Response to Referee #1

Major comments and questions

1. When INP concentration are increased to match the observed ice crystal concentration and the consequences for LWC is demonstrated please add what happens to the ice water content (IWC) in the model. If it is possible please compare with observations of IWC.

   Figure Ac (attached) indicates what happens to the IWC for the Control and Increased IN runs (IN-1 and IN-3) whilst Figure Bc indicates what happens for the Control and Surface Flux runs (with the formulation of the flux differing from the original paper following comments by the Anonymous Referee #2). None of our probes directly measure the IWC, but for comparison we have calculated IWC based on the binned size distribution of ice from the 2D-S.

   The IWC in Figure Ac suggests that increasing the number of INP by 6 orders of magnitude fails to increase the IWC to match the IWC inferred from the 2D-S measurements. Even though the ice crystals have reduced the LWC (Figure Ab) to below the measured LWC at Jungfraujoch, the modelled crystals are smaller in size than the crystals measured by the 2D-S, as the IWC is significantly smaller with similar number of ice crystals present.

   In Figure B, the IWC (Figure Bc) suggests that the inclusion of the surface flux increases the IWC when compared with the control simulation, but does not match, exactly, the IWC inferred from the 2D-S. There are a number of explanations as to why the modelled and measured IWC don't exactly match. However, a likely explanation is that the growth of the surface hoar crystals by vapour deposition in the model is not significant enough to increase the IWC to the measured IWC at Jungfraujoch. As the number of ice crystals agrees well between the model and measurements, the difference in IWC may be due to the small assumed size of the surface hoar crystals (10 µm). Smaller ice crystals contribute less to the IWC than larger particles, which suggests an increase in the size of surface hoar crystals in the model would be required to match the 2D-S inferred IWC.

2. The comparison with the calculated required updrafts velocities that enable MPCs is not done consistently. On one hand the measured updrafts are compared with the required updrafts calculated from observation and conclusions of the existence of MPC are made. On the other hand modelled updrafts are compared with the calculated updrafts base on modelled data for both simulations. But a comparison between the modelled and measured updrafts is missing. It should be done using the same linear axis including downdrafts.

   The reason for the two different plots here was really clarity, to show that the Korolev and Mazin threshold for the control run was significantly lower than the simulated updraft in the model, which isn't as clear in linear scale (as shown by Figure C). However, for sake of comparison of the wind speeds, we will include both in the same plot as required. The model updraft speed is quite different from the measured updraft speed, which doesn't seem to indicate the same downdrafts as in the model. However, the model doesn't accurately represent the steep orography as accurately as reality, and the steep gradients in the terrain cause higher updraft speeds than represented in the model. The difference has been discussed as a limitation of the Korolev and Mazin analysis.

3. As already mentioned by the authors the implementation of surface flux is kept very simple so far. Please discuss the consequences of these simplifications.

Several changes have been made to the flux in response to the comments made by the second referee. These include an additional minimum wind speed, and the conditions at which the flux is activated. The consequences of the simplification are discussed more below (in response to general comment 1a, 1c and 1d), and in the associated changes.

4. Explain why LWC is not completely depleted when increasing the ice crystal number concentration locally by surface flux processes but depleting when increasing by higher INP concentrations. Would the calculated updrafts as done for the study with increased INP allow MPC in the simulation with surface fluxes?

   Increasing the numbers of INP increases the numbers of ice crystals at all locations in the model, including the regions above the surface. These ice crystals then reduce the liquid water content by the Bergeron-Findeisen process. In the surface ice crystal source case (Figure B) the high ice crystal concentrations are confined close to the surface and thus liquid water is able to mix down from higher levels, or advect across to the summit, before the Bergeron-Findeisen process has had time to act.

   The updraft threshold calculated for Surf-3 is generally below the model wind speed when an updraft is present in the model, suggesting that even with the inclusion of the surface flux mixed phase cloud can exist in the simulation at these times (see Figure D). However, with the inaccuracies in the vertical wind velocity between the model and reality, the inclusion of this calculation doesn't seem to add any weight to the argument in the paper.

5. Is the WRF model respective the cloud parametrization able to distinguish different freezing mechanism? If so is deposition nucleation the dominating freezing process in MPCs?

   This is an interesting point. The Morrison scheme is able to distinguish freezing mechanism, and parameterisations for contact and immersion freezing are also included in the model. However, only the deposition and condensation freezing are increased in the increased INP runs, which needs to be outlined more clearly in the text, and the limitations that the mechanism has on this research. However, whichever parameterisation is increased to include additional INP, the LWC will still be reduced as the number of ice crystals increases. Hence to match the number of ice crystals witnessed at Jungfraujoch, the LWC will likely still be reduced in the model to below the LWC witnessed at Jungfraujoch.

Editorial comments

1. Timeseries: axis, axis title and titles are hardly readable (too small)

   This relates to a formatting issue that I didn't spot previously. Each of the plots were landscape A4 in the manuscript, and have been shrunk to fit the ACPD format. I will ensure these are correct on submission.

2. Do not use different axis for two comparable plots (figure 6)

   Corrected with just 1 plot.

3. Label bar description (units) in Fig 12 can't be found easily. Placed somewhere in between the other plots.

   Label units moved into a clearer place on Figure 12.

4. Better use INP instead of IN as suggested in http://www.atmos-chem-phys.net/15/10263/2015/acp-15-10263-2015.pdf

   This will be changed throughout.

5. Typing error: pp 25658 line 26: Bergeron-Findeisen (i is missing)

   Correction made in text.

<u>Anonymous Referee #2</u>

<u>General comments</u>

1) In their paper, the authors follow the assumptions of Lloyd et al (2015) and test if a flux of ice crystals emitted from the surface can explain the high ice number concentration observed at Jungfraujoch. Their results show that emitting such flux increases modelled ice number concentration and allow getting simulated number concentration closer to observed values. The source of this surface ice crystal flux is surface hoar crystals present at the snow surface.
   a) The authors mention the conditions required for surface hoar formation in the text (P25665 l. 15-25) but they do not really check if these conditions are present in the simulations. Before assuming that a flux of surface hoar crystals can be emitted from the surface, I highly recommend them to show that realistic conditions required for surface hoar formation are present around Jungfraujoch in the simulation. Surface hoar forms at the snow surface due to deposition of water vapour from the air onto the snow surface (Colbeck, 1988, Stoessel et al. 2010). Therefore, during growth conditions a water vapour flux toward the snow surface is required. This is for example the case when humid air is present above a snow surface on clear winter night when radiative cooling lowers the surface temperature of the snow (Stoessel et al. 2010). Horton et al. (2014) showed that factors affecting surface hoar growth and shrinkage were captured by modelling the latent heat flux. The authors could study the latent heat flux between the snow surface and the atmosphere in the WRF control simulation and provide an estimation of the occurrence of favourable conditions for surface hoar formation during the study period. How does the occurrence of favourable conditions compare with the conditions used by the authors for emitting the particle flux (air temperature is below 0C and supersaturated with respect to ice; P 25666 l25 to P 25667 l 2)? The conditions they use may generate the emission of ice crystals towards the atmosphere even when conditions are not favourable for the presence of surface hoar at the snow surface. In the current version of the paper, it is hard to believe that the assumption of a flux of surface hoar is realistic.

      To more realistically include a flux of surface hoar into out model, we have used the latent heat flux between the atmosphere and the surface modelled by the NOAH land surface model to assess when surface hoar can occur during our model simulations. As described by Horton et al. (2014), when there is a positive upwards flux of latent heat the deposition of vapour occurs and contributes to the growth of surface hoar.

   b) The authors use an adapted version of the aerosol flux from frost flowers (Xu et al. 2013). The authors should keep in mind that the fact that frost flowers can served as a source of aerosols is not widely accepted. In laboratory experiments, Roscoe et al. (2010) showed that no aerosol could be observed from frost flowers, despite winds in gusts up to 12 m/s. They concluded that frost flowers are unlikely to be the major direct source of sea salt aerosol. This limitation should be mentioned in the paper. Note that this point does not concern surface hoar at the snow surface and the fact that surface crystals can be removed from the snow surface by wind.

This will be noted in the text.

c) The flux in Eq. 5 depends only on wind speed and gives a positive flux even if the wind speed is equal to zero. This formulation is not realistic for the emission of any crystals from the snow surface (blowing snow or surface hoar). As mention by the authors (P 25666 l. 13-15), surface hoar is removed from the snow surface when wind blows the crystals in the atmosphere. The physical processes involved are similar to the ones observed when snow at the top of the snowpack is transported by the wind with a transport in saltation and turbulent suspension (e. g. Pomeroy and Gray, 1995). Therefore, similar to the initiation of snow transport by the wind (Schmidt, 1980; Guoymarc'h and Mérindol, 1998; Clifton et al, 2006), a threshold wind speed is required for the transport of surface hoar by the wind. The authors should at least introduce a threshold wind speed in their adaptation of the aerosol flux from Xu et al (2013). For example, a value of 4 m s?1 at 5 m above the ground typical for fresh fallen snow could be used. The authors should also better justify (P 25666 l 15-20) why they use a formulation different from the typical formulations used to represent the emission of blowing snow particles in the atmosphere (Gallée et al, 2001; Lehning et al, 2008; Vionnet et al. 2014).

A Wind speed threshold of 4 ms^{-1} has been included at suggestion of Anonymous Referee #2., as clearly if the optimal wind speeds for the development of surface hoar are speeds of 1-2 ms^{-1} (Hachihubo and Akitaya 1997), a greater wind speed would be required to transport the crystals from the surface into the atmosphere. With the adjustments made, the new fluxes do not significantly change from the fluxes presented in the discussion paper, and are plotted in Figure B.

The use of the frost flower flux indicates a simple representation dependant only on wind speed, and provides an exponential relationship with wind speed. The formulation of the blowing snow particles in, for example, Vionnet et al. (2014) suggests a much more complicated representation of the snow layer, with suspension and saltation layers.

The reasons for the more simplistic calculation are as follows

a) As snow particles being blown in the saltation layer are being carried along the surface, these are unlikely to be blown into the atmosphere and affect the ice concentration at Jungfraujoch, which is measured several metres above the surface. Hence only the suspension layer needs to be considered.

b) The double-moment equations 2 and 3 from Vionnet et al. (2014) describe terms for the number of particles of snow and the mass mixing ratio of the blowing snow. In their equation 2 (see below), the flux of particles of blowing snow is dependent on terms for advection, turbulence, sedimentation and sublimation.

$$\frac{\partial N_S}{\partial x} + \underbrace{u_j \frac{\partial N_S}{\partial x_j}}_{\text{Advection}} = \underbrace{-\frac{\partial}{\partial x_j}\left(\overline{N_S' u_j'}\right)}_{\text{Turbulence}} + \underbrace{\frac{\partial}{\partial x_j}\left(N_S V_N \delta_{j3}\right)}_{\text{Sedimentation}} + \underbrace{S_N}_{\text{Sublimation}}$$

Within the Morrison scheme, both sedimentation of ice and the sublimation of ice are represented, so terms for these sinks of surface hoar are not required in our flux. Hence the modelling of our flux only needs to include the advection and turbulence terms of the Vionnet et al. (2014) equation for blowing snow.

c) Comparing Figures 8a and 8b in Vionnet et al. (2014) suggests the relationship between the blowing snow mass flux and the wind speed is exponential, similar to the flux expressed by Xu et al (2013). Whilst the magnitude is clearly different for these fluxes, this is dependent on the availability of surface crystals.

Hence whilst perhaps the derivation of the blowing snow flux in Vionnet et al. (2014) is more complicated than the flux used in this article, the surface ice crystal flux here includes sedimentation and sublimation effects and has similar exponential dependence on the wind speed as the more complicated Vionnet et al. (2014) blowing snow flux does. However, the flux does not include the same turbulent effects that the blowing snow does, so this may be a limitation of the flux that should be examined in any future publication.

d) Two additional comments regarding the formulation of the surface crystal flux are:

- In Equation (5), at which height above the ground is considered the horizontal wind speed? Is it the same value as in Xu et al (2013)? It should be at least mentioned in the paper and if the values are different the authors should discussed the impact.

  The horizontal wind speed used in equation 5 is taken from the surface of the model. Whilst not explicitly outlined Xu et al. (2013), the implication is that the wind is also measured at the surface. This will be included.

- The authors assume a size of 10 microns for emitted surface hoar crystal. This value is small for ice crystal emitted from the surface and will have a large impact on the sedimentation of the particles. During blowing snow events, the diameter of blown snow particles ranges typically between 40 and 200 microns in the first meter above the snowpack and follows a two-parameter gamma distribution (Nishimura et al, 2005, Gordon et al 2009, Naaim Bouvet et al, 2010). The authors should discuss this assumption and the expected impact on the number of ice crystals in the atmosphere.

  The inclusion of small ice crystals is essentially to give the surface hoar that is blown in the atmosphere an initial mass to assess whether a surface ice flux would affect the ice concentration. Increasing the size would lead to faster sedimentation of the ice crystals, which might complicate the analysis. The actual mass of the ice crystals emitted is unknown. In absence of any knowledge of the size distribution of the ice crystals, we chose 10 microns as it was small enough to allow us to assess whether the process has the potential to explain our observations. We are planning to undertake a future experiment to measure the flux and size of surface emitted crystals, but until then we decided on 10 microns.

  However, after performing these calculations we realise the choice of small ice concentrations may explain why the model IWC is lower than the IWC inferred from the 2D-S (See Figure B). Investigating the sensitivity to initial ice particle size is a topic worthy of study in the future.

2) Based on the analysis of Lloyd et al (2015) the authors consider that blowing snow cannot explain the ice number concentration at Jungfraujoch and that a second source of ice crystals from the surface must be considered. This is based on the lack of a relationship between the number of ice particles and the wind speed found by Lloyd et al (2015) in the observations at Jungfraujoch. It would be very interesting if the authors could carry out a similar study using the simulated values. How does simulated ice number concentration in simulations Surf-6 and Surf-3 compare with simulated wind speed? As done by Lloyd et al (2015), the authors could pick up events identified as blowing snow event and non blowing snow event.

   Having done this comparison, the ice number concentration is more heavily dependent of wind speed than Lloyd et al. (2015). In Figure Ea, a comparison of wind speed to ice concentration is shown. At horizontal wind speeds greater than 4 ms-1, there is a strong correlation between the ice concentration and the wind speed, suggesting the strong influence of the flux above the minimum horizontal wind speed at which the flux is active. When compared with the findings of Lloyd et al. (2015) the ice crystal concentration in the model is more dependent on wind than the observed ice crystal concentrations.

   This suggests that while the ice concentrations provided by the flux may be accurate in comparison to the 2D-S, the flux is more dependent on wind speed in its calculation, and hence if the flux does contribute to the high ice concentrations at Jungfraujoch it is not solely dependent on wind speed.

   Hence the existence of the flux could be in occurrence, but is more complicated that the flux representation in the model, with less dependence on wind than the present flux.

   In reference to comparing the specific blowing snow and non-blowing snow events, the short cloud periods that the Lloyd et al. (2015) data covers (at most 2-3 hours) makes the model and 2D-S data difficult to compare over these periods.
   The different temporal resolutions of the model (hourly outputs) and the 2D-S data used in Lloyd et al. (2015) (10 second data), mean that the model has approximately 3-4 outputs during each cloud period, and it is difficult to compare the ice concentrations and wind with such little data. Hence the wind and ice concentrations are compared over the entire campaign in the model, and not splitting into blowing snow cases and non-blowing snow cases.

Specific comments

1) P 25654 l. 27 P 25655 l. 3: a map of the simulation domain showing the topography would help the reader to better figure out how looks the topography in the region. On this map, the authors could also mention the location of the Jungfraujoch station and the 3 other AWS stations used for model validation.

   I have included a map the topography and the location of Jungfraujoch and the Meteo Swiss stations (Figure F).

2) P 25656 l 24: the model validation is based on a comparison between simulations and observations at four stations (including Jungfraujoch). The validation is purely based on a visual comparison between observed and simulated time series over the period of interest. The authors should include a more quantitative evaluation and compute error statistics such as Bias and Root Mean Square Error for each meteorological variable, and each station. They could use a table to summarize the results.

The Bias and Root Mean Square Error has been calculated and summarised in a table in the text. The statistics have also been referred to in the text to support the validation of the model.

3) P25657 l 18-21: at which level is taken the simulated ice number concentration: at first atmospheric level or at the real altitude of Jungfraujoch?

   The simulated ice concentration is taken at the first atmospheric level.

4) In Section 4, the authors compare observed ice number concentrations with modelled values from different simulations. Time series are shown on Fig. 5, 7, 10 and 11. Based on a visual comparison, the authors discuss if a given process can explain the high ice number concentration observed in orographic clouds at Jungfraujoch. It would be interesting for the reader to have complementary figures showing for example scatter plots of observed and simulated ice concentrations.

   These have been done for the control and IN-3 simulations from Figure 5 and the SF-3 simulation from Figure 11, and will be added to the supplementary material (See Figures G-I). Figures 7 and 10 do not show a significant change from the control run, these are probably not as important.

Technical comments

Text

- P 25656: l. 19-23: The description of the stations used for model validation should be part of the Methodology section.

  This will be changed so section 3 is now section 2.4.

- P 25656: l. 20-21: please mention at which height above the ground are measured wind, air temperature and humidity.

  Measurement is taken at the first atmospheric level, which will be mentioned in the text.

- P 25666: l. 23: mention the units of phi.

  The units of phi, Number per square metre per second, will be included in the text.

Figures

- Fig. 12 and 13: add the prevailing wind direction on the maps or in the caption

  Added the prevailing wind to the figure 12 caption, which is referenced in the figure 13 caption.

Response to Short Comment

There are no specific issues here that need to be answered, apart from clarifying the speculative nature of the inclusion of the surface flux, and the need for upwind field observations of the flux to determine whether the flux is occurring and influencing the ice concentrations.

References

Hachikubo, A. and Akitaya, E. (1997). Effect of wind on surface hoar growth on snow. J. Geophys. Res., 102, 4367-4373

Horton, S., Bellaire, S., Jamieson, B. (2014). Modelling the formation of surface hoar layers and tracking post-burial changes for avalanche forecasting. Cold Regions Science and Technology, 97, 81-89.

Lloyd, G., Choularton, T. W., Bower, K. N., Gallagher, M. W., Connolly, P. J., Flynn, M., Farrington, R., Crosier, J., Schlenczek, O., Fugal, J., and Henneberger, J. (2015): The origins of ice crystals measured in mixed phase clouds at High-Alpine site Jungfraujoch, Atmos. Chem. Phys. Discuss., 15, 18181-18224.

Vionnet, V., Martin, E., Masson, V., Guyomarc'h, G., Naaim-Bouvet, F., Prokop, A. Lac, C. (2014). Simulation of wind-induced snow transport and sublimation in alpine terrain using a fully coupled snowpack/atmosphere model. The Cryosphere, 8, 395-415.

Xu, L., Russell, L. M., Somerville, R. C., Quinn, P. K. (2013). Frost flower aerosol effects on Arctic wintertime longwave cloud radiative forcing. Journal of Geophysical Research: Atmospheres, 118(23), 13-282.

Figures

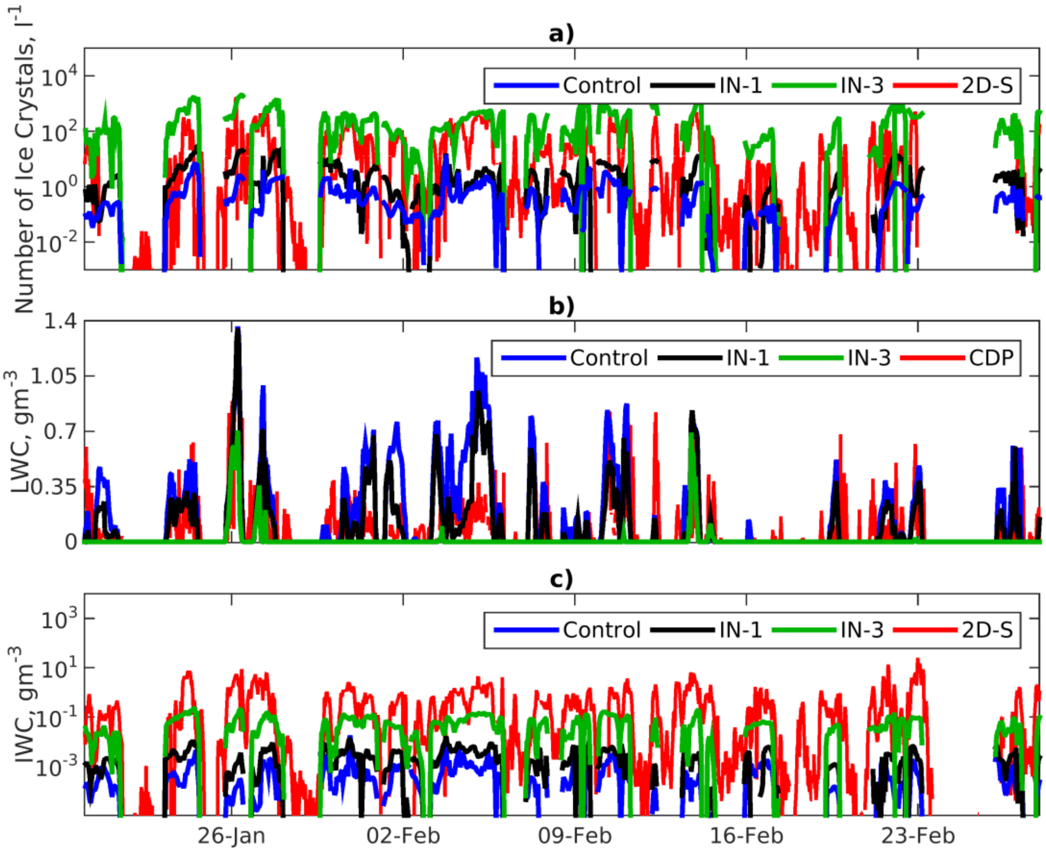

Figure A: a) Comparison of 2D-S ice number concentration measured at Jungfraujoch during the INUPIAQ campaign with the ice number concentration from the Control, IN-1 and IN-3 WRF model simulations. b) Comparison of the CDP LWC measured at Jungfraujoch during the INUPIAQ campaign with the LWC from the Control, IN-1 and IN-3 WRF model simulations. c) Comparison of IWC inferred from 2D-S measurements at Jungfraujoch during the INUPIAQ campaign with the IWC from the Control, IN-1 and IN-3 WRF model simulations.

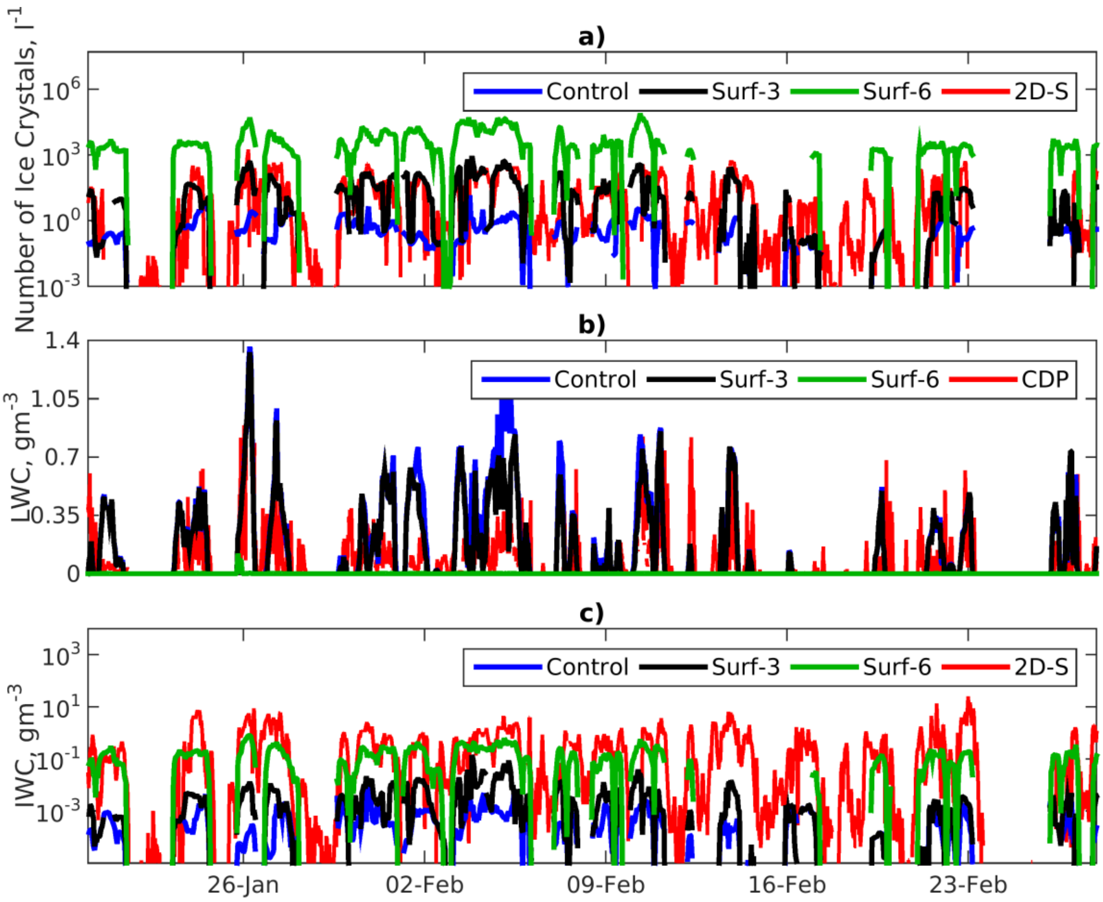

Figure B: a) Comparison of measured 2D-S ice number concentration at Jungfraujoch during the INUPIAQ campaign with the concentration from the control WRF model simulation, and the Surf-3 and Surf-6 simulations which included the addition of crystals from the surface. b) Comparison of measured LWC at Jungfraujoch during the INUPIAQ Campaign with the LWC from the control WRF model simulation, and the Surf-3 and Surf-6 simulations, which included the addition of crystals from a surface flux. c) Comparison of IWC inferred from 2D-S measurements at Jungfraujoch during the INUPIAQ campaign with the IWC from the Control, Surf-3 and Surf-6 WRF model simulations.

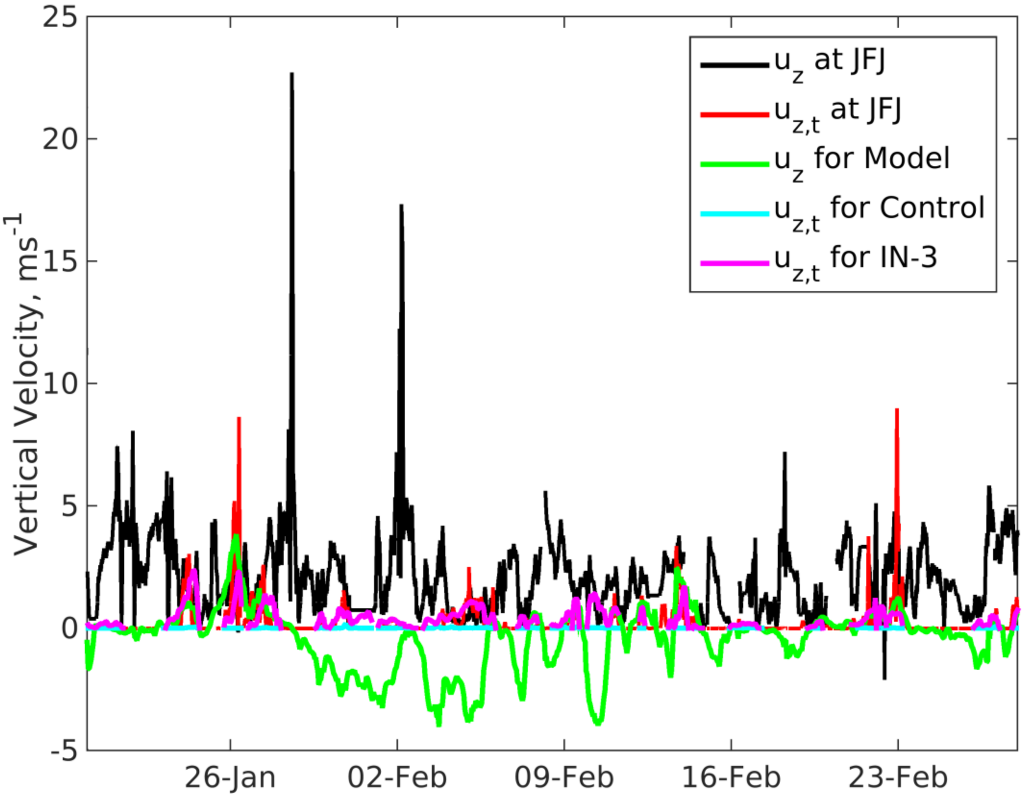

Figure C: Analysis of vertical velocity with the updraft threshold required for the presence of mixed-phase cloud for both measurements at Jungfraujoch and the Control and IN-3 simulations. The updraft threshold is calculated following Korolev and Mazin (2003).

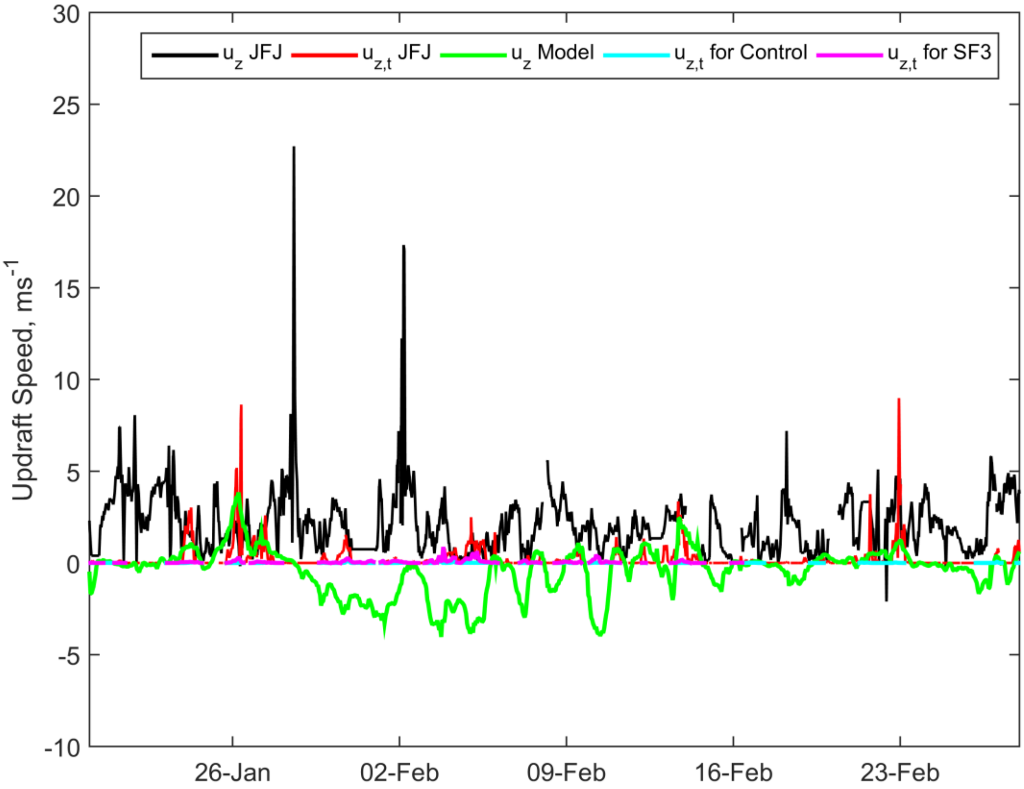

Figure D: Analysis of vertical velocity with the updraft threshold required for the presence of mixed-phase cloud for both measurements at Jungfraujoch and the Control and Surf-3 (SF3) simulations

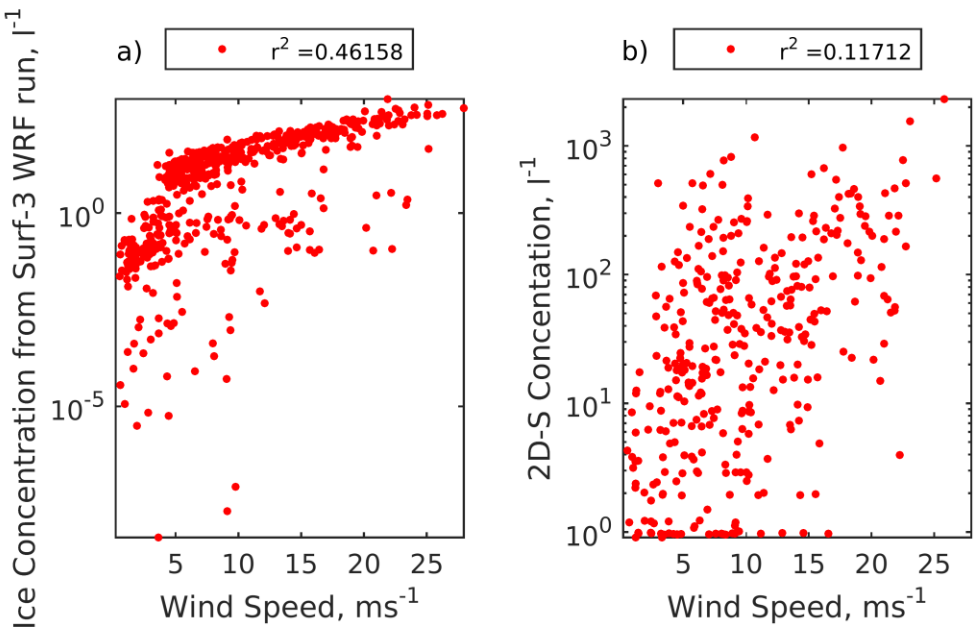

Figure E: A comparison of the wind simulated in the Surf-3 simulation with a), the ice crystal concentration simulated in the Surf-3 simulation at Jungfraujoch, and b), the 2D-S ice crystal concentration at Jungfraujoch.

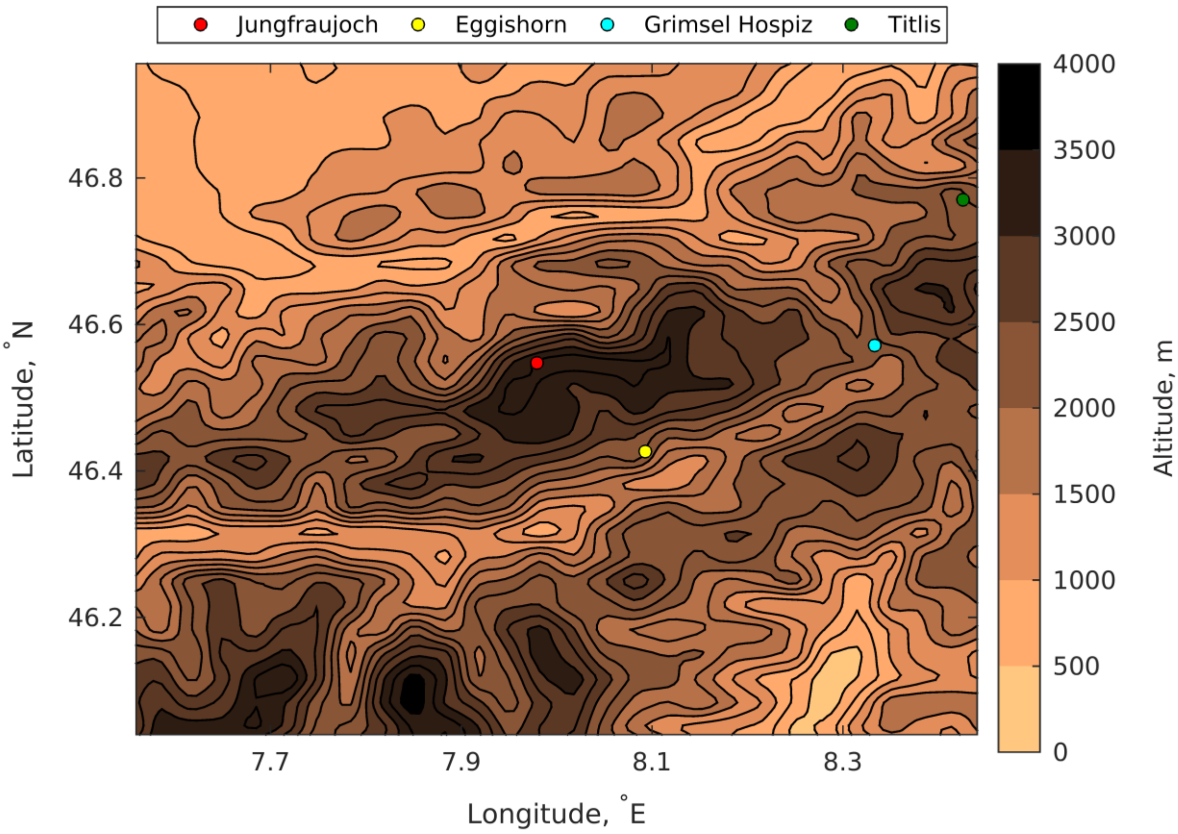

Figure F: Location of MeteoSwiss Observation Stations relative to Jungfraujoch.

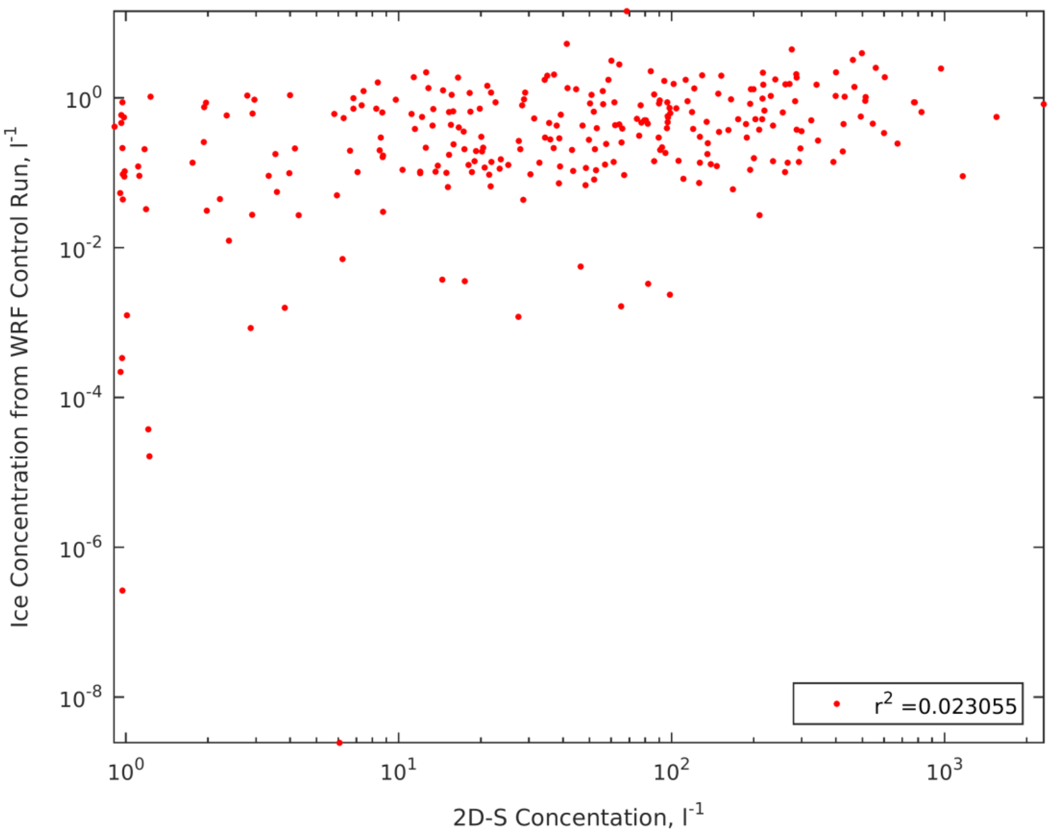

Figure G: A comparison of the 2D-S concentration measured at Jungfraujoch with the ice concentrations at Jungfraujoch from the WRF Control simulation.

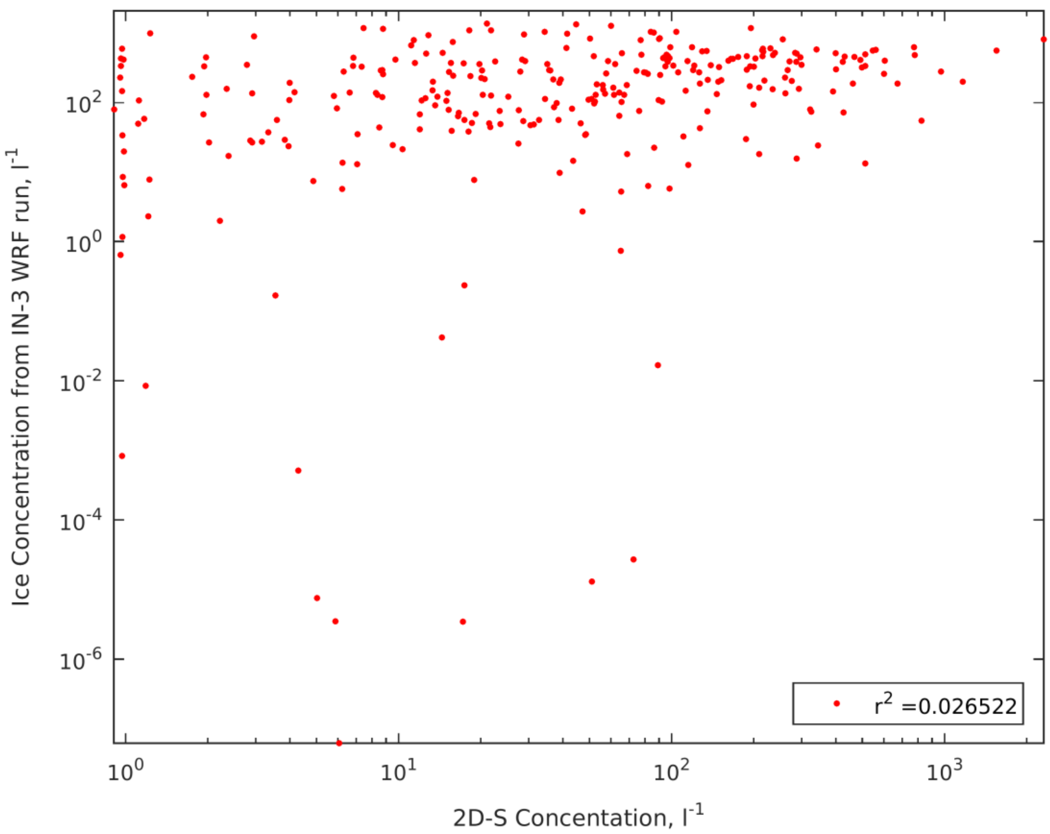

Figure H: A comparison of the 2D-S concentration measured at Jungfraujoch with the ice concentrations at Jungfraujoch from the WRF IN-3 simulation.

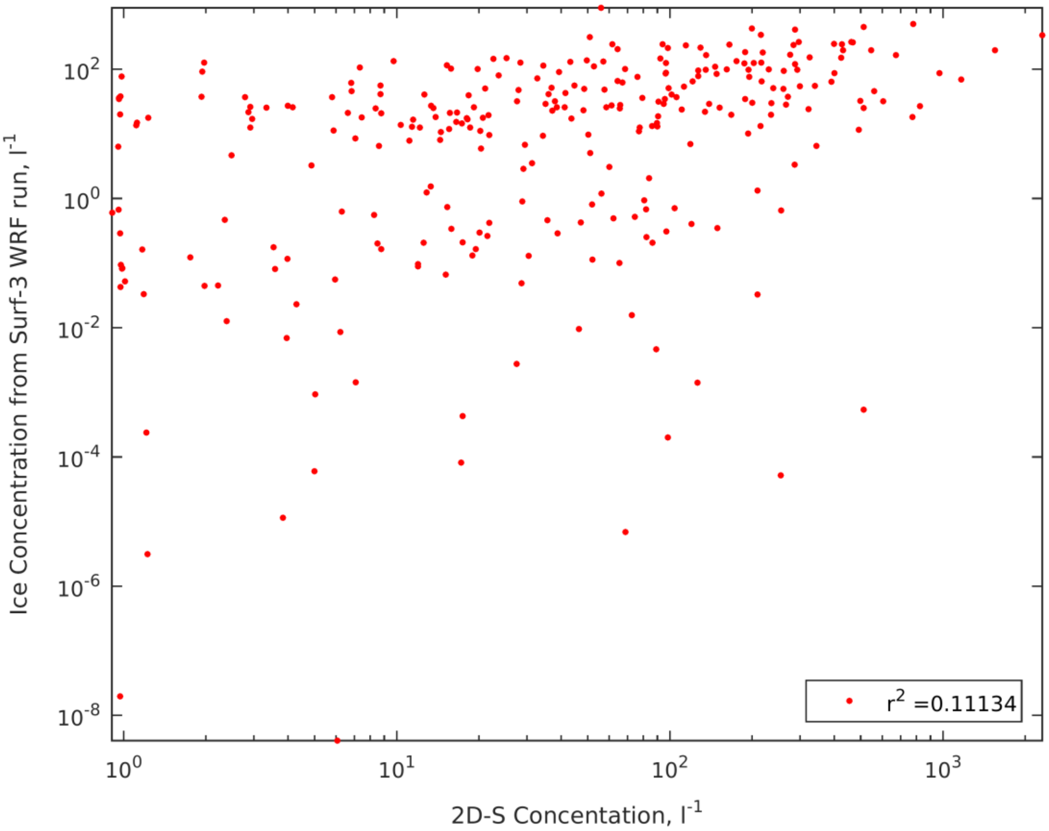

Figure I: A comparison of the 2D-S concentration measured at Jungfraujoch with the ice concentrations at Jungfraujoch from the WRF Surf-3 simulation.