Referee 2

Many thanks for your feedback. All our responses or comments are written in green through the text.

General comments

The article is devoted to measurements of $d^{18}O$ and O_2 concentrations in the atmosphere by absorption of DFB laser emission at 760 nm. The excellent sensitivity is presented in short (20minutes) and long (several hours) times. The results of measurements are in nice agreement with that of IRMS. The methods of calibration of the device are suggested for continuous monitoring of O_2 concentration and $d^{18}O$.

Specific comments

At the same time many details of the experiments are missing.

- Experimental setup is very useful for understanding further.

We refer to previous work for the general instrumental setup, we clarified this point in the manuscript. Thus, no figure or general description of the technique is needed, see our responses to similar comments by the other 2 referees.

- It seems the device long time stability obtained owing to high stability of cuvette (temperature of ~mK fluctuation). Nevertheless, no info in the text about the actual reasons for the stability was provided (precision of measurement, response time of feedback).

We modified the paragraph discussing this point to make it clearer that the whole instrumental optical assembly, including the laser and the pressure gauge, are temperature stabilized, with in particular a cell stability at the mK level. It is well known that by temperature stabilization of an optical setup one reduces the measurement drifts due to changes in optical beam trajectories thus in the signal amplitudes on the photodetectors and also in the amplitude of parasitic interference fringes due to scattered light. This is very general and very well known by developers of optical instruments. Furthermore, and more in general for any measurement scheme, other sample parameters (such as pressure in our case) affect the final measurement. Thus, temperature stabilization also reduces drifts on the measurement (and/or stabilization) of these parameters and thus on the final measurement of concentrations or isotopic ratios. Considering the broad audience of this journal we have added a small section for explaining these considerations.

- Far wings of absorption line contour are determined by Lorentz (apart from pure Doppler) in all models (Voigt, Rautian etc.). The difference in the applied model in the “main body” of line contour ($O^{16}O^{18}$) should be shown. One can estimate using cavity parameters the number of points (cavity modes) on that are not more than 10. (It is possible to see also the fluctuations around $O^{16}O^{18}$ line contour in fig.1.) The question is – is this enough to get the difference in calculation? In text lines 187-203 it is very difficult to understand how it was done. Apart from temperature influence on cavity length mechanical instabilities (for instance by outer pressure fluctuations) should be mentioned.

This comment is not clear to us. In order to make sense of the first part we have to assume the referee is not referring to the $O^{16}O^{18}$ line but to the $O^{16}O^{16}$ line (main isotopologue). Indeed, this is the line having missing points (12 data points actually) due to excessive intensity of the absorption at line center, as explained in the manuscript. As the referee states, the profile is mostly Lorentzian on the line wings which are the only part of this line appearing in the spectrum. The referee then seems to refer to small differences between the HITRAN simulation and the measured spectrum which are visible in the wings of this line, in fig.1. However, the figure does not show the spectral fit but a simple simulation based on spectroscopic line parameters from the literature. The fit of OF-CEAS spectra which are actually used to obtain concentrations and isotopic ratios matches the experimental spectrum much better than the HITRAN simulation and would not be distinguishable from it. To address this point, we added the residuals of the fit as a bottom panel in fig 1.

- Lines 206-215. Why the reflectivity of the second configuration is absent? “Less parasitic fringes” at lower finesse configuration means better spline/averaging of the signal (FSR cavity modes did not change). If it is so, single scan time of the laser frequency and time constant of the detector provided.

Since we specified that the finesse was half we assumed one could easily see what change of mirror reflectivity (R) that corresponds to, given that the finesse is proportional to 1/(1-R). We now explicitly added the reflectivity and cavity finesse for both mirrors sets in this paragraph. However, we are not sure why is the referee speaking about “splines”. Anyway, we added a sentence about the laser scan time and the detector response time at the end of the same paragraph.

- Line 304. “An influence of O_2 concentration on $d^{18}O$ of O_2 was expected.” Sound like a general rule, but is only valuable for the method applied.

We are sorry but we do not understand this comment, since the sentences after line 304 explain in detail why we do expect such a dependence for our instrument. If this is general or not to other methods is not relevant to the discussion. However, we tried to improve this discussion and make it more precise and clearer.