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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratosphere–troposphere exchange (STE) in the vicinity of North Atlantic cyclones" *by* P. Reutter et al.

Anonymous Referee #1

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This is a nice study documenting stratosphere-troposphere exchange (STE) in the vicinity of extratropical cyclones using Lagrangian methods developed extensively in previous studies. In particular, 33 yr of ERA-Interim reanalyses are used to quantify exchange relative to the position of sea level pressure minima and during the lifecycle (intensification, maturity, and decay) of extratropical cyclones. The north Atlantic basin is the focus of this study and the figures and discussion are well composed with respect to the findings and previous literature. There are certain aspects of the analysis, however, that may bias the results. Primarily, the choice to use only the position of the surface cyclone to identify related transport is a major concern. Such a choice likely

impacts the findings related to cyclone-relative transport location and attribution of total STE to extratropical cyclones, both major themes of the present study. I expand upon these concerns in the comments listed below and offer suggestions for resolving them to improve the paper. In addition, the motivation for the present study is somewhat weak in the current draft of the paper (general comment #1 below).

General Comments

1. Introduction: - Paragraph of history of STE studies on cyclones (Pages 2539-2540): While this summary is certainly informative, it would benefit from some better synthesis of these studies and reduction in length. Currently, it reads more as a collection of thoughts than a building block for the paper.

- There is little pointed motivation for the current study in the Introduction. Following the summary of previous work, the text jumps into the focus of this study and the questions it intends to address without providing emphasis on what gaps in our understanding remain and why quantifying them in the present study is worthwhile.

2. On the cyclone identification: What is the reasoning for using surface pressure alone to identify STE within cyclones rather than in tandem with a level near the extratropical tropopause? It seems to me that, for reasons you already outline in Section 4.1 and evidence given in Figures 9 & 10, considering only transport within the domain of the surface cyclone biases your understanding of the process. I expect this choice erroneously leads to one of the main findings of the current study: that most STT is confined to the west of cyclone center during intensifying and mature stages (due to westward tilt of cyclone with height) and near the cyclone centre during the decaying stage. There is no justifiable reason to me to consider only those particles that cross the tropopause within the domain of the surface cyclone rather than the upper-level cyclone, since these features are part of the same dynamical system. Using the upper-level cyclone identification would provide more representative results regarding the location of STE with respect to cyclone position and improve the broader attribution

of STE to cyclones. I suggest the authors use both surface and upper-level cyclone positions to better quantify transport associated with these flow features and its occurrence relative to cyclone centre.

3. On the discussion comparing Figures 9 & 10, specifically lines 15-23 on page 2551: I am not convinced that the differences in exchanged mass between phases for TST are not comparable to the differences for STT. The magnitudes of TST are about half as large as that for STT in these plots and using the same scale likely hides some of the differences between phases. For STT, the magnitude of mass exchange increases by a factor of $\sim$3 between intensifying and mature phases, with values ranging from $\sim$0.1 to 0.2 x 10ˆ15 kg at SLP < 970 hPa during the mature phase. For TST, the factor increase in mass exchange between phases appears to be similar to me, but the magnitude at SLP < 970 hPa is between $\sim$0.75 and 1.25. I suggest the authors look at ratios between phases in these distributions to develop a clearer understanding of how STT and TST compare during cyclone lifecycles.

Specific Comments

Page 2540, line 1: “date” should be “data”

Page 2541, line 14: An outlook for what?

Page 2544, line 23: What characteristics classify intensification as “explosive”? It would help to specify that here and possibly reference the work of Fred Sanders, John Gyakum and Lance Bosart. e.g., Sanders & Gyakum (1980): Mon. Wea. Rev., 108, 1589âĂŤ1606; Bosart (1981): Mon. Wea. Rev., 109, 1542âĂŤ1566.

Page 2545, lines 26-28: What is the typical lifetime of a North Atlantic cyclone?

Page 2548, line 27: “s445” should be “445”

Page 2549, lines 19-20: “includes” should be “include”

Page 2550, lines 25-27: Citation? Also, I assume you are referring to upper-level wind

speeds?

Page 2553, line 12: “A very strong” is subjective, not scientific. Suggest replacing with “One”

Figures 9 & 10: The grey and black lines are difficult to see atop the colour-filled background here. Also, there needs to be a scale provided for the density in these plots.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 2535, 2015.