Atmos. Chem. Phys. Discuss., 6, S412–S421, 2006
www.atmos-chem-phys.org/acpd/6/S412/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

Received and published: 1 April 2006

This manuscript presents an algorithm for automated detection of ship tracks using MODIS radiances, analysis of a single MODIS scene selected for its abundance of ship tracks, and computations of the radiative forcings for the scene attributable to the ship tracks. The introductory material suggests that the authors expect that the global radiative forcing of ship tracks may be non-negligible, and thus it can be inferred that an automated ship-track detection algorithm with microphysical retrievals could be used to assess the near-global radiative forcing of ship tracks coincident with the daily overpasses of the Terra and Aqua satellites. This manuscript thus establishes an important stepping stone toward such a goal, but a number of issues should be addressed before it is suitable for publication.

Radiative Forcings

Although the single analyzed scene is populated with a great density of extensive ship tracks (unlike the global stratocumulus field), the radiative forcings are of some interest on their own. The longwave forcings they compute are surprising and somewhat puzzling. They compute a reduction in thermal emission that compensates for 20 percent of the increase in outgoing solar flux at the top of the atmosphere, implying that the longwave emission from the ship tracks themselves is reduced more than 8 W/mˆ2 relative to the surrounding cloud, even though they have assumed that the altitude of cloud top is fixed at 1 km for all their radiative computations. This large reduction is unexpected, given that the average liquid water path (LWP) in the background clouds is over 170 g/mˆ2, which should be great enough for the clouds to be opaque for the bulk of their thermal emission. That is, the clouds should be emitting nearly as blackbodies, but by fixing the altitudes of cloud top and cloud base, the temperature of the cloud is presumably fixed. So what is causing the nearly 10 W/mˆ2 reduction in emission for the ship tracks in the authors computations? I recommend incorporating some stand-alone radiative computations to explain their surprising result.

Furthermore, it should be noted that observations during the MAST project provide evidence for increased entrainment of overlying air in the ship tracks studied. Consistent with this, a large number and wide variety of modeling studies (with mixed-layer models, higher-order Reynolds' averaged Navier-Stokes models, and large-eddy simulations) likewise indicate that entrainment increases with cloud droplet concentrations in marine stratiform clouds. Thus, by fixing the altitude of the clouds in their retrievals, the authors are thus likely missing the primary contributor to infrared compensation that presumably occurs in ship tracks (because cloud-top is higher than the surrounding clouds). In large-eddy simulations that I am familiar with, aerosol-induced changes in cloud-top altitude dominate any changes in LWP for overcast stratocumulus (that is, thermal emission always decreases with cloud-top height, regardless of the tendency of LWP changes), though LWP is significantly smaller than the mean value retrieved for the MODIS scene analyzed by the authors. Furthermore, in these simulations the changes in outgoing net longwave flux never exceed 3 W/mˆ2 for a given meteorological scenario with different cloud condensation nuclei (CCN) concentrations. So, if the authors are not representing the primary determinant of longwave emission changes (as far as I can tell), it is unclear why they find any non-negligible large longwave forcing, let alone nearly 10 W/mˆ2 on average.

Given the possibility that changes in thermal emission are primarily driven by changes in cloud-top altitude, is it feasible for the authors to modify their retrieval scheme to use a MODIS radiances in the mid-infrared to retrieve cloud-top altitude? If not, I would suggest that they admit this shortcoming in their manuscript, and qualify their findings correspondingly.

Other issues regarding radiative forcings:

- The manuscript should provide the wavelength ranges that correspond to the authors' classifications of solar and terrestrial radiation.

- Is infrared scattering included in the radiative computations?

- Given the substantial infrared compensation of the solar forcings, it might be mentioned that on a diurnal basis the compensation is likely to be considerably greater, since there are no solar forcings at night.

Other Issues

1. I agree with the first reviewer that the most novel aspect of this manuscript is the automated algorithm for ship-track detection (and I agree with the first reviewer's point that it would appear the algorithm is missing a sizable share of the ship tracks that are evident in the near-IR imagery). But the thresholds used in the detection algorithm are not given. If an objective of this manuscript is to publish an algorithm that others can use, I would recommend that the manuscript list the thresholds that the authors

used. Also, it is stated that the thresholds used in the algorithm have been manually optimized. Would these thresholds need to be manually adjusted on a scene-by-scene basis? If so, it might be noted within the manuscript that the algorithm is not quite automatic.

2. The abstract states that ship tracks are poorly studied, which is unclear. Is the implication that there is little in the way of published research on ship tracks, or that it is the authors' opinion that the research has been poorly done? Although this manuscript cites a small fraction of the literature devoted to the topic (which is not a problem in itself, since the manuscript is not intended as a review article), this under-representation of citations together with this particular statement in the abstract might be interpreted by readers as indicating a scarcity of published research on the subject. The authors should clarify what they mean by poor in this context.

3. The abstract and the main text state that the negative net radiative forcing corresponds to cooling, but no evidence for cooling is provided. Temperature changes in response to all the forcings in the system, and the authors provide no evidence that the only forcings relevant to ship tracks are radiative. In fact they imply just the opposite when discussing changes in latent heat. I would avoid extrapolating the computed radiative forcings to response of the system, and instead would limit discussion to the forcings themselves. Otherwise, I would explicitly note that all discussion of the system response is speculative.

4. It is stated on p. 1035 that 7 percent of the pixels in the scene are classified as ship tracks. But that fraction would seem to be inconsistent with the data presented. For instance, in the abstract (and elsewhere) it is stated that the mean top-of-atmosphere (TOA) reflectance (presumably what is meant by reflectance is actually net flux) is increased by 41 W/mˆ2, while for the whole scene the ship tracks increase the TOA net flux by 2 W/mˆ2. The only way that I can interpret these two as consistent is if ship tracks comprise less than 5 percent of the scene (I would recommend including an clarification of the two sets of forcings given in the abstract, as I was puzzled by the

seemingly contradictory statements when I first read it). Note that a fractional coverage of ship-tracks under 5 percent is consistent with all the radiative flux changes given in the tables, and with the changes in cloud optical thickness given in Table 1. Also, it's unclear why the average LWP and cloud droplet effective radius changes given in Table 1 appear to be inconsistent with those other changes. All the radiative forcings and cloud optical thicknesses are consistent with the same fractional coverage of ship tracks through the expected relationship:

$$A = F\,S + (1-F)\,N$$

where A is the average for all low-cloud pixels, F the fractional coverage of ship-tracks, S the average for ship-track pixels, and N the average for pixels without ship-tracks. But this expression gives F values for LWP and cloud droplet effective radius that are different from each other and different from those for the radiative forcings. The manuscript should explain how the scene-average LWP and cloud droplet effective radius are computed, which will presumably resolve the apparent inconsistencies between F computed for different parameters.

5. It is stated on p. 1027 that Han et al. (2002) "estimate a possible decrease of liquid water in the cloud" as a result of increased CCN concentrations. This statement is misleading on two counts. First, Han et al. did not study aerosol plumes in clouds. Instead they compiled statistics of correlations between retrievals of LWP and column droplet number, which mixes in meteorological changes with aerosol and cloud changes. Beyond the attribution problems associated with meteorological variability, that study does not provide information that indicates the direction of cause and effect (are the aerosols affecting the clouds, or vice versa, or both?), so it is inaccurate to state that the study implies any effect of CCN on cloud properties. Second, Han et al. reported that in roughly one third of their sample, column droplet concentration correlated with an increase in LWP, in another third there was no change in LWP, and in another third the two were anti-correlated. It is misleading to ignore two thirds of their analysis.

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6. In the introductory discussion of LWP changes in ship tracks, I would cite at least three other articles that report measurements of LWP changes in ship tracks: Platnick et al. 2000 (LWP decreased on average in 3 ship tracks observed by airborne remote sensing), Ackerman et al. 2000 (LWP decreased on average in over 60 in situ ship-track interceptions), and Coakley and Walsh 2002 (LWP decreased on average by 15 percent in a sample of hundreds of ship track segments observed by AVHRR).

7. In the paragraph discussing the reason for the visibility of ship tracks in the near infrared, I would cite Coakley et al. 1987, who provided an explanation nearly 20 years ago, rather than only citing a co-author's article evidently reporting the same thing two years ago.

8. Presumably both retrieval methods assume a horizontally uniform cloud, and are unreliable in broken cloud cover and in clouds with sub-pixel variability. Are such pixels included in the retrievals, or are they identified and omitted? If so, how are they identified, and what fraction of each sample of pixels to they represent?

9. From the discussion on p. 1032 it would appear that the authors assume that the dispersion (std. dev. divided by the mean size) of the cloud droplet size distributions is independent of cloud droplet number concentration. This would seem to be a shortcoming of their method, since there is considerable evidence that indicates the dispersion increases with cloud droplet number concentration, on average. How sensitive are the retrieval results and radiative forcings to what seems to be an unrealistic assumption? Alternately, how do the retrievals and radiative forcings change if the dispersion is allowed to increase with cloud droplet number concentration in accord with observed correlations?

10. Why aren't the changes in channel reflectances shown in Fig. 7 extended to cloud optical thicknesses below 5, where the look-up table method is used?

11. What is the point of Figure 9 and its brief discussion on p. 1034? I would either expand upon the point with some quantitative analysis, or just omit figure and associated

text.

12. It is stated on p. 1036 that the smaller selected area allows a more detailed analysis of the ship tracks. Unless I missed something, there is no more analysis of the smaller selected area than there is of the entire scene, so I don't understand the statement to the contrary. Also, some description of how the smaller area was selected should be provided.

13. Why are there so many pixels without cloud water or cloud droplets in the distribution of non-ship-track pixels shown in Fig. 13? I would not think it even-handed to compare cloud properties of a sample that includes cloud-free areas with a sample that does not include them. Would it not be more even-handed to compare only cloudy pixels, and simply report the number of non-cloudy pixels separately? If the implicit point is that the ship tracks don't include any cloud-free pixels, might that condition be guaranteed by the detection algorithm itself, rather than the properties of the actual clouds? As is stands, the scene could include cloud-free areas far from the ship tracks, which would reduce the mean LWP of the non-ship-track pixels, giving the appearance of a LWP increase in the ship tracks (as seen in Table 1), but I'm unconvinced that there is any physical significance to such a LWP change. The logic regarding sample selection needs to be explained better, and possibly revised.

14. It is stated on p. 1036 that because the increase in ship-track LWP is not obvious in the grey-scale plots, the change in LWP is uncertain. I don't know how to interpret this statement. Would not a different choice of grey scale allow the LWP to be obvious, given the 30 percent increase in the mean LWP shown in Table 1? Also, I would think that the choice of grey scales as they relate to the prominence of changes between samples would not lead to uncertainty if the numerical analysis of the pixels shows a statistically significant change. The authors' uncertainty in this regard should be explained more clearly. Is the uncertainty related to the fact that they don't believe their detection algorithm is working as expected, the sample selection is misleading (see point 14, above), or what?

15. Regarding uncertainty of analysis, I would think it would be worthwhile to include an assessment of the statistical significance of the differences between the ship tracks and the low-cloud pixels without ship tracks, particularly regarding the LWP changes. For instance, on p. 1037 it is claimed that the change in LWP in the ship tracks is not significant, but there is no analysis of statistical significance provided. How statistically significant is the 30 percent increase in the mean LWP?

16. It is stated on p. 1037 that Albrecht 1987 and Han et al. 2002 indicate an influence by emissions on the lower part of the cloud. Where does this notion come from? Neither of those articles were studies of ship tracks. Also, Albrecht was studying trade cumulus clouds, not stratocumulus, and ship tracks aren't seen in the former. Also, Albrecht's model has only two layers – sub-cloud and cloud, and thus cannot distinguish between lower and upper parts of a cloud. Furthermore, Han et al. 2002 used satellite retrievals for their entire analysis, and like the present study, those retrievals are generally biased toward the top rather than the lower part of the clouds.

19. I would normalize the frequency distributions in Fig. 13, making it easier to compare and contrast two samples with very different sample sizes, as information about the relative sizes of the two samples can be given elsewhere. It's also unclear why a logarithmic scale is used, since the manuscript seems to be largely focused on the general trends (rightly so), not the tails of the distributions.

20. It is stated on p. 1037 that the cloud droplet number distribution shows a shift of the maximum from 70 to 160/cc, but the maximum values increase from 400 to 800/cc, according to Fig. 13. I'm guessing that the authors meant to refer to the modal rather than the maximum values of the distributions.

21. It is stated on p. 1037 that the small change in LWP for the smaller selected area is not significant. It would help to back this up with a quantification of the statistical significance of the change.

22. It is stated on p. 1037 that the distribution of LWP in the smaller selected area does

not change, but the ship-track LWP distributions in Fig. 13c appear to be different from those for the pixels not in ship tracks, so it's unclear what the authors mean.

23. Why are the shortwave forcings greater for the entire scene than for the smaller selected area at the TOA, but reversed at the surface? The cause of this reversal is not immediately obvious to me (and may not be so obvious to others, either). I recommend that the authors provide an explanation in the text.

24. It would seem to be an over-generalization to state without qualification that Durkee et al. 2000b found an average (mean or median?) lifetime of 7.5 h for ship tracks. That lifetime is for a very limited area for one month of one year, and it should not be implied by the authors that the average lifetime of that sample of ship tracks is representative of the global population of ship tracks.

25. It is claimed on p. 1041 that drizzle only occurs when cloud droplet effective radius exceeds 14 micrometers in stratocumulus. However, the study of van Zanten et al. 2005 have evaluated this hypothesis with field measurements, and find an absence of any evidence for a monolithic, simple threshold of droplet effective radius associated with the presence of drizzle.

26. It is claimed on pp. 1041 and 1043 that the latent heat associated with drizzle reaching the surface should be considered when assessing the role of ship emissions on the atmospheric energy budget. I can understand the high-profile role of possible changes in latent heat in heavily precipitating convective systems, but not in lightly drizzling boundary-layer clouds that are tightly coupled to the surface. In such clouds, surface fluxes of heat and moisture (beyond drizzle itself) and entrainment rates also vary in response to changes in drizzle. I would not lean exclusively on latent heat changes in such a system when discussing responses of the system beyond those induced by radiative forcings. The same problem is repeated in Section 5.

27. It is claimed on p. 1042 that it was demonstrated n this study that ship tracks in otherwise clear sky conditions have to be examined with care. Instead, the point was

just asserted without any analysis to support it.

28. It is misleading to state on p. 1042 that clouds that form in air with fewer CCN, or even those with fewer cloud droplets, necessarily have larger droplets. There are a number of other assumptions regarding cloud water content and the dispersion of droplet size distributions implicit in such a statement, and I would explicitly state those assumptions.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1023, 2006.