Author responses on behalf of all co-authors of "Balloon-borne match measurements of mid-latitude cirrus clouds" by A. Cirisan et al.

We thank both anonymous reviewers for their careful reading of the manuscript and for their suggestions and critical questions that helped us improving our paper. In the following, the referee comments are given in italicized, the author responses in roman fonts. Page and line numbers refer to the original manuscript.

Anonymous Referee #2

General Comments:

1. The manuscript is a very comprehensive presentation of the current understanding of cirrus microphysics in the context of the specific "match" observations that were made. At times I was somewhat overwhelmed by the depth of detail presented, and ultimately became bogged down by the persistent intensity and sheer length of the manuscript. I would like to see the authors sometimes employ simpler (but still accurate) descriptions, reducing the level of detail and intensity, and therefore making the paper comprehendible by non-experts. This would not necessarily require a "watering down" of the scientific findings, only an adjustment from the current presentation style of unceasing technical details to a more fluid, readable prose. I would also encourage the authors to allow a native English speaker to refine the grammar and word usage. I've provided a few examples of this below.

We admit that it would be desirable to "employ simpler (but still accurate) descriptions". The most straight forward way to achieve this is to remove large parts of the technical descriptions of the section on the mass accommodation coefficient and of the section on the heterogeneous nucleation, and to have this material in appendices. This is what we did, and we think that the paper gains readability. Thanks.

2. The measurement artifact of the Snow White profile over Zurich is alluded to severaltimes throughout the manuscript but a full description and its implications for the matchstudy are not fully addressed until the very last paragraph before the conclusions section.This discussion should be moved forward to completely dismiss the large andpersistent supersaturations implied by the Snow White sounding over Zurich. Thiswill allow the reader to focus solely on the model-measurement differences in aerosolbackscatter that comprise the primary emphasis of the paper.

We would not agree that the primary emphasis of the paper is the discussion of the model-measurement comparison in backscatter, as the discussion of RH is just as important. Here we would argue that we refer to the SnowWhite artefact already in the abstract ("by a measurement error, such as a contamination of the sensor housing of the SnowWhite hygrometer"), then again in the observations Subsection 4.2. To emphasize the likelihood of a measurement artefact early on, we clarify the statement in Subsection 4.2 by writing: "as we will see below, the accuracy in one of the measurements has most likely been severely compromised leading to a systematic error of likely rather 20-30 %. Indeed, in Subsection 4.5.6 we will largely dismiss the large and persistent supersaturations implied by the SnowWhite sounding over Zurich. This does not affect the discussion of model-measurement differences in aerosol backscatter that comprise a major focus of the paper." Furthermore, we introduce a new subsection title for the treatment of the potential artefact: "4.5.6 Potential error in SnowWhite measurement".

3. The word "phase" is used in several contexts throughout the manuscript and sometimes the meaning is not exactly clear. Can a different word be used, especially in the context of timing? Examples: Page 14 Line 18 (P14 L18) "mixed-phase cloud"; P16 L22-23 "misjudges the phase of the warming"; P17 L21 "represent a case with poor phase matching"; P19 L17 "a pronounced warming phase"; P22 L12 "cloudy phase";P24 L9 "apply fluctuations in phase";

We agree with the referee and have cleaned up our usage of the word "phase". We continue to use it in the context of "physical phase" and phase of the small scale temperature fluctuation, but replace it by other words in the other contexts (time, timing, onset, part, process).

4. There should be some mention of the uncertainties of the COBALD sonde measurements because they are quantitatively compared to model results. Is a 10% difference between the modeled and measured aerosol backscatter ratios within the uncertainty of the COBALD measurements?

Exactly this information is provided in Section 4.2 Observations on P14 L18-19 right at the place where it should be: "The error analysis for COBALD limits its uncertainty to ±8–10% of backscatter ratio at 870 nm." For clarity we have added to point 5 in the conclusions that the 10% difference between the modeled and measured aerosol backscatter ratios is indeed within the uncertainty of the COBALD measurements.

5. Figures 5, 7, and 9-12 should be expanded to fill entire page widths so they are more easily viewed and comprehended. Figure 5 is especially difficult to understand because there are so many different traces in each panel and the panels are so narrow. Why show the HC2Humicap profiles here if they are not used in the paper? Might the HC2 profile over Zurich give even the slightest indication that the Snow White profile is anomalously wet?

Concerning the size of the figures we agree and will write to the ACP production office and make exactly this suggestion. Concerning the Rotronic HC2 we would argue, that at present there is so much debate in the community about the reliability of various humidity sensors that we would like to keep this as an auxiliary measurement in the figure. (Note that we corrected the name of the sensor to Rotronic HC2). Furthermore, in Subsection 4.5.5 "Comparison of measured and modeled relative humidity" we add a reference to the Rotronic sensor supporting the hypothesis that the rainy boundary layer might be responsible for a measurement deficiency: "The latter is supported by the measurements of both SnowWhite and Rotronic HC2, showing RH < 100% in Payerne and RH > 100 % in Zurich."

6. Is the 2-km spatial resolution of the weather model high enough to be expected to replicate the sounding observations? What is the typical path length of the light scattered back to the COBALD detector? Could there be substantial differences in the backscatter ratios over horizontal scales much smaller than 2 km? Might this contribute to the model-measurement differences for aerosol backscatter?

In the instrumental Section 3.1 we added: "....therefore COBALD measurements are only carried out at night. Geometric overlap between emitted and collected light cones is established at 0.5 m distance from the instrument, and due to geometry of the optics the dominant signal contribution is confined to a range of 10 m. COBALD data analysis provides ...". Of course we expect substantial differences in the backscatter ratios over horizontal scales much smaller than 2 km. This is exactly the reason for performing ensemble runs, because we do not know what the temperature fluctuations are at scales smaller than 2 km, and therefore do not know what the small-scale cloud variability is. Resulting differences certainly contribute to the model-measurement differences for aerosol backscatter.

Specific Comments:

P3 L5: "under the extreme humidity conditions in the upper troposphere" makes it sound as if the UT is a very wet region instead of a dry (but sometimes saturated) region.

Extreme → special

P6 L22: what is a "match radii"? Is this a tolerance for spatial coincidence?

Yes, exactly. Added in text.

P7 L24: The "balloon radiosondes" were not "carrying the payload". The balloons carried payloads "comprised of a Snow White : : :." (this is a correct use of "comprised")

Thanks, changed accordingly.

P8 L15: How is the light backscattered by molecules and aerosols considered a "net signal"? (i.e., what is the "gross signal"?) How is the Rayleigh scattering by air molecules determined in regions where aerosol loading is not very low?

Good point, we changed "net" to "total". For further explanation we refer to the webpage: http://www.iac.ethz.ch/groups/peter/research/Balloon_soundings/COBALD_sensor

P10 L20: for me the phrase "within radius space" is jargon.

Sorry, "within radius space" is normal mathematical terminology. There is geometric space (e.g., x, y, z or θ, φ, z) and radius space (r), within which particles are moving (as function of time t). We do not know how to "improve" this. Left unchanged.

P12 L13-14: The "nights" were not analyzed, their meteorological conditions were.

Thank you. Changed to: "Meteorological conditions of more than 500 nights were analyzed during a 2.5 yr period with respect to the suitability to perform match flights."

P12 L15-16: "finite" has no purpose here. Are 15.6 and 25 km "a few kilometers"?

Improved.

P14 L12-18: There's no need to repeat the Figure 5 caption here.

We have shortened the corresponding paragraph.

P14 L28: what are the CI values that indicate particle sizes > 3 micrometers between 6 and 13 km?

CI ~ 15 - 20. Added.

P17 L6: "balloon tracks" indicates spatial paths. "Balloon temperature profiles" is more appropriate here.

Corrected.

P19 L21-27: The vertical axes in Figure 9 are pressure but the text here discusses some features in terms of altitude.

We improved this by adding the appropriate pressure information.

P20 L2-4: I can't visualize how clouds clearing "further downstream" of Zurich would produce "a sunny morning over large parts of Switzerland"

We remove the reference to the sunny morning.

P21 L28: "during the last years" is vague

Deleted.

P23 L2-4: This sentence is very awkward. How about: "These fluctuations have a much greater impact on cirrus properties than do the differences between the three fluctuation types."

Much better, thank you.

P24 L2-4: Do the following two statements contradict one another? "The scatter in M between different fluctuations is substantial." "…while the differences between the particular type of fluctuations are less important."

Not at all. The metric *M* is a measure of the deviation between model and measurement, and differences between different fluctuations are large. Conversely, the differences between the particular types of fluctuations, as shown in the tables for the run with the best fluctuation member, are less important. For clarity, we repeat the qualifier "for the run with the best fluctuation member" now also in the main text.

P25-P26: This is the point where I really started to get bogged down. Must all the theoretical equations appear here? Can these sections be written in terms of conceptual understanding instead of mathematical rigor?

No, you are right, this technical material does not need to be in the main part of the paper. We move it into appendices for those readers who are interested in the theoretical details.

P30 L28 and P32L1: "Reason is …" is poor grammar. "The reason is …" is better. "This occurs/occurred because …" is even better.

Thanks, changed accordingly.

Over what temperature range does the saturation vapor pressure over ice change by 15% per degree?

10-15% in the temperature range of interest (-35 °C < T < -60 °C).

P31 L19: "a very unlikely coincidence" is better than "a large coincidence".

Done.

P31 L29 and P32 L19: Please change "household data" to "housekeeping data", or even better "engineering data"

Changed.

P32L2 and P32 L12: "ohmic heating" is more commonly called "resistive heating"

Done.

P32 L6: "droplet size of approximately 1 mm diameter?" Does this describe the surface area of ice exposed to the air passing through the intake on its way to the chilled mirror?

Yes.

P32 L22-24: "critical design reviews" is vague. Are you referring to the Snow White's baffle that prevents air from flowing straight through the region housing the chilled mirror? Other instruments do not have this impediment to air flow because it greatly increases the potential for measurement contamination.

We specify what we mean by adding "such as investigating the reliability of the chilled mirror temperature measurement or the requirement of baffles that prevents air from flowing straight through the region housing the mirror".

P33 L20-22: "Changes in microphysical properties of the clouds are not suited to lead to a significant improvement of the cloud modeling" is an awkward sentence. How about "The cloud modeling is not significantly improved by changing the microphysical properties of the clouds."

Thank you for the suggestion. We change this sentence to: "The cloud modeling is not significantly improved by only changing the microphysical parameters of the clouds (such as surface impedance)."

Anonymous Referee #1

Specific comments:

1) Abstract

'Observations of persistent high supersaturations with respect to ice inside cirrus clouds are challenging our understanding of cloud microphysics and of climate feedback processes in the upper troposphere.'

I recommend to reformulate this statement, since persistent high supersaturations are not yet observed in mid-latitude cirrus, only in very cold tropical cirrus. In addition, persistent high supersaturations in mid-latitude cirrus are theoretically understood and it is demonstrated by model calculations (e.g. Spichtinger and Gierens, 2009, ACP: Modelling of cirrus clouds – Part 2: Competition of different nucleation mechanisms) that the in-cloud supersaturation strongly depends on the number of available ice nuclei (IN) that freeze heterogeneously.

To my understanding (?) the goal of this study could be to provide measurements of the evolution of supersaturations in mid-latitude cirrus using the match technique and explain them by microphysical modeling since it is very difficult/impossible to observe the evolution of cirrus from aircraft.

We do not lose content by removing the word "persistent" from the first sentence in the abstract, which will already help merging the reviewer's and our perspective. Beyond this we would argue that there are of course publications on high supersaturations in mid-latitude cirrus. For example, Krämer et al. (Ice supersaturations and cirrus cloud crystal numbers, Atmos. Chem. Phys., 9, 3505–3522, 2009) report on upper tropospheric observations outside and inside of cirrus clouds from aircraft and balloon measurements performed in recent years, representing Arctic, mid-latitude and tropical field campaigns (see their Fig. 3). Therefore we prefer to leave the rest of the abstract unchanged.

2) Introduction, Page 4, line 11 ff:

'... very high in-cloud supersaturations are difficult to comprehend, since ice crystals should readily deplete the supersaturated water vapour leading to a fast decrease of supersaturation. Even though several potential theoretical explanations for persistent supersaturations have been alluded to, this phenomenon and the frequency of its occurrence remain difficult to understand (Peter et al., 2006, 2008).'

It is known that in case heterogeneous freezing occurs first and only few ice crystals form, the supersaturation decreases only slowly. In addition, in a new study of Cziczoet al.(2013, Science: Clarifying the dominant sources and mechanisms of cirrus cloud formation) it is shown from observations in mid-latitude cirrus in the US that heterogeneous freezing dominates the formation of cirrus, at least in the field campaignsinvestigated in this study. I suggest to include both -the influence of heterogeneous freezing on cirrus evolution and the new findings of Cziczo et al. (2013)- into the paper.

We agree and now explicitly refer to the potential importance of heterogeneous ice nuclei, including citations to Spichtinger and Gierens (2009) and to Cziczo et al. (2013). We do so in the introduction (P4 L10-13) and in Subsection 4.5.3 on heterogeneous nucleation of ice (P26 L19-21). We also performed more numerical simulations with ice nuclei, see below.

3) 4.4 Microphysical analysis based on COSMO-7 trajectory fields without small-scale temperature fluctuations and
4.5.1 Small-scale temperature fluctuations.
The need of small-scale temperature fluctuations to reproduce the microphysical properties of cirrus clouds is already demonstrated in two publications coming from the same group of scientists as the actual manuscript (Hoyle et al., 2005, JAS and Brabec et al., 2012, ACP). I don't think that it is necessary to repeat this type of model study here - especially since it shows the same as the other studies, namely that it is difficult to reproduce measured cirrus properties without superposing small-scale temperature fluctuations to the air mass trajectories. I recommend to mention the earlier studies and remove the sections from the manuscript.

The novel and very important aspect in the present manuscript is the recognition that a rigorous application of $(dT/dt)_{ss}$ ensemble calculations is indispensable in the comparison of BSR and RH_{ice} measurements and model results. While we did not have any idea about this requirement in Hoyle et al., we took a first step in this direction in Brabec et al., however, very much as an add-on analysis. Furthermore, the investigation and use of different fluctuation sets for clear sky, cloudy sky and – as a combination of both – "all sky" conditions is provided here for the first time. So is the estimate of these fluctuation sets based on the temperature measurements of the balloon ascents. Finally, the metric *M* introduced by Eq. (3) has never been used before. This leads to the discussion of Table 2, which is essential. We do not see how we could easily shorten this important part of the study.

4) 4.5.2 Mass accommodation of H2O on ice
To my opinion this section is far too long. In addition, it is shown by an earlier model study which is comparable to this one (Gensch et al., 2008, ERL: Supersaturations, microphysics and nitric acid partitioning in a cold cirrus cloud observed during CRAVE 2006: an observation–modelling intercomparison study), that the very low mass accommodation coefficient reported by Magee et al. (2006) can lead to unrealistic cirrus microphysical properties.

We accept the reviewer's criticism and removed most of the section to an appendix. More recently and more appropriate than the Gensch et al. (2008) citation is that of Skrotzki et al. (2013), who reject the laboratory results of Magee et al. We cite their study. Note that the application of the results by Magee et al. is not to defend them, but because they should provide an upper limit of an effect caused by $\alpha < 1$.

5) 4.5.3 Heterogeneous nucleation of ice
As already mentioned, the in-cloud supersaturation depends quite strongly on the number of IN (see e.g. Spichtinger and Gierens, 2009, ACP) with a higher number of IN can cause higher in-cloud supersaturations (homogeneous freezing might be entirely suppressed). Thus, I recommend to introduce one or two additional case studies with varying IN number. This would give a broader view on the possible conditions for the cirrus evolution and the statement that the observed supersaturations could not be reproduced by varying the microphysical model parameters would be more robust.

According to the suggestion of the reviewer we have performed these additional calculations and show them in Table B1 in Appendix B. These additional calculations do not change the conclusions drawn in the paper.

6) Table 4:
It would be desirable to see more model results than only backscatter, e.g. ice number and supersaturation.

Ice number and supersaturation are presented as contour plots in Fig. 9 and 11. As the paper is already pretty long, we refrain from adding more material. Also note that aerosol backscatter is itself a measure of ice number density, while relative humidity (and supersaturation) depends strongly on the temperature in the very moment of the measurement and, hence, is not considered to be suitable to judge the quality of the cloud modeling. Given the present measurements, the backscatter ratio is the best metric to objectively judge the quality of the match between the model results and the measurements.

7) Figures 9 - 12:
Only the model results including pure homogeneous ice nucleation are shown. I suggest to also show the results including heterogeneous freezing with varying IN concentrations. I also suggest to show additional model parameters such as ice number and size (mass mean diameter).

Showing the results in the form of figures would be too much for this paper. However, triggered by the reviewer's comments we performed additional simulations with 20, 50, and 100 L^{-1} ice nuclei (in addition to the 10 L^{-1} that were already addressed) and show these in Table B1. As mentioned above, these results do not change the conclusions of the paper.