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Referee comment on acp-2022-420

Anonymous Referee #2

Referee comment on "Gravity-wave-induced cross-isentropic mixing: a DEEPWAVE case study" by Hans-Christoph Lachnitt et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-420-RC2, 2022

In this study, the authors investigate the effects of gravity wave breaking and the resulting turbulence on mixing around the tropopause. The study is based on a research flight over the Southern Alps during the DEEPWAVE measurement campaign. In-situ measurements of N2O and CO above and below the tropopause, upstream and downstream of the mountains, have been used to diagnose mixing, while gravity waves and turbulence were analysed in detail using temperature and wind measurement data. The authors report breaking gravity waves and air turbulence close to the tropopause, and a resulting alteration in tracer structure, which shows that significant mixing events have occurred in the affected atmospheric regions.

Overall, I believe the results presented in the manuscript are of a very high standard and clearly merit publication in ACP. The effects that gravity waves have on atmospheric composition and dynamics are still poorly understood, but of high relevance for quantifying tracer transport and large scale dynamics of the atmosphere. The main topic of the paper is therefore highly relevant and of considerable interest to the community. The methods and analysis in this work are generally solid and clearly presented, the analysis of cross-isentropic transport is especially detailed. The figures are well prepared. Presentation of results is also very clear in most parts, I would only suggest to make the mathematical notation more consistent in a few places and to clarify the identification of different flight segments discussed in the text (see minor/technical comments).

My two more substantial observations are given below, followed by a list of technical corrections and minor suggestions.

General points and related observations:

1) The UTLS region is known for sharp tracer gradients, both horizontal and vertical. The

horizontal length scales of tracer filaments resulting from various stratosphere-troposphere exchange (STE) processes (like, for example, planetary wave breaking) can be much smaller than the dimensions of the flight pattern considered here. The structure of N_2O distribution in UTLS, as described in this paper, is shaped by STE and can be affected by various STE events. Since the speed of the aircraft is much larger than that of the wind, the air masses sampled downstream of the mountains are most likely not the same air masses as sampled upstream (or are they at least partially the same?), and thus it is possible that these air masses already had different N_2O-θ profiles even before crossing the mountains. It would be good if authors could comment on such a possibility, or argue why it would not significantly alter the tracer gradient analysis results. It was briefly mentioned that aircraft was not flying through tropopause folds, but air with altered tracer structure could have been advected from elsewhere. I realise that this problem may indeed be very hard to address using only data from airborne in-situ measurements, but there are other arguments that could be made. For example, model data showing no complex structures in the typical stratospheric or tropospheric tracers upstream of the mountains before the flight could strengthen the argumentation that leads to the main conclusions of the paper. N_2O data would, of course, be best, but ozone and water vapour, which should be available from ECMWF IFS, could already tell a great deal about possible influence of earlier STE events on the observed air masses. Alternatively, dynamical histories of the sampled air parcels and their surroundings could be investigated. There are also a few interesting details in the manuscript that might make this point more relevant:

a) The mechanism for modification of N_2O-θ relationship by cross-isentropic transport and mixing, as described in Figure 8, predicts that air above the mountains should include air masses that fall in between the compact upstream/downstream relationships (shaded region in Figure 8b). Therefore, in Figure 9, one would expect to see some black points (observations over the mountains) in between the compact relationships in blue and red (upstream and downstream data). However, the upstream data forms a compact relationship quite distinct from all the remaining higher-altitude flight leg data, especially in the 316-320 ppbv N_2O range. Could this be a possible indication that some of the upstream air masses might have a different composition than over-the-mountain/downstream air masses had before being affected by GWs?

b) Section 4.3 and the conclusions state that certain features of the results "can be seen as the result of the turbulence occurring potentially previously on this level". If the results suggest that the composition of (at least some of) the observed air masses was significantly affected by the previous turbulence/mixing processes, would it not be natural to ask if all the observed air masses were affected equally?

2) After a dynamical process, such as wave breaking, causes cross-isentropic transport and scale breakdown in the tracer structure, tracers are further (mostly isentropically) mixed by molecular diffusion (e.g. Balluch and Haynes, 1997). The N_2O-θ relationship is a great tool for characterising the cross-isentropic transport, but it would also be interesting to see to what extent the air masses that were transported across isentropes are already mixed into surrounding air. Maybe analysing the different air parcel groups from Figure 9 in N_2O-CO space could shed some light on that? Or was N_2O-CO analysis inconclusive for these air masses?

a) Another interesting feature of Figure 9 is that although the downstream air masses occupy roughly the same range of potential temperatures as the upstream ones, they have a much narrower range of N_2O concentrations (close to the mean N_2O value) with no outlying points in the rest of the upstream N_2O range. Could this potentially suggest that the turbulence over the mountains, which the downstream air masses have experienced for just a few hours, has already mixed the affected air masses quite efficiently, and further isentropic mixing (which would normally be slower) is less relevant here? Again, maybe N_2O-CO relationship could be used to confirm this?

Minor and technical points:

P 1, L 21: The phrase "conserves the effect" is confusing. It is not quite clear to me how an effect itself (as opposed to physical quantities or the results of the effect) can be conserved. This should perhaps be rephrased.

P 2, L 1: "Orographic gravity waves [...] may affect the large scale stratospheric circulation." Clearly, there is still a lot to be learned about orographic GW forcing and the effect they have on the general circulation, but is there really any doubt whether orographic GWs have an effect at all?

P7, L10: What exactly is meant by "analysed PV"?\

P8, Figure 4: The wave packet seen between 170.1° E and 170.6° E in the higher altitude leg has a very nice and regular vertical wind w and θ relationship (π / 2 phase shift), just as one would expect from linear wave theory. The same longitude range of the lower altitude leg, however, has an interesting θ structure that does not correspond that well to w. It might be interesting to see if ECMWF IFS predicts similar structures, as these may be related to wave breaking/reflection.

P9, L1: The word "where" should be replaced with "were".

P9, L2: Most literature (and the rest of this paper), provide amplitudes as positive numbers, "+/-" should therefore be omitted for consistency. Also, since fluctuations of potential temperature are mentioned, it would be good to provide their amplitude as well.

P9, L7: The phrase "[...] indicative for at least a kinematic breakdown [...]" should probably be replaced with "[...] indicative of at least a kinematic breakdown [...]". Also, the whole sentence is confusing, it is not quite clear what the word "but" in L8 refers to.

P10-P11: I may have missed something simple or misinterpreted the terms used, but the discussion of observed GWs in Section 3.3 appears to contain contradictory statements. For example, the terms "lower/upper flight leg" seem to refer to lower/higher altitude flight segments in most of the discussion on P10 and P11. Also, the long horizontal wavelength wave mode is stated to be "totally absent in the lower leg" (P10 L4), and "partly seen in VH in Fig. 3 around 17:45" (P10 L3). However, according to Fig. 3, the aircraft was flying at the lower of the two main altitude levels (i.e. flying the "lower leg"?) around 17:45 UTC. Perhaps in some cases "lower/upper leg" refers to lower/higher altitude, and in some cases to downstream/upstream? In any case, I feel that the terms used for flight segment identification should be updated in the entire Section 3.3, so that no guesswork is needed. For example, one might consider only using the term "flight leg" for a straight (geodesic) flight segments, and adopting other terms to refer to longer portions of the flight.

P11 L3: Dissipation is indeed a likely explanation for the change in vertical wave energy flux, but one must not forget that GWs are often reflected at the tropopause, which complicates the interpretation of energy fluxes in this region. In any case, the turbulence observations in this paper provide a stronger argument that wave energy is indeed dissipated in the altitude range considered here.

P11, caption of Figure 6: Duplication of the article "the".

P12 L14: Strictly speaking, there is nothing "between $\theta < 328.1$ K and $\theta > 326.3$ K", as the two intervals overlap. I would either write "layer between $\theta = 328.1$ K and $\theta = 326.3$ K" or, preferably, "a layer with $326.3 \text{ K} < \theta < 328.1$ K".

P12 L15: I cannot see any green crosses in Fig. 6, perhaps this should refer to green squares?

P12 L5: The notation "$\partial N2O / \partial\theta$" as given in L4 is clear, concise and well-defined. Therefore, I cannot see any benefit of subsequently introducing so many different terms (N2O-gradient, N2O-θ gradient, N2O-θ slope, decrease of N2O with potential temperature θ, N2O decrease with respect to θ, ...) to refer to essentially the same thing.

P14 L10: Firstly, notation "θ' $N2O'$ ratio" is a bit odd. "θ / $N2O'$ ratio" or "the ratio of θ' and $N2O'$" would already be better. Secondly, as explained in Section 4.1, the ratio $N2O'$ / θ' depends on integration time and is therefore mathematically not the same as "N2O-θ gradient". The fact that the measured gradients do not actually depend on the integration time too much (in a reasonable range of integration times) is a meaningful finding that supports the main claims of the paper. It is hence important not to confuse the readers by implying these two quantities are one and the same.

P18, L3: After inspection of Figure 7, it would seem that both θ and N2O concentration amplitudes given here are peak-to-peak values, actual amplitude values should be half that.

P18 L7: Same issue as P14 L10. I cannot see a good reason for using primed quantities here and unprimed quantities in L9.

P18, caption of Figure 12: Main text discusses various wave time scales, and not length scales. Therefore, the figure should have wave period labels.

P18 L14: Perhaps the authors meant "negative vertical N2O gradient"?

P20 L9: The phrase "periods ranging from 8-16 km" should be replaced with "wavelengths of 8-16 km"

P23 L30: Again, the expression "The tracer conserves the effect of [...]" should perhaps be rephrased.

References:

Balluch, M. G., and Haynes, P. H. (1997), Quantification of lower stratospheric mixing processes using aircraft data, J. Geophys. Res., 102(D19), 23487– 23504, doi:10.1029/97JD00607.