Anonymous Referee #1 Received and published: 19 January 2020

General

The paper presents a modelling study on the direct aerosol effect on climate. The authors distinguish between clear and cloudy skies. The approach is probably state of the art although, however, to my opinion, a very simple one. Let me start with my general impression: We have satellite lidars delivering global 3-D aerosol distributions (profiles!) with detailed aerosol typing (in terms of optical, microphysical and even chemical composition and thus refractive index characteristics) around the globe from the surface up to stratospheric heights and also producing 3-D distributions of clouds layers, their thermodynamic phase, frequency and cloud cover. In addition, we have sophisticated passive remote sensing techniques, again, delivering very detailed information on cloud layering, cloud heights, cloud types, cloud cover, and thermodynamic phase. In view of all the available and complex global 3-D cloud and aerosol data sets, I am a bit surprized that teams of modellers still use rather simple approaches (here Eq.(1)) to investigate and estimate the role of aerosols (natural and anthropogenic ones) in the climate system with the goal to answer the very important and 'ultimate' question: What is the contribution of anthropogenic aerosols to climate change? Even if global MODIS column information on AOD (and maybe cloud occurrence and cover?) is included in the study, . . . is that sufficient to obtain a realistic picture on aerosol effects on climate? The global aerosol distribution (profiles) used in this manuscript is rather simple so that question arises: Does the modelled global aerosol climatology really reflect the real world?

Response: To the authors it is unsure whether the Reviewer has understood the advanced global modelling and approach applied in this study. It is incorrect that we use Eq 1 to investigate anthropogenic and natural aerosols, our application of Eq 1 is used for anthropogenic aerosols. However, the global modelling includes natural aerosols and simulations of aerosol vertical distribution on a high temporal scale. The Reviewer mention a large set observational data the authors are aware of, but the apparent misunderstanding by the Reviewer is that these observational datasets only provide the present aerosol abundance. E.g. we have already referred to two studies comparing the AeroCom models against CALIPSO in the discussion (Koffi et al., 2016; Koffi et al., 2012). In this study we have the aim of investigate anthropogenic aerosols and the observations cannot provide a clear distinction between anthropogenic and natural aerosols and therefore model information is required. We refer to several studies using observational studies to estimate the cloudy sky RF, and we mention they have large limitations not only on abundance, but also natural and anthropogenic aerosols have large differences in aerosol optical properties. After presenting the earlier studies using observations, we have noted the following: '_Note that the above-mentioned studies investigate the present, total aerosol abundance which consist of anthropogenic and natural aerosols, whereas in terms of RFari only the anthropogenic aerosols are considered_**'. In the beginning of the introduction we have now underscored that the estimate of RF is for anthropogenic aerosols. We disagree with the Reviewer that our approach is simple, even though Eq is simple.**

Maybe, there are meanwhile modelling groups and thus papers in which the measured global aerosol distributions and measured global cloud distributions are used to model the impact of aerosols on global climate conditions, and these authors here just want to offer an alternative way, a more simple, rather basic approach to estimate the aerosol effects on climate? Maybe that is the reason for this simple paper but at the end the main question is still: Can we believe in these results when such a simple approach is used?

Response: see above

And, are you sure that you cover the full spectrum of anthropogenically caused aerosols. What about all the dust in the atmosphere especially over Central and East Asia, is that all natural? Clearly: NO! But how to consider that in the model? Did you consider that in the simulations? Probably not! The paper is worth to be published, no doubt! The list of authors is full of well-known experts, and the paper is a valuable contribution to the climate debate, but the authors should at least try to provide some answers to my concerns. Yes, maybe I am 'naive' . . . as an experimentally working specialist for aerosol and cloud profiling, and my comments indicate that I am not familiar with the modern modelling world but I am probably not the only one who has trouble with the concept and content of this paper. Maybe, I completely missed the point and the overall message of the paper, but again, I will be probably not the only one. So, we need a more critical discussion on the paper approach itself in this paper.

Response: We agree that anthropogenic dust aerosols are not included in the models applied in this work and only a very few in current state of the art models. We have therefore added the following sentence in the discussion: 'All the global models that supplied simulations for this study treat the major anthropogenic aerosol components sulphate, organic aerosols, and black carbon, some also treat nitrate, but none include anthropogenic dust aerosols which have highly uncertain radiative effects.**'**

Details:

P2, l40: Bellouin et al. . .. this is obviously not a publication, there is no year of publication, nothing. So, that is not an acceptable statement. Please improve!

Response: This paper is now published, and we have now included a complete reference.

P2, l50: . . . biofuel BC emission inventory is much higher than used in previous global modelling . . .. Bad wording? What do you want to say?

Response: Sentence rewritten.

P2, l62: Eq (1) is the most basic (trivial) approach, right? Or is there even a more simple one? On the other hand, the atmospheric system is so complex, and modern instrumentation fill the aerosol and cloud data base since 20 years, continuously. You seem to ignore all this! You separate (anthropogenic) aerosol particles in absorbing and non-absorbing ones, nothing else. Is that sufficient? You introduce AC as cloud fraction! Obviously it doesn't matter whether we have one layer, two layers, three layers of clouds, whether we have liquid-water clouds, mixed-phase clouds, cirrus . . .or even complex cloud mixtures and layering, and it is also not essential whether the aerosol is below the lowest cloud layer, between the different cloud layers, etc. . . Just one parameter is sufficient: AC! For the entire globe! For rather different climate zones? One AC value everywhere. . .? This is quiet a surprizing and 'universal' assumption. The other way around, what did I miss here? Please clarify, other readers (not familiar with climate modelling) may think the same. . ., may have the same problem with the paper. Maybe all the referenced papers show that it is sufficient to have just AC to describe the impact of clouds on the aerosol radiative effect around the globe from the tropics to the poles.

Response: All the global models simulate the spatial variation in the vertical profiles of aerosols and clouds and their composition and optical properties. We underscore that the simulations are not only done for a layer, but the study is based on complex global aerosol modelling. See further comments in the response to the main comments. When determining the radiative forcings of

RFcloud, RFclear etc, all model grid boxes are utilized, where the vertical distribution of the clouds and different aerosols and their optical properties will affect the radiative forcing calculations.

P3, l70: aerosols above clouds, below clouds. . . Only these two scenarios, not more are need to be modelled and considered? . . . although the world is full of complex aerosol and cloud layering. . . and large areas over the oceans downwind of polluting continents in the northern hemisphere . . . are 'affected' by this complex layering?

Response: This is clearly a misunderstanding by the Reviewer, it is certainly not only aerosols above or below the cloud. The models applied in this study have between around 20 to 60 vertical layers in the atmosphere. See further comments above.

P3,l93: When using Stefan Kinne's aerosol climatology, did you at least check how good the agreement between CALIPSO aerosol profile observations (in combination with MERRA and CAMS simulations) and Kinne's aerosol climatology is? I speculate: Yes, you did that! My 'spontaneous feeling' is that this quiet simple aerosol profile climatology is not in good agreement with the real world. So, please comment on this!

Response: In the MACv2 climatology the distribution of AOD (where the monthly local statistics of AERONET/MAN corrects the multi-model median of AeroCom phase 1 maps on a regional basis) a distinction is made for AOD from coarse particles (dust, seasalt) and AOD from fine-mode particles (pollution, wildfires). To approximate the aerosol vertical distribution (via vertical scaling of the local monthly column AODc and column AODf data of the MACv2 climatology), scaling factor from 20-year averages from ECHAM5-HAM model simulations are applied. Hereby a single model (ECHAM) was chosen (over a global model median) because in tracer studies and in comparisons to CALIPSO data (paper by Sarah Guibert) this model behaved very well and was not so 'vertical transport-happy' as many other models in that comparison.There was a consideration to replace the vertical aerosol distribution of aerosol with (more observational) data from Calipso. That this has not been included (so far) as CALIPSO data put more aerosol closer to the surface even in comparison to CAMS (e.g. in dust-outflow regions). More importantly CALIPSO data cannot distinguish between fine-mode AOD and coarse-mode AOD. Note than for the study of anthropogenic aerosol only the fine-mode AOD is relevant as anthropogenic aerosol is predominantly an added fraction to the fine-mode AOD. The aerosol vertical distribution also needs to be seen in the context of the cloud altitude placement (The MACv2 climatology distinguishes between high mid and low level clouds, where low clouds are near 1km above the ground, mid-level clouds at ca 3km above the ground). Since random cloud overlap (clouds at 3 altitudes require 8 separate simulations for each permutation) is assumed the cloud-free fraction in MACv2 is on average only at 30%). In MACv2 there is a significant fraction of for optically thin high-only cloud fraction, which may explain a relatively negative forcing for cloudy skies in the comparison. The model description is updated in the manuscript to include information on the vertical profile as follows: *'The Max Planck Aerosol Climatology (MACv2) method combines aerosol column optical properties for fine-mode and coarse-mode sizes (of an AeroCom phase1 model median regionally adjusted by AERONET/MAN monthly statistics) with MODIS surface albedo data, ISCCP cloud properties and vertical scaling by size-mode from 20 years of ECHAM-HAM aerosol simulations. The anthropogenic properties is defined as a fraction of the fine-mode, where the fine-mode AOD scaling factor prescribed from AeroCom phase1 simulations.'*

I would suggest to include a figure with a sketch of your basic aerosol-cloud scenarios considered in the model. Show a cloud layer (provide information on the cloud height, then visualize AC, that means, the cloud should not cover the full sketch from left to right, and then indicate aerosols (just a mixture of black (absorbing) and yellow or white points (non absorbing particles). Scene 1: aerosol below the cloud, Scene 2: aerosol above the cloud layer, Scene 3: aerosol in the clear part of the sketch, if there are more scenes in the model, please continue with further scenes. . ..

Response: The model simulations are complex with (multiple grid boxes and) multiple vertical layers with clouds and aerosols of different properties found at different height, all of which varies with time and geographical location. Since the Reviewer has misunderstood that we've just do simulations for one cloud layer (see above and the comment below), we refrain any further response to this comment.

P5, l127: Result section: My only one question . . . throughout this section. . . was at what height is the cloud layer (for which we have a fixed, constant AC)? Obviously you only consider liquid-water clouds in the lower troposphere. A cloud layer at, e.g., 1 km height (boundary layer top) almost everywhere. . .. around the globe. Maybe it is stated somewhere and I missed it unfortunately. But what about the impact of all the midlevel cloud fields (partly glaciated. . .) and the extended subvisible cirrus fields around the globe. . . , no impact on the aerosol related radiavtive effects?

Response: Again, the model simulations contain complex treatments of clouds at all altitudes around the global and this reviewer comment is a bit off mark. We have included a sentence in the Result section making it clear that although aerosols are found to have a large effect when located above low clouds, all placement of different aerosols types in relation to cloud are treated in the models, be it above, within or below clouds, for different cloud types (low, mid and high, liquid, mixed and ice).

The rest of the paper sounds ok (consistent) . . .. for a non-modelling atmospheric scientists traveling around the globe and measuring the rather complex world of clouds and aerosols in regions with very high amounts of haze and dust (which is partly triggered by human activities) and partly complex aerosol layering up to the tropopause, . . . and, in contrast, in very pristine areas with simple cloud and aerosol layering as in your model.

My 'basic' comments may be confusing but the goal is to improve the paper, not to destroy it.

Koffi, B., Schulz, M., Bréon, F.-M., Dentener, F., Steensen, B. M., Griesfeller, J., Winker, D., Balkanski, Y., Bauer, S. E., Bellouin, N., Berntsen, T., Bian, H., Chin, M., Diehl, T., Easter, R., Ghan, S., Hauglustaine, D. A., Iversen, T., Kirkevåg, A., Liu, X., Lohmann, U., Myhre, G., Rasch, P., Seland, Ø., Skeie, R. B., Steenrod, S. D., Stier, P., Tackett, J., Takemura, T., Tsigaridis, K., Vuolo, M. R., Yoon, J. and Zhang, K.: Evaluation of the aerosol vertical distribution in global aerosol models through comparison against CALIOP measurements: AeroCom phase II results, Journal of Geophysical Research: Atmospheres, 121(12), 7254-7283, 2016.

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