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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "STEFLUX, a tool for investigating stratospheric intrusions: application to two WMO/GAW global stations" *by* Davide Putero et al.

Anonymous Referee #2

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In this study, the authors present a new tool, called STEFLUX, to select trajectories having crossed the tropopause downward at some time in the past days and arriving into a user-defined geographical box within a prescribed time-window. The trajectories are selected among a large set of pre-computed trajectories based on the ERA-Interim reanalysis from the ECMWF. Doing this, this is presumably a fast-computing tool since no trajectory computation is needed.

Output data allow for various applications, such as assessing the occurrence frequency of stratospheric intrusions (SI) in the lower troposphere at any place on Earth at regional scale, but also characterizing preferred entry regions in the UTLS, travel times until the target area, etc. The paper presents an illustrative case study, a skill assess-

ment study with respect to SI detection based on (mainly ground-based) observations, and finally a climatology over 35 years of SI events over two focal areas.

STEFLUX is certainly a promising tool which may be helpful for a scientific community larger than the authors' research team. The paper itself is fairly well-built and written, and the presented scientific material and discussions are of good scientific quality.

Therefore, I recommend the publication of this study in ACP, but not before the author take in consideration the following comments and propose a revised version of their manuscript. I would appreciate if the authors could pay particular attention to my general comments 3 and 4.

General comments

1. Method originality not fully clear

While reading Section 1, it is not straightforward to know what is new in the STEFLUX method compared to existing methods based on backtrajectories. For instance, one could wonder why don't the author simply initialize backtrajectories from the target regions and see if their cross the tropopause at some time in the past?

I guess one major advantage of the method is computation speed, and this is due to the fact that it works from pre-computed backtrajectories. But this is not clearly stated in the text.

More generally, I think the Introduction could be developed and depict more explicitely the state-of-the-art in the domain: what are the different types approaches? what are their drawbacks or limitations? etc. The originality of the STEFLUX method should thus be more emphasized.

2. Representativity of a deep valley station

At several places in the text, it is suggested (e.g. when mentioning the "overpass" effect) that SIO may be missed at the surface stations because their measurement may not always be representative of the free troposphere at regional scale owing to local mountain meteorology. I think this concern is especially true for the NCO-P station, which is located in the bottom of a deep valley. Even in conditions of down-valley flow, it is likely that air has been in contact with the surface before reaching the observatory. Ozone in particular may have experienced deposition, and surface ozone concentrations may be lower than those encountered in the free troposphere. Valleys are indeed known to be net sinks for ozone (see e.g. Furger et al., Atmos. Env., 34, 1395-1412; Wotawa and Kromp-Kolb, Atmos. Env., 34, 1319-1322).

Even in the cited references (Bonasoni et al., 2010; Cristofanelli et al. 2010) little is said on the station representativeness at regional scale (except in the monsoon season at night). It would be worthy if this question could be briefly discussed somewhere in the paper (e.g. in Section 2 when the station are presented).

In contrast, I am much more confident in the regional representativeness of the mountain-top site Monte-Cimone (of course, apart from anabatic conditions) and the suitability of the site to detect deep stratospheric intrusion, although it is at much lower altitude.

3. SIO detection criteria too imprecise

In Section S1.4 (supplementary material), the SIO selection criteria are presented in a too vague and qualitative manner (and therefore the criteria appear to be subjective). For instance, what does "significant variation of daily P" mean? What is the threshold to consider the variation is significant? Further, is the current pressure daily mean compared to the value the day before?

One could ask such questions for almost every items of the two lists. The authors must present their study in a reproducible way, and those criteria are central elements. This

section should be rewritten in a much more rigourous and quantitative manner, with the concern of study reproducibility.

4. Missing discussion on backtrajectory maximum duration

In this study, tropopause crossings are considered up to 5 (= 1+4) days prior the trajectories reach the target box. But if one goes sufficiently deep backward in time, any trajectory ending in the target box crossed the tropopause at some time in the past. On the contrary, if the trajectory maximum duration is reduced below some value, no SI at all is detected.

Actually, the target region can be found to be from 0% to 100% of the time under the influence of stratospheric intrusions, depending on the chosen trajectory maximum duration. This parameter appears to be of central importance in the STEFLUX tool. I think a sensitivity study to this parameter should be presented (especially in relation with the results (percentages) given in Section 4.2.1), or at least, the choice of 5 days (which obviously comes from the work by Skerlak et al., 2014) should be carefully discussed and justified.

This leads to a more general question: any sufficiently long-lived molecule in the troposphere resided in the stratosphere at some prior times. What is the typical lifetime of a stratospheric intrusion in the troposphere, and when should one consider the air mass composition as being no longer influenced by the stratosphere?

I think these points are crucial in this study and deserve thorough discussions.

5. Links with ENSO, QBO and sunspots poorly convincing

In Section 4.2.2, the authors claim that some IMFs are correlated with various indicators (of ENSO, QBO, solar activity), but I find that Figure 5 and 6 poorly support these results (at least when examined by eye). Could these correlations be demonstrated more clearly, for instance by means of scatterplots?

Beyond this, correlation is not causality. A correlation is interesting to consider only if one suspects some mechanism linking two quantities. In the text, the possible link between ENSO and STE is discussed, but to a much lesser extent the links with the QBO and the solar activity. Could the authors discuss or even speculate a bit more about this?

6. Balance between paper main body and supplementary material

The article main body is quite concise in it present form, and I think there is perhaps room for moving important elements from the supplementary material into the paper main body.

For instance, the criteria to detect SIO are of primary importance in the study and could appear in the article, as well as Table S1, and perhaps also Figures S4 and S5.

Specific comments

p.1, l.2: The use of upper-case letters suggests that "STEFLUX" is an acronym. In this case, could the authors make it explicit at least once in the abstract and in the main text body? If it is no acronym but a simple proper noun, I suggest one should write "Steflux".

p.1, l.19: Please consider to change "relating" by "linking".

p.1, l.9-10: "show still" → "still show".

p.2, l.14, "anticyclonic": Do the author mean "cyclonic" instead?

p.2, l.17, "due to anthropogenic emissions": I would specify: local or regional. Please also consider that local or regional biogenic emissions may also alter atmospheric composition with respect to the tropospheric background.

p.2, l.17: "make" → "makes".

p.2, l.27, "Many different methods are based on this combined approach (...) and vary considerably between different measurement sites.": These statements are supported by no literature reference. Could the author cite here a list of references or at least a review paper on the topic? What are those considerable variations between the method? Could the author be a bit more explicit? See also my general comment 1.

p.2, l.27, "occurring over": reaching? detected?

p.2, l.33-34, "Moreover, ...": It seems that this potential application is not illustrated in the paper. Could the author justify this statement?

p.3, l.5: "to it" → "on climate".

p.3, l.19-20: This statement is questionable and deserves further discussion. See my general comment 2.

p.3, l.24-25, "starting at the measurement site": this is too imprecise, especially concerning the altitude. Was the true site altitude or the model surface altitude used to initialize the backtrajectories?

p.4, whole Section 3.1: even though the case study clarifies well what STEFLUX is (Sect. 3.2), Section 3.1 presenting the tool is confusing. Especially, it is hard to distinguish what comes from Skerlak et al. and what is specific to STEFLUX. Beyond this, a number of elements from Skerlak et al.'s methodology are mentioned in the text (trajectories extended 4 days prior to tropopause crossing; 3D labeling) but it seems these details are not needed in STEFLUX or at least in this paper. If really not needed, these information items are confusing and should be removed. Otherwise, it should be explained why they are important. More generally, I think that the whole Section 3.1 should be rewritten and clarified.

p.4: title of Section 3.2 could be changed to "Illustrative case study".

p.4, l.29: The box centered at NCO-P is hardly visible in Fig.1b. Anyway, a reference to this Figure is not useful in this sentence, and mention to Fig.1b could be simply removed here.

p.4, l.30: “recorded” can be removed.

p.5, l.5: in the present form of the paper, the criteria are actually introduced in the supplementary material, not in Section 2. See also my general comment 6.

p.5, l.11 and ff.: it seems from these lines that there are three different output files from a STEFLUX run, but it is not clear what is in those files. This should be clarified (perhaps in Section 3.1).

p.5, l.17: "indicated in previous studies ..." → "identified as a preferred region for tropopause crossing in previous studies ...".

p.5, l.26, "they still maintained a stratospheric signature": poor expression, please rephrase.

p.5, l.33: the choice of an horizontal extension of $3^\circ \times 3^\circ$ should be justified briefly.

p.6, l.2-3, “The selected time periods were the same as in Sect.2”: please specify.

p.6, l.4: “a table listing ...” → “Table S1 listing ...”.

p.6, Section 4.1.1: What is the criterion to tag a day as SI day according to STEFLUX? Is only one box crossing at any moment of the day and of any duration needed? The author should specify this in this Section. (See also the corresponding comment from the Anonymous Referee #1.)

p.6, l.11, “at the two measurement sites”: not needed and a bit confusing, please remove.

p.6, l.24: “subtle” is unexpected as adjective for the inter-annual variability. Please rephrase.

p.7, l.1, “criteria coverage”: please define. Is it the fraction of time when the data used in the criteria are simultaneously available? Every criterion does not use all the data:

what does happen when one data is missing for one criterion but another criterion is fulfilled? Or none other fulfilled? Is the day tagged as SI/non-SI day or discarded? Please clarify.

p.7, l.22 and ff.: I had a hard time to understand those contingency tables. Considering for instance Table 1(a), does 55 means that during 55 SIO events, STEFLUX detected more than 50% of time of the episode as SI? Does 148 means that during 148 SIO events, STEFLUX detected less than 50% of time of the episode as SI? etc. Please explain a bit more how those numbers should be interpreted. See also the comment from the Anonymous Referee #1 concerning the definitions of accuracy and false alarm rate: how exactly are the presented scores calculated?

p.8, l.2 and 5: the capture rates given in Table 2 (22-27%) are closer to one quarter than to one third.

p.8, l.20-24: In case of long travel time and high mixing, can one still consider the air mass as a stratospheric intrusion? See my general comment 4 on stratospheric intrusion lifetime.

p.9, l.10: this again is related to my general comment 4: is it really relevant to be irrespective of the degree of mixing and dilution in the troposphere?

p.9, l.23-25: could the author explain this statement?

p.9, l.32: "If divided seasonally“ → "Considering seasons separately”

p.9, l.33 and p.12 l.15: "southward of“ → "south of”

p.11, l.12 “does not exhibit as” → “exhibits no”

p.11, l.18 “defined” → “user-defined”

p.11, l.19 “representative” → “illustrative”

p.12, l.17 “both of the“ → ”both“; “significant” → “statistically significant” p.13, Appenzeller and Davies, 1992: insufficient reference.

p.16, Table 2: missing "(b)".

p.18, figure legend, l.3: "Sect. 2" → "Sect. S1.4". See my general comment 6.

p.19, figure 3: the STEFLUX and SIO panel columns could be interchanged, so that the panels (a-d) are numbered in the same order as in the text. Why do the box plots in the upper panel have no whiskers?

Supplementary material

p.1, l.18: do the authors mean gamma-spectroscopy?

p.1, l.25: "total column OF ozone".

p.2, l.9-10: This sentence is not fully clear. What does "centered at an ending altitude" mean? Is 490hPa the real altitude of NCO-P? In the same vein in l.12, do the trajectories reach Mt. Cimone at its real altitude level? What is the corresponding pressure level?

p.2, Section S1.4 (SI selection criteria): see my general comments 3 and 6.

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