We greatly appreciate the comments of the referee, which have helped us considerably improve our paper. Detailed responses to the referee's comments are given below.

1. **Referee B**: Page 10730, lines 12–14 (Abstract): "The net direction of transport is determined by the relative magnitudes of the upward advection of water vapor and the downward transport associated with microphysical processes." Please insert already here a short statement on the result of the study.

1. **Authors**: We have revised the abstract according to the referee's suggestion.

2. **Referee B**: Page 10730, line 21 ff: I miss the reference (and discussion) of Schiller et al. (2009).

2. **Authors**: Schiller et al. (2009) presented observations of water vapor at different locations in different seasons. They performed Lagrangian trajectory calculations assuming freeze-drying to the minimum saturation mixing ratio along the trajectories. They also discussed the role of convection in moistening the stratosphere.

   On the other hand, our work is built in a Eulerian framework, and concerns with wave forcing on clouds, cloud physics and mesoscale processes. We only consider in-situ formed clouds that are not directly related to convection.

   In the introduction we need to guide the readers towards the focus of our work. Hence we feel that it is appropriate not to reference Schiller et al. (2009) in this section because the themes of their work and ours are quite different.

3. **Referee B**: Page 10731, lines 18–21: "…horizontal transport over this region leads to freeze-drying of the air to a very low water vapor mixing ratio. This hypothesis helps to explain why the stratosphere is drier than the saturation water vapor mixing ratio indicated by the mean tropopause temperature." Do you mean that the stratosphere is drier since the saturation water vapor mixing ratio is higher in the stratosphere (due to the higher temperature) than in the TTL? Please explain.

3. **Authors**: Before air enters the stratosphere, it may be advected horizontally through regions colder than the horizontally averaged (mean) tropopause, and dehydrated to a saturation mixing ratio lower than that indicated by the mean tropopause temperature. In other words, the mass of water vapor transported into the stratosphere may be determined by the saturation mixing ratio at a temperature less than the mean tropopause temperature. This hypothesis offers an explanation for why the observed stratosphere contains less water vapor than the saturation mixing ratio at the mean tropopause temperature. We modified the text to make this clearer.

4. **Referee B**: Page 10735, lines 5–6: "Ice nucleation $\mathcal{I}$ is based on the formula for homogeneous freezing derived experimentally by Koop et al. (2000)." In the last years, it is found that the ice crystal numbers in the TTL are much lower than

expected from homogeneous ice nucleation theory (McFarquhar et al., 2000; Thomas et al., 2002; Lawson et al., 2008; Krämer et al., 2009; Jensen et al., 2010). See also comment 8.

4. **Authors**: The observations in McFarquhar et al. (2000); Thomas et al. (2002); Lawson et al. (2008); Jensen et al. (2010) are snapshots of individual clouds. On the other hand, Krämer et al. (2009) provided statistics of a number of clouds sampled during different flights. If the number of observed clouds in the data set is sufficient, the statistics would capture clouds at different stages in their lifetimes. Our response to the referee's comment is based on the data of the later paper.

   According to Krämer et al. (2009, their Sect. 3.5.2), at temperatures less than 205 K, ice number concentration (n_i) was observed in the range 0.005–0.2 cm^{-3}. High n_i (more than 0.1 cm^{-3}) occurred less frequently than low n_i (less than 0.1 cm^{-3}).

   In our simulation, the cloud lasts for 8 d, between $t = 1.7\,\mathrm{d}$ and $t = 9.7\,\mathrm{d}$ (Fig. 6). Based on the cloud lifetime, n_i in our simulation is in fact consistent with the frequency of occurrence given by Krämer et al. (2009, their Fig. 9, top panel):

   - The spatially averaged n_i between $t = 2.0\,\mathrm{d}$ and $t = 3.0\,\mathrm{d}$ (a period of 1.0 d) is between 0.1 cm^{-3} and 0.2 cm^{-3}. Hence $0.1\,\mathrm{cm}^{-3} < n_\mathrm{i} < 0.2\,\mathrm{cm}^{-3}$ in $\frac{1}{8} = 12.5\,\%$ of the time.
   - The spatially averaged n_i between $t = 3.0\,\mathrm{d}$ and $t = 5.0\,\mathrm{d}$ (a period of 2 d) is between 0.01 cm^{-3} and 0.1 cm^{-3}. Hence $0.01\,\mathrm{cm}^{-3} < n_\mathrm{i} < 0.1\,\mathrm{cm}^{-3}$ in $\frac{2}{8} = 25.0\,\%$ of the time.
   - The spatially averaged n_i between $t = 5.0\,\mathrm{d}$ and $t = 9.7\,\mathrm{d}$ (a period of about 5 d) is less than 0.01 cm^{-3}. Hence $n_\mathrm{i} < 0.01\,\mathrm{cm}^{-3}$ in $\frac{5}{8} = 62.5\,\%$ of the time.

5. **Referee B**: Page 10735, lines 7–11: "The deposition coefficient of water vapor on ice is assumed to be 0.01, which is larger than the experimental value suggested by Magee et al. (2006) but smaller than those suggested by cloud modelers (e.g. Kay and Wood (2008)). Sensitivity of our model results to the deposition coefficient will be discussed in a subsequent paper."

   The sensitivity of the ice crystal number on the deposition coefficient of water vapor on ice is quite large (see e.g. Gensch et al. (2008)). I think it would be better to show the sensitivity of the model results to this already here.

5. **Authors**: We also see large sensitivity of the ice number concentration and the cloud behavior on the deposition coefficient in our simulations. These simulations are not shown in this manuscript. The cloud behavior is also sensitive to other parameters, including the wave amplitude and period, the size and location of the moist patch, the relative humidity inside and outside the moist patch. We are currently organizing these simulations to write the follow-up paper.

In this manuscript, we focus on the mechanism in which the large-scale Kelvin wave forces TTL cirrus. This is the first time that a temporally and spatially varying, external forcing is treated in a dynamically consistent formulation in a model. Hence the formulation of the forcing requires detailed discussions. Adding the sensitivity tests to this first paper renders it too long and difficult to read. A follow-up paper allows more space to thoroughly discuss all relevant sensitivity parameters.

6. **Referee B**: Page 10738, line 9: "For this simulation, we set $w_0 = 1.8\,\mathrm{mm\,s^{-1}}, \ldots$" How did you derive this value? It seems very slow to me.

6. **Authors**: w_0 is chosen such that the amplitude of the temperature perturbations associated with the Kelvin wave matches those observed by Immler et al. (2008). Vertical velocities of this amplitude are typical of large-scale waves.

7. **Referee B**: Page 10739, lines 10–12: "The amplitude of the temperature perturbations near the tropopause is 2.0 to 2.5 K in this simulation. Temperature perturbations in Kelvin waves were observed by Immler et al. (2008) to be up to 8 K, but typically 2 to 3 K."

   What about small scale temperature perturbations caused by gravity waves? As far as I know they are believed to be a major source of cirrus clouds in the TTL, see e.g. Jensen et al. (2010)?

7. **Authors**: TTL cirrus may be forced by temperature perturbations from a variety of sources, including, but not limited to, gravity waves (Jensen et al., 2010), large-scale equatorial Kelvin waves (Immler et al., 2008), and midlatitude intrusion (Taylor et al., 2011). Large TTL cirrus, from several hundred to a few thousand kilometers wide, are most likely formed by temperature perturbations associated with large-scale waves.

   In the vertical plane along the equator, equatorial Kelvin waves have the same characteristics as gravity waves, though at different wavelengths and periods. In the follow-up paper, we will discuss simulations of TTL cirrus forced by waves at a few other wavelengths and periods, and/or by a mixture of waves.

8. **Referee B**: Page 10743, line 25–27: "During the formation of the cloud, ice nucleation followed by ice growth quickly reduces the supersaturation ratio S_i within the cloudy region from 0.6 (ice nucleation threshold) to close to zero (Fig. 5b)."

   The quick reduction of the supersaturation ratio S_i (Fig. 5b) is linked to the high ice crystal number (Fig. 6a). Typically, in the TTL at low temperatures and low ice crystal numbers high supersaturation can persist over quite a long time (resulting in a broad $\mathrm{RH_i}$ frequency distribution). In Krämer et al. (2009) the mean ice crystal number in this temperature range is $5\,\mathrm{L^{-1}}$, in your study the initial value is around $100\,\mathrm{L^{-1}}$. I am wondering how realistic the study is, especially when the gas phase water remaining in the presence of cirrus is critical. Also here (as in comment 4)

I miss sensitivity studies showing a range of possible scenarios, in particular those showing a lower ice crystal number and respective slow decrease of gas phase water.

8. **Authors**: As discussed in reply 4, because the period in which n_i is high (above $0.1\,\mathrm{cm}^{-3}$) is short compared with the entire cloud lifetime, the frequency distribution of n_i in our simulation is consistent with the statistics of Krämer et al. (2009).

   On average the simulated cloud is close to saturation. However, there are subsaturated and supersaturated cloudy areas, as shown by the minimum and maximum S_i in Fig. 5b. The frequency distribution of S_i inside the cloud in our simulation (shown below and in Fig. 8 of the revised manuscript) is reasonable compared with Krämer et al. (2009, Fig. 8) and MacKenzie et al. (2006, Fig. 7). We have added a short discussion regarding the frequency distribution of S_i to Sect. 4.2.

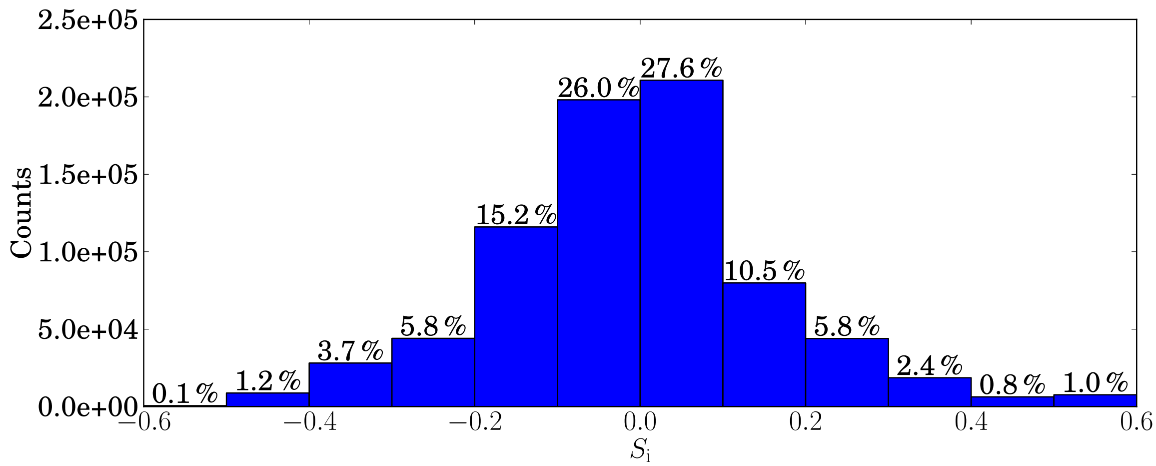

Figure 1: Frequency distribution of the supersaturation ratio (S_i) over the cloudy grid points and all model's output times.

9. **Referee B**: Page 10744, line 17: "4.3 Consistency with observations." I suggest to change the title to "4.3 Consistency with observations and other studies" since also model studies are discussed.

9. **Authors**: We changed the title to "Comparison with observations and other models" because our simulation is consistent with observations but different from other modeling studies.

10. **Referee B**: Page 10745, lines 18–19: "Observed IWC ranges from just $10\,\mathrm{mg\,m}^{-3}$ (Lawson et al., 2008) up to $10\,\mathrm{mg\,m}^{-3}$ (McFarquhar et al., 2000)." Please compare also with the IWCs from the climatology of Schiller et al. (2008).

10. **Authors**: Thank you for pointing out this paper to us. We have revised the

manuscript to compare the IWC in the simulation with the climatology of Schiller et al. (2008).

11. **Referee B**: Page 10745, line 23 ff: "The average diameter of ice crystals is between 7 µm and 10 µm throughout most of our simulation (Fig. 7b). This is consistent with ice crystals of approximately 10 µm in diameter in TTL cirrus observed by Voigt et al. (2007) over Brazil, and Peter et al. (2003) over the western Indian ocean. On the other hand, Lawson et al. (2008) reported larger ice crystals with an effective diameter of approximately 18 µm for a TTL cirrus over the tropical Eastern Pacific."

    In the study of Voigt et al. (2007), only measurements up to about 20 µm are performed (see Fig. 8) and it represents only one case. A much broader study of TTL and stratosphere cirrus is presented by De Reus et al. (2009). I suggest to show that instead. I also suggest to include the study of Krämer et al. (2009) in the discussion. They show frequency distributions of mean ice crystal sizes over the whole cirrus temperature range which agree well with your simulations for TTL temperatures.

11. **Authors**: In the study of Voigt et al. (2007), the instrument was able to detect ice crystals up to about 30 µm, but no crystal larger than 22 µm was observed. We have added a sentence to the caption of Fig. 8 in the original manuscript, which is now Fig. 9 in the revised version, to clarify this fact.

    As suggested by the referee, we have revised the manuscript to include the observations by De Reus et al. (2009) and Krämer et al. (2009).

    The size distribution of n_i in De Reus et al. (2009, their Fig. 2) has been normalized, and hence cannot be added into our figure. However, we have revised the manuscript to discuss their data in the text.

12. **Referee B**: Page 10746, lines 4–9: "However, the number of larger ice crystals in the simulated cloud is significantly smaller than observed." This could be due to the initial high number of ice crystals and rapid reduction of $\mathrm{RH_i}$, hindering the ice crystals to grow to larger sizes.

    "Nevertheless, because the observed number of crystals larger than 20 µm is small (note the logarithmic scale in Fig. 8a), they do not contribute significantly to the bulk properties, such as the average radiative heating rate, IWC and ice number concentration." The ice crystals larger than 20 µm can contribute significantly to the IWC, in fact in most cases the IWC is dominated by those crystals, even if their concentration is small.

    Please discuss both points in the paper.

12. **Authors**: As we argued in replies 4 and 8, n_i in the simulation decreases to small values matching the observations as the cloud ages. Also, there are regions in the cloud that are supersaturated. The frequency distribution of S_i in the simulated

cloud is reasonable. Therefore we do not think that the lack of large ice crystals is due to the initial high n_i and rapid reduction of S_i.

Large, spherical crystals do not exist in the simulation because they fall very fast. However, we suggested in the manuscript that it is possible to have large crystals if they are columnar or plate-like.

The sentence ("Nevertheless ...") pointed out by the referee is indeed incorrect and has been deleted from the revised manuscript. The mass of a spherical crystal is proportional to the cubic of the crystals' radius. Hence large spherical crystals may contribute significantly to the IWC even if their number concentration is small. However, the same does not hold for plate-like crystals, whose masses are proportional to the square of their widths, and columnar crystals, whose masses are linearly proportional to their lengths.

As discussed in Sect. 4.3 of the manuscript, when a few percent of the number of ice crystals in the simulation is replaced by plate-like and columnar crystals while *the total ice mass is conserved*, large ice crystals as observed in several case studies may be explained by the model. This indicates that these large, non-spherical crystals contribute only a few percent to the total ice mass.

13. **Referee B**: Page 10747, lines 24-28: "Another essential difference between Jensen et al. (2011)'s simulations and ours is the number of ice crystals nucleated when the cloud is formed. Jensen et al. (2011) tuned their heterogeneous nucleation scheme so that the number of ice crystals nucleated is $60\,\mathrm{L}^{-1}$, following measurements by Lawson et al. (2008). On the other hand, homogeneous nucleation in our model produces an average number of $200\,\mathrm{L}^{-1}$ up to a maximum of $5000\,\mathrm{L}^{-1}$ (Fig. 6a)." Then I would say that the study of Jensen et al. (2011) is more realistic! Please discuss that.

13. **Authors**: In Lawson et al. (2008), the observed large sizes of ice crystals (mean diameter of 18 µm, some were larger than 100 µm) suggest that the observations were not carried out during ice nucleation. Hence tuning the nucleation scheme to match the observed ice number concentration may not be appropriate.

14. **Referee B**: Page 10748, lines 1–4: "In our simulation, more ice crystals are nucleated, hence sedimentation of smaller ice crystals occurs at a slower rate, which allows sufficient time for the radiatively induced dynamics to develop." Same comment as for the last point.

14. **Authors**: In our simulation, n_i is high initially, but it decreases by a few orders of magnitude during the cloud lifetime. In fact, n_i matches observations as the cloud matures. Because the time (with respect to the cloud lifetime) at which the observations were carried out is not known, we do not consider the simulation unrealistic because it does not match the observations during cloud formation.

15. **Referee B**: Page 10748, lines 7–13: "In our simulation, although many ice crystals (on the order of $10^2\,\mathrm{L}^{-1}$ to $10^4\,\mathrm{L}^{-1}$) nucleate homogeneously, in less than a day the number is reduced to values comparable to or smaller than $100\,\mathrm{L}^{-1}$ (Fig. 6a)." The initial number of ice crystals strongly affects the gas phase water vapor evolution, particularly in TTL cirrus (see also comments 4 and 8). Thus, I do not see that the argument that the number of ice crystals reduces after one day enhances the reliability of the study.

   "Hence we argue that homogeneous freezing cannot be ruled out as a viable nucleation mechanism in the TTL." In case homogeneous freezing would produce a high number of ice crystals during cirrus formation, which would reduce after about a day to the observed numbers, than these high numbers would also appear in the observations. So I also do not see the argument for homogeneous freezing.

15. **Authors**: High n_i ($100\,\mathrm{L}^{-1}$ and above) was in fact observed (Krämer et al., 2009, Fig. 9, top panel), though less frequently than low n_i.

   Homogeneous freezing can be ruled out only if low n_i is observed during ice nucleation. So far as we know, no observational study has claimed that their measurements of ice crystals in the TTL were taken during ice nucleation.

16. **Referee B**: Page 10752, lines 9–11: "The ... microphysical properties (... ice number concentration) of the simulated cloud agree reasonably well with observations." I have a problem with this statement, to my opinion the ice number concentrations are too high. That leads me to the question: if the cloud microphysical properties of the simulated cloud is not right, how solid are the results? I suggest to perform sensitivity studies (as I stated before) to estimate the magnitude of the effect of cloud microphysics to the stated results: Page 10753, lines 12–16: "Under the conditions specific to our simulations, the radiatively induced upward transport of water vapor dominates over the downward transport by microphysical processes. The net result is upward transport of water vapor, which is equivalent to hydration of the lower stratosphere."

16. **Authors**: We agree that it is very important to discuss the sensitivity of the model results and will do so in the follow-up paper. To clarify that this result applies only to the specific case presented here, we changed the title of the manuscript to "Cirrus and water vapor transport in the tropical tropopause layer. Part I: a specific case modeling study". The title of the follow-up paper will be: "Cirrus and water vapor transport in the tropical tropopause layer. Part II: sensitivity of model results".

17. **Referee B**: Page 10753, lines 21–22: "The sensitivity of model results to the relative humidity of the surrounding air will be discussed in a subsequent paper." Following my previous comment, I also suggest to include this sensitivity study into the paper.

17. **Authors**: In fact, several other simulations that we have performed using different initial parameters and model conditions (not shown here) show different cloud behaviors. However, we argue that the current simulation is consistent with observations and hence can stand on its own. Considering the importance of the sensitivity study, the number of parameters that require testing, and the length of the current manuscript, we think that the sensitivity study should be separated into another paper.

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