Response to Editor's comments, 8 May 2015

Dear Editor,

We thank you for your most recent comments and address them line-by-line below.

The Editor comments are left in bold font for ease of reading.

Yours faithfully,

Dr Liang Guo, Dr Andy Turner & Prof. Ellie Highwood.

Editor comments

Your answers to the reviewers comments are often deflecting the improvement that the reviewers asks you to include in the paper. You cannot just answer with general comments without making the requested changes or add-on to the paper.

Let me give you 3 instances where I found your replies not precise or not pointing to the changes requested. Please point to me to the exact changes made in the text.

We thank the Editor for pointing this out and realize our previous response may have been lacking in detail, particularly in its pointing to changes made in the manuscript.

However, some of the points raised by reviewers are points of argument. Merely making changes or additions to the manuscript to satisfy the reviewers, who are not infallible, does not further the scientific process. Co-authors Turner and Highwood are current and former Editors of QJRMS and ACP respectively, so we are well aware of the pros and cons of the peer review process.

In the below responses we will highlight explicitly where we have made existing changes, where we make new changes following this round of review, and also where we believe the reviewers are asking us to make changes that are not supported by evidence.

We look forward to further discussions surrounding this manuscript. We also note that we thought that these responses had been satisfied at the previous stage of peer review (notified to us 7 April, containing at that stage only the Editor comments described at the end of this document). Nevertheless, we feel it is important to get the paper right.

For clarity, we present each response starting on a new page.

First instance:

Recent study showed that the CMIP5 models fail to simulate the post-1950 decreasing trend of monsoon rainfall (Saha et al., 2014). The failure of representing large-scale changes caused this issue in most of the CMIP5 models. Only 8 out off 48 CMIP5 models used is able to capture the monsoon rainfall features (Saha et al., 2014). This indicates that skill of simulating Asian summer monsoon is very poor in most of the CMIP5 models. Authors need to comment if models are poor in simulating essential features, how much reliable the model-simulated impacts of 20th century aerosol emissions on rainfall trends.

We regard this as a contentious point and poor logic. We have read the Saha et al. (2014) study in detail. It is a short GRL paper available as DOI: 10.1002/2014GL061573. It categorically does not perform assessments of the skill of simulating the Asian monsoon in current climates compared to observations. It does not assess the mean monsoon rainfall (mm/day over India in the 20th century). It does not assess the simulated pattern of monsoon rainfall in CMIP5 models (e.g. with pattern correlation metrics or RMSE for maps of the Indian region). It does nothing to compare the seasonal cycle of monsoon rainfall with observations (the most fundamental aspect of the monsoon) or of the ability of these models to represent the monsoon circulation. Thus it is not correct to state that Saha et al. (2014) have assessed the skill of monsoon simulation in the CMIP models [as in comprehensive studies such as Annamalai et al., 2007 or Sperber et al., 2013, that we already cited (admission: Turner is a co-author on Sperber et al.)]. We therefore contend that the reviewer's comment, *"This indicates that skill of simulating Asian summer monsoon is very poor in most of the CMIP5 models"* is disingenuous.

What Saha et al. (2014) *have* done is to assess the trend of monsoon rainfall in the CMIP5 models. They have then assessed the simulation in those CMIP5 models of two hypothesized SST-forced means of reducing monsoon rainfall. One originates from an Annamalai study that showed in a single GFDL model that warming and associated rainfall with diabatic heating over the Western North Pacific led to Rossby-forced descent over South Asia (admission: Turner reviewed that Annamalai study). The other mechanism relates to more rapid warming of the southern Indian Ocean and its impact to weaken the large-scale tropospheric temperature gradient and thus the monsoon. There may be some merit in the Saha et al. (2014) study suggesting that CMIP5 historical simulations cannot represent warming processed in the Southern Indian Ocean or the Western North Pacific (although even this has problems) but by itself that says nothing about the monsoon simulation (using measures of the monsoon skill as we describe above). Since CMIP5 historical simulations are uninitialized (not begun from well-observed ocean conditions) then they will not capture the correct phase of well-known decadal time scale modes of variability (e.g., the PDO or AMO, that are known to alter the monsoon). Nor will the phase of these modes of variability be the same between different CMIP5 models. Therefore the monsoon in a CMIP5 model may be responding correctly to whatever SST trend manifests in that model. We do not know, since Saha et al. (2014) don't assess it. We believe the closing sentence in Saha et al.'s abstract, *"Proper representation of these highlighted geophysical processes in next generation models may improve the reliability of ISMR projections"* is therefore erroneous, stated as if Western North Pacific or southern Indian Ocean warming can simply be better parametrized in a model.

The latter part of this reviewer's comment then asks how reliable we can regard the trends in 20th century CMIP5 simulations, given that Saha's measure of skill is itself the trend. We now address this in an amended form of our original response to this comment.

In order to interpret any trend, we must consider its drivers, and if those drivers are being successfully simulated. There is little doubt that in idealised future scenarios to the end of the 21st century (such as doubled CO2 concentrations), or even the IPCC-style economics scenarios (such as SRES or now the RCP), monsoon rainfall is shown to increase (e.g. see the review in Turner and Annamalai, 2012, but also by many others and in both IPCC AR4 and IPCC AR5 reports). Inherent in these scenarios is the dominance of greenhouse gas forcing and relatively low aerosol: doubled CO2 scenarios involve no change in aerosol; the RCP such as RCP4.5 or RCP8.5 show much reduced emissions of aerosol by the end of the 21st century over India, China and much of the globe.

We also have good theoretical arguments to link warming with increased moisture supply to the monsoon. This has been covered extensively in IPCC AR4 and AR5 and relates to the warmer Indian Ocean yielding a lower atmosphere that can hold more moisture, that is advected over India and results in enhanced moisture convergence (and thus rainfall) over India in a warmer world.

The 20th century differs from these future scenarios, since there is also a strong radiative forcing from aerosol emissions. Such aerosol emissions are strong locally (India) and to a lesser extent in the northern hemisphere as a whole.

The suggestion is that anthropogenic aerosols, particularly scattering aerosols such as sulphate, are acting to counter the effects of greenhouse warming over South Asia, and even override them. That sulphate aerosols can do this to the monsoon is not a new idea: see the cited works of Bollasina et al. (2011) for India, or of Polson et al. (2014) for the northern hemisphere monsoons as a whole, for example. Emissions and burdens of sulphate aerosol over South Asia and the northern hemisphere have increased strongly since the mid-20th century.

An outcome of our paper is that it is only the CMIP5 models that most faithfully represent aerosol processes (including both direct and indirect effects) that can capture the observed declining rainfall trend. Without the indirect effect, a model can thus not truly represent the full magnitude of negative radiative forcing. Including the indirect effects therefore gives more realistic physics. Our own analysis of other studies such as Ramesh and Goswami (2014) and Saberali et al. (2014) suggests that the few models that do reproduce the observed trend *do* contain indirect effects of aerosol.

Given the flaws we feel are in the reviewer's logic and in the Saha et al. (2014) paper, as we have now extensively outlined above, we made no additional changes to the manuscript in our original response to the first round of reviewer comments (on the system as file "acpd-14-C12554-2015-supplement.pdf").

In response to these latest comments we add some brief additional sentences to the new manuscript in order to discuss the reliability of trends and to acknowledge Saha's work. We stand by our earlier view that to make further changes would be inappropriate.

We now insert the following sentences at lines 113 on page 4 of the introduction: "Various authors have questioned the ability of CMIP5 models to capture observed monsoon rainfall trends over India (Saha et al., 2014) and therefore the reliability of CMIP5 at making projections of future monsoon rainfall (Ramesh and Goswami, 2014; Sabeerali et al., 2014).

In our study we shall demonstrate that it is only when aerosol indirect effects are included, and therefore when more physical processes are being represented, that these models are able to capture the observed trends.”

Furthermore, we would point out that in the conclusions of our paper (line 398 on page 18) we have already included the sentence, “While we generally have more confidence in our models if the present-day simulations perform well at simulating the mean monsoon, its seasonal cycle, and variability (Turner and Annamalai, 2012; Ramesh and Goswami, 2014; Sabeerali et al., 2014), a key novelty of our study is that it is only the models containing aerosol indirect effects that can reasonably be expected to represent the observed trend.”

Instance 2:
In the Conclusion section, authors discussed about the increases in monsoon rainfall in future CMIP5 projections. Sabeerali et al. (2013) found that most models produce too much (little) convective (stratiform) precipitation compared to observations. Is a too strong aerosol effect causing this issue? Better to add remarks on this issue in current context of the work. Whether aerosol indirect effects are enhancing the convective rainfall in the model?

We thank the Editor for again bringing this comment to our attention. This time we have opted to add an additional sentence to discuss this, but again feel that further attention to one study is inappropriate. The Saberali et al. (2013) study suggests that models, in a future warmer world, produce too much extra convective rainfall too readily with warming.

However, do note that other studies than the one that the reviewer recommends do not find consistent conclusions around the aerosol or aerosol indirect effects and their effect on convective rainfall.

For clarity, we repeat here our response arguing against the reviewer's comment (an amended version of that contained in our original rebuttal file "acpd-14-C12554-2015-supplement.pdf") and then mention the sentence we have added and its location.

We note that future projections of increased monsoon rainfall are not an outcome of this study, but a reference to a considerable body of reviewed work (e.g. Turner and Annamalai, 2012, and IPCC AR4 and AR5). While it is possible that increased aerosol can increase cloud droplet number concentration (Nd) and thus decrease stratiform cloud (Wood, 2012), this would suggest that inclusion of aerosol indirect effects leads to a more faithful representation of the physics involved. Our search of previous studies of the interaction between aerosol indirect effects and convective clouds, however, suggests that they are generally performed using radiative-convective equilibrium models or large-eddy models (i.e, much simplified compared to GCMs), and there have been no consistent conclusions to support the viewpoint that aerosol indirect effects are enhancing convective rainfall in models. Since representations of these interactions with convective clouds are generally not present in the models we cannot say from these model studies that too much convective rainfall is being caused in future projections owing to aerosol indirect effects.

Finally, we note that caution must be used in interpreting the comparison between convective and "large-scale" rainfall as identified in models with the convective and stratiform rainfall measured by products such as TRMM, as performed in the Saberali et al. (2013) study.

We have thus added a brief statement to our concluding paragraph at line 439 on page 20: "One study has suggested that future projections under enhanced greenhouse warming may be biased by a simulation of too much convective rainfall (Sabeerali et al., 2014), however there is no evidence that strong aerosol forcing is causing this and indeed it is suggested by other studies that inclusion of aerosol's ability to change cloud droplet number concentration (i.e., indirect effects) may give a more realistic simulation of stratiform convection (e.g. Wood, 2012)."

Instance 3:
When you answer this specific comment I need you to be specific about which sentence was added. I cannot make a wild guess, I need the exact sentence(s) to judge if you address the reviewer's comment:
(1) Page 30641, Lines 4-7: Whether the simulated aerosol distributions (aerosol optical depth, column burden) are too high over China? All the CMIP5 models are very poor in the simulating the aerosol-distributions over South Asia, especially Indian region. If the emissions are large enough, then aerosol distribution or deposition should be wrong. Rewrite the sentence.
We address the second part of the comment first. The reviewer states that "all the CMIP5 models are very poor in simulating the aerosol-distributions over South Asia", without offering any evidence. CMIP5 models all feature the same prescribed temporally and spatially varying aerosol emissions according to the IPCC/CMIP5 protocols. The models then derive their own aerosol burdens based on dynamical processes and other physics such as deposition with rainfall etc. We have already included in Figure 5a,b the CMIP5 multi-model mean picture of sulphate and black carbon aerosol loading (mg/m^2; the result of these prescribed emissions and the various modelled dynamical and physical processes). Fig. 5 clearly shows a concentration of aerosol on the northern plains of India, up against the Himalayas. This is consistent with minimal transport from key emissions source regions. It is also consistent with much other published literature on aerosol and India, for example Bollasina et al. (2011, observed sulphate AOD shown in their Supplementary material) clearly demonstrates the high burden of aerosol over northern India just as in CMIP5. Such a spatial distribution can also be seen across many years in recent MODIS satellite observations of AOD, e.g., as in Mehta (2015, Atmospheric Environment, Fig. 3, doi:10.1016/j.atmosenv.2015.03.021).

The reviewer primarily asks us about the aerosol burden over China and whether it is too high in CMIP5. We assume the reviewer asks this since in the conclusions of our original manuscript we included statements that, "while the large late-20th century emissions of aerosol in China are enough to reduce rainfall when only direct effects of aerosol are considered, over South Asia the sulphate emissions are not large-enough to reduce monsoon rainfall without indirect effects." However, we had made this statement without including a suitable figure.

In the revised version of the manuscript, we included a new Fig. 6, reproducing Fig. 5 but for the Chinese domain. This clearly demonstrates that the aerosol burden for sulphate (and also black carbon) is much higher for China than it is for India.

There is certainly large uncertainty in measurements of natural and anthropogenic aerosols over South and East Asia (especially complicated during the monsoon season when remote-sensing measurements may be unreliable due to cloud cover). Thus it is very difficult at present to assess the CMIP5 model skill at simulating these distributions. However we are confident that the common emissions used to drive the CMIP5 models represent our best estimate for regional scale aerosol distributions over India and China.

Regardless of the agreement with surface observations that do not exist at multiple sites, the multi-model mean aerosol loadings over China are much stronger for sulphate (approximately double) as shown in Fig. 6 of the new paper). This helps inform our closing hypothesis that aerosol loadings may be large enough over China for the direct aerosol effect to act alone in weakening East Asian monsoon rainfall, and warrants further investigation

For clarity we confirm that a new Fig. 6 was added to the manuscript, along with text on page 18 of the conclusions starting at line 408:

"Figure 6a and b show that while black carbon loading over China has a similar magnitude to India (comparing Fig. 6b and Fig. 5b), the sulphate loading over China is as twice large as that over India (comparing Fig. 6a and Fig. 5a). This supports our hypothesis that the aerosol burden over China may be large enough that even the direct radiative effects of aerosol alone are able to weaken rainfall over China during summer. Figure 6 reveals further interesting features. The maximum low-cloud cover during summer is situated over southwest China at the lee-side of the Tibetan consistent with observations. This is the region in which we note the strongest increase in cloud droplet number concentration, due to the overlap of high sulphate aerosol loads and low-cloud cover (Fig. 6d). Over much of central and southeast China (where rainfall declines the most as in Fig. 3), there is weak low-cloud cover and thus the indirect effect is likely to make little difference (e.g. compare Fig. 3b and c). This configuration of low-cloud and aerosol loading over China is different from that over India."

Last, but not least, you answers to my comments are too brief. I would like you to do more than add a single sentence to answer them:
We are sorry that we did not respond in a satisfactory manner to these comments in the most recent round of review. We reproduce here the original editorial comment for clarity.

The second referee deems that there are not enough new findings in this manuscript to warrant publication. Your answers point out that your main finding is that CMIP5 models that include only the direct-aerosol effect are unable to reproduce the observed trend of the Asian monsoon in recent decades.
Although, your findings are summarized in your abstract, you should have more explicitly stated them in the introduction. Please show how your study stands apart from previous published ones and brings new insights into this Asian monsoon trend. In addition, you should strengthen this point has in your conclusions.

To address this comment from the editor we have added new sentences in both the introduction and conclusions.

In the **introduction**, we now emphasize the new aspects that the rest of our study will show in relation to the role of indirect aerosol effects (and thus a fuller representation of the real physics) and their role in better simulation of observed monsoon rainfall trends. On page 4 lines 113 in the last paragraph of the introduction, our sentences now read: "…Various authors have questioned the ability of CMIP5 models to capture observed monsoon rainfall trends over India (Saha et al., 2014) and therefore the reliability of CMIP5 at making projections of future monsoon rainfall (Ramesh and Goswami, 2014; Sabeerali et al., 2014). In our study we shall demonstrate that it is only when aerosol indirect effects are included, and therefore when more physical processes are being represented, that these models are able to capture the observed trends."
Note that this sentence has been amended slightly from the previous manuscript version, expanding on the concerns raised in **instance 1** above.

Similarly for the **conclusions** we emphasize that, while it is necessary for models to be able to simulate the mean, seasonal cycle etc. of the monsoon in order for us to have more confidence in their future projections, a key finding of our work is that it is only when aerosol indirect effects are included (i.e. more fully representative of aerosol effects operating in nature than if only direct effects are included). On page 18 at line 398 we write, "…While we generally have more confidence in our models if the present-day simulations perform well at simulating the mean monsoon, its seasonal cycle, and variability (Turner and Annamalai, 2012; Ramesh and Goswami, 2014; Sabeerali et al., 2014), a key novelty of our study is that it is only the models containing aerosol indirect effects that can reasonably be expected to represent the observed trend."

After you address these remarks I will re-consider the paper towards being accepted in ACP.
We thank the Editor for taking the time to consider our manuscript.