The submitted manuscript describes a system used to stabilize the output frequency of a high powered Nd:YAG laser. The laser design and the system are described in detail as well as the testing done on the system to ensure functionality and long term stability.

In my opinion, the submitted manuscript is very well written from a technical perspective. I also believe the manuscript is within the scope of AMT. However, given how the manuscript is written, I personally would have submitted it to a more optics/laser focused journal. The sourcing is lean but I think sufficient as well. I have no major issues with the technical detail from manuscript as written and would suggest it be published subject to some very minor revisions that I think would clarify a few confusing details.

One issue worth mentioning in a general comment is that while I think the manuscript is within the scope of AMT, the reader needs to bring motivation and knowledge of the atmosphere with them. As I read this manuscript, the laser is clearly targeted at a scientific application (Doppler wind measurements) but the description seems to have almost surgically removed the application. For example, while NLCs and winds are mentioned in the intro in a general way, it seems odd to me to have only mentioned wind accuracy in the conclusion. From an engineering perspective, the content is great. From an atmospheric science perspective, several things remain unclear to me: does this locking method improve your measurements noticeably or are other sources of error (for example shot noise) dominant? What was the accuracy you achieve before this system? Does this system enable scientific exploration that is otherwise inaccessible? These seem to be the most relevant questions to me as an AMT reader.

We thank the reviewer for spending time and work in examining our manuscript. For clarification we would like to mention that the journal relation of our manuscript is GI (Geoscientific Instrumentation, Methods and Data Systems) instead of AMT. We chose this journal because our aim is to publish the technical aspects of the FCaM system rather than its scientific application. For this reason we mentioned NLC and wind measurements only in a general way by giving some basic numbers, so that the reader can assess the quality of the technical solution. In the following we reply to the individual comments (in blue color).

1. The title refers to ALOMAR but the manuscript mentions the Kühlungsborn RMR lidar as well. I might consider a more generic name if there are 3 such systems not all running at ALOMAR.

The capability for Doppler wind measurements as well as methods for quality monitoring were implemented at the ALOMAR lidar many years ago. Also the FCaM system was developed and installed for this lidar. Only in recent years we started to standardize the RMR lidars operated by IAP and implemented proven solutions from ALOMAR in the Kühlungsborn lidar. For this historical reason we prefer to keep the title.

2. In my opinion, while it is true that strict stabilization of the laser frequency is required for wind measurements, the manuscript hardly mentions it. This is why I comment that I would have targeted a more optics focused journal. In response, I would suggest:
    1. Link the frequency accuracy to your wind measurement accuracy in the abstract.
    2. Comment on how much of your error budget for winds is occupied by frequency stability. Here I simply mean, if you have improved your wind measurements

markedly, show that. If your main error source is shot noise and the stability is contributing less to that budget, it would be good to know.

3. Describe the level of improvement this locking system provides over your previous system.
4. With an accuracy of ~11 cm/s, can you describe what scientific questions can be addressed that were not addressable with a previous stabilization system?

As the journal relation is GI instead of AMT we refrain from discussing these topics, which would be definitely necessary for AMT.

3. Line 28: Is 4 picometers approximately equivalent to 4 GHz? Not 4 MHz?

Yes, that's a typo, the correct relationship is: 4 MHz => 4 fm. We will change the text accordingly.

4. Line 96-97: In the text, you specify 2 hours to reach stability but only show 1 hour. I would tend to show 2 hours of data in Figure 2 to show how stable looks. For example, I wonder if the voltage jumps become less frequent of disappear completely.

We will change Figure 2 to show the time period from 8 to 10 UT. While there were 7 voltage jumps during the first hour, only 2 jumps occurred during the second hour.

5. Line 113: It is not clear to me why a diffuser is helpful here. I would think increasing the angular distribution of light would be generally detrimental to etalon performance. Can you please comment?

The diffuser produces a uniform illumination of the etalon. This increases the efficiency of the system and reduces sensitivity to changes in the angle of the incoming beam. The latter is particularly important for the power laser beam which is guided approx. 2 m by mirrors over the laser table before entering the etalon. In general, the diffuser makes the system more robust.
See also, e.g.,
D. Rees et al.: Stable and rugged etalon for the Dynamics Explorer Fabry-Perot interferometer, https://doi.org/10.1364/AO.21.003896
Xusheng Xia et al.: Method to improve the resolution of a non-parallel Fabry-Perot etalon, https://doi.org/10.1364/AO.57.008757

6. Line 119: I am a bit confused here. I can take this statement to mean either that you read data from 1 intra-pulse period every second from the seed laser or that you read 100 and average/sum them into 1 data point. Can you clarify?

We read data from only 1 intra-pulse period every second from the seed laser.

7. Line 126-127: I presume given that Figure 5 and 7 use fractional pixels that you are fitting a curve to the data. Is that true? If you are fitting something, can you say what that is? If you are just using the raw data, can you specify how FWHM is calculated and how you get to fractional pixels?

The processing chain is as follows:
- Determine peak maxima (integer) and background level in the 1-d array of intensity as function of radius in pixel coordinates.
- Following the intensity values from a peak maximum in left and right direction until the background level is reached yield arrays for the left and right slopes of the peak.

- For each slope array: find the two intensity values between which the half maximum value of the peak lies.
- Linearly interpolate the half maximum value using fractional pixel values yield left and right positions of the FWHM value. The difference between these positions yields the FWHM value in fractional pixels.
- The sum of the left position of the FWHM value and the half of the FWHM value yields the position of the peak maximum in fractional pixels.

8. Line 133: This statement is a bit vague in my opinion. Adding parallelization ad infinitum will slow down your code eventually with communication (not calculation) being the bottleneck. Is this statement just meant to say that you are doing calculations on an FPGA in a parallel way or are you talking about multi-threading in your software?

LabVIEW is an inherently concurrent language which makes it relatively easy to program multiple tasks that are performed in parallel via multithreading (https://en.wikipedia.org/wiki/LabVIEW#Parallel_programming, 2025-09-23). Additionally the code on the RT level was structured in a way that more complex calculations are separated from time-critical parts of the code and the data transfer between them is done by FIFO structures. So it is mostly multithreading on the RT level that matters. To further improve the timing, user interaction and file operations (writing logs etc.) is outsourced to the client running on a windows computer which communicates with the server (embedded controller) via network shared variables. We will add such information to the manuscript.

9. Figure 4 Inset: I am not clear why you would use time as the x-axis. Wavelength makes more sense to me.

We agree and will change the x-axis to wavelength scale.

10. Line 157: I would modify this statement to "…light enters the LPS continuously".

We will change the text accordingly.

11. Line 169: 10% of data seems rather large to leave without a physical explanation for lack of correlation. Are there any physical processes that could explain this data?

To be honest, we were surprised at how well the spectrometer reproduces the frequency variations of the seed laser while locked to an iodine absorption line. Presumably the spectrometer is responsible for the 10% disagreement rather than the seed laser stabilization setup. The upper panel of Fig. 5 covers data in a LPS peak position range of about 0.8 MHz, corresponding to a camera pixel range of only 0.04, and the lower panels show decreasing correlations with decreasing LPS peak position ranges. These numbers are simply at the measurement limit.

12. Figure 5: I would change your colors to accommodate red/green colorblind readers.

We will change the colors.

13. Line 222: Do you mean resonance-fluorescence lidars? My understanding of fluorescence lidar is that the spectral features are relatively broad (order 1-10 nm at least). It is not clear to me that any amount of precise laser locking should affect that.

The spectral features are indeed relatively broad compared to the linewidth of typical narrowband lasers (few GHz compared to typically <150 MHz). However, in order to measure wind speed

(Doppler shift) precise knowledge of transmitted wavelengths is required. The detectors of Doppler fluorescence lidars are typically broadband, which means the Doppler shift of the scattered light is not measured. Rather, the absorption spectrum of the trace species (e.g. sodium) is measured in the lab frame at a minimum of three wavelengths. In the frame of the trace species' atoms, the wavelength at which atoms can absorb photons does not depend on the environment. Comparing this 'fixed' absorption spectrum to the absorption spectrum measured in the lab frame yields the Doppler shift (wind speed). Hence, the stability of the laser affects directly the precision of wind measurement. The stability requirements are actually a factor of two higher compared to the Rayleigh wind lidar because the measured Doppler shift is only half as large.
See for example C. Y. She et al.: High-spectral-resolution fluorescence light detection and ranging for mesospheric sodium temperature measurements, https://doi.org/10.1364/AO.31.002095
We will be more precise in the last sentence of the manuscript.