

## ***Interactive comment on “On the transitions in marine boundary layer cloudiness” by I. Sandu et al.***

**Anonymous Referee #3**

Received and published: 7 December 2009

This manuscript describes an effective application of techniques used in previous studies to put satellite observations into a perspective that allows for the interpretation of the time evolution of cloud fields. By combining satellite observations and trajectory analyses, the transitions of boundary layer clouds from near solid to broken cloud conditions are studied for 4 ocean basins. This manuscript is well written, the analysis techniques used are solid, and the findings are important in describing the characteristics of the transitions and factors that might affect these transitions. There are, however, some minor issues that can be addressed to improve the clarity and answer some questions that readers may have about the analysis techniques and the interpretation of the results.

Issues and Questions:

C7957

In the discussion about conditional instability in the subcloud layer (page 23591; line 18-20), the usage of this term is non-standard. By definition (e.g. see AMS glossary) conditional instability exists in a layer if the lapse rate is greater than the moist adiabatic lapse rate. Such a condition is generally met in the subcloud layers associated with both marine stratocumulus and fair-weather clouds. Thus the real intent of this remark is unclear. The moistening of the subcloud layer that occurs when transports into the cloud layer are inhibited can increase CAPE that can then help support cumulus clouds in the cloud layer.

In Section 2.1; paragraph 2, the discussion of forward and back trajectories in the last sentence is confusing. First, since there is no previous mention of forward trajectories, the assumption must be that all prior discussion is for the forward trajectories. Second, a more specific description of the two sets of back-trajectory calculations needs to be made. It appears that one set is made at a 200 m starting level, and the other is made from a 2000 m starting level and both use the same starting points, time, locations, etc. But this is not clear from the description given. Are the results sensitive to the starting level in the boundary layer? What are the inherent spatial uncertainties in forward and back trajectories of this type? Is there anything in the literature that can address this issue? Do all trajectories (back and forward) remain in the boundary layer? Do all the back trajectories that start at 2000 m stay above the BL? These points may affect how well the satellite observations line up with reality in terms of time and space.

In choosing the trajectories for the fast and slow response (page 23600; line 4), why not just take the average for day 3 and then select the lowest and highest 30 percentiles of these average? This should give a larger signal than using the 3-day averages, since all the cases in the first day have a similar cloud fraction that may temper the 3-day average.

In Sect. 2.2.3; paragraph 2, there is reference to precipitation estimates from GPCP. But this quantity and its source is not given. What is the accuracy of these estimates? Is there a threshold above which the values can be considered reliable and significant?

C7958

If so, what is the threshold? A citation and something about the accuracy of these estimates, particularly for shallow BL clouds, would be helpful. Without these details, the discussion about the variation of the precipitation rate along the trajectory has little value. Thus where the authors state that the “the transition from stratocumulus (to) broken shallow cumulus fields typically occurs before the precipitation at (the) surface become significant” (page 2358; line 11-12 ) it is difficult to justify the merit of the statement. What is meant by significant? In a few sentences later the authors do indicate, however, that this quantity is difficult to measure (or in this case to retrieve). But how difficult and what are the uncertainties?

A major finding from section 5 is that the cloud transition estimated along streamlines is similar to that observed along the trajectories. Although this was nicely demonstrated, it is unclear that this is a new or unexpected result. The flow in all the regions considered is strongly controlled by the subtropical anticyclones. Since these systems tend to be nearly stationary and evolve slowly in time, one would expect a good correspondence between trajectories and streamlines. This point is not raised explicitly in this section, although there is a short reference to the steadiness of the trades (page 23604; line 5). Given the anticipated nature of these results, it is unclear that the extensive discussion given on this topic is warranted. A more concise and focused argument would improve the content density of this section.

Minor Editing: There are a couple of places in the text where there are missing words or incorrect word usages that should be corrected. A careful re-read may indicate others.

Page 23596; line 16. Confusing sentence. Should masses be singular?

Page 23600; line 5. “Hereinafter” is not a word.

Page 23600; line 12. Change “latter” to “later”

---

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23589, 2009.