

Interactive comment on “ACE-FTS observation of a young biomass burning plume: first reported measurements of C₂H₄, C₃H₆O, H₂CO and PAN by infrared occultation from space” by P.-F. Coheur et al.

Anonymous Referee #1

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General Comments

The paper describes the analysis of a particular occultation sequence from the ACE-FTS instrument where the absorption features of a number of biomass-burning related molecules have been detected in the spectra. The authors are careful to state which molecules they believe they can detect and which are doubtful, and the spectral signatures of the former look convincing. Comparisons are made with earlier in situ measurements within such plumes but, given the unusually high altitude of this particular plume, it not possible to draw any firm conclusions as to whether or not the retrieved

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composition is in line with previous observations.

Data from a chemistry-transport model are used in support of this study but only in the limited sense of identifying the source of the plume as local. This, I have to say, is not convincing: the measurement location (Fig.3b) seems outside the predicted local biomass burning in Africa and, judging by the shape of the plume, the wind seems to be blowing the CO in the wrong direction. On the other hand, given that this seems such a complete chemical model, why not include the modelled CO and other chemicals for the plume and "background" profile locations in the tables and figures for comparison?

There have been previous papers on detection of biomass burning signatures using the ACE-FTS instrument, by different groups, but since these are probably the first reported detections for some molecules from satellites, a new paper is justified.

However, the major deficiency in this paper is the lack of any sort of error analysis of the results given. Even with a simple least-squares fitting procedure it should be possible to assign some statistical error bars to the tabulated results and figures.

Specific Comments

Novelty: There are a number of differing claims to the novelty of the data, from "first reported detections using infrared occultation spectroscopy from satellites" (Abstract, line 16); "first reported measurements from infrared occultation sounders" (p7916, line 4); and "first reported simultaneous measurements by means of infrared spectroscopy from satellites" (p7919, line 3). All of which have subtly different meanings (eg as far as I know, there has been only one satellite infrared occultation instrument - ACE - unless you also include ATMOS which, being a shuttle experiment, was not strictly a satellite instrument).

Vertical Resolution: On p7910 line 13 the average vertical resolution of ACE-FTS is given as 4km, yet in Fig.6 the profiles are plotted at 1km resolution and on p7917 it is claimed that the maxima for various species can be located to within the same 11-12km

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interval. Please clarify.

HNO₃ retrieval: p7911, line 4: "it is worth pointing out that we do not use [the standard ACE-FTS L2 HNO₃ product]". Why not? Is there something wrong with it?

Ozone: Why is ozone not retrieved? Ozone enhancement is considered a good indicator of photochemical processing in biomass plumes so a low ("background") ozone concentration in the plume would have been more evidence that this is indeed a recent plume.

Apodisation: Are apodised or unapodised spectra used? The ACE-FTS team generally prefer unapodised but, particularly when looking for small signals, the "ringing" effect caused by the truncation of the fourier transform can produce spurious signals if not modelled correctly - I do not say that that is what is seen here but it should be stated whether or not apodisation is applied.

Interfering Species: On p7913 line 9 it is stated that the v2.2 profiles of all interfering species are adjusted to provide the least best achievable fits. Unless some sort of constraint is applied (particularly for the weaker absorbers in a particular spectral region) this is a dangerous procedure which may create more spurious features.

Background Continuum: Is a background continuum also fitted for each spectral region? For the broader featured absorbers, such as PAN and acetone, this can make a significant difference to the answer.

Plume Origin: If the plume is assumed to be recent, ie of local origin, and is of sufficient strength to be lofted to high altitudes, it seems strange that its origin cannot be more convincingly located with the use of the ATSR data. (I assume these are actual fire detection data and not some climatological distribution).

CH₃OH & HCOOH: On p7914 line 20 it is stated that the retrieved concentrations of these molecules "are consistent with the average mixing ratios earlier reported for aged plumes". However, in aged plumes I would expect the mixing ratios to be considerably

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reduced (due to mixing with background air) compared to a recent plume. Does this actually mean "consistent" in the sense that one might eventually evolve into the other?

Species not detected: p7916, lines 6-14: it would have been useful to have the detection limits for these additional molecules so that the absence of any detectable signal can also provide information on the upper values of the concentrations.

Ethene lifetime: p7916 line 18: Mauzerall et al are quoted as giving a lifetime of half a day. In Table 1 of their paper, that value is given for 0-4km altitude. For this particular plume, wouldn't the 8-12km altitude figure of 1.2 days be more appropriate?

Yokelson et al results: On p7918 it states that the Yokelson results are "measured just above a fire in Mozambique" which is a slightly misleading statement since it suggests a comparable situation to the ACE-FTS observations which could also be described in the same way. The Yokelson results (from their Table 2) are the "initial emission ratios" extrapolated back in time to the emission source, so not directly comparable with these (or any other) subsequent plume observations. Unfortunately Yokelson et al do not provide any information on the time evolution of these ratios so it is not possible to make any adjustment to their results, but it should be made clearer that these data differ in that respect.

Sunrise or Sunset?: I understand that all ACE-FTS occultations are either at local sunrise or sunset but I didn't notice it mentioned which applies here - presumably has a significant bearing on the photochemical processing in a recent plume. I assume that the "ss" notation for the occultations studied here refers to sunset, but I am not familiar enough with the orbital mechanics to know whether a "sunset" from the instrument point of view necessarily corresponds to a local sunset.

Applicability to other cases: The plume is clearly untypical in reaching such high altitudes but, having read the paper, I am still unclear as to whether this is just coincidental or whether such a clear signal is *only* detectable because it is at high altitude? Clearly the absence of cloud is a prerequisite, but would line-broadening, water-vapour, haze

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production etc prevent such a retrieval at low altitude? I accept that the authors have just analysed a single case, but I think it would be helpful to make some further statement as to the potential of ACE-FTS as an instrument for routinely measuring such biomass burning products.

Table 2: Apart from adding some 1 sigma uncertainties to the retrieved values, it would be useful to have the Mauzerall values here as well and, perhaps those from the model. Also, rather than take the single Beira case from Yokelson et al, I suggest using their "Study Average" values, plus 1 sigma variability (which usually incorporates the Beira values).

Figure 3: Perhaps it is a problem with the colour scale, but it seems that the total CO over S America (a) is significantly larger than the biomass burning contribution (b), in which case: where does the extra CO come from?

Figure 5: It would be useful to have some quantitative measure of the improvement in the fit generated by the additional molecules, eg write the chi squared statistics before and after the fit on the figures.

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