Reply to Reviewer #2
(Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-584)

We thank the Reviewer for the time she or he spent on the manuscript. The comments helped to improve the manuscript. In the following, the Reviewers comments and the corresponding responses are listed. The page and line references given by the Reviewer relate to the manuscript in discussion. Numbers given from our side relate to the revised manuscript.

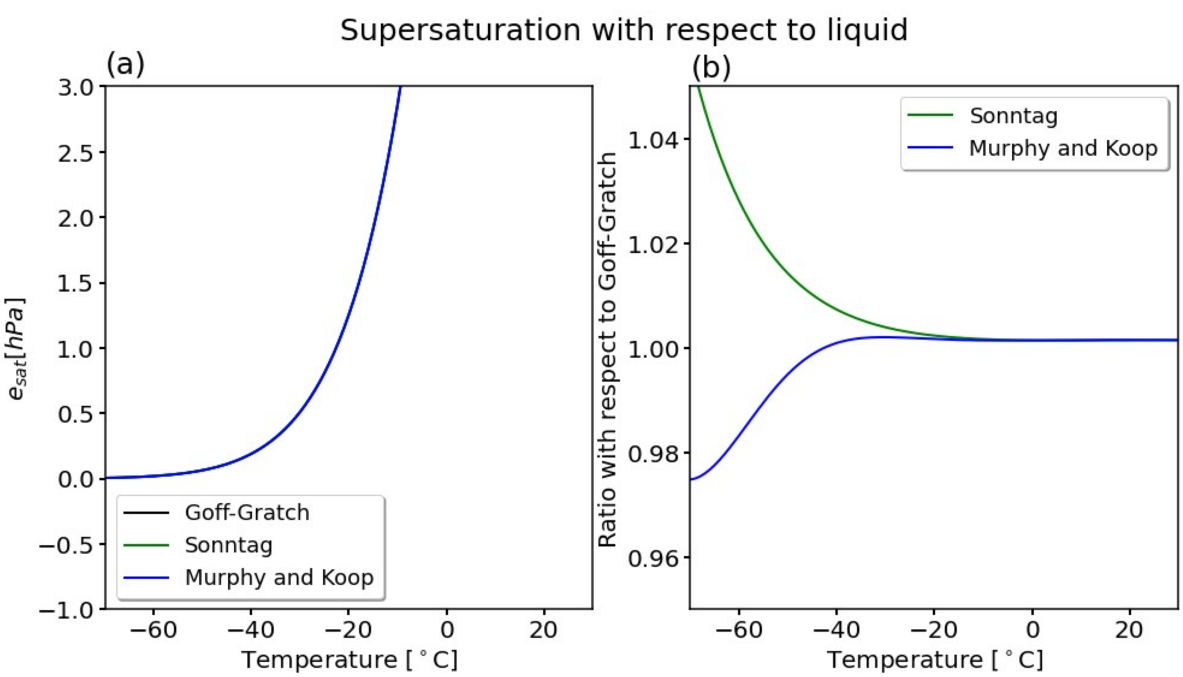
For better legibility, the Reviewer comments are highlighted in **bold** and changes in the manuscript are in *italic*.

Comments

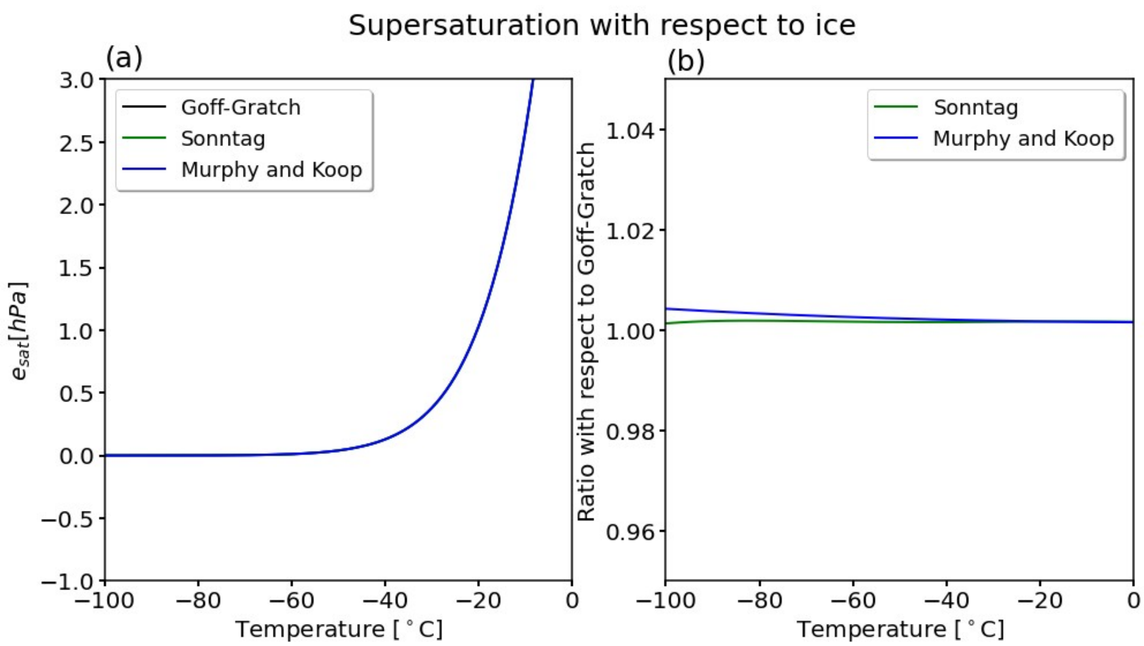
Is there a reason that for the calculations of the saturation water vapor pressure over liquid water the equation after Sonntag (1994) is used? Wouldn't it be more consistent to use the Murphy and Koop (2005) method also for e_sat_liq (as it is already done fore_sat_ice)?

The saturation water vapor pressure e_{sat} is calculated by using polynomial approximations of the Clausius–Clapeyron-relationship. Multiple approximations and equations do exist. For e_{sat} over liquid water $e_{sat,liq}$ and ice $e_{sat,ice}$ the equations after Goff and Gratch (1946); Goff (1957) are regarded as the reference, for example in Alduchov and Eskridge (1996) or Gueymard (1993). Commonly, radiosonde manufacturers use the equation after Sonntag (1994) to calculate $e_{sat,liq}$. Similarly, Spichtinger et al. (2003), Immler et al. (2008), and Rädel and Shine (2010), who also analyzed radiosonde observations, used the equation after Sonntag, which we follow for consistency.

To analyze the differences in $e_{sat,liq}$ as well as $e_{sat,ice}$ calculated from Goff and Gratch, Sonntag, and Murphy and Koop, we compare absolute values to derive relative differences with respect to Goff and Gratch.



The first figure below shows (a) the calculated saturation $e_{sat,liq}$ with respect to liquid water using the equations of Goff and Gratch (black), Sonntag (green), and Murphy and Koop (blue). Sub-panel (b) shows the ratio between Sonntag (green) or Murphy and Koop (blue) with respect to Goff and Gratch. Over the plotted temperature range from -70 to 30º C the differences are of at most 4% for Sonntag in the extreme case of -70º Celcius.



The second figure shows (a) the calculated supersaturation with respect to ice using the equations of Goff-Gratch (black), Sonntag (green), and Murphy and Koop (blue). Sub-panel (b) shows the ratio between Sonntag (green) or Murphy and Koop (blue) with respect to Goff-Gratch. Over the plotted temperature range from -100 to 0º C the differences are below 0.2%. Based on these minor differences in calculated supersaturation we argue that the selected approximations for the supersaturation do not influence the results over the temperature range applied here.

A part of this discussion is added to the Appendix to the manuscript as it could be useful for the interested reader.

In order to help readers better understand the results, a rough quantitative estimate of the possible disadvantages of higher / lower altitudes would be helpful.

The Reviewer points to an important topic, which has to be considered for flight planing and avoiding ISSR. Though it is beyond the scope and capability of this study to provide quantitative estimates. The topic is complex and dedicated studies are ongoing to determine and quantify the effects of flying at lower or higher altitudes. Nevertheless, we added the following paragraph to the manuscript to point the interested reader to already existing studies.

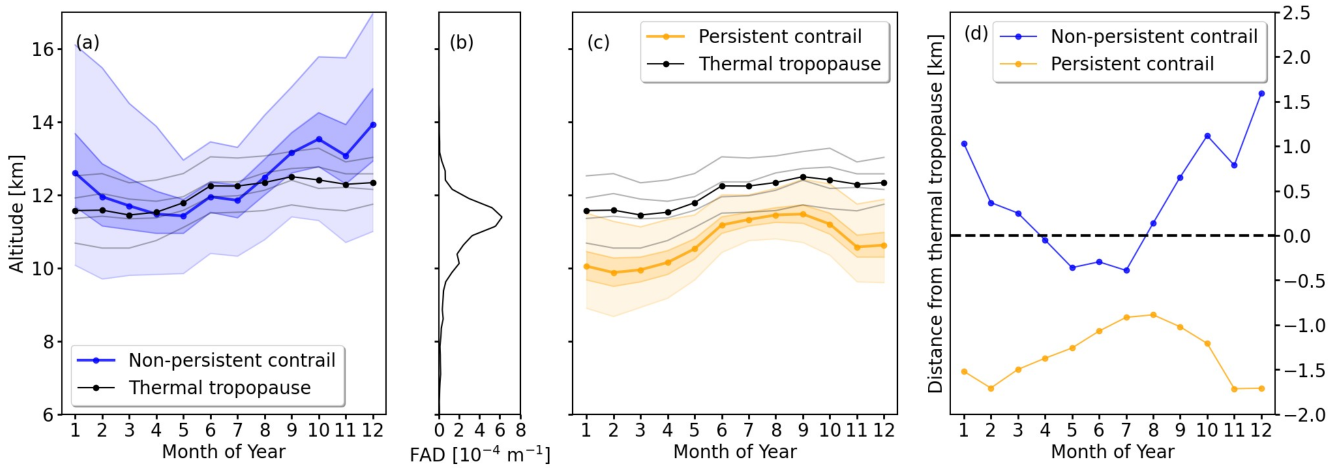
> *[…] One has also to consider that aircraft tend to fly at / or close to their optimal flight levels. Flying lower or higher may be sub-optimal in terms of fuel consumption and/or require adjusting the aircraft airspeed. Furthermore, depending on an aircraft characteristics and*

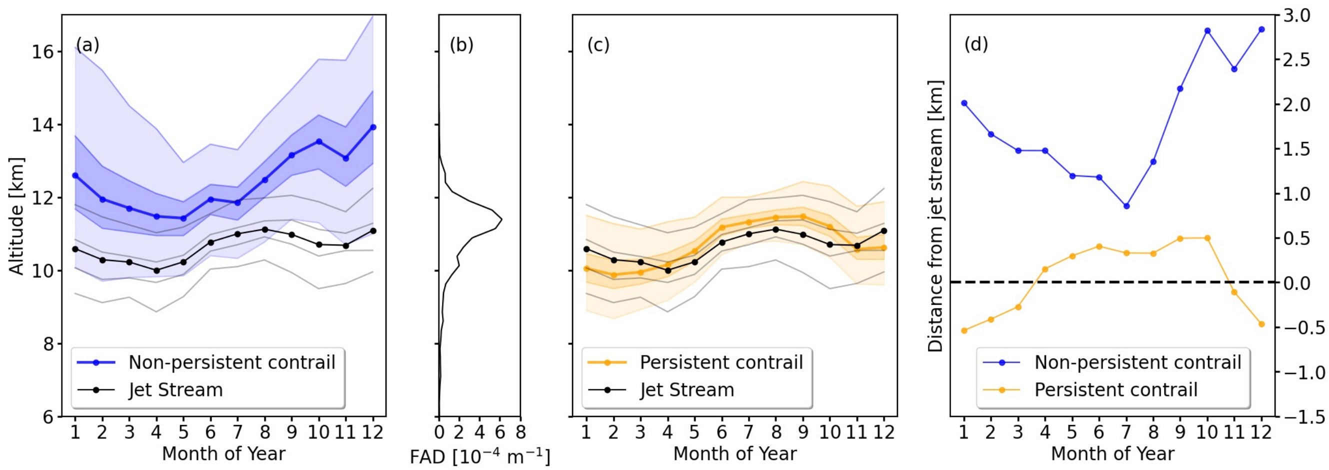
payload, it may not always be possible to fly higher. Beyond that the interactions between flight altitude, aircraft performance, engine emissions, and radiative impact of possible contrails are complex. To estimate the effect of flight altitude changes dedicated studies have been performed, for example by Frömming et al. (2012), Dahlmann et al. (2016), and, more recently, by Matthes et al. (2021).

Furthermore, a quantification is not possible as we a missing the import point of the ice crystal size after the contrail formation process. In case of hydrogen engines, chemistry calculations would be required.

The illustrations are very clear and meaningful. Nevertheless, an indication of the percentiles for the tropopause (Fig. 5) and jet stream height (Fig. 6) would be desirable in order to gain an impression of their variability.

Following the suggestion of the Reviewer we added lines for the 20, 40, 60, and 80th percentile. The revised plots are shown below and exchanged in the manuscript.





P11L258: „Profiles for which the temperature inversion was weak and the TT altitude was not clearly identifiable are removed from the analysis.“ What exactly do you mean

with „weak“? How do you define this „weakness“?

We agree with the Reviewer that ‘weak’ is an insufficient description. Therefore, the paragraph has been rephrased to explicitly describe the selection algorithm.

> *[…] For each RS profile, γ is calculated with Eq. 9 and the location of the smallest γ (in absolute value), i.e., the local minimum in T , between 8 and 14 km is set as the TT. For profiles, where the altitude of smallest γ was equal to 8 or 14 km, the TT altitude was not identifiable and was removed from the analysis.*

P14L332: "The smallest distances are identified in March and November with the jet stream at the same altitude as for R2.“ For me it seems that the distance in April is smaller than the distance in March. Typo?

This is correct. The distance is smallest for April. The typo was corrected.

> *The smallest distances are identified for April and November with the jet stream at the same altitude as for R2-PC*

P12 L278 "similarly to R2-PC conditions“ - you mean R1-NPC? Typo?

The Reviewer is right. We refer to the R1-NPC. The typo was corrected.

> *The largest vertically-integrated occurrence is in winter followed by spring, autumn, and summer, similarly to R1-NPC conditions.*

P14L331: "Similarly Fig 6b..." you mean Fig 6c?

The reviewer’s statement is correct and the typo was corrected.

> *Similarly, Fig. 6c visualizes the vertical distribution for R2-PC.*

Minor Comments

P12 L273. R1-NOC should be R1-NPC

The typo is corrected and ‘R1-NOC’ is replaced by ‘R1-NPC’ .

Sometimes R2 is used, other times R2-PC (same for R1 and R1-NPC). Please use consistent labeling in the text and graphics.

The nomenclature has been homogenized throughout the paper. Please see the diff file.

References used in the author answers:

Alduchov, O. A. and Eskridge, R. E.: Improved Magnus Form Approximation of Saturation Vapor Pressure, J. Appl. Meteorol., 35, 601 – 609, https://doi.org/10.1175/1520 0450(1996)035<0601:IMFAOS>2.0.CO;2, 1996.

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Immler, F., Treffeisen, R., Engelbart, D., Krüger, K., and Schrems, O.: Cirrus, contrails, and ice supersaturated regions in high pressure systems at northern mid latitudes, Atmos. Chem. Phys., 8, 1689–1699, https://doi.org/10.5194/acp-8-1689-2008, 2008

Rädel, G. and Shine, K. P.: Evaluation of the use of radiosonde humidity data to predict the occurrence of persistent contrails, Q. J. Roy. Meteor. Soc., 133, 1413–1423, https://doi.org/10.1002/qj.128, 2007

Spichtinger, P., Gierens, K., Leiterer, U., and Dier, H.: Ice supersaturation in the tropopause region over Lindenberg, Germany, Meteorologische Zeitschrift, 12, 143–157, https://doi.org/10.1127/0941-948/2003/0012-0143, 2003