Atmos. Meas. Tech. Discuss., 8, C3582–C3597, 2015
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Atmospheric Measurement Techniques Discussions

Interactive comment on "Sensitivity of remotely-sensed trace gas concentrations to polarisation" *by* D. M. O'Brien et al.

D. M. O'Brien et al.

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Received and published: 27 October 2015

Before listing our responses, an important clarification is required.

- We submitted the original draft on 2015-05-06.
- On 2015-07-14 we received reviewer comments from the quick review stage, and we were advised by the editor that minor revisions were needed.
- We submitted a revised manuscript on 2015-07-22 incorporating the changes recommended by the reviewers, and the manuscript was published for discussion in AMTD on 2015-08-24.

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- On 2015-10-23 the comments of anonymous reviewer #2 were posted. *However, his comments were identical to those he made on the initial draft; they did not address the revised paper that was published in AMTD.*
- Therefore, in this response, we have provided the same responses that we made during the initial quick review. The references to pages and line numbers are to the initial draft, a document that is not publicly available.
- Because our responses to the comments of reviewer #2 were included in the discussion paper published in AMTD, we have not made any further revisions in the light of the comments posted by reviewer #2 on 2015-10-23.

We thank the reviewer for his constructive and helpful comments. The reviewer's comments are shown below in cyan, while our responses are marked in blue. Revised text is in black.

While preparing this response, we noticed two typographical errors, which we have corrected in the final version of the manuscipt uploaded on 2015-10-27.

Comment 1

Introduction P2, Line 51: "The polarisation sensitivity of the geoCARB spectrometers imposes strong, wavelength dependent signatures upon the spectra, which potentially could cause unacceptably large errors in retrieved concentrations of CO2, CH4 and CO." This statement is poorly supported in the rest of the paper.

The quoted sentence has two parts. The main clause, that the spectrometers impose strong, wavelength dependent signatures upon the spectra, is amply demonstrated in the text, for example in Fig. 4. The second clause, that the signatures might cause

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large errors, is disproved in the rest of the paper; it was meant only as a straw-man proposition, typical of the many that are raised in the discussion of a new instrument. Our paper blows the straw-man away.

The spectrally-dependent variations shown in Figure 4 are smoothly varying, and not obviously correlated with CO2, CH4, and CO absorption features. Any model that fits for the slope of the continuum (as an albedo slope or radiance scaling) should be adequately compensate for such features.

We agree entirely. Our intuition suggested the same conclusion, but we needed to quantify the effect, which we have tried to do in the paper. We have investigated how other parameters, such as the albedo slope, may compensate for the effects introduced by the gratings. As is often the case, the answer is complex, because many parameters interact, but the positive conclusion is that the errors in retrieved column-averaged concentrations are small.

Features more strongly correlated with the spectral structure of the absorption bands (like those associated with Rayleigh scattering in the A-band) would produce much more serious errors, but no such errors are shown here.

Our modelling includes Rayleigh scattering. Because Rayleigh scattering is much stronger in the A-band than the other bands, we agree that it could produce errors, potentially more serious than those arising from the spectrograph itself. However, our results indicate that the errors are not large, and that they can be reduced by preflight polarimetric calibration.

We think that our original sentence is acceptable as it stands. Nevertheless, in the hope of making our meaning clearer, we have modified the sentence as follows:

> The polarisation sensitivity of the geoCARB spectrometers imposes strong, wavelength dependent signatures upon the spectra, which raises the question as to whether such signatures might cause unacceptably large errors

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in retrieved concentrations of CO_2, CH_4 and CO.

Comment 2

Section 2: I only spot checked the equations in Sections 2 and 3. It appears to be consistent with those in O'Brien et al. (2013).

We certainly hope so!

Comment 3

Section 3, Pg 6, Line 49: "Stokes vector after reflection from the north-south mirror will be" Try "The Stokes vector after reflection from the north-south mirror will be"

Accepted. "Stokes vector" has been replaced by "the Stokes vector" throughout the text.

Comment 4

Section 5, Pg 11, Line 56: "The meteorology at each target was based on forecasts from the European Centre for Medium-range Weather Forecasts (ECMWF), interpolated to the time and location of each observation." What time of year? What year? Nine pages back, in the introduction, we have learned that these are times near solstices and equinoxes, but that is all we know.

We have added a footnote listing the dates used for the simulations. We believe a footnote to the sentence quoted by the reviewer is appropriate, because the specific dates have little impact on the results. We chose dates near the solstices and equinoxes to capture the seasonal dependence. As already stated in the text, the observation times were fixed at solar noon and three hours before and after. The polarisation is most

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affected by the geometry, which varies significantly during the course of a day and also with season. The text of the footnote is repeated below.

> The specific dates for the simulations were the twenty-first of March, June, September and December in 2012. The equinoxes and solstices were chosen to capture the seasonal dependence. The only significance of the year 2012 is that data was already on hand for the geophysical variables; we expect similar results for other years. Three observations were simulated for each day, at local solar noon, three hours earlier and three hours later.

Comment 5

Pg 11, Line 60 (and pg 12 line 100): "Calipso" CALIPSO should be in capital letters. CALIPSO is an acronym for "Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations."

Agreed. We have ensured that CALIPSO appears in upper case throughout the text, and after the first occurrence we have expanded the acronym.

Comment 6

Pg 12, Line 85: "Generally in simulations of this type, random noise would be added to the unpolarised intensity in accordance with the noise model for geoCARB, and the resulting signal would be regarded as a measurement (or measured spectrum). However, because the focus of this study is the bias caused by polarisation, random noise was not added."

This choice was somewhat surprising. It would be more realistic to apply the same random noise to both experiments 1 and 2. The absence of realistic noise, combined with the use of the same forward model in the simulator and retrieval algorithm (which

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only differs in its treatment of the surface) will produce “optimistic” results. What is the effect of adding realistic noise on the primary results of the paper?

We replaced the sentence quoted above by the text below, which explains and justifies our approach.

> Generally in simulations of this type, random noise would be added to the unpolarised intensity in accordance with the noise model for geoCARB, and the resulting signal would be regarded as a measurement (or measured spectrum).
>
> However, in this study random noise was not added for the following reason. For every retrieval, differences between the true and retrieved values of the parameters can arise via many mechanisms, including:
>
> 1. differences between the absorption coefficients and radiative transfer models used for the forward simulation and for the retrieval algorithm;
> 2. the influence of the prior and algorithm controls, such as the stopping condition;
> 3. random noise added to the simulated spectra.
>
> The last source is the most understood, and its magnitude can be quantified easily by the posterior uncertainties returned by the retrieval algorithm, the calculation of which uses the instrument signal-to-noise ratio. Furthermore, random noise in the spectra generally will not cause a bias, because the radiative transfer problem can be linearised in the vicinity of the true solution. Consequently, we can concentrate on the biases introduced by factors other than random noise (such as the first two items listed above). Since the model errors and the random noise (items 1 and 3) are statistically independent, including the effects of random noise simply widens the bias distribution by the width of the random uncertainty. As the focus of this

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study is the bias introduced by polarisation effects, it was judged that the effects would be easier to spot in the narrower error distributions calculated without random noise.

Comment 7

Section 6, Pg 12, Line 95: “In contrast to the measured spectra, which were computed using polarising surfaces with directional reflectance, the modelled spectra assumed that the surfaces were non-polarising and Lambertian, with albedo varying linearly with wavelength.”

This statement raises several questions: First, these are not “measured spectra.” They are “simulated measurements” (with no noise or calibration errors).

In the absence of an instrument and any data, we have to pretend that the simulated data represent measurements. However, as the reviewer points out, calling the simulated data measurements is not correct, so we have resorted to enclosing the first appearance of measured in quotes and adding in parentheses the words “in reality simulated”. Thereafter we simply refer to measured spectra, assuming that the reader will understand that these are pretend measurements.

Second, how non-Lambertian were the surfaces used here? How structured were the bidirectional reflection functions (BRF’s)? We later learn that the typical degree of polarization is 2%. What was the largest degree of polarization?

The BRDF for each geoCARB pixel was taken to be that derived from MODIS at the centre of the pixel. The spatial resolution of the MODIS BRDF is 1 km. In the O_2 A-band, CO_2 weak band and CO_2 strong band, the BRDF was interpolated from the two nearest MODIS bands; in the CO band, where MODIS does not make observations, we assumed the BRDF was the same as in the strong CO_2 band. The polarised component of the surface reflection was introduced by combining the MODIS BRDF with a

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parameterisation of polarisation effects derived from POLDER observations by Nadal and Breon. We believe that we have captured the BRDF and the polarisation signature of the surface as well as possible with current data. Rather than repeat this information in the current paper, we cited Polonsky et al. where the material was reported earlier.

Concerning the 2%, please see our reply to the reviewer's comment "Pg13, Line 31".

Finally, in the introduction, we find the statement: "Instead it assumes that the surface is non-polarising, but it generates polarising elements internally." Here, it indicates that the retrieval algorithm assumes that the surface is Lambertian and unpolarized. Does this mean that all "polarizing elements" are attributed to clouds and aerosols or added to the Rayleigh scattering?

In the sentence from the introduction, the context makes clear that "it" refers to the retrieval algorithm. As the reviewer correctly points out, the retrieval algorithm also assumes that the surface is Lambertian. Therefore we have replaced "the surface is non-polarising" with "the surface is Lambertian and non-polarising" in the introduction. In answer to the reviewer's question, the "polarising elements" are attributed to scattering by clouds, aerosols and molecules. In an attempt to make the introduction clearer, we have added the following:

> The source of polarisation within the retrieval algorithm is via scattering by clouds, aerosols and molecules.

By way of further explanation, our goal is to retrieve X_{CO_2}, X_{CO} and X_{CH_4}, whereas the polarising properties of the surface are of less interest. As reported for OCO by O'Dell et al., assuming that the surface is Lambertian (and non-polarising) has little impact on the accuracy of retrieved X_{CO_2}, but simplifies the retrieval model significantly. The reason is easily understood. For an instrument that measures only the intensity component of the Stokes vector, the distinction between two surfaces, characterised by a polarized BRDF on one hand and a Lambertian albedo on the other, is lost when the

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atmosphere is non-scattering. When the atmosphere contains small amounts of scattering material, and the component of the intensity reflected directly from the surface dominates, the difference between the spectra for the two cases also is small, and may be neglected. Hence, in the retrieval algorithm polarisation is introduced only through scattering by clouds, aerosols and molecules (Rayleigh scattering). In contrast, a detailed model of polarisation by the surface was included in the forward model used to simulate the spectra (the pseudo observations).

Comment 8

Pg 12, Line 97: "Thus, while the modelled surface was based on reasonable prior information, it differed in detail from the measured surface."

What was the prior used for the surface? Most such models simply retrieve the surface reflectance from the continuum, without an explicit prior. Also, what was the prior used for CO2, CH4, and CO? What it the same as the answer, except for the small (3ppm, etc.) random perturbations, or was it substantially different?

In fact, our "reasonable prior information" for the albedo was derived from the spectra using a selection of frequencies, mostly in the continuum, and a radiometric model that assumed the atmosphere was free of cloud and aerosol. The estimate so obtained then was used as both the first guess and the prior in Rogers' optimal estimation. Thus, the method we used was essentially that described by the reviewer. This explanation has been added to the text, and the modified paragraph is reproduced below.

> In contrast to the measured spectra, which were computed using polarising surfaces with directional reflectance, the modelled spectra assumed that the surfaces were non-polarising and Lambertian, with albedo varying linearly with wavelength. An estimate for the albedo was derived from the spectra using a selection of frequencies, mostly in the continuum, and

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a radiometric model that assumed the atmosphere was free of cloud and aerosol. The estimate so obtained then was used as both the first guess and the prior in Rogers' optimal estimation. Thus, while the modelled surface was based on reasonable prior information, it differed in detail from the measured surface. This difference ensured that simulation followed by retrieval was not a circular process, and in fact was open to the range of errors we expect with real data.

Concerning the prior profiles of CO_2, CH_4, and CO, the following has been added to the text.

For each day, each observation time and each (approximately) north-south scan line (through Agra, Wuhan or Alice Springs), the prior profile of CO_2 was taken to be the average of the profiles at all of the target pixels along the scan line. This was judged to be a fair prior, neither too optimistic nor too pessimistic, and indicative of the accuracy possible with large-scale averages predicted by general circulation models. Prior profiles of CH_4 and CO were calculated similarly.

Comment 9

Pg 12, line 98: "This difference ensured that simulation followed by retrieval was not a circular process, and in fact was open to the range of errors we expect with real data."

This approach will introduces a systematic bias, but does not provide any information about the effects of random errors, which may affect your results differently, depending on the specification of the prior.

We have addressed this point in the discussion above on random noise.

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Comment 10

Pg 12, Line 101 and 103: "...two types of aerosol plus water and ice clouds ..." Try: ...two types of aerosol plus liquid water and ice clouds ...

Agreed. We have added "liquid" to the text and checked for similar occurrences.

Comment 11

Pg 13, Line 26; "In a sense this experiment represents the worst case, because it assumes that no pre- flight radiometric and polarimetric calibration has been performed."

No "radiometric" calibration as well? This suggests that you simply scaled the observed continuum to the simulated continuum, which I don't believe was the case here. If your simulated measuremens assumed some conversion from volts to radiometric units (watts or photons/sec per square meter per steradian per micron), and assumed that the observed amplitudes scaled linearly with input radiance, you have assumed some radiometric calibration.

Agreed. Our wording was incorrect. We have removed the reference to radiometric calibration.

Comment 12

Pg 13, Line 31: While the "typical" degree of polarization is 2%, what is its distribution? Were there any very much larger or smaller values? Did they pass your post processing screens?

Lines 29–39 in our original manuscript were incorrect, and did not describe the calcu-

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lations we performed, so we are very thankful for the reviewer's questions. We have replaced lines 29-39 with the text and figure below, which we believe address the reviewer's comments.

The degree of polarisation, defined by

$$P = \sqrt{Q^2 + U^2 + V^2}/I, \quad (1)$$

varies strongly across the absorption spectrum, peaking at the line centres and falling to a backgroud level, determined principally by the surface and Rayleigh scattering, in the continuum between the lines. At wavelengths in the cores of the lines, photons are likely to have been scattered higher in the atmosphere by molecules, clouds and aerosols, which typically have stronger polarisation signatures than the surface. Fig. 9 shows the mean and standard deviation of the degree of polarisation in the O_2 A-band for the ensemble of soundings in the frames passing through Agra, Wuhan and Alice Springs on the selected days and observation times. In order to illustrate the degree of polarisation likely to be encountered in the almost clear conditions required by the retrieval algorithm, the mean and standard deviation in the left-hand panel of Fig. 9 were computed from the ensemble with cloud disabled. Thus, in this ensemble, polarisation is generated by the surface and by scattering from aerosols and molecules, but not from clouds. The right-hand panel applies to the ensemble with cloud enabled.

The full text of the caption for Fig. 9 is as follows.

Mean and standard deviation of the degree of polarisation simulated at the top of the atmosphere in the O_2 A-band. In the left-hand panel the soundings with cloud were discarded, so the sources of polarisation are the surface and scattering by aerosols and molecules. The right-hand panel applies to soundings with cloud.

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Comment 13

Pg 14, Line 59: “While the differences in the average biases shown in Table 2 appear small, they nevertheless are important, because even small biases on large spatial scales can lead to significant errors in surface fluxes of CO2”

The mean bias and variance shown here is actually quite large for an experiment that uses “perfect” gas absorption coefficients, similar forward models in the simulator and retrieval algorithm and assumes no random noise. Could it be that biases introduced by these relatively weakly polarizing land surfaces are much smaller than those introduced by the simplified surface BRF or cloud/aerosol model? This seems to be reinforced in your Conclusion section (pg 15, line 73), where you state: “The ability of the retrieval algorithm to predict the polarisation state is limited because internally it assumes that the surface is non-polarising and Lambertian and that aerosols and clouds are composed from fixed types whose scattering (and polarising) properties are assigned, fixed and usually inconsistent with the real atmosphere. This inability leads to an irreducible minimum error when the algorithm is applied to a realistic ensemble of surfaces and atmospheres.” This leads us to wonder what would happen if random noise was added.

We disagree that the the errors are “quite large”; in fact they are not so different from those arising in other studies of this type. As we state in the conclusion, the retrieval algorithm makes assumptions about cloud and aerosol that rarely coincide with those used in the forward model. This is an important source of bias. We reiterate that we have attempted to isolate the errors introduced by polarisation. We have done so by comparing histograms of bias for scenarios with and without polarimetric calibration data. The differences between these histograms we have attributed to polarisation effects, because all other factors were identical. Furthermore, as explained above, we did not add random noise because it would have broadened the histograms, and consequently made small changes more difficult to detect.

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Comment 14

Pg 15, line 85; However, generally they are small, though they remain significant for XCO2." Do these biases vary systematically with airmass or latitude?

Presumably the biases vary systematically with airmass and latitude. However, our study shows that, with proper preflight calibration, the biases associated with polarisation are secondary. Therefore, while the dependence of the biases on airmass and latitude is intellectually interesting, it has low priority. It would be an excellent study for a student during the Phase A development of geoCARB.

The frames and observations times we selected were designed to generate a wide range of latitudes, illumination zenith angles, observation zenith angles and relative azimuths. Therefore, potentially we have the data to investigate the dependence on airmass and latitude, but such a study is beyond the scope of the present paper.

Comment 15

Pg 16, Line 92: "Thus, even in the presence of significant polarization at the entrance aperture," What is "significant polarization?" The only number cited is 0.02. That is not very "significant". This may be "typical" for the land surfaces considered here, but biases associated with the degree of polarization would still be of great concern because they might indicate spurious sources and sinks.

As explained above, lines 29–39 (which contained the number 0.02) in our original manuscript were incorrect, and did not describe the calculations we performed. That text has been replaced, and the new Fig. 9 shows clearly that the simulation spectra were significantly polarised.

We agree that all biases are important, because they might indicate spurious sources and sinks. This is a problem in common with GOSAT and OCO-2. However, we believe

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that our results show that the polarisation biases are unlikely to dominate, provided that the polarisation characteristics of geoCARB are quantified before flight.

Comment 16

Pg 16, Line 95: "Through radiometric and polarimetric calibration before launch using the procedure defined in this study, errors from polarised surfaces and clouds can be reduced to negligible levels." You might want to qualify this. This is true in the presence of the other simplifications in the current model, which yield relatively large biases. Would this still be true in the presence of much smaller systematic biases in the retrieval algorithm's forward model?

Agreed. We have changed the text to

> . . . can be reduced to negligible levels compared with other systematic biases in the retrieval algorithm. If in the future the latter can be reduced, then polarisation biases would need to be re-examined.

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Interactive Comment

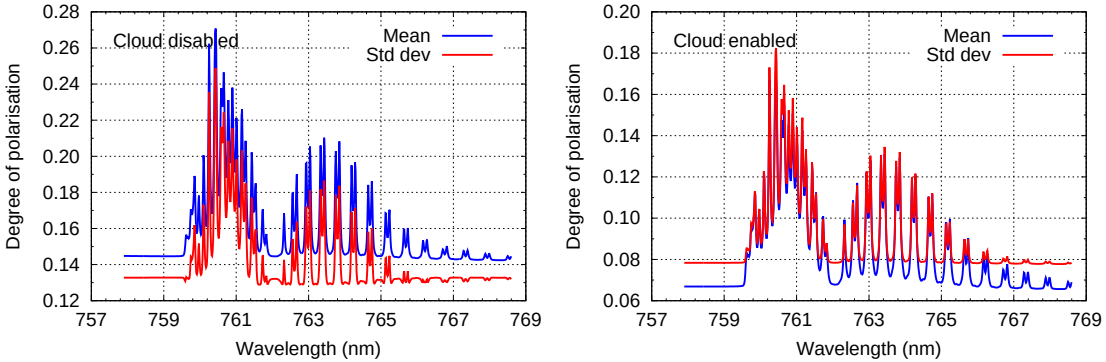

Fig. 1. Mean (blue) and standard deviation (red) of the degree of polarisation simulated at the top of the atmosphere in the oxygen A-band for cloud free (left) and cloudy (right) ensembles.

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