

## ***Interactive comment on “Dynamical characteristics of ice supersaturated regions” by K. Gierens and S. Brinkop***

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### **1 Reply to reviewer No. 1**

The reviewer states that the scientific impact of the paper is incremental and that the results are somewhat expected. This is absolutely true and we have no problem with this statement. However, nobody has analysed this kind of data before and thus it was not *a priori* clear what we would find. It is good that we did not find gross contradictions to what one might have expected, otherwise our confidence in ECMWF's representation of supersaturation would have been damaged. Fortunately, there was none such problem.

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## 1.1 Spatial scales (comment 1)

Satellite and aircraft data are generally higher resolved horizontally (but not vertically) than forecast model data. This has no effect at all on the results of our study, which are based solely on one single type of data, i.e. forecast data. As long as we do not mix data from various sources there is no problem with different resolutions.

Satellite, aircraft, and other data are merely mentioned in our paper to support the discussion section, to seek for consistencies and inconsistencies. If we find consistency, it's fine. If we find an inconsistency, non-matching resolutions can be part of the explanation, but there are other possibilities as well.

## 1.2 Duration of the study (comment 2)

When we started the study in April we decided to first look at one month only in order to see whether there are statistically significant differences between the non-conditioned and ISSR-conditioned datasets at all. Then, the fact that the IFS was updated in Nov. 2011 implied to us that we could not currently analyse a whole year or at least four seasons without the risk of spurious jumps in the analysis. Now we have asked Dr. Forbes from ECMWF for details of the update to cycle 37r3 and he told us that the change in the treatment of supersaturation referred to mixed-phase clouds only and did not affect the upper tropospheric ice-only regions. Thus we analysed more months. Since the new results add little to what we have already seen in the original manuscript, we present them in a supplement. The paper itself is only slightly adjusted where necessary.

According to additional information by Dr. Forbes, there was another update to the model in June 2012 to cycle 38r1. This update actually affects the upper tropospheric supersaturation treatment to a small degree, and for this reason we did analyse previous months instead of the most recent ones.

### 1.3 Deep convection (comment 3)

Indeed a separation of regions with and without recent occurrence of deep convection would be interesting, but it cannot be easily done with the forecast data alone, even if we would take the IFS field “top of atmosphere net thermal radiation” as a proxy for the indication of *current* deep convection. We would have to compute trajectories either forward from regions of deep convection (however without knowing from which altitude to start) or backward from ISSRs until the trajectory finds a deep convective tower somewhere. Susanne Nawrath (2002) has implemented such a strategy for her interpretation of tropical humidity data from MOZAIC, but not especially for ice supersaturation (her PhD thesis “Water Vapor in the Tropical Upper Troposphere: On the Influence of Deep Convection” can be obtained from <http://kups.uni-koeln.de/view/creators/Nawrath=3ASusanne=3A=3A.html>). Nawrath has shown:

With the help of backward trajectories and satellite data of cloud top temperatures the origin of the measured air was traced back to areas of deep convection. It is shown that the meridional exchange is not very strong. For most measurements the convection is not more than 10 degrees of latitude away. Particularly in the ITCZ there is also only a very weak zonal circulation so that most measurements are taken very close to the originating convection. This means also that the distances between convective clouds can not be very large. The analysis of air that was measured close to convective clouds shows that the mean relative humidity with respect to ice lies near 80% on all pressure levels and in all seasons as well over the South American continent as over the ITCZ.

The humidity distributions shown by Nawrath shows a tail into supersaturated states at all considered times along her trajectories, however with an overall decreasing probability with time increasing since deep convection. These results point to deep convec-

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tion as the ultimate source of ice supersaturation in the tropical UT and this is probably generally accepted. Our paper acknowledges this role of deep convection as well. In the paper we have shown that, on average, vertical velocity decreases and divergence increases with altitude in the tropical ISSRs, a clear indication of deep convection as a major source of ISSRs. A trajectory analysis like Nawrath's would extend our study to an unjustified degree and give it a completely new direction.

The last statement in this comment is not understandable, namely “... is needed to determine if vorticity, divergence, or cyclonic conditions influence tropical regions”. What is meant with “influence tropical regions”? These fields are dynamical characteristics of the tropical regions but how and in what a sense should they influence them? Furthermore, what has this to do with the question how the flow in ISSRs differs from the flow in the rest of the tropics?

## 2 Reply to reviewer No. 2

### 2.1 Missing motivation (comment 1)

ISSRs are investigated now in some detail since more than ten years. Topics have been frequency of occurrence, climatology, statistics of supersaturation within ISSRs, temperature distributions and moisture distributions inside ISSRs and elsewhere, geometrical properties. As far as we know, there was no study up to date that compared the dynamics within ISSRs and outside, so it was natural scientific curiosity that motivated us to make this analysis. As indicated above, we did not expect revolutionary results, but we expected certain differences in the distributions and we wanted to see and analyse them and to interpret them in the light of previous results. Such a study can as well be regarded as an evaluation of the supersaturation prediction of the IFS and is thus not merely an academic exercise.

The histograms show that some distributions differ quite remarkably from gaussians. Thus it makes sense to consider not only mean values and standard deviations but higher moments as well.

The motivation for the study in general and for using the four lowest moments will be given in the revised version.

## 2.2 Study duration (comment 2)

See the reply to reviewer 1, comment 2, above.

## 2.3 Additional dynamic and thermodynamic variables (comment 3)

Temperature and absolute humidity distributions within ISSRs vs. outside of ISSRs have been considered in many papers, some of which are cited in our introduction. In contrast, dynamical fields in relation to ISSRs have not yet been considered so far, apart from two case studies which are quoted as well. So this is the first such study as far as we know, and we started of course with fields that are easily available on the ECMWF's meteorological archive in the same data collection as relative humidity. This is a justified and reasonable strategy. ECMWF also offers data on potential-temperature levels, but only dynamical data, no humidity. Data provided on PV levels contain humidity, however only one level (probably 2 PVU) is stored on the archive. The suggested extension of the study to PV levels is thus not easily possible and we will not try it for the current paper. Furthermore we doubt whether a study of PV (or other dynamical quantities like Montgomery potential) would lead to more novel results than those presented here.

A more convincing recommendation is to our view the consideration of trajectories. As we argue in the paper, it depends on the airmasses history whether it becomes ISSR

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or not, and the dynamical fields alone can only give hints where to look for ISSRs and where not. Thus trajectory calculations backward and forward from detected ISSRs can give valuable and interesting insight, for instance in typical lifetimes. Such a study is currently performed at this institute as a master thesis and will be published in the near future.

## 2.4 Warm conveyor belt (comment 4)

It seems that the additional point that the reviewer wants to have in the discussion is already there:

... Wylie et al. (2002), for instance, reviews older analyses of cirrus occurrence in relation to the jet stream and Rossby waves. He shows dense and opaque cirrus to occur close to the ridges of Rossby waves where the flow is anti-cyclonic. On the contrary, the sky is clear of cirrus in the cyclonic trough part of the Rossby wave. A case study of a long-lasting ISSR over Europe (Spichtinger et al. 2005a) shows the ISSR in anti-cyclonic flow along a high pressure ridge as well. Moist air ascends on the western side of the ridge in a warm conveyor belt and descends on the eastern side. ...

Thus, we have already mentioned that warm conveyor belts lead to ISSRs and feel no necessity to expand on this, in particular since the reviewer argues for shortening and focussing the discussion. We agree that this could enhance the readability of the paper and we will do it. New results from the additionally analysed months are so similar to what we have already presented that further discussion is hardly needed. To shift part of the discussion into the introduction makes to our view not much sense, because the discussion of results and procedures should logically follow the presentation of the results and procedures.

### 3 Reply to the short comment by P. Bechtold

Of course it is NOT co-incidental that ISSRs in the tropics occur in uplifting divergent flow; it is a result of recent deep convection, see above. If our text could be misunderstood in this way, we will have to clarify this. What we think to be coincidental is only that ISSRs in the tropics are preferentially related to anticyclonic flow, because this results from the poleward transition from westerly to easterly flow. These are large-scale (1000s of kilometres) phenomena which are certainly not directly causally related to deep convection and ice supersaturation. Neither is anticyclonal flow the condition for ice supersaturation nor vice versa.

In contrast, in the middle latitudes the warm conveyor belt is a feature connected to the anticyclonal flow type in the upper troposphere and it is the ultimate source of the high humidity in many ISSRs. In this sense, anticyclonal flow supports the formation of ISSRs, thus there is a more direct causal relation.

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