Author's response to Review Comments on
Irvine EA, Shine KP 2015: **Ice-supersaturation and the potential for contrail formation in a changing climate.** Earth Syst. Dynam. Discuss., 6:317-349 10.5194/esdd-6-317-2015

Original reviewer comments are in normal font, our replies are in bold italics. Intended changes to text are shown in quotes "..."

Reviewer 1

This is a very interesting paper for all those concerned with the climate impact of aviation, in particular due to persistent contrails. Such contrails only form in ice supersaturated regions and it is therefore important to know how the frequency of ice supersaturation will evolve in a future warming climate. Furthermore this is of interest for those concerned about the climate impacts and feedbacks of cirrus clouds since their formation needs substantial ice supersaturation as well. The latter topic is not touched upon in the paper, which is reasonable in view of the problems current climate models have to represent ice supersaturation at all. For the latter reason the authors had to use relative humidites above model-dependent threshold values as proxies for the presence of an ISSR. To my opinion this is justified. This paper is well written and easy to comprehend. The only thing I miss is a comment on the statistical significance of the observed changes. Otherwise I have only a couple of minor comments. I recommend publication of this paper.

We thank the reviewer for the helpful comments

Major issue

Nothing is said about the statistical significance of the observed changes. There is "considerable interannual variability" (page 329), thus the question on the significance of the results seems justified. You could include __-bars on the curves in figure 5 such that the reader gets a feeling of how far the curves deviate at 2100 from the historical values. T-tests or non-parametric tests on the 2D-fields could be performed to check significance. I see that the changes are quite substantial in the tropics, so it might be that they are beyond doubt. If so, please say so.

After our 10 year smoothing is applied, the amount of unforced variability is small indeed compared to the signals in the polar and tropical regions and so we agree with the reviewer that the changes are beyond doubt. We propose to add text to say "the changes in the smoothed time series are clearly larger than any internal variability". As indicated in the original text, we are more circumspect about the mid-latitude changes, as there is no consensus between the models and do no claim any significance.

Minor issues
Although this paper is very well written, there are several instances where I found minor jumps in the logic. These can be fixed easily.

Page 319, line 22: Instead of "This study" please write "The present study". The word "This" otherwise leads back to Marquart et al., which is probably not meant.

Thank you – the change will be made

P. 320, l. 21/22: Please rewrite the sentence in the following form: "The consensus is that under climate change there will be a decrease ... in the upper troposphere ...". (Otherwise I read that there is a consensus in the upper troposphere).

Thank you – the change will be made

P. 321, l. 3: The sentence ending in "Marquart et al." talks about the tropics. As the next sentence talks immediately about the highest flight levels and the stratosphere, the reader is misled because one wonders why you are talking about the tropical stratosphere where air traffic is very low. Please clarify that you are now talking about the extratropics.

Thank you – the clarification will be made

P. 322 (bottom)/323 (top): How are these monthly means computed? I assume you compute daily RH values and average them. Is this correct?

This will be clarified – we do not compute RH values – they are taken directly from the archived CMIP5 data from each individual model, and represent the time-mean RH (rather than the RH of the time-mean temperature and absolute humidity)

P. 328, sect. 3.2, 1st par.: You might add that the changes are substantial, namely about one third of current values.

Thank you – the change will be made and also highlighted in the abstract and conclusions, as it is an important point.

P. 332, l. 11: temperatures are lower, not colder

Thank you – the change will be made

Reviewer 2

This paper focusses on the question, how large the potential influence of projected temperature and humidity changes in the upper troposphere may be on future ice supersaturation and persistent contrail climate impact. To this end, a multi-model analysis of the respective parameters, in particular of the parameterized frequency of ice-supersaturated regions, is made from standard climate projections available from CMIP-5. Conclusions for actual aircraft induced impacts in the future must remain speculative, as the effect of projected air traffic changes is not included. This limited approach may look trivial to some, yet I think it is very helpful to understand and to assess this somewhat neglected aspect of a complex issue, viz., contrail climate im-pact research. The paper is well-written, honest and balanced in its conclusions, and the physical reasoning for explaining the results is well-conceived (I'm particularly fond of section 3.2!). I know of two previous studies to address a similar issue (Mar-quart et al., 2003; Minnis et al., 2004), of which the latter is not mentioned in this pa-per (perhaps because it does not address ice-supersaturation explicity?). Yet, I en-courage the authors to add a discussion (if possible) of Minnis et al.'s results, which seems possible as they also show dedicated results for mid-latitudes.

The present paper should certainly be published after a minor revision.

We thank the reviewer for the helpful comments

I) Major comments

• The definition of a model-dependent threshold to mark actual ice-saturated regions is crucial, yet it is motivated adequately in section 2.2., and may stand as a standardisation setting for the present paper.

Thank you – we do not believe any action is required as a result of this comment.

• While this is a very detailed comment, referring to the beginning of section 2.2, it is of general relevance. Frankly speaking, I think the term "ice-supersaturated regions" forms a clean-cut definition of a region where the air is saturated with respect to ice. Yet, in the context of this paper it is employed to indicate "regions potentially carrying persistent contrails and contrail cirrus" by adding a temperature threshold criterion. There's nothing wrong with this, the reasoning for the definition modification (p. 324, l. 1) being quite comprehensible, but you might adjust the wording in p. 323, l. 29 to avoid the formally self-contradictory definition of ice-supersaturation used now.

We agree with the reviewer and have adjusted the wording and also adjusted the wording in the abstract to make clear that a temperature threshold is applied. In addition, throughout the paper we now refer to Cold ISS (CISS) rather than ISS, for clarity.

• There is no mentioning throughout the paper of the topically similar work of Minnis et al. (2004), who used measured humidity trends in the upper troposphere to project contrail changes. I strongly suggest to discuss the results of the present paper in context of those observation-based findings, at least in the concluding section.

Thank you for reminding us of this study. Minnis et al. derived relative humidity trends for the period 1971-1995 from an early version of the NCEP re-analysis and we will incorporate a mention of this study in the introduction, and will emphasise the need for observational monitoring in the future in the conclusion. In the main text we propose

"Minnis et al. (2004) analysed upper-tropospheric relative humidity trends, derived from reanalyses, for the period 1979-1995, over northern-hemisphere mid-latitude regions, in the context of changes in contrail and cirrus occurrence. They found relative humidity decreases of up to 6% per decade, although they noted that data quality issues meant that these trends should be "viewed with some scepticism" because of date quality issues."
while in the conclusions we note
"In time, improvement in the global observing system may allow a robust evaluation of the model-derived humidity trends, which would impact on the confidence with which those trends can be viewed."

II) Minor remarks

1. p. 318, l. 24: From my point of view, contrail cirrus climate impact cannot be regarded to make a "large" contribution to anthropogenic climate change. Thus, I suggest to limit this sentence to "Because they make a substantial fraction to aircraft climate impact (e.g. Lee et al., 2009), many …"

We agree – the change will be made

2. p. 319, l. 7: The authors may consider here additional references to Schumann et al. (Journal of Aircraft, 2000), who gave observational evidence for the impact of engine efficiency, and Marquart et al. (2003), who made dedicated sensitivity tests for the respective effect on contrail radiative forcing.

Thank you – we will include these two references as suggested.

3. p. 320, l. 2: To emphasize the link of ISS to contrail cover, it may worthwhile to add the following text and reference: "However, the close link and comparability between ISS and potential contrail cover has been clearly demonstrated by Burkhardt et al. (2008)."

We agree that this is a useful point to make and will amend the text

4. P. 322, l. 1: "…historical simulation simulates the present-day climate …" sounds funny to me, perhaps change to "… historical simulation tries to reproduce the present-day climate …"

We agree a better wording is needed and now say "the historical simulation aims to reproduce"

5. p. 322, l. 28: I would like to see a reference here.

We presume that the referee refers to our statement that 250 hPa is a typical cruise altitude. We have now added reference to Wilkerson et al. (2010) specifically "(see e.g. Wilkerson et al. 2010 who show peak emissions at about 10.5 km, with the vast majority of flights cruising at between 10 and 12 km (about 200 to 260 hPa))"

6. p. 323, l. 3: "high humidity regions"? Do I guess correctly that you are meaning "humidity at high altitudes" (or "upper tropospheric humidity")?

We agree this is ambiguous – it was meant to be "regions of high humidity" (we think the high-altitude is implicit in the context of this paper) and we have modified the text.

7. P. 323, l. 22: "Air traffic …", please try to unravel this sentence by simplification.

We will re-write this sentence to make it less convoluted to say "Air traffic growth is projected in all three regions , particularly in the tropics; for example, Owen et al. (2010) predict five times as much air traffic in some regions in 2050 compared to 2000, for the A2 scenario (their Figure 2) used in the 2007 IPCC assessment (Riahi et al., 2007), on which the RCP8.5 scenario is based."

8. p. 327, l. 21: I think I generally understand the general reasoning with respect to model biases in this subsection. Still, it strikes me why (e.g.) MPI-ESM-MR can reproduce closely the ERA-Interim ISS frequency in northern polar latitudes (Figs. 2a, 2e), when it captures specific humidity quite well but has a -5K cold bias in that region. To my impression this should imply extreme (relative) dryness. It may be helpful, beyond giving largely general statements, to un-ravel the combination of effects for this or some other appropriate example.

We think the reviewer loses sight of the fact that we have model-dependent relative humidity thresholds to define the ISS, which is discussed in Section 2.2 and illustrated in Figure 1. Also, if the humidity is well modelled and the temperature is too low, then the model has "relative moistness". For this particular model, the top 10% of RHi points is obtained by using a relative high RHi threshold. We will add extra discussion that the effect of model temperature biases is "***ameliorated to some extent (at least at the global-mean level) by the choice of a model-dependent CISS threshold (Table 1 and Figure 1).***"

9. p. 329, l. 26: "… may be less significant in terms of persistent contrails …", do you mean that some or many of the additional contrails will be too thin to in-crease the contrail coverage? This may be true but not for sure (see Marquart et al., their Fig. 3). Perhaps, limit the statement to "… less significant in terms of persistent contrail climate impact …", which is fully in line with the reasoning of this paragraph.

We agree with this nuanced wording

10.p. 331, l. 6: Please, change to "Our analysis …", as the statement doesn't hold for general ISS research.

We agree with this nuanced wording

11.p. 331, l. 19: This sentence confused me a little bit, what is meant by "other levels"? And why should the agreement between models facilitate an extrapolation of findings at one level to other levels, anyway?

We think our wording was not clear – we meant that the analysis at 250 hPa is likely to hold for a wider range of cruise altitudes other than 250 hPa, based on the fact that the monthly-mean analysis shows the change is similar across this range. The wording will be improved to say "which suggests that the increase in CISS frequency predicted in this region at 250 hPa will be also occur at other cruise altitudes."

12.p. 332, l. 3: If there is anything to be gained from existing publications on the GFDL-ESM2G simulations that may help to understand the strange behaviour of that model in the tropical upper troposphere, it ought to be mentioned here. If not, it would be regrettable, but not due to your fault, so leave it this way …

We agree that the GFDL-ESM2G simulation is different, although we would not necessarily call it strange. We are unaware of any detailed discussions of this behaviour.

13.Section 4: I see some reason to mention Fig. 3 from Marquart et al. (2003) in this concluding discussion section, because it supports a lot of expected consequences for contrail cover formulated here.

We agree – we will include a short discussion to say "The results are broadly consistent with those of Marquart et al. (2003), where the focus was on predicting changes in contrail cover for specified distributions of air traffic growth, rather than the frequency of CISS. In their simulations, the impact of climate change reduces 2050 contrail cover by 20% compared to the case with no climate change, with that decrease concentrated in the tropics."

Reviewer 3

This manuscript addresses an important question concerning the future changes in potential contrail coverage as a result of climate change, .. In my opinion the scientific contribution and the quality of the manuscript fulfills all the requirements to be published in ESD in its present version.

We thank the reviewer for these very positive statements.

Minor suggestions: Section 4 line 17 "are" should be "is".

We agree – the change will be made

The distribution of the profiles in Fig. 7 could be changed to increase their size.

This is a typesetting issue in ESDD which chooses to use landscape format on figures which are fine in portrait mode.

Reviewer 4

General comment:
... Generally, this is an interesting topic and the study provides new and interesting results about the change of ice supersaturation in the tropopause region ..

We thank the reviewer for the helpful comments

Major points
1. For the investigations the authors use just daily data for one pressure level (250hPa) for the investigation of ice supersaturation in the tropopause region. They argue that most of the relevant flights will occur around this pressure level. There are at least three concerns, which should be discussed by the authors:

(a) It is not clear how the pressure level 250hPa is represented in the model data. Obviously, the model levels will have a certain extension representing a vertically thick layer. The authors should indicate which vertical extended layer is represented by the level 250hPa; is it a layer centred at p = 250hPa with vertical extension of 50hPa (since they indicate other pressure levels as 150, 200, 300hPa, etc.), i.e. representing the range 225 − 275hPa?

This is a misunderstanding of the nature of the CMIP5 output and we must clarify this point in the revised manuscript (in particular around 322:26-29). The 250 hPa CMIP5 data is standard output required by CMIP5. Each modelling group interpolates data from their own model grid on to this pressure level. We will add text to say "(in the UTLS regions these are 500, 250, 100 and 50 hPa, with each CMIP5 modelling group interpolating to these pressures from their own model's grid)".

(b) From MOZAIC/IAGOS measurements (see e.g. http://www.iagos.fr/web/) it is known that a large portion of long-distance flights is located in the range p < 250hPa or even in the range p < 220hPa. Thus, an investigation of pressure level 250hPa might just give a part of information relevant for contrail formation. The authors should think about extending their study including the pressure level 200hPa, since most of the relevant long-distance flights would be covered by these two levels. Of course, the question about the vertical extension of the pressure layer is related to this issue.

See also our response to the previous point. We need to be explicit in the revised manuscript (at 322:26) that the daily data is only available on a very limited number of levels (in the UTLS region these are 500, 250, 100 and 50 hPa). Hence 250 hPa is the only one suitable for this analysis. Of course we are aware that 250 hPa is an not a perfect proxy for cruise altitude (but neither is it a bad one) but this is precisely the reason why we also present the monthly-mean data for which more levels are available (see 323:2) to provide at least some check of this.

(c) The use of daily data might also cause some underestimation of ice supersaturation frequency. Our knowledge about life cycles of ice supersaturation is quite limited. It is often assumed that large scale dynamics with time scales of days triggers ice supersaturation in the tropopause region. However, recent studies (e.g. Irvine et al., 2014) indicated that Lagrangian life times of air parcels in supersaturated conditions might be smaller than 24 hours. Thus, the authors should describe carefully, how this influences their investigations; probably, just a lower limit can be derived from their evaluations. A similar issue constitutes the use of monthly mean data for other vertical layers.

We agree that the time resolution is an issue, but are surprised to see this labelled as a major point. We will add text to say "We note that even the use of daily data will fail to resolve ice supersaturated regions with shorter lifetimes". However, the reviewer should recognise that the Lagrangian lifetime calculated in Irvine et al. 2014 represents the time that an individual parcel remains saturated, not the duration of the region of ice supersaturation itself.

2. The temperature criterion for the definition of ice supersaturation seems a bit artificial and might lead to artificial biases. It is true that the temperature limit of T = 233K coincides almost with the Schmidt-Appleman criterion, although the limits would be possibly situated at lower temperatures (see e.g. Gierens et al., 1997, figure 1). However, for the pressure level of 250hPa I would expect such low temperatures (i.e. T < 240−245K) that the frequency of occurrence for pure supercooled water should be very small if not almost zero (see e.g. Pruppacher and Klett, 2004, fig. 2-33). The introduction of the temperature criterion could result into an artificial bias for the data evaluation, as already indicated by the authors. Since some models seem to tend to higher temperatures in the tropopause region, the frequency of occurrence for ice supersaturation could be masked by the temperature criterion. Thus, it is not clear how robust the results are.

Therefore I would suggest additional evaluations:

(a) The authors should carry out the same data evaluation with no temperature criterion or with a changed criterion (e.g. setting the threshold to T = 238/243K). This should provide a hint about the robustness of the results. The existence of ice supersaturation is not only important for persistent contrails but also for the formation of natural clouds, thus investigations without a temperature threshold would provide additional information.

(b) If the authors would prefer to stay with the temperature criterion of T < 233K, they should introduce a second data category, i.e. T _ 233K and carry out the same investigations for this category (maybe with the additional constrain of T < 243K or similar constrains to avoid liquid water). This would give an answer about the robustness of the results, too. In addition, they could study the transition between the two cases, which would also provide additional information about potential contrail formation (concerning the Schmidt-Appleman criterion).

We cannot agree that the application of a temperature criterion is artificial in the context of persistent contrail formation – as the Reviewer points out, it is a consequence of the Schmidt-Appleman criteria, and it is clear that Reviewer 2 agrees with our approach. Nor do we understand the statement that the temperature criterion could lead to an artificial bias. A bias in what? We will add additional text to point at 324:4 to make clear that the Schmidt-Appleman criteria is not, in reality, a fixed temperature but is dependent on other parameters - specifically: "although we note that in reality the threshold temperature is somewhat dependent on altitude, humidity, fuel type and engine efficiency (Schumann, 1996).". We also note (see response to Reviewer 2 comment 8), that the application of the model-dependent relative humidity threshold for ISS acts to ameliorate the effect of the temperature bias. Concerning point (a) we do note the relevance of our study to the formation of natural clouds, but we also make clear that this is not the focus, nor the motivation, for present study (see also comment by Reviewer 1), which is firmly on the subject of persistent contrails. Nevertheless we will add text at the end of Section 3.2 to indicate global-mean impact when applying no temperature threshold:

"Since the ISS changes without application of the temperature threshold are also of interest, beyond the context of contrail formation, we briefly comment on the ISS trends. Since the tropics dominate the global-mean, and the tropical CISS results are strongly influenced by the temperature threshhold, the global-mean ISS trends are expected to be less strong than their CISS counterparts. The global-mean values corresponding to the time-period in Table 1 are -1.5% (EC-EARTH), +4.9% (GFDL-ESM2G), -0.004% (HadGEM2-CC), -1.5% (MIROC5) and -1.2% (MPI-ESM-MR). All models show an increase in polar regions, albeit less strong than indicated for CISS in Table 2, while all models show a decrease in the tropics, with the exception of GFDL-ESM2G which shows an increase, which hence strongly inlfuences the global-mean response in that model. As will be discussed in Section 3.3, the GFDL-ESM2G model has a quite different predicted relative humidity response in the tropical upper troposphere compared to the other models discussed here, with increases near 250 hPa".

3. The authors discuss the results in a quite qualitative manner. However, the origin for changes in relative humidity and thus in the frequency of occurrence of ice supersaturation remains unclear. The authors should try to investigate, which variables contribute to increase/decrease of ice supersaturation dominantly. For instance, it is not clear if changes in temperature or in specific humidity contribute most to changes in ice supersaturation. It is not clear to me, if the available data is good enough for investigating such quantitative issues, but the authors should at least comment on that issue.

We agree that it is useful for the reader to understand the reasons for the change in relative humidity distribution, but we note that this has been the topic of major studies already. The purpose of our paper is to understand the consequence of the changes in the context of contrails. Accordingly, in the introduction we have added text to state "Wright et al. (2010) and Sherwood et al. (2010) discuss in detail the reasons for the changing distributions of relative humidity. Briefly, the tropical decrease is driven by the vertical and poleward expansion of Hadley circulation and the changes in temperature in regions where air parcels reaching the upper troposphere are last saturated. In the extratropics, changes in relative humidity are largely driven by temperature changes. In the context of contrails, a further mechanism is at play, because contrail formation is dependent on the air being below a given threshold temperature (Schumann, 1996 and see Section 2.2)". Further we do not agree that we have been qualitative in the impact of the change in threshold temperature on tropical ISS frequency – Figure 6 addresses this issue in a fully quantitative manner.

Minor points:

1. The representation of the thermal tropopause is usually not very good in climate models. Actually, the vertical gradients are usually weaker than in nature due to coarse resolutions. Thus, it is not clear to me how a misrepresentation of the tropopause height in the models might influence ice supersaturation in the tropopause region. Maybe the impact is not that strong, but it is not clear at all. The authors should discuss this issue in more details, regarding the quality of representation of this transport barrier in climate models.

We frankly do not know what to do with this comment, beyond noting that the reviewer labels this as a "minor point". It starts with an unreferenced assertion about the mis-representation of the tropopause (and the cause of that mis-representation) and then concludes with a request that we discuss a point that goes well beyond the topic of our paper. We have presented a warts-and-all analysis of our selected models, in terms of their representation of temperature and humidity in the upper troposphere (see especially Figure 3), the origins of which likely go well beyond the representation of the tropopause (for example, vertical moisture and heat transport).

2. A more quantitative evaluation of the 2D distributions of annual ISS frequency should be carried out (figure 2).

We have added text in the relevant paragraph of Section 3.1, giving a more quantitative evaluation as requested.

Technical comment:
The colour bar for figure 2 is very hard to read. Please change it by including more colours for a better discrimination of ISS frequency.

We should have pointed out that Figure 4 includes the labelled contours of the present day CISS distribution. We experimented with several different versions of Figure 2, and decided the current version was best for giving a visual feel of the distribution of areas of high CISS. We will amend the text in Section 3.1, and the caption of Figure 2, to make this clear.