We would like to thank the Referee for the constructive comments and helpful suggestions on the manuscript, which helped us to further improve the clarity of the paper. Below, we give detailed responses (in blue) where appropriate.

General comments:

Unfortunately, despite the promising title, abstract and introduction, this study provides more questions than answers. The reader is not told until L53, after an Introduction loaded with information related to the Oldest Ice challenges that this study only reports on the development of a new laser spectrometer for multiple gas analytes. This study does not include any information about the sublimation technique, i.e., how the gas sample will be extracted from the ice. This makes it impossible to assess the potential efficacy of the system for ice core measurements, which makes the last sentence (L377) seem a little premature, and the title, abstract and introduction seem misleading. (…) the instrument is a long-way from being well-suited to ice core analysis. In particular, the section beginning at L337 focuses on one of the challenges of using laser spectrometers for small gas volumes – that the volume of gas must be kept constant. Figure 10 nicely shows the problems this causes for the measurements. As the sublimation system is not described and no results reported, it is difficult to judge how effectively this problem will be dealt with via the calibration method suggested.

We regret that the Reviewer was misled by the title/abstract of our manuscript and we apologize for not meeting his/her expectations. In the revised version, we make clear very early on, i.e. in the abstract and the introduction, that this is only the first of two papers on this topic. The second paper will describe the sublimation extraction in detail and will be submitted next year. However, a thorough description of the QCLAS itself, as done in this manuscript, is already a formidable challenge and easily fills one manuscript. The multi-species capability of our unique dual-laser instrument and its unmatched performance for small air samples of only 1 ml STP in batch mode with similar or even better performance than off-the-shelf spectrometers in through-flow mode and high sample pressures, in our opinion represents a substantial scientific progress and easily justifies publication on its own. Regarding the issue of pressure adjustment, we accentuate in the revised manuscript that (i) using our calibration we can correct for the pressure dependence and (ii) that we designed our QCLAS inlet system in such a way that we can adjust the pressure of our standard gases to the pressure of the ice core derived sample within 0.04 mbar. Moreover, our multi-gas calibration routine ensures that we always have a standard gas that is similar in concentration and carbon isotopic signature to the sample derived from the ice cores. This implies that the important issue concerning the size of the ice core sample is already solved.

At two points I felt that more technical information could have been provided: L83 on the selection of absorption lines, and~L102 on the beam characterization. The figures are all clear and helpful and the paper itself is well-written.

Added a table with the information about the selected absorption lines. Values for the beam waist and the optical power were also added to the main text.

Other comments:

Abstract L10: Where are these precision values from? I can't find their origin in the manuscript. Values at L288 are different. Please state that these are 1 sigma values, if that is the case.

There was a mistake regarding the precision values for N_2O and CH_4. They should read as given at L288 as pointed out by the Referee. Replaced the wrong values and added the information about the 1σ.

L11: How do "Repeated measurement cycles of air samples" demonstrate "an excellent accuracy level"?

We realize that this statement was difficult to understand in the given context. We were referring to repeated calibration runs, which agreed very well within the instrument measurement uncertainty. The wording was changed accordingly.

Introduction: Nice summary of modern measurements and the Oldest Ice campaign but it doesn't provide much context for this technical study on spectrometer design.

The instrumental development with its specific design has been fully driven by the ice core project, and it is important that this becomes clear in the introduction. Nevertheless, we are thankful to the reviewer for his feedback, and we revised our manuscript to inform the reader early (both in the abstract and the introduction) that this paper describes the QCLAS development.

L68-71: Both studies cited here use Picarro instruments that do not utilize cavity-enhanced technology.

The Picarro instruments are based on cavity ring down technique, and the signal is enhanced through the cavity length. Therefore, it is correct to refer to as cavity enhanced technology in this context.

L77-78: How were these values chosen? Are they 1 sigma values?

Added to the manuscript: " *In order to allow for authoritative interpretation of the observed glacial/interglacial changes in the biogeochemical cycles of these three greenhouse gases, a signal-to-noise ratio of better than 5 for the centennial to multi-millennial variations found in ice cores for CO_2, CH_4, N_2O, and $\delta^{13}C(CO_2)$ over the last glacial cycles is required. This results in precision targets of these parameters for high-quality ice core analyses of 0.5 ppm, 2 ppb, 2 ppb, and 0.04 ‰, respectively. These targets are either comparable or better than the best ice core analysis systems available to date.*"

L151: Last sentence is repetition.

To avoid confusion we rephrased this sentence: "*Thus, this concept allows for a simpler alignment mechanism and hence it also minimizes the dead volume behind the rear-mirror that would be required otherwise.*"

Section 3.1, L255, sentence beginning "Our setup..." I didn't catch the meaning here.

To clarify the text, the following modification was applied: "*Currently, custom-made MPC requires about 300 s to be completely evacuated and subsequently filled with another gas. This relatively rapid exchange time is due to the improved leak-tightness and minimal surface interactions, and it is well within the constrain defined by the Allan variance minimum.* "

L258: Can the authors further explain why d13C-CO2 and CO2 behave differently to the other analytes during the Allan-Werle test? Why do the d13-CO2 measurements drift significantly when the others do not?

The $\delta^{13}C(CO_2)$ is a ratio given by the two measured quantities of $^{12}CO_2$ and $^{13}CO_2$ and, therefore, the random errors of these quantities sum up in the ratio. Furthermore, the $\delta^{13}C(CO_2)$ is also temperature sensitive as now mentioned explicitly in Sect 2 in the context of line selection. See also our reply to the comments of Referee #2. To clarify this detail we added the following text:
"*This different behavior of $\delta^{13}C(CO_2)$ can be understood by considering the facts that i) this quantity is a ratio between two measured quantities of $^{13}CO_2$ and $^{12}CO_2$, therefore, the random errors of these quantities sum up in the ratio, but correlated drifts are eventually eliminated, and ii) the $\delta^{13}C(CO_2)$ is highly temperature sensitive as discussed in Sect. 2 in the context of line selection.*"

L270-271: The Allan-Werle shows the optimum time to integrate the data over to obtain high precision. How does this ensure high accuracy?

If one can measure a reference gas right after the unknown gas sample was analyzed and keep this alternating measurement cycle within the time period defined by the averaging time corresponding to the Allan variance minimum, then the accuracy can be as high as the precision, assuming that the reference is accurately known. In other words, precision is a necessary but not a sufficient prerequisite for accuracy.

Figure 9 and associated text: Much of this underestimation of all the analytes is attributed to optical saturation...this seems to be a significant problem because the off-set changes with pressure. What is preventing you from reducing the intensity further? The 'neutral density filter' mentioned at L118 doesn't seem enough here.

We already reduced the laser initial intensity by a factor of 10. Further reduction would lead to a decrease of the SNR because of detector noise, which then would negatively affect the precision. For the targeted precision, we have to make this compromise between enough signal and potential optical saturation. In any case, our calibration routine takes care of systematic effects introduced by slight optical saturation. These details are now included in the revised manuscript.