Atmos. Chem. Phys. Discuss., 6, S68–S73, 2006
www.atmos-chem-phys.org/acpd/6/S68/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

ACPD
6, S68–S73, 2006

Interactive Comment

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.

Anonymous Referee #2

Received and published: 14 February 2006

General comments

This paper's claims are that (i) there exist two distinct states of convection in the cloud-topped marine boundary layer (MBL); (ii) aerosols suppress precipitation; (iii) by suppressing precipitation, aerosols have a primary impact on the cloud cover by changing the convective state; (iv) positive feedbacks are associated with each convective state that maintain the MBL in that particular state; (v) the transition from closed to open cells has a major impact on global temperatures.

Scientific advances in all fields are made via a common framework in which novel hypotheses are developed and then tested using empirical data. This empirical data, besides supporting or refuting the initial hypothesis, may provoke the need to develop new hypotheses, and so on. Contributions to the scientific literature should therefore contain novel hypotheses or new data/analyses that support or refute existing hypotheses. This manuscript contains neither. I discuss my reasons for this below, by addressing each of the authors' claims separately.

Specific comments on the claims made in the manuscript

(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.

(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).

(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)

(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

ACPD
6, S68–S73, 2006

Interactive Comment

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. 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. 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. Also, what is the positive feedback that maintains the aerosol population in closed cells?

(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.

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. 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.

Additional comments

1) Is it correct to call these cellular configurations Benard convection?

2) I cannot understand what the arrows represent in Figure 2. 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. Further, what evidence is there for the bimodal distribution of cloud heights presented in Fig. 2c?

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”.

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

ACPD
6, S68–S73, 2006

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

References:

Agee, E. M., T. S. Chen, and K. E. Dowell: 1973, A review of mesoscale cellular convection. Bull. Amer. Meteorol. Soc., 54, 1004.1012.

Albrecht, B. A., 1989: Aerosols, cloud microphysics, and fractional cloudiness. Science, 245, 1227-1230.

Atkinson, B. W. and J. W. Zhang: 1996, Mesoscale shallow convection in the atmosphere. Rev. Geophys., 34, 403.431.

Baker, M. B., and R. J. Charlson, Bistability of CCN concentrations and thermodynamics in the cloud-topped boundary layer, Nature, 345, 142-145, 1990.

Bretherton, C. S., and Coauthors, 2004: The EPIC 2001 stratocumulus study. Bull. Amer. Meteor. Soc., 85, 967-977.

Coakley, J. A., M. A. Friedman and W. R. Tahnk. 2005: Retrieval of cloud properties for partly cloudy imager pixels. J. Atmos. Sci., 22, 3-17.

Comstock, K., S. Yuter, and R. Wood, 2004: Reflectivity and rain rate in and below drizzling stratocumulus. Quart. J. Roy. Meteor. Soc., 130, 2891-2919.

Comstock, K., C. S. Bretherton and S. E. Yuter. 2005: Mesoscale Variability and Drizzle in Southeast Pacific Stratocumulus. J. Atmos. Sci.. 62, 3792-3807.

Gerber, H., 1996: Microphysics of marine stratocumulus with two drizzle modes. J. Atmos. Sci., 53, 1649-1662.

Pawlowska, H., and J.-L. Brenguier, 2003: An observational study of drizzle formation in stratocumulus clouds for general circulation model (GCM) parameterizations. J. Geophys. Res., 108, 8630, doi:10.1029/002JD002679.

ACPD

6, S68–S73, 2006

Petters, M. D., J. R. Snider, B. Stevens, G. Vali, I. Faloona and L. Russell, 2005: Accumulation mode aerosol, pockets of open cells, and particle nucleation in the remote subtropical Pacific marine boundary layer. J. Geophys. Res., 111, D02206.

Stevens, B., and Coauthors, 2003: Dynamics and Chemistry of Marine Stratocumulus–DYCOMS-II. Bull. Amer. Meteor. Soc., 84, 579-593.

vanZanten, M., B. Stevens, G. Vali, and D. Lenschow, 2005: Observations of drizzle in nocturnal marine stratocumulus. J. Atmos. Sci., 62, 88-106.

Wood, R, 2005: Drizzle in stratiform boundary layer clouds. Part I: Vertical and horizontal structure. J. Atmos. Sci., 62, 3034-3050.

Yum, S. S., and J. G. Hudson, 2002: Maritime/continental microphysical contrasts in stratus. Tellus, 54B, 61-73.

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