note that an improved documentation system is not the responsibility of the Systems group, nor is it within the scope of the AdL project. The new DCC is a LIGO Laboratory infrastructure scope item.

There were (are) a number of documentation issues or deficiencies encountered for Initial LIGO:

- i) Inadequate or missing design & implementation documentation: An easier-to-use DCC, and one which returns more benefit to the users, should help in improving the rate of capture for this documentation/data. However this is also in part a cultural problem and diligent management must also be applied.
- ii) Lack of an organized key or structure ("documentation tree") to find documentation in the DCC: The new DCC permits the users (subsystem personnel) to organize their documents in a logical manner. Some common and required structure will also be imposed by Systems Engineering. In addition Systems Engineering will in the final design phase build a "documentation tree" or hyperlinked index for all critical design and implementation data. This documentation tree will then be maintained throughout the duration of the AdL project.
- iii) Poor search tools to help find documents in the DCC: The new DCC is reported to have improved search tools.
- iv) No or little design source file archiving and control: For mechanical drawings the intent is to also capture all source (CAD) documents in the PDMWorks vault. The new DCC will be used for electronics source file capture. The project employs better software configuration control tools and procedures than it did at the beginning of iLIGO.
- v) Tracking of part history, especially electronics: During commissioning and operations in particular, the life history of electronics boards (test data, revisions, re-test results, etc.) were not tracked with the paper traveler system that had been put into place. A few people/groups have been looking at the requirements and implementation approach for an electronic database for on-line entry and tracking of part history by serial number. It is not yet clear if or how this integrates into the new DCC.

- f. How will operability and maintainability be enhanced in these machines, which are far more complex than initial LIGO? Most entries into laser hazard areas to date (and most safety incidents) arose from people needing to check whether a cable was connected or if the beam routing on a table was still the way it used to be, not to actually do any work in these areas. (For example, we depended on long-term alignment stability of suspended optics to ensure stability of beam routing on optical tables.) Do we intend to continue this or will we require use patch panels external to sensitive areas, remote sensing and correction of beam alignments on output tables, etc. Will we accept needing to go into hazard areas to reboot machines?

All critical beams will be routed by fast steering mirrors so that alignment can be accomplished/controlled remotely.

Test boxes for emulation to test & isolate problems.

- g. The adopted contamination control document is the one created for initial LIGO installation and the one conformed to during operations. However, I believe a

separately derived contamination control document was provided by another party after installation was completed, but never followed. Have we become any smarter about necessity or cost-effectiveness of these procedures in the meantime? Has that been considered?

We are not aware of any contamination control document provided by another party for proposed use by LIGO Lab or AdL in particular. We have initiated a study (by Mike Zucker) to look at the requirements for particulate and non-volatile residue (NVR) requirements for AdL and the implications for each phase of work (UHV part preparation, assembly, storage, installation and maintenance). We have also gathered input from the NIF project for consideration. Our principal concern is reduction in particulate contamination since scatter and absorption loss in the iLIGO COC was almost entirely due to particulates. A number of specific measures, beyond iLIGO practices, have been, or will be, taken to reduce particulate contamination, such as the use of nitronic 60 inserts to reduce particulate generation, the use of FirstContact(TM) on our optics, post assembly phase cleaning (to reduce the particulates due to integrated exposure), improved contamination control procedures (per NIF recommendations), etc. The contamination control study, a part of the FMP subsystem, should be ready for a first review later this year. We do not anticipate at this time requiring any significant investment in equipment to support UHV preparation and assembly (which is scheduled to start in early 2009).

- h. Observatory staff have often been surprised by shipments arriving? We have also been told by NSF that we need to have better tracking and storage of equipment items from time of arrival. Should these not be included in the generic requirements and standards?

Good point. Top level guidance and requirements for tracking, shipping and storage will be added to the generic requirements document. Also a reference to the intended bar code inventory system and its use will be added when available.

- i. We have had long down times associated with cable/connector sets that have just worn down and failed. Do we need some manner to handle how many times we make/break connections over the lifetime of equipment? Some other remedy? Just bear it?

The plan is to have all cables manufactured & pre-tested out-of-house. The other problem we had was un-mating of non-latching connectors. We require use of latching connectors in AL.

- j. Initial PEM was a guess; does AdLIGO take any responsibility for having a suitable PEM deployment? Who does?

There is no scope for additional PEM instrumentation or channels for AdL. Operations has responsibility to maintain, repair and refurbish the existing PEM sensors.

- k. We have repeatedly found poor mechanical designs for auxiliary components, like periscopes, baffles or baffle stands, etc. How do we pass down requirements for resonance and mass envelopes for these types of components?

29. I think a diagram (or many diagrams) showing the flow down of requirements to subsystem is needed to avoid subsystem leaders being surprised and certainly to allow outside

reviewers to understand how subsystems are meeting all the requirements that abound in many separate documents. Does SYS plan to provide this? When? (FR)

We don't; we've never found these to be useful. The "systems team" is comprised of the leads for all subsystems. We have frequent meetings and discussions to help minimize interface disconnects or mis-understandings about requirements. In addition one or both of the two systems leads, Dennis and Peter, are members of the review panels for every subsystem review. Each subsystem is required to prepare a requirements document which re-states the relevant system level requirements and their derived requirements. Trade studies or analysis of a systems nature performed in support of a subsystem (e.g. the COC material choice, the allowable polishing surface error spectrum, mass and modal limits for seismic platform payload elements, etc.) always have input and guidance from Peter and/or Dennis and are generally also discussed in systems meetings or forums.

30. The electronics needs to be analysed together with the instrument, especially that electronics which touches on the sensitivity. (RW)

a) The electrostatic drive is new and the plans need to be shown.

The driver is designed and built by Nick Lockerbie. There is currently a prototype that has been built and tested, and the design and test data can be found in T080038-00 ("Prototype Electrostatic Driver – User Guide"). This prototype will be further tested at LASTI on the quad suspension, starting in summer 2008. One issue people have worried about is coupling of the ESD signals to unshielded OSEM cables; this will be investigated in the LASTI tests.

b) What are the plans for the wavefront sensing system and its electronics.

A design is underway that leverages off the LSC RFPD design using a series resonant readout topology. The ASC detector will be available in a hermetic package for in-vacuum work. Prototype work is underway for a demodulator cell to be used in both LSC and ASC RF demodulation applications.

c) What is now the strategy to be used to avoid RF oscillations in the electronics,

Testing is to be conducted into more realistic loads for "bench-test". This has involved using the same type and length of cables, as well as the actual loads when possible.

Time domain testing of the output stages of amplifiers is now routinely performed by driving the outputs of amplifiers with a square wave through a resistor to check the damping under real load conditions.

d) What is the strategy to avoid cross talk in the analog and digital electronics, are circuits now individually shielded, is it mandated that all feeder lines are truly balanced at input and output

More use of symmetry in circuit layout due to improvements in CAD technology, that way the channel to channel isolation is consistent. Better definition of allowable cross talk would help at the requirements level. Improved shielding and cabling practices based on R&D done on shielded cable at CIT. Much more use of twisted pairs and balanced differential signal transmission. Balanced lines are the standard signal transmission method for inputs and outputs in ADLIGO. That being said, there will always be exceptions on a case by case basis that yield more optimal solutions.

e) Are there plans to have direct feedback from the commands to determine actual switch settings

rather than inferred ones,

Yes.

f) How is the DC power generated and distributed,

After a recent demonstration yielding reasonable results for a switched mode DC power solution, this is gaining momentum as a probable choice. A bulk supply at 48VDC will be distributed to passively cooled remote modules. The remote modules will generate the required voltages for point of use. Local linear mode regulators and filters are used at the board level as needed.

g) What are the magnetic fields expected from the electronics and how do they interact with the suspension system.

We don't have a way to make a reasonable estimate for AC magnetic flux strength. The suspensions (ie, the effect on test mass motion) are much less sensitive than iLIGO: magnets only down to the penultimate stage, giving ~2 orders of magnitude more isolation; PM magnets are the same size as iLIGO, but paired with shielding magnets that should give a factor of several decoupling; factor of 4 increase in mass.

31. There is no heritage in parts of the suspension system being proposed.

Questions arise about its actual performance: does it have thermal noise at the expected level, is there creaking and acoustic emission in the design being proposed. No experiment is doing the noise qualification. Is the experience on GEO actually relevant to advanced LIGO since the frequencies in advanced LIGO are so much lower and that is where the sensitivity is needed. What is actually known about cross coupling in the suspension system in particular between displacement and angle. What is the program to find out that the suspension has both low enough acoustic emission, low enough cross coupling and low enough thermal noise at low frequencies to function for LIGO. (RW)

There is no program to study acoustic emission or suspension thermal noise. I'm not sure what you mean regarding cross coupling.

32. The wavefront alignment system is the most complex and least robust system in initial LIGO. What does the control matrix for this system look like in advanced LIGO with its signal recycling mirror and what is the hierarchical nature of the system. How does it rely on the interferometer coordinate system and how on the ground based coordinate system. (RW)

From the ISC conceptual design, here is a WFS matrix (signal per normalized angle):

A/ $[\alpha_0]$	PR	SR	DITM	CITM	DETM	CETM
REFL f1 $\Psi_G = 5^\circ$	0.2858	0.000	-0.0007	0.0130	0.0009	-0.0185
OMC 2f2 $\Psi_G = 160^\circ$	0.0006	0.0187	0.0086	-0.0175	0.0010	0.0006
OMC f2mf1 $\Psi_G = 90^\circ$	0.0001	-0.0002	0.0846	0.0001	0.0003	0.000
POP f1 $\Psi_G = 90^\circ$	0.1249	0.000	0.0014	0.3198	0.000	-0.2810
DP f2 $\Psi_G = 10^\circ$	0.000	0.0231	-3.2937	-0.0291	3.6038	0.0008
ReH f1 $\Psi_G = 90^\circ$	-0.5148	0.000	-0.0063	-1.4314	0.0002	1.5389
OMC 2f2	0.0120	0.0312	-0.0385	0.0782	0.0265	-0.0038
POP f2 $\Psi_G = 90^\circ$	0.0436	0.0001	-0.0017	0.1118	0.000	-0.0981

Some DOF are pretty well coupled together. The controls aspect has not yet been modeled. We anticipate needing a bandwidth of about 10 Hz in some loops to stabilize the angular instability. Don't really understand the question about the influence of the ground base coordinate system on the WFS.

33. The plan to make incremental steps in advanced LIGO is useful for many reasons, alone that there is too long a hiatus in data collection for the LSC in the original plan but also that we operate better in a mode where there is interleaving between construction, commissioning and data taking. The idea of making a run after the installation of the new seismic isolation system and the new suspensions and before the high power and amplitude recycling is incorporated is an excellent place to make the cut. It would be useful for a host of reasons (the desire to try squeezing, the possibility of bringing VIRGO into the planning....) to see a trial schedule. For example, is this to be a run with one detector in LIGO and the mate being the VIRGO detector. Does it require H1 and L1 both being ready, what about H2 which is now a 4km system. (RW)

In the discussion of operating modes, is the model for operating compatible with the NSF schedule milestones for the Project? If not do operations require installing and removing and re-installing components to comply with NSF Project milestones? (FR)

Here is David S's story on modes and NSF milestones:

The plan we agreed to with the NSF for acceptance of the interferometer requires that we show that the interferometer can be locked for two hours with all length degrees-of-freedom under control. We should carry a plan at this point that is consistent with that. We could choose to spend some time before acceptance with a transparent or non-existent SRM, and could collect GW data under those circumstances, but our plan at this time must include the agreed-upon demonstration of all DOF under control before the interferometer is accepted. Once the interferometer is accepted, we can of course configure it in the way we think leads to the best physics and the long-term goals of AdL in the large sense.

We can revisit this at a later time as we approach acceptance, but can't deviate from the promised goal at this time; our systems and ISC plans and activities must support the promised goal.

I don't think it should be difficult (and I think it would be advisable) to keep the alternatives for commissioning paths open in the interim, and clearly persuasive arguments may be developed which we can consider at the right time.

34. How are the arguments of the importance of the low frequency performance (control of thermal noise, radiation pressure noise...) modified if the regression of gravity gradient noise is effective in reducing this noise to 50%, 20%, 10%, 1% between 1 to 20Hz. (RW)

I don't think the arguments change, but I'm not sure I really understand the question.

35. What evidence exists that the coating loss and point scattering loss will be better for advanced LIGO than it was for initial LIGO (factor of 1.5 to 2 improvement in loss assumed in

[the document](#)).

We have metrology data from the LASTI optic (full size optic coated at LMA). It showed a total-integrated-scattering of about 12 ppm; this is comparable to or a little better than some iLIGO test masses (in lab testing). It didn't show a lot of excess point scatter. There is some evidence in the literature that scattering from coated surfaces can be in excess of the standard TIS formula, by a factor that increases with increasing microroughness; if there is anything to this, it says we should specify low microroughness (around 1 angstrom).

One of the potential polishing vendors showed us metrology data from another project, where they had achieved an rms distortion of about 0.3 nm over a similar aperture; this is significantly better than iLIGO optics.

From Stan Whitcomb:

System Design (T010075-01)

36. In separate discussions, I have been told that the plan for AdvLIGO is to keep this system design at a very high level, and to document subsystem requirements in the individual subsystem documents. I recognize that there are many ways to approach a project like AdvLIGO and this may be effective. However, it makes a review of this type very difficult. Since the subsystem requirements are so distributed, it is difficult for a review team to know if the requirements are complete, compatible and meet the over all system requirement. With that in mind, it would be valuable to hear from the System team how they ensure that the subsystem requirements are managed.

Please see the answer to item 29 above.

37. It appears that much of the system design is really contained in the Bench models which generate the sensitivity curves. What is the validation history of Bench? How is its configuration controlled? If a committee like this wanted to review the origin of Figure 2, how would we find the parameters that were used to generate it?

Each component has a different validation history. Rather than go through these (and admittedly the validations are generally not documented), let me say what I think needs further validation:

- Quantum noise: the code that includes losses was checked against the previous version (which itself had been checked against numerical code), but I don't think anyone has gone through and checked it line-by-line; also checks with numerical code including loss should be done
- Mirror thermal noise: the substrate terms (that don't matter) have been checked, and of course the coating terms *were not checked adequately*, but we are about to incorporate new code for the coating terms, and this will need to be checked
- Suspension thermal: current code was checked against other formulations (M Barton), but we'll need new code to handle the new fiber geometry

Parameters: I tried to put all the parameters that determined the main noise sources in the document (e.g., the parameters that are changing in the different curves of Fig 2 are given in Table 2); I might have missed some of course.

38. Section 2.2, Non-Gaussian Noise: I find this discussion a bit unsatisfying.

Ideas for making this more satisfying are quite welcome!

39. Section 2.5. It appears that 16384 was chosen for the data acquisition rate, simply because that was the Initial LIGO rate. Was any consideration given to the science drivers for this? Shouldn't this be documented here.

Science drivers ... yes, some motivation for the upper end of the bandwidth should be given; will look into this. See also answer to Q #7.

40. Table 1. By specifying the maximum power on the PRM, all subsystems will meet their requirement by delivering a lower value. I don't think this is what you want. I suggest you specify a range of powers to be delivered.

Yes, will edit accordingly (5 W – 125 W).

41. First paragraph on page 4. It is implied, but not stated that all interferometers will be commissioned in the same modes. Is this correct? Has any consideration been given to commissioning one of the LHO interferometers in narrow band mode?

First, a narrowband mode will be added to section 3.1. If commissioning means 'initial commissioning' (a la iLIGO), I think it's likely we'll decide to commission all three in the same mode. If commissioning refers more generally to operations, then certainly at some point they will probably be operated in different modes, including the narrow band mode. We'll note that the ISC group is concentrating on developing the sensing and controls for the zero-detuned mode.

42. Table 2/Figure 2. The case for implementing mode 2 (which may involve considerable effort) does not appear to be strong. Is there really a science case for the extra effort involved?

The current estimate (with the correction to the coating thermo-optic noise) is that there is a 10% range increase in mode 2 (rather than the 3% given in the document). And of course these estimates could change in the future, in either direction, so it seems too early to discard mode 2. But as noted just above, ISC work is concentrating on mode 1.

43. Section 3.6, third sentence. Please fix the English. I am not sure that the historical approach is the right one for this document, but if so the arm cavity finesse has not been 1200, the specification for the arm cavity finesse has been 1200.

Section 3.12, the last sentence seems incomplete. I suspect a reference to Table 3 is required.

Will fix both of these items.

44. Section 3.14. I find this section lacking. There is a statement that there is a significant potential problem, and a statement that “some mitigation method might be adopted in the future”. Please give us a status on your efforts to scope this problem and to deal with it.

Please see answer to Q #23 above.

45. Table 4, the caption says that there are still significant uncertainty in the tantala parameters. How do these uncertainties affect the predictions of noise? What is being done to reduce the uncertainty?

Actually not much, now that the thermo-optic noise calculation has been fixed. But there are more measurements underway: Andri G will make more measurements of reflectivity versus temperature of a coating sample, at different wavelengths than 1064 nm (this helps distinguish dn/dT from α); there are other ideas for measuring tantala parameters on a sample that has a single layer of tantala applied to it (so one doesn't need to rely on assumed parameters for the silica layers).

46. Tables 4 and 5, it might be useful to have a reference for the various parameters that are shown.

Sure.

47. Section 4.2. It would be good to have curves for these noise contributions.

Section 4.2.2. It appears that only hydrogen is considered. What is the pressure of water vapor in the current facilities? And how does this contribute? It would also be good to calculate the allowable pressure for a generic AMU 100 and AMU 200 organic molecule, given the statement (page 13) in the Generic Requirements document that the hydrocarbon outgassing requirements were being relaxed.

See answer to Q #17 for water.

To be clear, the statement in the Generic Requirements Document (E010613-02, section 1.4.7) points out that the hydrocarbon outgassing requirements should be comparable to initial LIGO (and not as tight as indicated in the referenced document, T040001). The analysis in T040001 will be revised to include the very significant pumping speed, and nearly infinite capacity, of the beam tubes in deriving allowable pressures for high AMU molecules.

48. Section 6. really need more on these items. Which subsystem is dealing with each one? How is it being managed? What are the schedule for resolution and who is monitoring each one? See items 20-26 above, and in the imminent document on Systems plans.

49. Appendix A, I am surprised that in a document which is at such a top level seems to be the appropriate place to document the inclusion of loss in the SR cavity and photodetector efficiency.

OK, will revise and place this discussion in the ISC design document.

From Ken Strain:

50. T010075-01, section 2.4 Environmental Sensing: incomplete?

See discussion above under #6 and #28.j.

51. T010075-01, section 3.14, Instabilities: understandable why incomplete, but could it be better?

See response to #23 above and also refer to the imminent plan document for details on how we will pursue resolution of parametric instabilities.

52. T010075-01, section 3.1 is this the place to call out a definite lower power limit for design purposes? i.e. a lowest power at which PSL and IOO must deliver a noise spec. (probably less than mode 3, 20W)

Yes, will do.