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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global large-scale stratosphere-troposphere exchange in modern reanalyses" *by* Alexander C. Boothe and Cameron R. Homeyer

Anonymous Referee #1

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The authors perform an analysis of stratosphere troposphere exchange (STE) in four state-of the art reanalysis data sets (ERA Interim, MERRA, MERRA2, JRA 55). They apply Lagrangian analysis to diagnose exchange using the thermal tropopause as reference surface. STE is subdivided according to spatial directions of exchange 'lateral' and 'vertical'. Before starting the reanalysis comparison, they compare their method with the results of a recent analysis of STE from Skerlak et et al., who used a PV-based threshold. They find differences, which are based on the different methods and motivate their own lapse-rate-tropopause (LRT) approach partly from these different results.

The reanalysis data are analysed for time period of 15 years, despite longer analysis

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time periods would have been possible. They find substantial differences between STE in the reanalysis data sets. Whereas JRA and ERA Interim are STT dominated, MERRA 1 and 2 are TST dominated, according to the authors. Mass fluxes are shown and exhibit significant differences and net mass fluxes partly deviate significantly from zero, which is mentioned, but not explained or discussed in detail.

The manuscript could in principle make an important contribution to the field, since a consistent comparison of exchange between different reanalysis data is of very high interest. However, the authors need to assess the caveats of their method and the consequences for the result. It seems to me that they miss a part of the processes particularly at the extratropical tropopause. This might be due to the method and procedure they have applied to diagnose STE. The non-zero net fluxes also could be an indication for this.

Further a careful quantification and discussion of the tropopause location and its determination is essential for the paper and needs to be included. Based on this and on the points below, the authors should discuss the results, which are of high importance and interest more carefully also in the light of the potential caveats of their methods of tropopause determination or differences in the tropopause location between reanalysis data sets.

1) The thermal tropopause itself needs to be assessed for the individual data sets, before analysing the exchange and probably before regridding (see also suggestions below). This point is crucial, particularly for the method as applied here. Which role plays the interpolation of the fields for the results, particular for the vertical coordinate and the location of the tropopause altitude?

2) The authors just perform a spatial classification of STE 'lateral' and 'vertical', which does not mirror the dynamical processes. For exchange between the subtropics and mid latitudes, where the tropopause break has a large vertical extent, this might work well. For the mid latitudes they might miss parts of the exchange (see comments below

with references) since there is no 'lateral' STE per definition of the method. This needs to be discussed as well and potentially lead to a potential bias e.g. in the fluxes.

I highly suggest to include the method of Skerlak et al., 2014, despite differences, since it allows for a further independent comparison also with previous results from literature.

3) As stated by the authors, one should expect the STE being mass conservative. This seems however not be the case. Since this is a central point also for the long-term STE time series the authors should also discuss carefully the caveats of their method.

4) The thermal tropopause definition in general, but especially in high latitudes is problematic, how does this affect the results (see e.g. Zangl and Hoinka, 2000)?

5) How do the results relate to other approaches?

Overall such a comparison is a valuable effort. However, in the current form the paper needs a major revision, particularly with regard to potential uncertainties of the method, which could contribute to the discrepancies between the reanalysis data sets.

MAJOR: Which role plays interpolation of the fileds for the results? Did the authors interpolate also in the vertical? If yes I think a sensitivity for at least one model should be done to assess the effect of interpolation ob the STE results.

Further the authors find the largest differences between the data sets for the 'vertical' exchange. This is not surprising, since it might be related to differences in the vertical resolution or the variability of the vertical wind in the models. Also the differences in the representation of the thermal tropopause might contribute to these differences, which in turn depends on the vertical resolution of the specific data set. I missed an assessment of this particularly for the extratropics (e.g. a monthly pdf of vertical wind for each month the extratropics).

Since the spatial coordinates play such a crucial role the authors need to systematically assess this: They should add plots (PDFs) of the tropopause height separated for the extratropics (seasonally resolved) and tropics for each data set. This should be done for the original data as well as for the interpolated data to get both, the differences between the data sets and the effect of regridding.

For STE: Evaluating the differences TP_tropical_press minus TP_extratropical_press between the different data sets is important since differences of the diagnosed separation between extratropical and tropical tropopause will directly affect STE results (see also comments further below).

Criteria: 'Lateral' STT: Is exchange across the extratropical tropopause possible, which is not 'vertical'? How are particles counted, which start in the troposphere, but follow downward sloping isentropes into the stratosphere? These parcels (initially lying below the extratropical TP) descend e.g. from above the polar jet and are mixed into the adjacent stratosphere above a trough). Such a parcel will descend, but gain PV? This is not an exotic process and does occur quite frequent (e.g. Pan et al., 2007, Pan and Konopka., 2012, see also Juckes, 2000). How is quasi-isentropic mixing in the extratropics in general treated? According to the classification no 'lateral' exchange is possible if the tropopause is below 200 hPa (i.e. at higher pressures). Also: Why does exchange above and below the jets need to be 'vertical' (p.6, l.33)? This is a limitation of the method and needs to be clearly discussed, also in comparison to Skerlak et al., 2014.

SPECIFIC: p.5, l.10-12: It has been shown in many studies (Gettelman et al., and references therein) that in the extratropics away from the subtropical jet very well represents tracer isopleths. This is due to the fact that PV is materially conserved under adiabatic conditions, which is not the case for the LRT. Notably the vertical gradient is included in the PV definition, which therefore inherently includes the thermal definition. Note further, that the -2K/km are an arbitrary definition for the thermal tropopause gradient in a similar way as a fixed PV threshold. Therefore, the above argument is not valid.

p.6, l.19: Isn't convection 'vertical'? How is it considered? p-7, l.15: What is 'systematic' upwelling? p.7, l.17: 'according to our knowledge... LRT method agree more closely with known transport mechanisms'. Please give a few references here, such a statement without references is very superficial. How does this compare with e.g. Juckes et al., 2000? Maybe the LRT method is very well suited for identifying exchange at location of the subtropical jet. At mid latitudes the the LRT altitude criteria probably fail in regions?

p.13,l.20: What is 'equivalent dynamics'? p.13,l.13-l.22; The arguments are confusing as well as the use of 'dynamical and physical differences': Why is the jet location a dynamical difference, the fold and tropopause physical differences? Both are related to the same physical processes, which control temperature gradients and pressure etc. and finally the location of these structures - based on the representation of physics in the respective reanalysis model.

Further: Why can TP altitude lead to changes in STE between the different models? If the tropopause location (and jet, folds) in each reanalysis is the result of differences in the respective model physics, it still might be self consistent within each reanalysis data set. One could get differences of tropopause height, location, jets etc. between different data sets without differences in STE.

Since 'vertical' exchange is so important - which role plays the variability of the vertical wind in the data sets?

p.14, l.13/13.: Vertical and lateral are inappropriate terms to characterize physical processes, It's just a spatial direction in (cartesian) coordinates, but STE is more complex as you show. Therefore please change the word 'processes' to 'direction'

p.14, l.25-27: This statement as it stands here is incorrect or at least misleading. In geometrical coordinates the direction could be downward, although the PV change can be positive. This were a TST in the physical víew accounting for thermodynamics, but an STT from geometrical aspects.

p.14, l.30: How is transport 'stratified'?

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p.15, l.12: Why is poleward transport the same as TST?

References:

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Pan and Konopka, On the mixing driven formation of the ExTL, JGR 2012, 10.1029/2012JD017876. (Fig,1)

Pan et al., Chemical behavior of the tropopause observed during the Stratosphere-Troposphere Analyses of Regional Transport experiment, JGR, 2007, 10.1029/2007JD008645 (Fig.8)

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