

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2022-28-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2022-28

Anonymous Referee #1

Referee comment on "Satellite remote sensing of regional and seasonal Arctic cooling showing a multi-decadal trend towards brighter and more liquid clouds" by Luca Lelli et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-28-RC1>, 2022

This paper uses extensive analysis of changes in the radiation balance over the Arctic to consider the causes and effects of different trends. It is a highly timely and very important study that should be published. It shows how satellite can be used to effectively address a question that has been very much deliberated in the scientific press; Do changes in clouds, either macrophysical or microphysical, due to climate change affect the radiation balance over the Arctic, especially when considering the accelerated ice melt and snow drawback and suggested changes in cloud microphysics and the potential importance in aerosols. It is very welcome and I do encourage the authors to revise and resubmit this paper.

However, it is abundantly clear that the study is not finished yet; in fact, it is so poorly put together and presented that this is the reason I feel I have to recommend that the paper is rejected at this stage. Paired with poor writing this just simply goes beyond the scope of a major revision. The introduction has no real thread and just repeats various statements as if they were of the same significance and the text doesn't lead up to the motivation and background for this study. The text quotes huge amounts of numbers but doesn't lead the reader to the important ones and it is much too long for the message (65 figure panels in the manuscript alone and another 44 in the appendices). The authors are piling definitions and numbers upon numbers and completely forget the narrative; the paper is basically unreadable and I wouldn't have read it if had not had the task of reviewing it – in fact, I gave up when I got to the discussion and conclusion section – which is almost a third of the paper. I'm just saying!

A few examples:

The statement that the sea ice will be gone by 2035 (line 11) is not representative of current understanding; yes, at the current rate it will eventually be gone but the recent IPCC report concludes that some ice will remain if we can keep the global warming below 2 degrees. The Arctic warming (line 12) is, however, probably larger than twice the global

average. Arrhenius (line 14) may be of historical importance but his method was likely incorrect and he was "lucky" while the concern of scientists and public about the fate of the Arctic (lines 15-17) is much more recent than the 1990's. This was when the first IPCC report was published and if you download that and have a look, you will find that to the extent the Arctic is mentioned it is mostly either in the context of how little we know or how badly the models deal with the Arctic. All these superlatives seem to be used to underscore the importance of the study, but on me they act as a turn-off; if you need to exaggerate this way, the result cannot be very important. But it is and the framing of important facts is also important!

Moving on, the reason that the clouds are considered a major reason for much of uncertainty in climate projections (line 18) is not that they affect the radiation (line 19-20); of course they are! It is because models describe clouds so poorly, because it is so very difficult to model. Satellite observations are an important part of this but the work cited on line 24 does not "rely on" (line 25) on satellite observations. It is well known that different retrievals based on AVHRR are very different (line 42-44); yet it is used again here without illustrating why we should now all of a sudden believe in this retrieval. The ice-mass loss for Greenland is attributed to a reduction in cloud fraction in summer (line 53-54) without a proper reference; I tend to believe that global warming has some influence as well. Ocean areas are quoted frequently without accounting if they are ice covered or not (first on line 59) which is a very important distinction; not all of the Arctic Ocean is always ice covered which is an important part of this study. Moreover, the Arctic seems to be defined as being everything between 60 and 85 degrees north. Not only does that miss a fair portion of the central Arctic; it also includes most of the Northern North Atlantic including Iceland and the Faroe Islands, large parts of which is never affected by sea ice, half of Sweden and Norway and almost all of Finland; much of this would not be considered Arctic at all.

While it is true that Pithan et al. (2014) identifies the vertical structure of the atmosphere (the lapse-rate effect) as the primary factor for Arctic amplification the difference to the next important process – the albedo feedback – is not large and the whole argument rests on models; not observations. By the way, saying that "temperature-related processes dominate the Arctic warming" is just plain thoughtless; what else is warming but a change in temperature? On Line 96 we are told there are three reasons for this paper only to be given four reasons. The whole introduction is just confusing, sometimes borderline wrong, and doesn't lead the reader to the conclusion that this study is important at all.

On Line 131 is an unexplained "common north parallel" and on the following line there is an unexplained "darkening of the Arctic". On line 142-143 there is a transition in June while the figures show a transition through the entire spring. This is followed by "transitions increasingly approaching the summer solstice" which I don't understand and an argument that the day with the largest solar radiation needs to be the seasonal demarcation; why then is spring followed by summer and not autumn? I can buy the seasonal division based on what I see in the figures; that makes sense to me. So please don't add unjustified arguments that only muddies the water.

Line 152; what do you mean by "individual downstream methodology"; downstream of what? What is an "aggregated IWP histogram" (line 157) and how is it different from any

other IWP histogram? The sentence "Broadband ... instead" (line 163-164) must be missing some words. Observations cannot be derived from models (line 185) and I for one cannot see the trends in Figure 4 (line 188); it may be there but it is not obvious from looking at the figure and a change in one area cannot be "compensated" (line 203) by a change in another area. On line 215-216 you "infer" things from changes in clouds without reference to what it is you actually do; the paragraph just ends with this statement. The red markers in Figure 6 are not mentioned in the fig caps and the different parts of Figure 7 (that could benefit from breaking into two) are sometimes referred to as "upper" and "bottom" (line 233) panels and sometimes as "left" and "right" (fig caps).

Changes in CTH are given in percent; is that wise? A 100 m change is a 100 m change and corresponding to roughly the same temperature change regardless of it is at 1 or 10 km, but the percentage change is quite different. On Line 245 you discuss a decrease "especially where statistically significant"; is there any point in discussing changes that are not statistically significant? Conversely the change in CTH is once quoted to be 6 m; is that a difference you feel comfortable with give the measurement accuracy, statistically significant or not? On line 261-262 you discuss a change that is "marked" on spatial but not temporal scales, but what is a change if it is not temporal? The whole section on CRF is very interesting and would benefit from knowing where this is; surface or TOA? By the way, what is BOA (Line 293); not in the list of acronyms.