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6, S870–S873, 2006

Interactive Comment

Interactive comment on "Impact of ship emissions on the microphysical, optical and radiative properties of marine stratus: a case study" by M. Schreier et al.

M. Schreier et al.

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General comments:

We would like to thank the reviewer for the detailed and constructive review. According to the change in radiative forcing, the reviewer is referring to the fact, that a decrease of thermal emission by $8 W/m^2$ in ship tracks can not be possible for a liquid water path of 170 g/m^2 . Here we disagree with the reviewer and we would like to explain our position: As can be seen in Fig.13 (Fig. 11 in revised version, respectively), the cloud parameters are not equally distributed over the scene. There are a lot of clouds with low optical thickness, where thermal radiation can be influenced from surface radiation. As one can see via the distribution of Fig. 13, there is no linear correlation between the



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results of radiative calculations of the scene and the mean cloud optical properties retrieved. Making assumption about the radiative behaviour of the cloud, as the reviewer did via the LWP, does not represent the scene that is studied here. To avoid this type of errors in our calculations, we estimate the radiative forcing in the paper by calculating the radiative forcing for every pixel (and not using mean cloud optical properties) - assuming an independent pixel approximation - and from this calculation of the radiative mean values are calculated.

Uncertainties in the simulation of the effect on the thermal radiation - for example assuming a constant cloud top height, parameterisation of absorption, ocean emissivity etc. - is now better addressed in the revised text. However, further investigations of changes in the thermal radiation are necessary, but would go far beyond the scope of this paper. The reviewer has asked to modify the retrieval scheme and to retrieve cloudtop-altitude. However, the retrieval of such low-clouds is not possible in an accurate way. Therefore we have assumed a constant cloud top height for thermal calculations but pointed to the uncertainties in the revised paper. Concerning the second comment regarding radiative forcing calculations, the wavelength-ranges are now explicitly mentioned in the revised paper and it is also pointed out, that the optical properties of the clouds were calculated according to Mie theory.

Other Comments:

1. The the ship-track-mask is slightly depending on the settings of the cloud mask algorithm. A paragraph was added (end of 4.1) to quantify the sensitivity of the cloud mask to the algorithm settings. We point out in the revised version of the text that the automatic detection fits for this scene type.

2. The phrase in the abstract is changed from "poorly studied" to "not entirely understood".

3. The increased backscattering was calculated for the top of atmosphere and in this way it is a loss of energy for the atmospheric system. This results in a reduction of energy available for sensible heat, latent heat, ocean surface temperature and any other

6, S870–S873, 2006

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

factors of the climate system.

4. As pointed out at the beginning, there is no relation such as A=FS + (1-F)N, because of the distribution of the cloud parameters as shown in Fig. 13. Because of the different distributions of optical properties and the non-linearity of radiative transfer calculations, calculation were done assuming independent pixel approximation and then mean values were calculated. We point this out in the error analysis of the ship-track-mask of the revised paper.

5. Han et al., 2002 is included in the reference list, because the paper points out different theories of changes in liquid water path, even the Albrecht effect. But we have added the other references that was proposed by the reviewer. In addition, the reviewer is right, our conclusion was wrong and the sentence is changed.

6. The articles recommended by the reviewer are cited in the revised paper.

7. The article of Coakley is now cited.

8. The retrieval is restricted to pixels with reflectance higher than 0.1, but always assuming a horizontally uniform cloud. Because of the possibility of fractional cloud cover or thin cloud artefacts on cloud edges also the smaller selected scene was analyzed where such artefacts are unlikely to occur.

9. Nakajima and King (1990) showed that optical thickness is weakly affected by the detailed shape of the size distribution.

10. Fig. 7. is replaced in the revised paper according to the first reviewers comments.

11. Fig. 9 is removed in the revised text.

12. The smaller selected scene is provided because it has thick clouds and no cloud edges or other artefacts impacting the cloud mask algorithm. Also, because of the thickness of the clouds, the possibility of uniform clouds with no cloud fraction in the pixels is more realistic. This is pointed out in chapter 3.2. of the revised version.

13. The statistical distribution of Fig. 13 only represents the low-cloud-pixel and there are no cloud-free-pixels used in the analysis. The small numbers of lwp and droplet number concentration result from the cloud edges, why we also analyze the smaller selected scene.

ACPD

6, S870–S873, 2006

Interactive Comment

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Interactive Discussion

Discussion Paper

14. The calculation at cloud edges can result in artefacts. That is, why we also use the smaller selected scene. There seems to be no evidence of changes in liquid water path for the smaller selected scene. Changing the greyscale would not change this fact (see also Fig.11).

15. We did an error analysis of the radiation effects via changes of the cloud mask parameters as proposed (see replies to reviewer 1). We also show a table presenting changes of mean cloud properties when changing cloud mask algorithm parameters.

16. The sentence is changed.

19. The graphs were not changed, as all details explained in the text of the manuscript are clearly traceable from the given distributions.

20. The maximum of the distribution-curve was meant, sentence is changed.

21. Looking onto the STD, one can estimate the non-significance of the liquid water path change.

22. There is no obvious change as in the other distributions. The sentence is changed in the revised paper.

23. The difference can be explained by uncertainty of the calculation when looking onto the standard deviation. Increased backscattering of the cloud can affect absorbed radiation above, inside and below the cloud changing the mean values of surface radiation and backscattering at TOA.

24. It was not the authors intention to claim for a 7.5 hours global lifetime of ship tracks. The intention was to give an example for a typical lifetime time of ship tracks and is taken from another study (Durkee et al.). This is pointed out in the revised paper. 25. The chapter is removed.

26. see above.

27. We point out and study possible misdetection due to surface albedo changes (see chapter 3.2. of the revised paper).

28. Sentence is changed.

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6, S870–S873, 2006

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