

Interactive comment on “A low-cost portable fibre-optic spectrometer for atmospheric absorption studies” by J. Bailey

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Response to Referee 1

(Reviewer comments in italics - my responses in normal type).

The paper under review presents a very compact fibre-optic Fabry-Perot spectrometer customized for solar absorption spectroscopy in the shortwave infrared spectral range. Instrument setup and calibration procedures are described and illustrated for the CO₂ absorption band at 6350 cm⁻¹. The paper recommends the compact spectrometer as a candidate for future remote sensing of atmospheric total column concentrations of CO₂, CH₄, and potentially other absorbers.

The paper is well written and the employed methodology appears solid and robust.

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Given high spectral resolution, low noise, and good spectral coverage, the measurements of the 6350 cm⁻¹ CO₂ absorption band look promising.

However, the paper lacks an estimate of the accuracy to be expected for routine retrievals of CO₂ (or CH₄) total column concentrations. The authors themselves recommend such an assessment “by using [the Fabry-Perot spectrometer] alongside a high-resolution Fourier Transform spectrometer” (p.1080, 1st paragraph). All referenced precursor studies [Kobayashi et al., 2010; Kawasaki et al., 2012; Petri et al., 2012, Gisi et al., 2012] include at least a first estimate of the retrieval accuracy to be expected based on a reasonable ensemble of atmospheric observations. I would consider such an assessment the heart of an demonstrator study.

I don't think it is appropriate to compare this paper with the four studies mentioned. Those studies were applications of commercially available instruments based on standard designs, and specifically addressed the use of them for greenhouse gas measurement. In this paper I have developed a completely new spectrometer of an innovative design, designed and built all the hardware and software for it, and evaluated its performance. None of this was necessary with the commercial instruments used in those other studies.

I totally agree that a study of the retrieval accuracy for greenhouse gas concentrations now needs to be done. However, it was never the intention of this paper to cover that specific application. There is nothing in the title or abstract of the paper to suggest that this is a paper about greenhouse gas total column measurement and retrieval. This paper is specifically an instrument paper describing a novel spectrometer and its performance. Greenhouse gas column measurement is an application of the instrument, one of a number of possible applications, but clearly the one that is of most current interest. I consider it logical and appropriate to divide the work into this instrument paper, and follow-up papers that describe specific applications of the instrument.

Therefore, I do not recommend publication in AMT. The paper requires a plausible

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investigation on the expected accuracy and on the usefulness of the spectrometer for the chosen science question – be it CO₂ total column measurements. This should be based on a reasonable set of actual atmospheric observations, preferably under various ambient conditions.

I respectfully disagree with the conclusion that the paper should not be published in AMT. Given the reviewers earlier comments ("well written", "solid and robust", "... look promising") the reviewer does not seem to have any problems with what is presented in the paper. The reason for not publishing is therefore, presumably, that without a discussion of a specific science application, the paper is not considered to be sufficiently significant. I disagree. I think that the presentation of a completely new design of atmospheric absorption spectrometer, and the demonstration that it can achieve high resolution, good wavelength coverage and high signal-to-noise for a fraction of the cost, size and weight of existing instruments (none of which is disputed by the reviewer), is a significant development and worthy of publication in AMT.

In particular the low cost of these devices is likely to make them of interest to other experimenters, who will be able to benefit from the experience presented here on what they can do and what the potential problems are.

I list some related concerns below.

***I would consider a ~13 min scan time per spectrum a major shortcoming of the technique and quite a challenge for data reduction.*

13 minutes is the scan time used for the spectra presented in the paper, but there is no reason why scans cannot be made faster. The FFP devices used can be scanned at rates up to 100s of Hz. In the lab we have carried out faster scans without any degradation in resolution. The practical consideration is that there is a trade-off between S/N per spectral point and scan speed. Improved detectors (e.g due to cooling, smaller detector sizes) will enable faster scans (as is mentioned in the paper in the "Conclusions and Future Work" section).

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The first issue that comes to mind is the moving sun implying a considerable difference in the slant airmass between the start and the end of the scan. Probably, this can be taken into account by the retrieval algorithm by choosing the correct solar zenith angle for each monochromatic transmittance calculation. Nevertheless, I would recommend describing such aspects in a section about data reduction.

Yes, we would plan to handle this in exactly the way described. A brief discussion of the issue has been added to the paper.

Brightness fluctuations of the solar source due to thin clouds, cirrus, or aerosols could cause major baseline fluctuations within 13 min. Even on much smaller timescales and thus, within sub-ranges of the spectrum, such fluctuations could be non-negligible. Thus, as the authors admit, the technique "requires completely cloud-free skies to produce quality spectra" (p.1080, last paragraph). In practice, I would expect major difficulties for gas concentration retrievals (if these baseline fluctuations are not monitored).

This is indeed an important issue as well as the fact that it limits the number of days that are available for making measurements. The proposed solution to this is already described in the paper (last paragraph of page 1080) . We propose to split off a fraction of the light before the tuneable filter using a fibre splitter and send this to a broad-band detector which will monitor such baseline fluctuations.

***For direct sun observations, it seems very useful to simultaneously monitor an O₂ absorption band with CO₂ and to ratio total column CO₂ by total column O₂. The ratio will cancel quite some instrumental and spectroscopic error sources and yield a highly accurate total column mixing ratio XCO₂. This is the standard technique currently used by the Total Carbon Column Observing Network (TCCON). In the view of a 13 min scan time and other instrument issues described in the paper, I would consider a reference O₂ channel highly desirable.*

I agree entirely. I have a 1270 nm Fabry-Perot (for the O₂ a-X band) ready for installing

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in a second spectrometer for exactly this purpose.

***Is the filter response and the instrument line shape stable with respect to changes in ambient conditions?*

The filter response appears to be stable except for small wavelength shifts of the "ripple" pattern which I assume are a function of temperature. The line shape is expected to be very stable since the main determining factor is the reflectivity of the FP mirrors which are completely protected inside the encapsulated FP device.

I have added a new short section to the paper on "Stability" to discuss this and related issues raised by Referee 2.

Fig. 7 shows the filter response function determined from fitting a modeled transmittance spectrum to a measured one and then, low-pass filtering the residuals. Is the modeled spectrum actually fitted (eg. in a least-squares sense) or does it just assume an a priori CO₂ (and H₂O) concentration? Could this filter response function be determined once in a calibration setup and then, be used for the actual target observations?

Yes - these are actual least squares fits and the CO₂ and H₂O concentrations can be fitted or fixed. The temperature dependence mentioned above probably means we can't use a single filter response function for all observations, but it may be possible to use a set of response curves to encompass the temperature variations.

Response to Referee 2

The article by Bailey reports on a potentially important technical topic, namely a low-cost and portable Fabry-Perot spectrometer for accurate atmospheric trace gas retrievals. The manuscript is very technical, mostly describing the instrument and each interface in detail. However, the integration into the scientific questions as a motivation for the instrument falls rather short and it is not at all clear whether this instrument could achieve the accuracy required for monitoring total column abundances over longer time-periods. Agreeing with reviewer 1, an actual analysis of the spectral data and

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comparison with benchmark measurements (e.g. TCCON) would be indispensable to evaluate the true potential of this method.

As already discussed in the response to referee 1 the purpose of this paper is to present a solar absorption spectrometer of innovative design and measure its performance in terms of the basic spectrometer parameters such as wavelength coverage, resolution, signal-to-noise ratio etc. It was not the intention of this paper to discuss the specific application of total column greenhouse gas retrievals, although this is clearly one of the main motivations for the development of such instruments.

Page 1068, Line 13: "spectrum of the sun" sounds like this is a prime reason, not the Earth's atmosphere. Please rephrase

Agreed - changed to "atmospheric absorption spectrum using the sun as a background source".

Page 1069, line 9: typo "Intereferometer"

Corrected.

Page 1070, line 16: Define "small loss of performance", by how much? In what sense?

The loss in performance is a degradation in resolution to 0.25 cm^{-1} at $1.66 \mu\text{m}$ - which is to be expected as a result of the reduced reflectivity of the FP mirrors away from their nominal wavelength.

These figures have been added to the "Resolution and Instrument Line Shape" section.

Most important part of such a network is long-term stability. How would the "creep" effect impact this?

Creep is more of a short term effect causing shifts in wavelength calibration within a days measurements. I wouldn't expect it to be an issue for long-term stability.

Page 1075, line 27: Define "majority vote"? The algorithm description is not very

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quantitative and words like "majority vote" don't help in that respect. E.g. Line 1, page 1076: "in most cases" why does it fail sometimes?

I have rewritten the description avoiding the use of "majority vote". The problem with the algorithm is that the similarity of the two CO₂ bands and the almost equal spacing of the lines within a CO₂ band can fool the pattern matching algorithm to match with the wrong CO₂ band or to match a CO₂ band one or two lines out of step. We get round these occasional mismatches in part by making several matches using different subsets of lines and choosing the match for an observed line that comes up most frequently (this is the majority vote).

ILS: This is very descriptive but at the heart of any retrieval algorithm. The extended wings are somewhat disconcerting. Can you quantify those? How well can you determine the ILS and how stable is it? These are questions that are most important to the absolute calibration of the retrieval as well as its stability in time.

At present we rely on the manufacturers claims that the line shape of the FP devices closely follow the theoretical Airy function. I agree that the line shape and its stability is an important issue for retrievals. The fact that the optical system of the devices is entirely in single-mode fibre and encapsulated within the device should provide a very stable line shape, and is a potential advantage over conventional FTS systems where misalignment of the optical system can affect performance.

However, we do need to check this with actual measurements, and are currently setting up a laboratory system that will enable us to measure the line shape accurately using narrow absorption lines from a gas cell.

A new section on stability has been added to the paper discussing these issues and including the two new figures described below.

Page 1080, line 1: "It is planned to test": I would suggest to wait until such a test has indeed been performed. As the paper stands right now, it is merely a qualitative

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instrument description but very hard for the reader to figure out whether such a system will indeed prove reliable and accurate in actual retrievals.

I think that the performance of the spectrometer in terms of spectral coverage, resolution and signal-to-noise ratio shown by the results in this paper is already impressive given the very low cost of this approach. I agree that more work needs to be done to demonstrate the capability of the instrument for retrievals of CO₂, but that is a particularly challenging application of a spectrometer in view of the accuracy needed to obtain scientifically useful results. I don't see why it should be necessary to solve all the problems involved in these difficult retrievals (many of which are raised in other referee comments) before the basic concept of this innovative spectrometer can be published.

The author mentions instrument tests at the University of Wollongong. Why not team up with some of the TCCON experts there and at least test some real retrievals and/or convolve co-located TCCON spectra for comparison?

This is exactly what is planned.

Suggestions for more intermediate corrections: An example of stability within a day would be to check whether the depths of Fraunhofer lines actually remains stable throughout a day. The author could create ratio spectra of the respective times (for spectral ranges that overlap) and show how CO₂ absorption depths change (maybe quantitative as a function of Airmass!) and how Fraunhofer lines remain stable (if they do! This would be a very good indicator of stability though).

Figure 9 already showed this to some extent, but I have made two new figures to show stability within one day and over several months. The first shows three spectra from the Oct 4th data set covering a little over three hours with airmass increasing. The wavelength region shown includes one strong and two slightly weaker solar Fraunhofer line (marked with ⊙). It can be seen that these remain stable while the other lines (mostly CO₂ lines) increase in-depth with airmass. The lower panel shows the differences of the blue and red spectra from the black one.

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The second figure shows the same thing for spectra spread over a six month period. Again the solar lines agree well although there is structure visible in the difference spectra which result from the changing solar Doppler shift varying from +650 m/s for 2013 Apr 11 to -160 m/s for 2013 Oct 4. Other features in the difference spectra are due to the small changes in airmass changing the CO₂ line depths. A weak line just before 6346 is an H₂O line and is strongest in the Jan 25 data set.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 1067, 2013.

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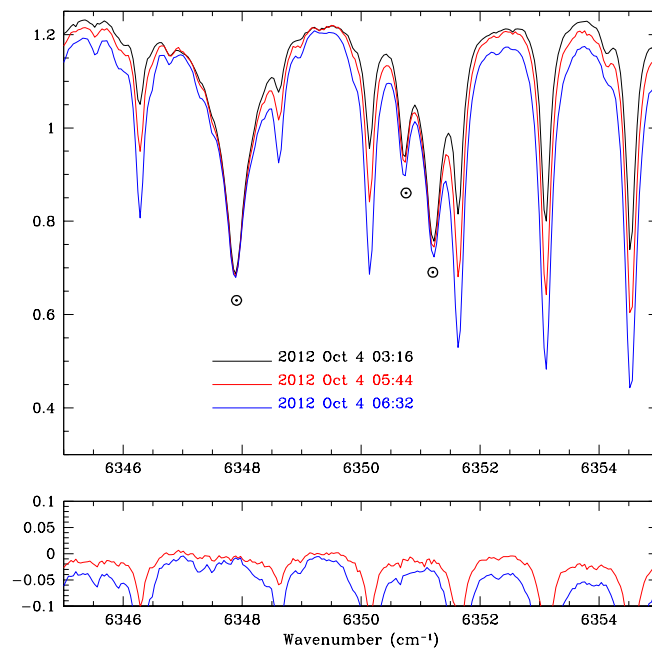


Fig. 1. New figure 10 - Comparing spectra during the same day

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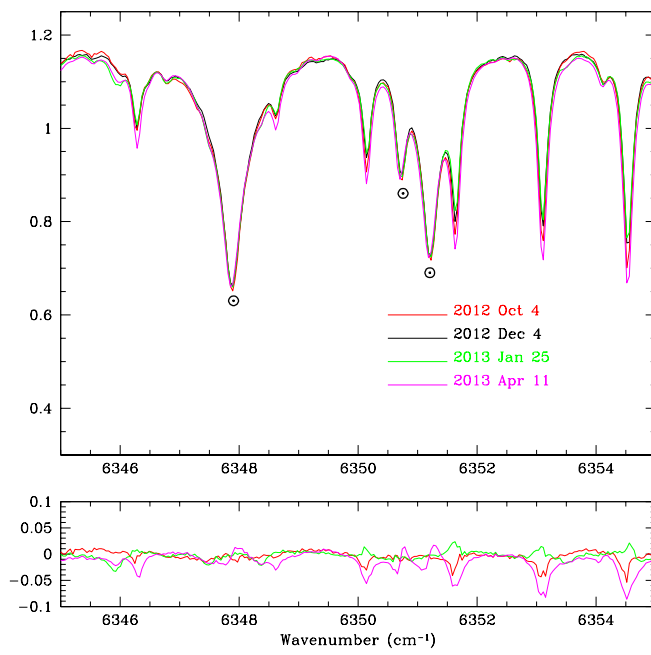


Fig. 2. New figure 11 - Comparing spectra over a six month period

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