Anonymous Referee #2

Dear Referee #2,

Thank you very much for your effort and your work on our manuscript! Your helpful comments and useful suggestions contribute to the scientific quality of the publication. More detailed answers to the major and minor concerns are presented below. If nothing else is written about the changes that have been performed we refer to the original version of the manuscript.

General comments: The paper is in general well written and addresses an quite important topic. However, there are major items to be resolved, which affects conclusions and value of the paper significantly.

Please find attached my comments (all major)

page 2014, line 3 : This sounds as bit strange as nowadays satellites provide cloud information in a high tempo-spatial resolution. However, the statement might be true as a stand-alone message but does not motivate why satellite data is not used. Either the micro-scale effects cancels out for spatial averages (e.g. satellite pixel size) then there effect on the climate should be expected to be minor, or they do not cancel out, then it could be expected that the effect is somehow observable by satellites (simply as a result of the law of energy conservation). More over, the in-situ measurements are used for the validation of ECHAM which has a much coarser temporal resolution than satellite data, e.g. from MSG. Especially for the validation of climate model output insitu data have the great disadvantage that they measure pointwise, whereas satellite data can be averaged to the climate model grid, which makes the data much more comparable. I think it should be discussed in more detail why satellite data are not used for these study or the sentence should by simply deleted. However, the disadvantages of point measurements (e.g. lack of spatial coverage) for climate studies should be discussed in more detail in any case.

Reply: *Thank for your comment. We agree that this sentence is a bit misleading, because no satellite data has been used for this study. Therefore I deleted the lines 3-5.*

Cloud satellite information is weak, especially for cumulus, but those small clouds cause shortterm radiation enhancements.

Fast fluctuating effects cancel out in satellite data and their impact on climate might be minor, but those short term and small scale fluctuations play a major role for biological and chemical processes. Satellite data include neither fast fluctuations nor small-scale spatial variations.

We have revised the passage and included the citations of Marra (1978) and Walsh and Legendre (1983) to underline the importance of fast fluctuating radiation fluxes and their impact in biological and chemical processes. We added an indication that in-situ measurements are point measurements.

page 2016: The approach for the clear sky radiation is a bit outdated and quite limited. The effect of variations in aerosols on the transmission is not considered by the model as a fixed amount of aerosols is assumed everywhere (only variation in H20 are considered). This is a significant drawback and corrupts the reliability of the estimated quantities of clout radiative forcing/effect which is the core of the whole study. The differences in the clear sky irradiance induced by variation in aerosol optical depths exceeds 100 W/m**2 along the ship tracks for AM2 (as it e.g. passes regions affected by desert storms and biomass burning). As these variations are not considered by the simple clear sky model conclusions on the resulting estimates for CRE might be quite misleading. As several easy to handle & free available clear models are available which performs much better, the motivation for the application of this limited model is quite unclear and unnecessary. Unfortunately, the use of this model is a quite essential limitation. In my opinion, the manuscript can only be accepted for publication if the

effect of aerosols is taken into account appropriately in a revised version of the paper. This can be done by the application of a sophisticated model, or perhaps, by the use of the observed clear sky irradiance (or both).

Reply: *We agree with the referee that a differencein the clear sky irradiance induced by extremely high aerosol optical densities can reach about 100 W/m². But nevertheless, this is an extreme scenario that occurred during the transsects at Saharan dust events only.*

For more clearly results we have recalculated the results given in Tab. 3 and Tab.4 without taking into account the days of Saharan dust events. This should minimize parameterisation errors and clarify the impact of large variations in aerosol content.

From the original datasets we have excluded following dust events: 22.-23.04.2007 (ANT-XXIII/10), 07.-09.11.2007 (ANT-XXIV/1), 05.-08.05.2008 (ANT-XXIV/4), 06.-09.05.2009 (ANT-XXV/5) and 31.10.-01.11.2009 (ANT-XXVI/1). We revised the given results in Tab.3, Tab.4 and the corresponding Fig. 3 to Fig. 6. We revised the given CREs in the text as well and added a listing of the excluded dust days. The differences without the dust events are small and to not lead to different physical results for the individual cloud type classes. The largest difference after the recalculation occurred for the SW CRE for Stratus/Altostratus (class 6): without the dust impact its value is -319.9 W/m² instead of -293.5 W/m²(before the recalculation). The mean net CRE changed from -33.5 W/m² (before recalculaton) to -32.8 W/m².

Note that in Tab. 4, class 7 was a typo for the delta F (CRE_LW): it was supposed to be positive, not negative.

For the remaining dataset without Saharan dust events the variations in aerosol optical densities are small (AOT at 550nm 0.1 to 0.18, see citations Tab. 1). The impact of the mean background aerosol has been taken into account when creating the transmission equation (Eq. 2). Because of that it is suitable to use the given parameterization of the surface clear sky insolation.

p 2016, line 14-20: In general, comparison at one station is not enough to provide reliable validation of a model. The statement (line 17) concerning the performance of the model are not proofed by validation and quite misleading. More over, the "mean" aerosol state at Sylt is quite different to that occuring along the ship tracks, hence, the comparison performed at Sylt are by no means representative for the current study.

Reply: *We agree that the validation (line 17) is not proofed by a peer reviewed paper. That is why we revised the passage and deleted the link to Sylt data. It was added that the impact of the mean maritime background aerosol on the transatlantic expeditions onto the transmission estimation was included.*

But nevertheless, Sylt is a maritime island with mean aerosol optical densities at clear sky that do not differ significantly from mean aerosol optical densities at the Atlantic. The composition might be different with more anthropogenic aerosol on Sylt. But a study of the aerosol composition is beyond the aim of this paper. For the mean aerosol optical densities at the transsects of ANT-XXIV/4, ANT-XXV/5 and ANT-XXVI/1 see citations in Tab. 1.

Table4 and other places: CRE is not measured but calculated, partly based on measurements,

yet also affected by simple clear sky model approach. The term measured

is therefore wrong and should not be used.

Reply: *We agree with the referee and changed "measured" into"observed". We changed it in Table 4, Fig. 5, Fig. 6 and the corresponding text passages as well.*

p2026, line 6: The results seems not to provide a clear evidence for the conclusion that "climate model is in general not able to ... due to broken clouds". Would it be possible that the climate model fails to model clouds (hence CRE) appropriately in general.

Reply: *The design of the model does not enable a surface insolation which is higher than the clear sky insolation. Therefore even for any kind of model clouds the SCM would not simulate the broken cloud effect. It would always simulate a mean insolation which is (due to the mean cloud transmission) lower than the clear sky insolation – even if you tune the model to much higher temporal and grid resolution.*

Conclusions: no meridional dependency, wrong result/conclusion due to the missing consideration of variations in aerosol contents along the tracks ?

Reply: *The recalculation of the dataset without extreme high aerosol contents has not shown significantly changes in the results. The parameterization of the surface clear sky insolation did include the mean background aerosol. Therefore one can not expect, that the remaining micro-scale variations around the mean aerosol content might affect the calculated cloud radiative effects significantly.*

An explicit aerosol characterization and study is beyond the aim of this paper.