

At the end of Section1 Daily alignment of the interferometer.

Lisa: Did you succeed in locking the DRMI without the arms?

Yuta: Yes. We could hold the lock for 30 minutes or so. But it was not always possible. If suspensions are crazy, we could not lock.

Lisa: But you managed to have a good alignment to adjust the central part.

Yuta: Right.

Maddalena: Why you cannot dithering for Yarm? When you are locked, you control both ITM and ETM.

Yuta: You mean, in this daily alignment scheme?

Maddalena: Yeah, once you pre-align with the camera, with this feedback and so on, I think you can lock. No?

Maddalena: You can dither to fine align both mirrors.

Yuta: Yes, I think that's possible. Once the loop was closed. But I think we didn't use it taking daily basis.

Maddalena: I mean it should be enough using the lock and dither tool, setting the good position. You can also center the ITMY.

Yuta: Masayuki, do you have any comments? Did you do beam centering on ITMY using dithering?

Masayuki: No, we only use the camera on ITMY.

Yuta: Is there any reason that you didn't do the dithering for centering?

Masayuki: Because we can't have the error signal for the dithering. I guess it is because of the frequency noise of the laser.

Maddalena: So your locking signals are too noisy?

Yuta: But maybe you could use the transmission of the IR to maximize the transmission. For ETMY, you could do this. Maybe it is also possible for the ITMY.

Maddalena: Usually you could use BSC to maximize the transmission while the alignment the cavity is dithering on the correct signal. So you got the two cavity mirrors centered and then you adjust the incoming beam and maximize the power. This is a suggestion.

Yuta: I am not sure if we tried or not. We should think about that.

Valera: I have a question. Is the temperature stability of the tunnel as expected?

Yuta: I am not sure but I hear the temperature stability is worse than expected.

Yoichi: If no one is in the tunnel, the temperature stability will be good. Unfortunately, this is not the case when there are a lot of machines and computers running. In the end, the temperature variation is large. If we do nothing, it can be several degrees. Miyoki-san and others did a lot of hard work to install the air-conditioner and heaters to stabilize the temperature. I think right now, especially when people are working for the installation work and so on, the amplitude of the temperature could change by 1 degree. Maybe I have the wrong number. This gives us a lot of drifts for vacuum chambers and suspensions. We are going to put heater systems and temperature control systems to stabilize the temperature of the chamber wall. We will see how well it works.

Lisa: 1 degree over which time period?

Yoichi: It depends. Let's say one week or maybe a few days. It really depends. Sometimes, someone introduce one machine, the temperature starts to increase. In that case, the temperature can change in about one or two days. The temperature of the tunnel is determined by the equilibrium between the generated heat from these machines and cooling capacity of the tunnel walls. The cooling capacity of the tunnel walls depends on the underground water level. The water is taking away the heat. That's why the cooling capacity has a seasonal feature. So even the heat generation inside the tunnel is constant, because of the seasonal variation of the underwater level, naturally the tunnel temperature changes. It really depends on the season, but the time constant of the variation is about, I believe, a week or weeks due to this mechanism.

At Section 2 [Yuta's slide](#) page 17 and page 18.

Lisa: For the simpler configurations, like just the arm, just PRMI, just DRMI, not the full interferometer, could you close the WFS in these intermediate configurations.

Yuta: Yes, this is the summary (page 18). For example, for single Xarm, we could close the ASC signals in SOFT and HARD basis. For PRMI, we can close PRM, PRC and also BS. For DRMI, we couldn't do that. Only some dither loops were once successful, but not wavefront sensor signals. For FPMI with both arms, we couldn't lock all SOFT and HARD mode. We could only close differential ETM and common ETM. Using AS, we could also close BS loops. For PRFPMI, we could basically close the AS loops, but we didn't have much time. This is the situation. For intermediate steps we could close some of them.

Lisa: Do you use these loops in your initial alignments? I mean, as you start to lock the interferometer, which we are doing in LIGO is, like closing WFS in the step of intermediate state, then you open up as you start to bring the arms in?

Yuta: For KAGRA we didn't do that. In the initial daily alignments, we just do dithering and we never close WFS loops. For example, when we do PRMI alignment, we just do dithering and don't close WFS.

Maddalena: In Virgo, we engage all the loops, and re-opens while bringing other mirrors.

Lisa: That seems the somewhat obvious way to try to make it more reproducible.

Yuta: I think for Xarm and Yarm, we closed some loops using transmission QPDs, but not WFS, so maybe we can try to do that.

Maddalena: Virgo is enough to use beats in locking phase. Use dither to center cavity mirrors. We don't engage WFS.

Yuta: So in Virgo, in locking phase you don't close WFS loops. Only dithering?

Maddalena: Close the PR, but for the arm, dithering is enough. It depends on the accuracy requirements.

Yuta: After the full lock, you will engage WFS. I see.

Valera: Why do you have to engage all control loops, otherwise you won't be able to lock? Or engage all angular control signals as zero as possible. Even before full build up, we open and scale, not all simultaneously, but as degrees become available, we engage.

Masayuki: The reason that we don't use WFS because our WFS is not very reliable. Yuta said it has a reproduce trouble. We are not sure why it is not reliable. Could be due to some offset that changes the zero point. We ever tried to use WFS to align arms. Some of them we can use but the others we cannot. If we have the WFS, we should use it for the alignment.

Maddalena: In Virgo it is essentially the same that we can use WFS when the arms and the interferometer is well aligned. So we align it before dithering in order to be sure we are in the alignment working point and then we can engage WFS. Yeah, this is not so reproducible.

Masayuki: I see.

Yoichi: The non-reproducibility may be to do with the birefringence of ITMs. As you may know, our sapphire mirror has high fluctuations of birefringence, depending on the position on the mirror. The amount of birefringence means the amount of light from s-polarization converted to p-polarization during the transmission through the ITM substrate. Because the birefringence has a kind of very complex pattern, I think the scattering to higher order modes in p-polarization depends on the beam position on the mirror. The small displacement of the beam on the ITM may cause a significant different response in terms of the higher order modes and especially RF sidebands, which don't have mode healing effect from the arm cavity. We are just suspecting or guessing. That could be the reason why KAGRA WFS is not so reliable.

Yuta: This is one of the example (slide page 17) simulated with FINESSE. This is the shape of the WFS signal with respect to the tilt, e.g., of common HARD mode. These plots shows WFS signal with and without TWE maps. As you see, the zero-crossing point is different and also the slope is different. If the beam position on the ITM changes, the TWE map changes, the zero-crossing changes, and maybe the slope changes. We suspect this is the case. For this simulation, we only include inhomogeneity and we

didn't include the birefringence. If we include the birefringence in the model, maybe the more complicated situation can help explain the reason.

At Yuta's slide page 21 and page 22

Maddalena: Which is the vertical and horizontal displacement of the test mass that you can get? Since you have temperature fluctuation, maybe you also have issues on suspensions.

Yuta: I don't have the number at the moment.

Yoichi: You are right that our vertical spring will change the height. But we have heater control, you know, we have the coil made actuators on the springs. With the feedback control, we try best to control the height and keep it as constant as possible. When the temperature changes by one degree, these feedback loops are close to saturation point. That is the problem. What we were doing e.g., during O3GK, was monitoring the temperature change and use the kind of warning system to tell people the loop is close to saturation. Then people rush to turn on the heater, or the cooler, or turn off one of them, to manually adjust the temperature into a better range. So that's what happened.

Maddalena: What is the accuracy band? How many micrometers can you keep?

Yoichi: With the DC control, it is pretty well. I think we can keep it in the micrometer level.

Yuta: That's for the room temperature suspensions. But for the cryogenic suspensions, maybe the situation is different?

Yoichi: The Type-A is half cryogenic suspension. At the room temperature part, the situation is basically the same. But for the cryogenic part, when it is cooled down to 20K from room temperature, the shrink is about 10mm in length, so the mirror will move up by 10mm. But we know it happens, and we push down the spring using the fishing rod mechanism to compensate the height change. After that, around 20K, the thermal change of the spring is very small. Even change the temperature a little, the height in the cryogenic part won't change much.

Yuta: But during O3GK, the test mass temperature is at 250K or so?

Yoichi: Yeah, that's the bad range. That's where the temperature change directly translates to the height change. The O3GK situation is very bad.

Yuta: For O4, we are planning to start at room temperature. Maybe that is an issue.

Yoichi: Yes, but it really depends on the temperature stability. The vertical spring of cryogenic payload is much stiffer than the room temperature spring. I think in the end the height change through the temperature fluctuation will still dominated by the room temperature part.

At Yuta's slide page 27

Lisa: How is the process of mode matching? How will the mode matching apply? Move the suspension?

Yuta: We don't have to open the chamber. We have an actuator to move PR2 and PR3 in longitudinal direction in probably 0.5cm.

Yoichi: Actually, I want to ask what's the mode matching value between the PRC and the arm cavity for LIGO? While Virgo has marginally stable PRC, the situation may be quite different.

Valera: 99%.

Yoichi: So our 90% is certainly very low. By the way, did you do the fine adjustment after installations of suspensions to achieve this value?

Valera: The Advanced LIGO design is good but previously in Advanced LIGO, it was 10% or 20% to improve.

Yoichi: I see. I thought I designed the KAGRA cavity very well.

Kentaro: Is there TCS in LIGO PRC?

Valera: Yes, but we don't use it.

Valera: What I said is for the total interferometer mode matching. I am not sure if you really asked the mode matching between PRC and the arm cavity. Total mode matching means the total input beam to the coupled arm, which is 99%. And I think that's your point, right?

Yoichi: That's right. That's a very good number.

Valera: The reflectivity of the arm is critical coupled. There is impedance match. If you subtract the impedance match and sidebands, estimation is possible.

Lisa: (Asking for the 90% mode matching ratio) Your number also includes the mis-alignment. So how much is the actual mode mismatch?

Yoichi: This number is measured by the mode scanning and counting all the non-TEM00 mode peaks and summing them up. That was 10%. So it certainly includes the mis-alignment component.

Yuta: Actually that's not true. The measurement we had for the 90% excludes the mis-alignment. So we exclude TEM01 mode.

Yoichi: So not higher than 01 mode. The lowest mode is subtracted.

Valera: And the impedance match is also subtracted.

Yuta: For the 90% measurement, we mis-align the PRM and measure the mode matching.

Valera: You actually mis-align the PRM, it is to the arm, not to the coupled cavity. That is the difference. The low finesse is the difference. For the arm, we think we also have good mode matching. Your measurements that you described is just the simple beam to the arm.

Yoichi: So it could be the problem is in the mode matching telescope rather than the PRC. In that case, adjusting the alignment of PR2 and PR3 may not be a good idea.

Lisa: You did lock the PRM, so you could make a number equivalent to what Valera called, right? By looking at the REFL beam. Do you have the number?

Yuta: We don't have the number. Maybe the situation is a bit different for KAGRA case as we have birefringence. The REFL port only sees the s-polarization because we have a Faraday which is polarization dependent. I am not sure if we can account for this issue for calculating the mode matching. But we have to check.

At the end of Section 2, Yuta finished the talk.

Lisa: What do you expect from the birefringence model? So you hope to find combination of signals that allow you to lock and align, or you just try to understand what you see.

Yuta: Both. We want to see if our interferometer is operating as expected. For example, if we lock the arms, the amount of p-polarization reduces. We want to check if this is coming from the mode healing effect for the birefringence or not. We want to understand these things from simulation. And also for the ASC signal, we did some investigation on where the signal can be obtained for alignment of each mirror. And we did simulations for this kind of sensing matrix. The sensing matrix may be very different if we include birefringence effect. Those are what we want to check with simulations.

At the last of the meeting.

Tomotada: (looking at Yuta's slide page 7) I am interested in what is the concrete idea or method to do the mode matching. Using the IMMT1, IMMT2, PR2 and PR3 is likely a problem.

Maddalena: In Virgo it is a bit different. What we do is to match the input beam to the long arm cavities. We keep the mirror, one of the two, and measure the amplitude of the higher order mode. Then we don't do the matching to the coupled cavities. We do only for the long arm cavities.

Maddalena: So about the numbers, we have something like 99% or 98%. But we also run with 90% for mode mismatch.

Tomotada: In O3GK, we somehow ever touched the IMMT1 and IMMT2, and their condition is different now. Maybe we should do the mode matching again. I am not sure about the concrete way. Maybe to measure the beam profiles and do similar things.

At Section 3.

[Kenta's slide.](#)

Page 6

Maddalena: How important is shift? With only one QPD, you can only control tilt.

Kenta: Yes, we control only tilt.

Maddalena: Have you estimated how much the shift affects your system?

Maddalena: How much is the UGF for this IMMT QPD loop?

Kenta: 0.1 Hz.

Lisa: What's the limit for 2 hours?

Yuta: 2 hours is just our original target. We can lock even longer of course.

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Maddalena: We can see input pointing and frequency noise with the main interferometer.

Maddalena: How about using QPDs between IP2 and IMC, using pick off? There's a system called BPC in Virgo.

Lisa: Maybe we should gather the information from LIGO and Virgo. We don't need to re-invent.

Yuta: I think we also have QPDs installed at PSL. Why aren't we using them?

Masayuki: We have them, but we never tried to use them.

Maddalena: Are those QPDs before the actuator or after?

Masayuki: Before the actuator.

Maddalena: It doesn't work.

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Maddalena: What caused the unlock of 15-hours lock?

Kenta: Earthquake.

Maddalena: You said you want to change the actuator. Why you want to change the piezo?

Kenta: We use PZT actuator. Not to change PZT.

Masayuki: I guess Kenta is saying to change the control scheme, not the actuator itself.

Kenta: Yes.