

Interactive comment on “Switching cloud cover and dynamical regimes from open to closed Benard cells in response to the suppression of precipitation by aerosols” by D. Rosenfeld et al.

D. Rosenfeld et al.

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Response to reviewer #2:

The reviewer states that “Contributions to the scientific literature should contain novel hypotheses or new data/analyses that support or refute existing hypotheses”. He is of the opinion that this manuscript contains neither, and therefore it should not be published. It is shown here that the reviewer is wrong on both counts.

The novel hypothesis of this paper:

Section 3 of the paper presents a novel hypothesis about the mechanism by which the

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amount of aerosols in marine boundary layer determines whether the marine stratocumulus will take the shape of closed or open cells. This hypothesis is original, because there is nothing that can be found in the literature that connects aerosols to reversals between closed and open cellular convection in the marine boundary layer. It needs to be emphasized here that this is not the same as the extensively studied relation between extent of cloud cover and aerosols. Here we go further and propose a mechanism that explains the observed shapes of cloud patterns in a way by which previous studies become links in this integrated new conceptual model. Admittedly, here we build a new conceptual model mainly by combining existing knowledge in a new way. The gap in our understanding of this mechanism until now, which is exactly what the hypothesis of this paper potentially answers, is highlighted at the bottom of page 8 the paper, quoted here again:

Wood and Hartman (2006) stated that “qualitative examination of geostationary satellite imagery suggests that a mesoscale cellular convection usually transitions from closed to open. This change often takes the form of small pockets of open cells forming over a few hours within regions of extensive closed cells, which then grow to become extensive areas of open mesoscale cellular convection.”

The new data/analyses of this paper are:

1. Figure 1 shows strong dependence of the indicated cloud drop effective radius and cloud fraction on the aerosol optical depth in four different meteorological and aerosol regimes of the Atlantic Ocean. These data are new. The questions that the reviewer raises with respect to the nature of the relations are irrelevant in the context of the reviewer’s argument that we do not present here new data/analyses. Furthermore, the reviewer contradicts him/herself by claiming that we do not show any new data and then criticizing these data because they are new and not yet well established relations.
2. Figures 3 and 4 show different pieces of data that have been seen many times before, but never related to each other this way. Here we show that:

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a) When the ambient clouds are composed of closed cells the effective radius is smaller than 15 micron, and then the ship tracks are distinct just by smaller effective radius, whereas cloud cover and cellular regime are the same within and outside the ship tracks. We assume that ship tracks are caused by excess CCN, and this is the main reason for the indicated reduction of the effective radius within the ship tracks. We further assume, based on that, that also outside the ship tracks the effective radius is inversely related to the concentration of the CCN. We will make this assumption explicitly expressed in the text.

b) When the effective radius is greater than 15 micron the ambient clouds break into open cellular. Then the clouds within the ship tracks remain with effective radius smaller than 15 micron and in a regime of closed cells.

c) When there are no ambient clouds at all (i.e., only when cloud are absent due to runaway rainout and cleansing effect), ship tracks are still formed in closed cells.

Nowhere in the literature these observations are being stated, although images showing the same can be cited (but not interpreted that way where presented).

3. The movie is clearly a new observation which is the first of its kind for the following reasons:

a) It is based on the new METEOSAT Second Generation geostationary satellite, using channel combinations that have never been used before in the published literature, for presenting day and night continuous coverage of cloud microstructure. This alone already worth the publication.

b) The movie shows pockets of open cells (POCs) that have clearly larger cloud droplets than the ambient closed cells, and tracks these areas for duration of up to four days and nights. This has never been done before. The movie demonstrates that there should be a mechanism that maintains the identity of the POCs, and supports the proposed mechanism which is the hypothesis of this paper.

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So we have established here that the two necessary ingredients for a scientific paper do exist in the manuscript.

In addition, the reviewer has problems with specific issues that are addressed next.

The reviewer perceives the manuscript as having five claims and makes the following comments on them, quoted here with our response following each such perceived claim.

The reviewer writes: "(i) There exist two distinct states of convection in the cloud topped marine boundary layer: That open and closed forms of mesoscale cellular convection are common forms (but by no means the only forms) of mesoscale shallow convection in the MBL is not a new idea and has been discussed in manuscripts as early as 1961. In fact, two review papers (Atkinson and Zhang 1996 and Agee et al. 1973) discuss other types of shallow convection (e.g. rolls) that can be found in the cloudy MBL."

Our response: The existence of closed and open cellular regimes is definitely not a new idea, and appropriately reviewed in section 1. The discussion pertains to weakly sheared boundary layer. It is true that rolls will develop in highly sheared conditions, which are not very common in areas dominated by marine stratocumulus. This mention will be added to the manuscript. It is already stated that open cells develop unconditionally on the aerosols when the surface is significantly warmer than the overlying air.

The reviewer writes: "(ii) Aerosols suppress precipitation: The authors present no precipitation or aerosol data whatsoever to support this claim. As a hypothesis it is hardly new (e.g. Albrecht 1989), and observational support can be found in the literature (e.g. Gerber et al. 1996, Yum and Hudson 2002, Pawlowska and Brenguier 2003, Bretherton et al. 2004, Comstock et al. 2004, VanZanten et al. 2005, Wood 2005)."

Our response: We indeed rely on the well established and referenced following links: a) Cloud drop effective radius at a given depth above cloud depth is dominated by CCN

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concentrations. b) Clouds start drizzling and precipitating when the effective radius exceeds a given threshold. Therefore, we conclude that: c) Clouds with a given depth start precipitating when the CCN concentration exceeds a given threshold. We have showed this in Figure 7, based on published data and references.

It is therefore very reasonable to infer the existence of precipitation where we do. Scientific advancement is being done by building on previous steps. We do observe aerosols in Figure 1. We infer the relative excess of aerosols in ship tracks with respect to the ambient environment. This is a very safe assumption. If one is not allowed to make such assumptions in scientific hypotheses the whole structure of observational science as we know it will collapse. We will make our assumptions more explicit in the manuscript.

The reviewer writes: "(iii) By suppressing precipitation, aerosols have a primary impact upon cloud cover by changing the convective state: Again, there are no aerosol data presented in this study, nor are there quantitative cloud microphysical retrievals to support this claim. Their qualitative depiction of cloud droplet size from the satellite data may well be prone to considerable errors in broken clouds (i.e. the open cells) due to thin clouds and partially filled pixels, both of which result in overestimates of the true cloud droplet effective radius (see e.g. Coakley et al. 2005)."

Our response: The inference with respect to aerosols relies on the ship tracks, as explained for (ii). Quantitative cloud microphysical retrievals are presented in Figures 1 and Figure 4. Cloud drop effective radius is a quantitative parameter, whereas the reviewer views it wrongly as qualitative. It is true that retrieved effective radius can be overestimated in thin and broken clouds, but it is quite accurate in clouds with optical thickness > 8 in the visible (Rosenfeld et al., 2004). However, comparing the MODIS products of cloud drop effective radius and cloud optical depth shows that this cannot serve as an alternative explanation in this case. In fact, comparison of the optical thickness to the effective radius of the case study shows that the optically thickest clouds have the largest drop radius, both in the closed and in the open cell regimes.

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A figure of the cloud optical depth will be added to the text to cover this valid concern that the reviewer raised.

The reviewer writes: "(iv) Positive feedbacks are associated with each convective state that maintain the MBL in that particular state: This is certainly not a new hypothesis, and can be traced back to a quantitative modeling study by Baker and Charlson (1990), and the authors do cite this paper. Again however, because the authors do not present any aerosol data, it is difficult to see how the work contributes to its refutation or validation."

Our response: The reviewer is correct by pointing out that the positive feedback leading to the bistatic stability is not a new idea. Indeed, this is reviewed in the introduction of the manuscript. The new idea here is the proposed mechanism that links the various processes that have described in the literature and further documented in this manuscript into a new physically consistent hypothesis, in which everything fits.

The reviewer writes: "The authors discuss that reducing the entrainment rate would lead to a reduction in the entrainment of free-tropospheric aerosols into the MBL and therefore prevent the replenishment the CCN population. Yet the authors present absolutely no evidence that there is indeed a reduction of entrainment in open cellular convection."

Our response: Page 4, lines 15-19 in the manuscript reads: "Most of the mixing with the FT takes place by the mechanism of inverse moist convection into the clouds, which is stronger with larger CF and greater LWP. Therefore, the decreased LWP, CCN and cloud cover reduce the entrainment of FT air into the MBL (Randall, 1980b; Stevens et al.,2005), and with that also reduce the replenishment of CCN from above (Jiang et al.,2002)."

The reviewer writes: "Comstock et al. (2005) find no systematic difference in the MBL depth in open or closed cellular convection from observations over the Southeast Pacific Ocean."

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Our response: But we do not suggest such a relationship, nor should one exist.

The reviewer writes: "Also, what is the positive feedback that maintains the aerosol population in closed cells?"

Our response: The answer is again in lines 15-19 of page 4 in the manuscript. Here they are repeated with changing the sign, for the most possibly explicit answer to the reviewer: Most of the mixing with the FT takes place by the mechanism of inverse moist convection into the clouds, which is stronger with larger CF and greater LWP. Therefore, the increased LWP, CCN and cloud cover enhances the entrainment of FT air into the MBL (Randall, 1980b; Stevens et al.,2005), and with that also increases the replenishment of CCN from above (Jiang et al.,2002).

The reviewer writes: "(v) The transition from closed to open cells has a major impact upon global temperatures: Again, there is absolutely no evidence to support this claim. At the very least, the authors would need to demonstrate quantitatively that the albedo is systematically lower for open cells than for closed cells. In addition, the authors make no attempt to ascertain the area of the globe that may be affected by such transitions. Even if the authors had addressed either of these issues using satellite data, it is no guarantee that the complex array of feedbacks in the climate system would result in a major impact upon global temperatures."

Our response: The sharp increase in cloud cover and optical depth of clouds when transitioning from open to closed cells has an obvious large effect on the clouds albedo. Here we are not interested in quantifying the amount of change, but rather in identifying the mechanism responsible for the transitions between closed cells, open cells and completely cleared areas due to the rainout-cleansing runaway effect.

The reviewer perceives the manuscript to claim that "The transition from closed to open cells has a major impact upon global temperatures" But all that is stated in the manuscript on this subject is: "This huge sensitivity provides a mechanism for large response of Earth global temperature to very small changes in the aerosols in the

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MBL.” In addition, the abstract states, based on that: “This can have a major impact on global temperatures.” Therefore, there is no basis for the concern of the reviewer. We identify a mechanism that can have a major impact on the global temperature. We do not determine the extent by which this mechanism actually incurs that impact, because it is much beyond the scope of this paper, as the reviewer points out.

The reviewer writes: "The transition from closed to open cellular convection, and the potential role that aerosols play in determining cloud cover in general are complex and important issues that are not fully resolved. A number of modeling studies have led to important hypotheses (e.g. Albrecht 1989, Baker and Charlson 1990) that the scientific community now is in the process of confronting with new observational datasets (Bretherton et al. 2004, Stevens et al. 2005, VanZanten et al. 2005, Petters et al. 2005), including detailed in-situ and surface radar remote sensing, aerosol sampling, and satellite measurements. This process requires careful and quantitative observational analyses, and thoughtfully designed modeling studies."

Our response: All is true. This manuscript builds on these recent advances and integrates much of them into a new conceptual model. This manuscript introduces this conceptual model with supporting evidence. Much more work of the community is required to validate the various links in the model and eventually simulate it. This hypothesis will be useful to direct the ongoing observational efforts mentioned by the reviewer.

The reviewer writes: "A manuscript that contains a single satellite image (and an incomprehensible movie) from which no quantitative measurements are derived, does not constitute a novel or worthwhile contribution to the body of literature on this subject. I therefore consider the manuscript to be unsuitable for publication in Atmospheric Chemistry and Physics."

Our response: A manuscript that provides a valid hypothesis does not need to contain any data at all, if it provides new insights to old observations. This is mainly what we do

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here, with a nice example from a single satellite image. This is the beauty of it. When considering the physics of the processes, we do not necessarily need much statistics, when observing the physical processes that manifest themselves almost in any satellite image.

If the movie is incomprehensible to the reviewer it is a technical issue that can be addressed by appropriate annotations. The fact that the reviewer cannot comprehend the movie and apparently much of the manuscript does not mean at all that the manuscript should not be published. Apparently the reviewer did not comprehend the main points of the manuscript, which are: Based on the following observations, most of which are made by others and dully referenced: 1. It is observed that CCN concentrations determine the re of cloud droplets. 2. It is observed that significant precipitation requires re to exceed a given threshold. 3. It is observed that Sc clouds in weakly sheared MBL are mostly arranged in closed or open cellular convection. 4. It is observed that the closed cells have small re and do not precipitate significantly, whereas open cells have large re and precipitate. 5. It is inferred that there are positive feedbacks for maintain both states.

Based on these observations, we propose a new conceptual model that at the bottom line relates the aerosol concentrations to the transitions between closed cells, open cells and clear areas where cloud cannot form due to dearth of CCN.

The manuscript provides one satellite image and one movie that are used as demonstration examples.

If this hypothesis in completeness is not new, the reviewer is required to provide reference that preempts our proposed mechanism in a similar detail level and observational support (including by references). If there is none, and if the reviewer cannot find a strong basis to reject the validity of the proposed hypothesis, it should be published, after making the points that were incomprehensible to the reviewer more explicit.

Additional comments of the reviewer:

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The reviewer writes: "1) Is it correct to call these cellular configurations Benard convection?"

Our response: Yes.

The reviewer writes: "2) I cannot understand what the arrows represent in Figure 2."

Our response: The orange arrows represent emitted thermal radiation (already stated in the figure caption). The thin black arrows represent air motions (will be added to the figure caption).

The reviewer writes: "In addition, the authors present Fig 2 as if this represents a Lagrangian transition from closed to open cells, but one would require images at different times to make this claim."

Our response: It is assumed that the transition front progresses. This is supported by the movie. Therefore, time and space are exchangeable. This explanation will be added to the manuscript.

The reviewer writes: "Further, what evidence is there for the bimodal distribution of cloud heights presented in Fig. 2c?"

Our response: There is no evidence for that. It is merely aimed to indicate a decoupling within the MBL that occurs at that stage. This will be clarified in the manuscript.

The reviewer writes: "3) What do the authors mean by "threshold of drizzle size" (Page 1182, line 8)? The coalescence efficiency of large cloud drops increases strongly with size but there is no clear evidence of any clearly definable "threshold". "

Our response: The drizzle formation rate increases with droplet size so highly non linearly that for all practical matters a threshold size can be used. The references are available in the manuscript.

The reviewer writes: "4) It would be reasonable to argue that the cloud fraction may increase with aerosol optical depth (AOD) simply because there are dynamically-forced

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changes in the cloud fraction that accompany the geographical patterns of AOD (Fig. 1 and Kaufman et al. 2005). For example, over the Eastern ocean basins (off the coast of California, Peru, Namibia), there are many cities, and the removal of aerosols by precipitation events is low. This perhaps allows a greater efflux of pollutants into these subtropical regions in precisely those areas with high stability that favor the formation of low cloud. Thus the cloud fraction can increase with AOD with absolutely no second indirect effect (suppression of drizzle by increased aerosol concentration)."

Our response: There are certainly no big cities in the south Atlantic. The relations are sharpest in the most pristine ocean area, i.e., the south Atlantic. This is so because the transition between closed and open cells occurs already at near pristine background aerosol concentrations.

The reviewer writes: "I believe this methodology to be fundamentally flawed without a better attempt is made to select only cases with a very narrow range of lower tropospheric stability. Simply using "high" and "low" stability, as the authors have done, is insufficient."

Our response: The fact that the effect is replicated in all instability slices at all geographical areas and climate regimes means that it is not related to the instability.

The reviewer writes: "How do they define "high" and "low"? At the very least, the authors should demonstrate no systematic change in stability with increasing AOD by plotting the mean value alongside the other variables."

Our response: The answer is given in lines 3-11 of page 6 of the manuscript.

The reviewer writes: "5) There is no good theoretical or observational basis for the suggestion that cosmic rays are a potentially important process in the pristine MBL."

Our response: This is debatable. The point here is that should there be such an effect, this is the most likely place for it to occur, and this is where it should be looked for. This statement will be added to the manuscript.

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Reference:

D. Rosenfeld, E. Cattani, S. Melani, and V. Levizzani, 2004: Considerations on daylight operation of 1.6 μm vs 3.7 μm channel on NOAA and METOP Satellites. Bulletin of the American Meteorological Society. 85, 873-881.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1179, 2006.

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