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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloudy sky contributions to the direct aerosol effect" *by* Gunnar Myhre et al.

Anonymous Referee #1

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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

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Discussion paper

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?

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?

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

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approach itself in this paper.

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!

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

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.

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?

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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!

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. . ..

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?

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.

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