ISC review – February 1st 2012

Questions from the review board and answers

1.1 General description of the sub-system

1) Benoit: Page 2, last paragraph of the introduction: The fourth modulation is explained, while the three first one are not yet introduced. May be this part could be put later, or we should introduced the other modulation frequencies.
I propose to leave in the introduction the mention of risk reduction strategy, with reference to the proper section, without explaining it is obtained with an additional modulation. This is explained in the corresponding section.

1.2 Interfaces with other sub-systems

2) Fulvio: Your chapter is stating a lot of requirements that will drive the activity of several other subsystem. These numbers are collected in tables but not all of them. I suggest to collect all the informations and (even if it is boring) to copy them in dedicated table grouped at the end or at beginning of the document.

I tried to make a summary of all requirements in section 1.2. I did not repeat all the numbers and tables written elsewhere in the document. If you think it is useful I can explicitly repeat all the requirements even in this first section.

3) Benoit: The interface section is very longue and duplicates some part of the text. One option could be to keep a much shorter version focusing only on the "assumption" used to do the various ISC computations and move the summary of consequences (or requirements) on the other subsystem and then end of the chapter. This would be a actually a better conclusion that the current one which do not bring really information.
This suggestion is in contrast with Fulvio's one. I personally agree with Fulvio, that a summary of all requirement, even if repeating some part of the text, is very useful. I wait for a decision of the review board on this point.

4) Benoit. Page 7, section 1.2.6 is a good example of a section which could be shrink and put at the end of the chapter.
Same as above.

5) Benoit: Should the interface section be at the end of the chapter since it gives constrains to other sub-system?
This is maybe a good suggestion. Moving the interface to the end will avoid problems of double definition of terms and things.

6) Benoit: Page 2/3: It may be good to first explain the goal of each of the three modulation frequencies before giving the constrains (resonant or not in the various cavities).
This will be solved moving the interface section to the end of the chapter.

7) Benoit: B1 beam: det is planning to demodulate it for monitoring purpose.??
That's fine for ISC. We have no need to demodulate B1

8) Luciano: Section 1.2.5, pag. 6: "…This poses stringent limits on the OMC filtering capabilities." : Could you give specifications on this limits?

The specification is given in term of the sideband power the OMC should transmit, which is 0.07 mW. The amount of sideband power impinging on the OMC depends on the configuration. I report here a table from Romain's presentation VIR-0581A-11:

From Optickle Simulation (m=0.1)
Personal Communication from G. Vajente

Powers BEFORE OMC

AdV configuration	Carrier	SB @ 6.27 MHz	SB @ 56.44 MHz
125 W, SR	80 mW	2.5 mW	160 mW
25 W, SR	80 mW	0.5 mW	32 mW
25 W, PR	90 mW	8.4 mW	116 mW

From these numbers one gets the following requirements for 6MHz OMC power transmission: SR125W = 1/36, SR25W = 1/7, PR25W = 1/120. And the following for 56MHz OMC power transmission: SR125W = 1/2300, SR25W = 1/460, PR25W = 1/1700.

9) Luciano: Section 1.2.6, pag. 7. At which frequency is intended the requirement for the RIN (10^-8 Hz^-1/2) in the power recycled case?

This requirement is almost flat in frequency, as shown in section 1.3.8, figure 1.4. Added a small comment to the text: "(almost flat in frequency)"

10) Fulvio: Please clarify the meaning of the sentence at the end of the paragraph 1.2.4

Quadrants on the end benches will be used to control two degrees of freedom of the arm cavities using DC (spot position) signals. Any motion of the bench will fake an angular signal coming from a real motion of the beam. The amount of beam displacement on the quadrant that correspond to a given bench motion depends on the optical properties of the end telescope, which is design to reduce the beam size from few centimeters to fraction of millimeter. The requirement for the bench angular and translational motions quoted in the document were computed taking into consideration the design of the telescope given to us by DET. Since the angular requirement is pretty tight, it was considered important to try an optimization of the telescope design to reduce the coupling from bench angular motion to spot displacement on the quadrant. This would relax the requirement on the bench angular motion. We can even afford some worsening on the coupling from bench translation to spot displacement on the quadrant, since the bench requirement with the present telescope is not very tight.

I modified the last sentence with the following:

"Since the angular requirement is pretty tight, it was considered important to try an optimization of the telescope design to reduce the coupling from bench angular motion to spot displacement on the quadrant. This would relax the requirement on the bench angular motion. We can even afford some worsening on the coupling from bench translation to spot displacement on the quadrant, since the bench requirement with the present telescope is not very tight."

11) Benoit: Page SBE requirement: The Telescope offer little margin for optimization. It will be discussed this week and the last sentence might be drop and numbers updated after the telescope meeting.

At the moment of writing of the TDR chapter (and even today) we did not know if there is margin for the telescope optimization. The chapter makes a summary of what we know as of today. If things we'll change in the future, we have to decide what to do.

12) Benoit: Page 6; electronic noise for the control sensing (old story...): The controls have been design assuming a noise equivalent to a shot noise limited 100mW beam. DET may take a little more light, if needed, to achieve this target, since it does not affect ISC.

As Benoit says, this is an old story. ISC group disagrees with this approach. We pointed out that reaching a project-wide agreement on this (and other) topics was important before writing the TDR chapters. Unfortunately we did no reach such an agreement. Therefore we can't write in the ISC chapter something we believe is wrong.
Requiring signals to be limited by shot noise is the correct way to proceed in our opinion. This is also shared by the Advanced LIGO project (see LIGO-T070247-00-I , section 5.2, page 34) and it is also proven to be feasible (beside Virgo photo-diodes, in REVIEW OF SCIENTIFIC INSTRUMENTS 78, 054704‘2007’ H. Grote shows the desig requirement).

13) Fulvio: The statement concerning the shutter in section 1.2.5 is extremely interesting. I wonder if this is matter for the Detection bench chapter. In any case I wonder if this is such a system is available on the market or it requires a dedicated R&D.

Indeed we quoted this result from an Advanced LIGO document. Since the configuration of Advanced Virgo is not very different from Advanced LIGO, we expect the same problem to exist in Virgo. The implementation of the shutter is indeed a problem that DET must tackle. It is maybe ISC role to point to the problem and confirm the LIGO computations, since they concerns lock loss events.

14) Benoit: Section 1.2.8 (SAT): Why not setting a goal on the improvement of the inertial damping?
As written in the text, the present performances are enough. We should not worsen the performances. Any improvement will help of course, but is not required.

15) Luciano: Section 1.2.7. DAQ – It looks like, according to DAQ sub-system, we won't be able to design a DARM control loop with a good phase margin. It is also written that we don't expect "huge improvements" from further studies. So we won't be able to lock the interferometer in a stable way?

If this is the case, you should stress this point in the conclusions. I don't think this is the case and possible point of mitigation for the total delay are indicated, for example, in the following section (1.2.8). In my opinion, this point should be better clarified.

The need of having a high unity gain frequency for DARM loop comes from the unstable optical spring generated by the DC-read-out offset and the signal recycling cavity. The present design of the DARM loop foresee a UGF of about 400 Hz. It might be possible to change the control filter to reduce it to 200 Hz. In this way a total delay of about 100-150 microseconds could be tolerated, reducing the phase margin of about 7-10 degrees.

If such a delay is not possible, we are in trouble, since we don't see today any way to stabilize the DARM loop and reach the required accuracy. However, this problem appears only in the dual recycled case.

16) Fulvio: In the case of SR configuration the requirement to extend the bandwidth up to 400 HZ could have an impact on the payload design. In fact it is not so eay to have mechanical resonances of large plates coupled to the payload infrastructure higher than 400 Hz. This point requires a deeper analysis and it is connected with the remark b).

This implications were not considered. The band-width of the DARM loop is 400 Hz in the present design. The reason of such large bandwidth is the fact that DC read-out plus signal recycling creates and unstable pole at about 40 Hz. The DARM loop must gain significantly at this frequency.

Since the implications of the DARM band-width are not at all trivial, we believe it is important to study more deeply the problem. We expect it might be possible to reduce the band-width to 200 Hz with some work on the control strategy. Not much more than this.

17) Benoit: Section 1.2.9: I'm not sure if it is really a requirement.
18) Benoit: Section on lock acquisition: We should give the specification for the forces, instead of not providing a number, even if the final design of the payload is not yet available and leave to choice of the magnets to PAY.
The goal of section 1.2 is to collect interfaces with other subsystems. The goal of section 1.2.9 is to explain that before choosing the new magnets a discussion with ISC will be needed.
We might express the result in term of force. The Virgo+ high power actuation gives about 15 um/V, which corresponds about to 4 mN for 1 V. A force 160 times lower of the 0.8 V we used in Virgo+ means 25 uN. But we need to discuss with PAY if this enough for other uses, like local controls, dampers, etc.

19) Benoit: Section 1.2.10: This is maybe so obvious and easy that it is not needed to add it
This is about the interface with INF, concerning the need of having a bench for auxiliary lasers in the end station. It is nevertheless something we should consider, even if easy to do.

20) Benoit: Page 5: In the old Virgo naming convention, the PO beam is probably B4 instead of B5.
21) Benoit: Table 1.10: (and alignement section) Why not using the Virgo names "B1,…) for the beam?
There are discussion going on for the naming of the beam. As soon as we agree on one single convention, we'll update the naming. For the moment, figure 1.1 acts as a "Rosetta stone".

1.3 Longitudinal control system: steady state control

22) Jerome: In GEO, to tune the SRM position we change the sidebands frequency used to lock the SRC (for example by 100 Hz). Will it is be the same for Advanced Virgo ? I am asking that because MICH and SRCL share the same sidebands (table 1.6), so does the tuning of SRCL will affect MICH?

This would not be possible in AdV, since the modulation sidebands must go through the IMC, so their frequency is fixed by the IMC FSR. We plan to change to offset added to the SRCL error signal. I've added a sentence in the TDR section 1.3.6:
"The Advanced Virgo detector in dual recycled configuration is expected to work with different tunings of the SRC. Here only the one which optimizes the sensitivity for NS-NS inspirals has been considered. The SRC tuning will be controlled changing the offset added to the SRCL error signal."

23) Benoit: Section 1.3: There are different possible choice for the SR detuning. What are the impact for the ISC point of view? A sub-section addressing explicitly this would be usefull
The error signal used for SRCL control has a linear range which roughly goes from wide band to the double of the offset foreseen as NS-NS optimized configuration. Therefore we will be able to control the SRCL detuning with a wide range.
The effects on longitudinal and angular control, noise couplings, instabilities, etc were not studied so far. The reason is lack of time and manpower. It remains as something to study in the next years.

24) Benoit: Page 11: The modulation index is set to 0.1. It would be nice to know why and even better,

it would be nice to have a sub-section somewhere discussing the modulation index optimization.
The choice of 0.1 comes from an input from INJ. This modulation index is for sure feasible at all frequencies (except the 130 MHz) with some safety margin. It is enough from the ISC point of view. Larger modulation indexes will be much more difficult and need a reconsideration of the present modulation scheme.

25) Jerome: It could be good to remind in the paper how the CARM feedback is applied, more precisely how to divide the feedback between NE/WE and the laser frequency (frequency range / actuation range).

Added at the end of section 1.3.1:
In addition to these ``mechanical'' degrees of freedom one should consider the laser frequency stabilization loop, which locks the laser to the mean length of the two arm cavities with typical band-widths of few tens of kHz (sec.~\ref{sec:ssfs}). The laser frequency will follow the motion of the arm cavity mean length, called CARM in the previous equations. The typical motion of the CARM degree of freedom, if left free, would correspond to thousands of Hz. To avoid such large fluctuation the CARM mechanical mode will be controlled, acting on the end mirrors, using an error signal coming from the reference cavity \cite{ssfs}. This is the same strategy adopted in Virgo+ and allowed also to extract complete position information to be used for the \ac{GIPC} system.

26) Jerome: For the power recycling mode, the Local Oscillator (LO) will come from a MICH offset. Did you consider that the laser amplitude and frequency noise will be much higher in that case ? With an offset from DARM, the laser amplitude and frequency noise is filtered by the pole of the arm cavity, so the LO is much cleaner (maybe even the spatial profile is more immune to aberration, but that is something to check).

According to the simulations we did with Optickle (briefly discussed in section 1.3.3), in the power recycling case, adding an offset to MICH instead of DARM increases the coupling at high frequency (above 150 Hz) but improves a lot at low frequency. More details are available in reference [6] VIR-0738A-11. This is at least true in the case there are no losses or finesse asymmetries. We are evaluating the effect of asymmetries, but no result are available yet.

However, the requirements on frequency and power noises discussed in sec. 1.3.8 do include the MICH offset.

27) Jerome: It could be good to explain where the 80 mW of power for the MICH/DARM offset comes from, since it is a very important number.

28) Benoit: DC offset. Actually, there is a section here, which gives good justification of where to put the offset, but do not discuss its value beside the one of top of page 13 (it is say that it is 80mW, but without too much explanation).

From the ISC point of view this came as a DET requirement. I don't think the ISC chapter is the right place to discuss it. From the ISC point the lower the offset the better.

29) Jerome: I do not see any reason why the LO at the dark power must be constant when the power changes from 25W in input to 125W ? I would think the DARM offset is constant but not the power at the output. When the input power is increased 5 times, the junk light that the 80mW has to dominate will be 5 times larger (optimistic case), so the safety margin will be reduced.

In principle I agree with you, but again this came as a requirement from DET. If you change the amount of LO field at dark port as a function of the input power, you would get, among other things, much more stringent requirements on the OMC filtering

30) Jerome: P15, just above 1.3.5. "Alternative schemes are being considered [11]". Could add in one line the alternative schemes and still keep the reference.

Added:
"such as controlling the CARM loop directly using the OMC error signal."

31) Benoit: Section 1.3.4: A non linear extraction of z(t) from equation 1.2 might relax the requirement and remove some of the non linear effect in the loop.
This non linear extraction will not work, as already discussed at (DET?) meeting on 06/10/2011. If the DARM signal is dominated by spurious noises at low frequency, there is no way to extract real residual DARM displacement information. Therefore it is not possible to do a non-linear reconstruction.

32) Jerome: P16, you give a requirement on the maximum acceptable power transmitted by the OMC for the SB. What about the higher order modes, can they also fluctuate in power in the frequency range of interest ?

I agree that HOM can fluctuate a lot. This has not been considered, since we did not have any good way to estimate how much the HOM of the carrier will fluctuate. We though of using B1p in Virgo, but that was anyhow dominated by sidebands.

33) Benoit: Page 15: Last paragraph: of 1.3.4: You should at least use the nominal values for the OMC length or probably better, leave this discussion for the DET chapter.
I used some number for which computations were already done. It seemed to be not useful to compute requirements using the old OMC design, since it is not fulfilling th requirements. And for the double OMC, I did not have time to think about the implications. Using a longer and higher finesse OMC to compute requirements gives more stringent requirements, so it should be safe.
The design of the OMC is clearly a DET problem, but requirements on the CARM accuracy is a ISC problem.

34) Benoit: Section 1.3.5, second paragraph: The assumption made is the power fluctuation of the sideband is 2%. But later, in the alignment section it is 0.1%. This should be made more consistent or the difference explained.
35) Benoit: 1.7.3: Where the 10**-3 requirement on power fluctuation is coming from? What would be the consequences of changing this number by an order of magnitude (both ways)?
These are two different kind of problems.
In the computation of alignment requirements we want to set stringent requirements for the power fluctuation of carrier and sidebands, since the simulation takes into account a perfect interferometer without defects, asymmetries or aberrations. For the carrier case the requirement of 1e-3 comes from the consideration than in Virgo we reached 1%, therefore an additional factor 10 seems safe, since the sensitivity will be much improved. We should implement simulations of the dependence of the sensitivity and noise coupling on alignment, but this is out of our possibilities now (lack of manpower, time and simulation tools). For the sidebands, we want to stay on the safe side. We know from Virgo experience than when defects and aberrations are present, sidebands are much more sensitive to alignment than expected from simulation.
In the case of the OMC requirement, staying on the safe side is the exact opposite. We should consider some kind of worst case scenario for sideband fluctuations, and assuming the same level of fluctuation as in Virgo+ seems reasonable.

36) Jerome: P18, table 1.6. Is there a simple explanation why from power recycling to dual recycling some degrees of freedom (MICH,PRCL) are not controlled with the same sidebands ?

It is indeed possible to use more or less the same signals. However, in each configuration the best error signal in terms of signal-to-(shot)-noise. So if a different signal is used, it is because it gives a bit more SNR.

37) Benoit: Section 1.3.6: This is the core of the ISC deliverable. It seems “lost” between different requirements. Could it go earlier, may be as 1.3.4?

I don't think this is really the core of the ISC work. This section contains the performances of the longitudinal control in steady state. ISC is supposed to be able to design also angular control and lock acquisition.
I think we should not move it earlier, since the present position is the best one to follow the logic of the document.

38) Benoit: Still section 1.3.6: It would be nice to give a short description of the design process and what are the filters obtained and sensing matrix used. BTW, I’m not sure it is wish to write that “There is no more room for filter optimization”, at least without explanation.
The design process is explained with greater details in the note VIR-0738A-11. I don't think they add much to the general reader, but if the review panel believes a larger number of details will be useful we can add them. To be consistent, the same level of details must be reached also in the angular control section.
However, sensing matrices are strongly frequency dependent, so we should add lots of transfer function plots or lots of tables, otherwise the information will be partial and useless. Moreover this information will be needed in all configurations. We will end up adding tens of plots or tables, with no clear additional information in my opinion.
The sentence “There is no more room for filter optimization” is true, but maybe a bit too short. We can replace with
“The filter optimization is a balance between having high gain below 2-3 Hz to be able to reach the accuracy requirements and string enough roll-off at higher frequencies (above 10 Hz) to avoid re-introducing control noise in the detector sensitivity. The stability of the control loop imposes some limits on the possible optimization. The present design is barely fulfilling the requirements with the minimum acceptable stability margins (30 degree of phase margin and less than a factor 2 of gain margin). Further optimizations in the present scheme are not possible.”

39) Jerome: P20, Table 1.7. Could you give a comparison of the SB relative amplitude noise given to what has been achieved with Virgo+. I would like to have an idea if the noise required is critical or we could have such level without too much special care.

I could not find any estimation of sideband amplitude noise in Virgo or Virgo+. The requirement for amplitude noise corresponds to -136 dBc/Hz, to be compared to the measurement shown in the INJ chapter, figure 7 of section 1.6.4. The measurement is below the requirement above 100 Hz, while it is still higher at low frequency. I would avoid repeating the same thing already written in the INJ chapter. However I can add a cross-chapter reference:
“Refer to [INJ chapter, section 1.6.4] for a comparison with the expected performances.”

40) Fulvio: In the section 1.3.8 can you specify better what it the meaning and the implications of the phase noise requirement " at the mixer input"?

Added to the document a better explanation:

“Modulation noises enters in the photo-diode output signal following two paths. The first one is through the noise at the level of the modulation, which is transmitted to the laser beam and enters on one of the inputs of the mixer. The second path is through noise introduced by the local oscillator distribution system, which enters on the other input of the mixer. The requirements given here apply to the total of the two contributions."

41) Jerome: P21, In the text, to follow the logic it is better if the part about the power recycling appears before the dual recycling case.

Done

42) Jerome: 1.3.10. In case of no etalon, what should be the requirement on the transitivity matching of the 2 arms ? to know if it is achievable or an etalon in the ITM is mandatory.

The requirement on finesse asymmetry is the one reported in section 1.3.9: 400ppm loss asymmetry and 1% finesse asymmetry. I forgot to recall them in the MIR requirement section. I added there the following sentence:
"To meet the sensitivity requirements, in the power recycled case losses asymmetries must remain below 40~ppm and finesse asymmetry below 1\% to be able to meet the requirements."

43) Benoit: Page 27: Etalon effect. The end of section 1.3.9 seems to explain that we need a good finess asymmetry only for the power recycling configuration. Do it means that we need the etalon effect (with the large cost overhead), "only" for the PR configuration? Anyway, the justification of the need for the etalon effect seems a little too short given the implications.

The requirements on finesse and losses asymmetries are stringent in the case of PR configuration. The choice of having etalon was already considered a good idea in ISC and OSD, since it allows a better handling of beams and provided an additional handle to cope with other asymmetries which were not simulated yet. This is explained in sec 1.3.10. Additional motivations should come from OSD.

44) Benoit: Section 1.3.8: Replace "technical noises" by power and frequency noise in several places, especially in the figure. BTW, do we need to keep all figure 1.4 to 1.7?
The name technical noises is explained in the section and was used for brevity. If desired we can change it. Figures from 1.4 to 1.7 are useful t understand the effect of asymmetries. I would not try to put everything in one single picture. We might leave only the requirement plot, but the general reader will lose some information in my opinion.

45) Benoit: Page 28: Is the finesse 445 or 450?
There might be some small differences in the number written here and there. The baseline value should be 445.

1.4 Longitudinal control system: lock acquisition

46) Jerome: 1.4.2 what is the expected input laser power to start to lock. And after a steady state is reached with this power, how long time does it take to get to 25W (roughly). Those numbers could be mentioned.

The input power should be low enough to prevent any significant thermal effects in the interferometer, but high enough so that the photo-diodes and quadrants used for the lock-acquisition and the initial alignment receive enough power. Exact values are to be decided, but we would probably start with an input power of 5-10 Watt.

During Virgo and Virgo+, about 10 minutes of the lock-acquisition were spent waiting for the thermal transients. It is expected that we can ramp up the input power in a similar time-scale.

47) Benoit: The figure 1.10 could probably be dropped since it is just an example.
The figure shows an experimental result of the technique which is planned for the lock of the Advanced

Virgo cavities. I think it is relevant.

48) Jerome: Still 1.4.2, could you add one sentence to say what is the technique use to lock MICH at half fringe using the DC power ? (a kind of dithering ?)

As noted in the text, this is a standard side-of-fringe locking using DC-signals only. In reality, we normalize by the power in the PR-cavity, so the error signal is B1p / B5 – offset. Added to the chapter:

"the ratio of the power transmitted to the dark port over the power impinging on the arm cavities (read with the PR pick-off beam) gives an estimation of the Michelson fringe value."

49) Jerome: Table 1.8, Could you add that the arm cavities are locked in the table (if it true) and does it take into account the change in input power from the start of the lock to the science mode or is it normalised to 25W ?

All states described in the table have the arm-cavities locked. As noted in the caption of the table, all values are calculated for 1 W in input power. The label "fringe offset" in the table has been changed to "MICH offset" to be more clear.

50) Jerome: P33, just before 1.4.3 "we do not expect large effects due to radiation pressure in the power recycled configuration". Does it mean the use of Optickle was not mandatory in that case and that Finesse could be use (and so the influence of aberration could be checked).

This sentence was mainly referring to lock acquisition. However, in the case of the longitudinal control this might be true: if a MICH offset is used for DC readout, we see no large radiation pressure effect. This means that we might use finesse. However, it would be safe at some point to be sure that radiation pressure really does not play a role in the case defects are present.

For sure the same is not true in the angular control case. Radiation pressure does play a role even in power recycled configuration.

51) Benoit: The section 1.4.2 is very detailed and could probably be trimmed down
This is a schematic explanation of the Variable Finesse technique which is planned to be used for the lock of AdV in PR configuration. People not from Virgo might not be familiar with it. I believe it contains useful information for the general reader.

52) Fulvio: It is matter of fact that the strategy for controlling the interferometer in the final configuration (125 W +SR) is not studied yet. This is explicitly mentioned but no plan and timeline for the study is included.

We have a strategy for the steady state control. You comment is true for what concerns the lock acquisition strategy. Studies on lock acquisition started few months ago and focused so far on the problem of arm cavity lock and power recycled lock. We are planning to build some strong know-how on the use of e2e time domain simulation, starting from next month. We can expect to have some results for the beginning of the next year concerning dual recycled lock acquisition.

1.5 Auxiliary lasers

53) Benoit: The section 1.4.3 and the introduction of 1.5 are not very consistent. It is a little bit unclear of the strategy. Are we just making the ITF compatible with the possible installation of the auxiliary laser, waiting for further lock acquisition studies, or is it the baseline?

54) Fulvio: It is not clear in the present form what is the importance of the auxiliary laser strategy. Did we study it to reduce the project risk? Is it useful in the context of future study for the controlling the

SR interferometer?

55) Fulvio: Moreover, if we need to apply this technique for the first two steps of Advanced VIRGO how far we are to have at least the conceptual design for the integration of the system with the main VIRGO optics?

The use of auxiliary laser is foreseen in the Advanced LIGO lock acquisition scheme for dual recycled configuration. Since we have so far no plan for the problem in Advanced Virgo, we can consider the use of auxiliary laser as a risk reduction strategy. It might even become a required infrastructure once we have some better idea on the lock acquisition scheme for dual recycled.

It has been already shown with Siesta simulations that if we do not change drastically the actuation mechanisms the auxiliary lasers will not be needed to lock the long cavities.
However the installation of the auxiliary lasers is already planned with the different subsystems (DET, DAQ, MIR, INF). Most of our optics will be host on an "in-air" optical table (close to the mini-tower) and so will be quite independent of the DET benches. The final design of the auxiliary laser table could be achieved when the end arm telescope parameters will be known. The optical scheme will be quite standard: a laser, a modulation system, a reference cavity, a telescope for the beam matching, some optics to control the beam alignment and its position and the different photo-diodes.
All optics we need on the vacuum benches have already been foreseen in their design. The coating of the mirrors for the second wavelength needs to be approved. The acquisition and control scheme is also already planned on the DAQ side. The remaining points to clarify are the design of the reference cavity needed to be able to lock with the auxiliary laser and the exact position of the table in the end arm building.

We can also add in the introduction:

"without changing the lock algorithm we can already reduced the locking force by at least a factor 5. It could even be reduced more by using complex algorithm, see part 1.4 ".

56) Benoit: Last line of figure 1.12 caption: Is there a price on the sensitivity (due to coating thermal noise) required by the use of auxiliary laser?
The new coating have been done to reduce at maximum the number of layers: between the two designs the width of the coating will be changed by 300 nm (on a total of 7 um). So we can expect that the impact will be negligible.

1.6 Sideband aberration risk reduction strategy

1.7 Angular control system

57) Jerome: Regarding the angular control, I keep wondering how is degenerated the control for PRM/BS and SRM. A sensing matrix would be welcome to appreciate the difficulty. Is there a way to understand why the angular control for PRM is from demodulated f1 while for BS is from demodulated f2, could be also the otherway around.

The BS and PRM error signals can be exchanged if needed by retuning the Gouy and demodulation phases. It has been chosen to use the best signal, in terms of SNR, for the PRM since it is strongly coupled with the Differential(+), especially in the final configuration. A table showing the central interferometer sensing matrix was added to the TDR. The SRM signal was decoupled using a hierarchical control scheme, since it uses drift control.

Added to the TDR:

	PRM	SRM	BS
PO f_1	103.4	0	-74.4
APp A DC	0	1	0
PO f_2	-2.1	0	-4.3

Table 1.11: Sensing for the central interferometer DOFs in case of high power configuration with SR cavity, the most coupled configuration.

The \ac{PRM} is controlled by using the first demodulation frequency while the BS using the second one. The \ac{BS} and \ac{PRM} error signals can be exchanged if needed by retuning the Gouy and demodulation phases. It was chosen to use the best signal, in terms of SNR, for the \ac{PRM} since it is strongly coupled with the Differential(+), especially in the final configuration~\cite{darm}. This sensing scheme should then assure the lowest PRM control noise re-introduction in the Differential(+) mode.

58) Luciano: Section 1.7.5, pag.43 **–** Could you specify starting from which frequency the QPD sensors on the end benches should be shot noise limited?

Quadrant signals are assumed to be shot noise limited from 10 Hz. Added a sentence to the text:

"In other words all quadrant photo-diode signals are assumed to be shot-noise limited everywhere above 10~Hz."

59) Benoit: Table 1.11: Give the frequency used for these requirements.
Suspended terminal detection bench displacement requirements . Above 10 Hz. Will be added to the table.

60) Benoit: Section 1.7.4: There is a discussion on the power per photodiode which seems more for the DET section. It might be better to talk more on the equivalent SNR assumed.
The amount of power per quadrant came as an input from DET. Concerning the equivalent SNR, we disagree, since it is the same point as for the longitudinal photo-diodes.

61) Benoit: Implicitly some of the quadrant will use galvo to center the beam. It would useful to explicitly state which ones.
A centering system should be present for all the quadrants, but in case of terminal benches the quadrants could be initially moved (in the pre -alignment phase) by translation stages since in science mode the beam will be centered on the quadrants thanks to the (-)-modes angular control.

62) Benoit: Section 1.5.4 (I assume 1.7.4, since there is no 1.5.4!) is long and would benefit of some internal structure; Removing some of the details of the history (like the end of the section) could also help.
I don't see how we can cut down the section. Even the "history" of the experience in Virgo is important to give a correct perspective to the simulation results and explain why we need to stay on the safe side.

63) Benoit: It is often complain that we don't have a good simulation, but the goal of developing it is not given, while we expect that as an ISC task (for longitudinal and alignment).
The development of a better simulation tool will be foreseen in the new version of the WBS. So far there are no results, so it was not mentioned in the TDR. We might add a sentence, where?

64) Benoit: Section 1.7.6: I'm not sure to understand the second paragraph.
We can try to rephrase it:
"The residual angular displacements and the AA control noise, evaluated with the Optickle simulation, are in agreement with the accuracy and the sensitivity requirements, for all the three configurations. No major issues are emerging from the control point of view even if some important aspects have been highlighted. The most critical degrees of freedom in terms of control noise are the (-)-modes, for all the three commissioning configurations"

65) Benoit: Section 1.7.7: It would be good to give the specifications before the telescope and may be leave the optimization in the DET section, especially since the telescope tuning does not seems easy.
To compute the requirements for the RMS displacement of the bench the angular error signal for the (-)-mode, which is a pure translation, has been analytically evaluated.
The error signal has been computed by using the beam displacement at the input of the telescope, the Telescope TF (for shift and tilt), the beam waist and the power impinging on the quadrant (to compute the quadrant signal).
The SBE seismic requirements could be evaluated at the input of the telescope only for the RMS of the bench shift. In case of bench shift, indeed, the effect of the telescope is the same for the bench and for the beam (both generate a pure translation for the beam).
In case of bench tilt the TF that have to be considered are different. The AA error signal computation takes into account only the Telescope shift TF while for the computation of the fake error signal due to the bench tilt uses the Telescope tilt TF.
Moreover for the computation of the bench requirements in the detection band, above 10Hz, the requirements have been computed by comparing the shot noise of the quadrant, which does not depends at all on the telescope design, and the fake error signals due to the bench displacement, which depend on the telescope design.
In conclusion it is mandatory to have the telescope TFs to compute the bench displacement requirements, except for the shift_RMS, and they cannot be evaluated at the input of telescopes.

66) Benoit: P. 38: Could you give the value of the (-) mode frequency?
It will be added to the text. It is approximately 1.1Hz.

67) Benoit: Last lines of figure caption 1.15 and 1.16 could be dropped.
Refers to "The control noise results to be compliant with the sensitivity requirements". If desired we can remove it.

1.8 Software requirements

68) Benoit: Most of section 1.8.1 could probably be dropped. I'm not sure also that the discussion about ALP (section 1.8.2) should be in this chapter.
Details might be removed.
I think the discussion about ALP is important, since it is an ISC task to develop the lock acquisition procedure and this includes requirements and suggestions for the automation software.

1.9 Summary and conclusions