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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Large-scale European source and flow patterns retrieved from back-trajectory interpretations of CO_2 at the high alpine research station Jungfraujoch" *by* C. Uglietti et al.

Anonymous Referee #2

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General comments

The paper report on a backtrajectory-based analysis of >4 years of continuous measurements of CO2 and O2 at the well-established Jungfraujoch (JFJ) mountain observatory. The authors introduce here two case studies of elevated CO2 concentrations and a backtrajectory analysis of the CO2 and O2 "high frequency" anomalies. These continuous measurements extend by a few more years the dataset presented by the authors in an earlier paper (Uglietti et al., 2008). This paper may be considered as a useful characterization of the JFJ station in terms of O2 and CO2 observations, with a

valuable discussion of airmasses origins and processes for different CO2-APO signatures. Overall, the essential novelty of this paper should be more clearly identified and stated.

Discussing high frequency variability of CO2 and O2 at a remote, mountain site is challenging and the authors deserve credit for attempting to tackle this issue seriously. However, the technique used (relatively low resolution back trajectory) has several limitation for this particular situation, including poor representation of convection and boundary layer processes, which are indeed noticed by the authors but not sufficiently addressed in the paper.

Backtrajectory analyses of transport of atmospheric compounds are numerous and the technique is well established, with now more elaborated tools available, such as Lagrangian particle disperson models (LPDM, such as FLEXPART) less sensitive to transport errors and more suitable for source region identification (see Han et al., 2005). The authors acknowledge that situation in their conclusions (p. 835, l. 14). Given the free availability and relative computational efficiency of these models, it is surprising that the authors did not use directly a LPDM. The authors further state (p. 835, l. 10) that "this study demonstrate the potential of trajectory analyses for classifying airmasses. . ."; I would argue that in fact the authors do not perform their classification based on backtrajectories but, instead, based on their measurements by selecting arbitrary regions in O2 and CO2 concentration space, which represent a significant difference. Automated, multivariate chemical composition based clustering has been shown as in, e.g., Lewis et al. (2007) for pollutants at Mace Head. Furthermore, it should be noted that the first demonstration of backtrajectory-based cluster analysis for atmospheric compound has been performed in the 80's (Moody and Galloway, 1988), and that improved LPDM-based cluster analysis now exists (e.g. Paris et al., 2010; Hirdmann et al., 2010). Choosing between the two approaches (observation-based vs. trajectory based) depends on the objectives of the study. In the light of some inconsistencies in the text such as the one noted above, this choice should clearly be better

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documented in the paper (see e.g. Cape et al., 2000 for such a discussion).

The paper is rather well written and clear, with good quality figures. However the flow of the paper is impeded by two case studies of high CO2 concentrations that do not bring new elements to our current knowledge. Instead, case studies could be better used to illustrate and validate some of the "cluster" analyses and discussions.

Specific comments

The title of the paper mentions "flow patterns. . .retrieved", this is slightly misleading because the paper does not really retrieves the flow patterns as such, it rather establish source receptor relationship (based on reanalysis flow patterns); a better title could be coined in this respect. I would also suggest mentioning O2 in the title for better visibility of the work performed.

p.814 l.5-6: actually the CO2 and O2 signatures are used to classify air masses, contrarily to what is suggested in this sentence

p.815 l.7 for the logic of the flow I would suggest to replace "greenhouse gases" by "CO2" after "For that reason. . ."

l.8 remove "constantly"

l.10 remove "of the changes in time", it is redundant with "evolution"

p.816 l.7"partly from oceans": a word may be missing here

l.11 "consummation" should probably be "consumption"? otherwise the sentence could be somewhat clarified.

l.12 replace "like" by "i.e." after "different fuels,. . ."

p.817 l.2 replace "it's" by "its"

l.6 "One of the most important sites in Europe. . .": I would suggest avoiding superlatives and ranking here and rephrase to say that JFJ is an important site to monitor

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atmospheric composition. . .

l.10 maybe the result of the papers cited here relevant to this study could be briefly mentioned.

l.13-14: here again trajectories are not used to classify, but CO2 and O2 anomalies are used to classify. Please rephrase correctly

Section 2.1. Please include longitude and latitude of JFJ

l.23: remove “also” after “North is. . .” (at the previous sentence it was northwest!)

p.818 l.14-15: I would suggest to remain factual and simply write that JFJ is a GAW station.

Section 2.2. Two O2 analysers are described, what measurements exactly are used in this study. This section should include a quantitative report on the uncertainties in O2 and CO2 measurements.

p.819 l.19-22: please provide the values of the concentrations used in the calibrations

l.24-26 In apparent contradiction with the sentence (“Data are archived through. . .”) I could not find the JFJ CO2 data neither from the GAW’s WDCGG, nor from the IMECC website. Non-CO2 data are nevertheless available from WDCGG. At least regarding GAW the sentence is misleading and needs to be changed. If the data are indeed available on one of these databases, web links would be useful. Note that GAW in itself is not an archiving service, but WDCGG is (on behalf of GAW).

p.820, last paragraph: here it is explained that 5 backtrajectories are run for each observation at different receptor points in the horizontal plane near JFJ, in a fair attempt to reduce transport error. However, why is the same not done in the vertical (i.e. 7 backtrajectories, with 2 above and below JFJ) ? Mountain stations’ altitude is notoriously difficult to represent in models, partly due to coarse topography (as noted later by the authors), and the uncertainty due to transport in the vertical may be more critical

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than in the horizontal especially for the identification of possibly thin plumes related to short term variability. I would also suggest reporting the number of vertical levels in the reanalysis used and the approximate vertical resolution near JFJ's altitude. These remarks are complementary with respect to the following comment.

p.821 L.1-5 this CO-based transport model validation exercise is very important and showing results (some text mentioning compound agreement values, e.g. offset and std dev, and/or a figure) would lend the whole study an improved credibility.

l.23 backtrajectories are notoriously not so good at representing convection and other BL processes. By selecting trajectories in the BL, the introduction of a bias in source-receptor relationship patterns is therefore probable. Could the authors explain how they tackle the problem of poor representation of convection?

Sections 3.1 and 3.2: At the beginning of section 3.2, the distinction between long term, seasonal and short term variability is exposed, but in Section 3.1 the authors have already discussed the seasonal variations, whereas they go on with long term and short term after the distinction is made, in Section 3.2. I suggest reorganizing the text more logically: 1) define distinction long/seasonal/short, 2) long term, 3) seasonal, 4) short term.

p.823 l.7-18: this paragraph is not a result and may therefore not pertain to this section 3 entitled "Results". I suggest that the text be reorganised and the section's title changed l.19-22: These two sentences should be in the "Methods" section, as are some parts of section 3.2. Could the authors detail in a short sentence or two how they de-trend the data beyond citing this reference, as this is highly relevant to the paper?

l.20: please change "Morimoto, S., et al." to "Morimoto, et al."

l.25 The same group observed (in Valentino et al., 2008, further citing Sturm, 2005) "a strong oceanic component contributing to the oxygen seasonal cycle even at the continental sites JFJ and PUY" based on other JFJ data. On the opposite, here, the state-

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ment that the seasonal APO amplitude is smaller than at other CarboEurope stations is in apparent contradiction. Does the high resolution dataset bring new information that is challenging previous flask data analysis? Maybe this is worth discussing here.

p.824 l.9: could the authors explain in the paper how they estimate the mean seasonal cycle? (simple monthly means? Harmonics?)

l.13 "as it would be linear": what is meant here? The whole sentence (starting with "Nevertheless we calculated. . .") is not very clear, please rephrase.

l.19: in the iterative procedure, what percentage of data points is rejected (and hence do not intervene in the calculation of the background)?

l.25 I would suggest to add at the end of the sentence ". . . for high frequency anomalies" as they removed the large scale flux effect by removing the seasonal pattern.

p.825 l.11 ">10 ppm above average": isn't 'background' intended here instead of 'average'? Please clarify.

l.20 Flask sampling: this is not described in the "Methods" section. Is the sampling automatic? At regular intervals? Only upon specific events?

l.22 please report the estimated uncertainty in the flask measurement.

p.828 l.15 I would argue that these are not really "clusters", and the wording needs to be changed (here and at other places in the paper) to e.g. "subsets", as in section 4.2's title. Here, the clutter is too strong (Fig. 9) and no group of points is obviously separated from the rest of the data.

Section 4.2: it would be useful to have the number or percentage of points in each subset relative to the total number of observations.

Section 4.2.3 & 4.2.4, titles: rephrase "negative CO2" into e.g. "negative CO2 excursion" or "depleted CO2", or any other reference to a delta-CO2 relative to the defined background. Same remark for "positive APO", etc. . . Please check the text for other

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occurrences.

Conclusion: see remark in the General comments above.

p.846 Fig 4: why are the error bars the measurement uncertainty and not e.g. the std dev of the data? If it is the measurement uncertainty, why does the measurement uncertainty vary with time (e.g. 30% more uncertainty on O2 in August relative to March)?

Figs. 7, 8, 10-15: - I recommend adding a sign for the JFJ position in these figures. - I also suggest to change the colour code to a light tone (toward white) instead of dark blue for low values (towards zero), in order to better visualize the relative weights of the residence time in the BL across the map and between figures. - Please mention in the captions, which subset number it has (in reference to section titles 4.2.1 and following) - Here, also please avoid the expression “negative CO2” and similar ones.

p.856 fig. 14: please move the sentence starting with “The main provenance of air masses. . .” to the main text instead of figure caption.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 813, 2011.