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Comment on acp-2022-28

Anonymous Referee #2

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-RC2>, 2022

Review of Satellite-based evidence of regional and seasonal Arctic cooling by brighter and wetter clouds by Lelli et al.

Summary:

This paper is an interesting assessment of satellite observations of trends in total albedo of the Arctic along with a number of other properties of the clouds that would impact that albedo. The topic is very relevant and timely, considering the rapid shifts occurring in the Arctic over the past couple of decades. Moreover, there is wide interest in understanding the potential contributions and feedbacks associated with clouds in the changing Arctic. The type of data used here is perhaps the only means for answering the questions that are posed. Thus, the topic and general approach are quite reasonable and worthy of publication. However, there are a number of major challenges with this paper that render it not publishable in its current form. The first of these is the lack of clarity in the writing. The introduction is a great example. It touches on important points and provides good references, but is generally not clearly written. As a person who has studied this field for many years, I was often confused by what was being stated. Some points are not entirely correct as stated and often the linking of one sentence to the next does not make sense. This lack of clarity is present through the rest of the manuscript (apart from the appendices, which happen to be quite clear). Heavy scientific editorial work is needed to clean up the writing, adjust sentence structure and wording choices, and to overall help make the points clear. Additionally, this work is focused on decadal trends that are often very small and, in most locations, not statistically significant. Thus, the whole analysis is really working on the margins of what is possible to conclude from the data sets that are employed. Yet there is very little effort put towards quantifying or characterizing the uncertainties that are inherent in the data used, as well as the implications of these uncertainties for the conclusions that are drawn. Lastly, the main point of the paper is cooling by "wetter clouds;" however, it has not been demonstrated that the clouds are actually wetter, and it might be that the clouds have simply shifted from more ice to more liquid water without a net change in mass. This point must be addressed. Further, the decline in surface albedo alone will lead to a stronger effective "cooling" by the clouds, regardless of changes in the clouds. The importance of this point has not been

appropriately addressed in the paper. Generally, there appear to be some mis-interpretations of cloud radiative effects. Below there are many comments regarding these general critiques. I believe the core of the work is a good starting point, yet major revisions are needed to bring this manuscript up to the standards that are expected for publication.

Specific Comments:

Line 36-37: This sentence is not clear to me. Perhaps: "the magnitude and variability of CFC depends on atmospheric conditions, cloud nucleation and growth, and the type of sensors used to measured it."

Line 37-38: I believe that the CFC of clouds (based on high-resolution data) is, in fact, bi-modal. This means that there is a relatively high occurrence of both high CFC and low CFC. However, this sentence appears to refer to the annual cycle of CFC. It appears that what you mean is that over the course of the year there are two relative maxima and two relative minima in CFC. If the later, please clarify and change the word bi-modal to something more appropriate. If I have mis-interpreted, please modify the sentence to be clearer.

Line 46-47: While I am a huge fan of clouds, I'm not sure this is the case. Large scale circulation patterns are particularly important for "modulation of energy flow exchange between the Arctic and its surroundings." Clouds are more important for the local exchange but are not themselves necessarily that important for the link between the Arctic and its surroundings. This sentence would be true if it started: "Clouds are an important atmospheric factor...."

Line 50-51: Here it states that the change in surface reflectance from melting cryosphere is offset by changes in clouds. However, on lines 57-58 it is stated that this compensation does not occur. Which is it? It seems to me that this sentence could be modified to say: ".....relative changes in surface reflectance could be offset by atmospheric...."

Line 58: I don't understand the "thus levelling out the recent pan-Arctic reflectance trend" statement. It was just stated that the albedo trends are NOT compensated by changes in cloudiness. I think the second part of this sentence should simply be deleted.

Line 62-68: This summary of He et al. is quite confusing and should be rewritten to ensure that the points are clear.

Line 69-76: Again, it is hard for me to following the train of thought in this paragraph.

Also, is it not obvious that “temperature-related processes dominate the Arctic warming”? Lastly, clouds only “amplify warming in the Arctic region” under certain conditions of sun angle and surface albedo; under other conditions they dampen the warming.

Line 77: Warming does not occur as a result of the release of greenhouse gases. Rather, there is warming due to an increased concentration of greenhouse gases in the atmosphere.

Line 77-78: The physical properties of clouds contribute to the tropospheric thermal emission. No need for “may” in this sentence.

Line 79: LWP/IWP is not the same as liquid/ice water content. One is an integral of the other. Please use the correct terminology.

Line 78-80: These properties regulate the LW as well as the SW. And this point is important because later in this paragraph it is stated that changes in cloud properties enhance or suppress CRF at the surface. Part of their ability to enhance or suppress is related to the balance of LW and SW effects from clouds, in addition to other factors.

Line 112-116: There is a list of criteria given here. These should be all included as a list and separated by semi-colons so it is clear what is part of the list and what is not part of the list.

Line 134: The darkening of the Arctic is possibly apparent during some months in spring at some wavelengths. But there is an even larger “brightening” of the Arctic in other months (mostly winter). First, it is not clear how this is the case with very little light in those months. Second, why is that feature not discussed in this paper, either as having importance for the geophysical system or as some indicator of uncertainty in the data stream?

Line 162-163: This statement suggests that broadband fluxes are computed using cloud properties. However, little information is provided on how these broadband flux derivations are made. There are naturally many inputs and assumptions to such a calculation so it is difficult to assess the validity of this approach with the information provided. Even with more information, derivation of radiative fluxes from satellite measurements, particularly at the surface, is a challenging process and multiple studies have shown significant uncertainties. It is essential to 1) describe what techniques have been used, 2) describe the uncertainties inherent in those techniques, and 3) discuss how those uncertainties impact the results that are presented.

Line 164-167: I assume that these values for F (LW, SW, +, -) are at the surface since you cite comparisons with BSRN measurements.

Line 173-174: This sentence needs to be rewritten for clarity.

Line 174-175: The text here has not really provided justification for the suitability of AVHRR cloud property retrievals for use in this paper. The prior statement simply says (I believe) that Philipp et al. showed that the trend in CRF has a low sensitivity to biases in cloud properties. This is different than saying that the cloud properties are suitable. The preceding statements also have not provided justification for how suitable the satellite retrieved cloud properties are. Moreover, there are numerous papers documenting how AVHRR derived cloud properties have large uncertainties. Thus, further justification for the utility of the derived cloud properties is needed here. Specifically, it is important to describe how the uncertainties in the cloud products impact the analysis conducted here.

Line 181-182: I do not know what this sentence means: "Any errors are minimized, when sunlight availability across the Arctic provides full coverage for the sensors' swath at highest latitudes." Does this mean that the errors are smallest when the sun is highest? Does this mean that there was a process in place (bias correction) to ensure that the errors are minimized at high sun angles? Something else?

Figure 3: There is a marked change in the standard deviation between the GOME and SCHIAMACHY data sets. This needs to be explained. And of great importance to this paper, the implications of this change on the ability to detect trends must be discussed.

Figure 3: There is a large discrepancy between SCIAMACHY and MERIS in the fall-winter. This discrepancy should be explained along with its implications for the results.

Line 188: "small downward trend". If I'm interpreting the numbers on Fig. 3 legend correctly, then the "trend" is actually within the 95% confidence interval of 0 (i.e., no trend). On line 190 you say that this is a "significant decrease." By this do you mean statistically significant? And if so, that should be stated clearly.

Line 189: "at time t." I assume this means at the given time of the year, so that information should be included.

Line 205-206: Yes, some of those negative anomaly areas are open ocean, but some of those are also over sea ice pack, and the year-to-year variability of sea ice extent is important here. Before attempting to draw this type of conclusion, it seems imperative to use the actual sea ice extent, which is readily available, to confirm that the negative

trends are indeed over open ocean as stated and/or to what degree that is true. I have the same comment for line 235 where changes are again related to regions of sea ice loss, but actual sea ice extent is not shown or discussed.

Line 208: Can you explain the Greenland trend?

Line 209-210: The text appears to be backwards. There is a strong negative trend in the Barents Sea in AMJ and a positive trend in JAS. And how is this similarly extraordinary to what was observed over Greenland?

Line 215-216: This statement that the JAS trends at 760 nm are large: First, is this specifically in reference to Hudson Bay or to all regions? And regardless of that, it is not clear from Fig. 6 that 760 shows particularly large trends relative to any other wavelength. What is the point of this statement? Perhaps this was intended to be part of the next paragraph, which appears to focus more on 760 nm?

Line 230-233, and Fig. 7 caption: What are warm liquid clouds and cold ice clouds? Are there cold liquid clouds (i.e., mixed-phase clouds)? Or do you simply mean liquid vs. ice clouds? If the later, then the "cold" and "warm" should be deleted. Otherwise, further explanation is needed.

Line 230-233. Also, it is not clear if the phase of clouds is shifting (i.e., more liquid clouds and less ice clouds, but the total CFC stays the same) or if there is the same amount of liquid and ice clouds but the actual COT of those clouds is changing. In other words, are these COT trends due to changes in cloud phase partitioning or the actual COT of clouds of a given phase when they exist? The statement made in lines 258-259 seems to suggest that the changes are due to occurrence fraction of liquid vs. ice.

Line 248-249: Fig. 7 and Fig. 8 do not show a positive trend in CTH over Greenland or Hudson Bay in JAS.

Line 252: I believe you mean Baffin Bay instead of Hudson Bay.

Line 255-256: I do not understand how this sentence starting with "Conversely" is actual converse to the prior sentence, which discusses opposing trends of liquid COT and ice COT. This sentence discusses the fact that liquid COT increases in both AMJ and JAS.

Figure 7: Make the colorbars the same for the liquid and ice COT fields so they can be

readily intercompared.

Line 256-258: Do you mean "nearly unchanged" (which would be a trend of 0) or do you mean that the trends remain similar for the different seasons?

Line 261-262: What does "for which a marked change of the spatial rather than temporal scale is observed" mean? The spatial distribution of changes is very similar to liquid COT and to some degree CFC. As for temporal scales.... The trends themselves (which are the temporal scale) are relatively large. Or do you mean temporal scale as in comparing the two seasons? In which case there is little difference between the seasons. This statement, and many others, are simply not clear.

Line 278-280: There are multiple ways to compute CRF and these are not consistent with each other. For example, some people use radiative transfer algorithms to compute the equivalent clear sky radiative fluxes by removing the cloud, but not modifying the other features like moisture or temperature that are associated with the cloudy air mass. Others will simply compare observed cloudy and clear states to get the radiative difference between these. There are a number of other considerations, such as the change of surface albedo under cloudy vs. clear skies, which could impact the results depending upon how this is accounted for. At a minimum this manuscript needs to clearly state how the CRF was derived so the results presented here can be realistically put into context of the past work on CRF done across the Arctic. Moreover, it is essential to understand how CRF was calculated so it is possible to interpret the CRF results (i.e., how much does the actual change in surface albedo due to decreased sea ice extent translate into enhanced effective cloud cooling?)

Line 285-286: The language is not precise here. The SW values quoted are not for cloud reflection, but for the difference in SW CRF. Similarly, clouds do not emit 36 or 43 W/m², but rather this is the LW CRF.

Line 288-292: First, it would be better to put the explanation for Greenland right after the Greenland result and the Atlantic sector explanation after the Atlantic sector results so as not to confuse the reader by jumping back and forth. Second, there are some basic misinterpretations here. With regard to the statement "darker surfaces of the Atlantic corridor and Baffin Bay emit LW more effectively": "darker surfaces" is typically referring to the amount of reflection, and thus impacts SW; the "darkness" of the surface does not directly impact the LW emission, rather that is the temperature, which happens to be higher over ocean than over sea ice but has nothing to do with the darkness of the surface. Additionally, the reason that SW outweighs LW in the Greenland Sea / Baffin Bay is not predominantly related to the liquid COT but rather to the surface albedo; at the relevant latitudes and a surface albedo less than 0.1, almost any cloud will result in SW outweighing LW effects (e.g., Shupe and Intrieri Fig. 7). On the other hand, over higher albedo surfaces like sea ice (albedo >0.6) the dominance of SW vs LW effects is much more sensitive to the cloud COT.

Line 293-294: Fig. D1 does not show climatological annual pan-Arctic total CRF, but rather shows trends in various CRF terms. Where do these numbers in the text come from? It would be very useful to include polar projection plots of the annual LW, SW, and total CRF to aid in the discussion around this part of the text.

Line 296-298: "Consequently, the Arctic surface is warmed by clouds throughout..." It is not clear what this statement is referring to. Presumably this is referring to the annual total CRF (which is not shown anywhere).

Line 304-312: These results are entirely consistent with the change in surface albedo. i.e., there is little change in the surface albedo over land surfaces, Greenland, and the N. Atlantic because there is no major shift in surface properties there. On the other hand, over the Barents/Kara in spring and over the whole Arctic Ocean domain in summer the declines in sea ice have led to a decrease in surface albedo. This leads to a larger negative SW CRF (independent of cloud changes), which increasingly outweighs the LW CRF.

Line 316: I don't know what "wetter Arctic clouds" means. In general, "wet" means water in any of its phases. But this paper has not established if there is in fact more water mass (LWP+IWP) in clouds. Rather, what has been shown is that often the changes offset each other, and the percentage change (Fig. 8) is often larger on the IWP decrease than on the LWP increase. Nonetheless, the information has not been provided here to demonstrate if there is in fact more condensed mass in clouds or not (although it could and should be). Additionally, the paper has shown some changes in the COT of liquid vs ice clouds, but this is not changing how wet the clouds are but rather how optically thick they are. Lastly, it has not been demonstrated here that anything has actually changed with the amount of cloud or the condensed mass of the cloud. The changes in liquid vs ice could simply be a shifting from ice towards liquid (i.e., same mass), which would be expected as atmospheric temperatures slowly warm. The paper needs further work to disentangle these key points and to draw the appropriate conclusions as to what is actually changing. For this specific statement highlighted here, if anything, the conclusion would rather be "...compensated for by more reflective Arctic clouds" (although to truly make that statement would require spatial maps and temporal trends for total COT).

Line 328: Figures 6,7,8 do not demonstrate a moistening of clouds. If anything, they might demonstrate an increase in COT, but even that is not clear in the aggregate.

Line 326-327 vs Line 330-331 vs. Line 338-340. In the first sentence, it is stated that the CFC trend over Greenland turns "strongly negative" after 1995, explaining Hofer et al. But then a few sentences later it is stated that over Greenland there is an "insignificant CFC trend." Then in the third set of lines, it again mentions the Hofer et al. work and the decreasing CFC leading to more insolation. So which is it? If there is a decreasing CFC (ala Hofer et al) then there would be less clouds and more of the Greenland Ice Sheet contributing to reflected irradiance. Assuming the surface is slightly more reflective than the clouds, this would explain the total change in TOA reflectance observed in this dataset.

Line 335: Again, unless there is evidence showing wetter clouds, all you have shown is that they might be more reflective.

Line 345: It depends on how cloud forcing is defined, but it generally also depends on the solar zenith angle and the surface albedo.

Line 346: Here and elsewhere, including on the legend of Fig. 10, use liquid instead of water. Water can be liquid, ice, or vapor.

Line 348-349: First, SW CRF dominates over LW CRF only for certain locations with sun high in the sky and surface albedo low. Second, how is the conclusion about CFC modulating mainly LW determined? In looking at the right half of Fig. 10, there is a general distinction for both LW and SW trends based on CFC (i.e., open vs. filled circles cluster on opposite sides of the panels).

Line 350: Perhaps: "In the last two decades the net radiative effect of clouds on the surface is decreasing."

Line 351: Perhaps: "Clouds cool the surface when they diminish the net SW flux by more than they enhance the net LW flux." The original statement is not true as stated.

Line 351-352: The above statement is simply true, regardless of any change in the clouds. It is possible that the SW CRF becomes MORE dominant over the LW CRF as the clouds become more optically thick and more reflective. This appears to be somewhat the case in JAS and not really in AMJ. Overall, these last three sentences of this paragraph need to be re-written in a way that accurately describes CRF, cloud effects, and the implications of changes in COT.

Line 353-367: One of the main points of the paper is that there are changes to the overall reflectance of the system that are NOT due to changes in CFC. But in this paragraph, there is a discussion about the sensitivity of CRF (SW, LW, and total) to changes in CFC. So, are changes in CFC important or not? And if not, why the lengthy discussion about this sensitivity?

Line 362-364: Sea ice retreat can happen earlier in some locations, such as the Barents. Leading to the unique values for the Barents seen in the AMJ panels of Figure 10.

Line 364-367: Enhanced convergence of moisture could be a possible mechanism to

explain why there might be changes in cloud properties (although this paper has not shown that there is a change in the total condensed cloud mass). However, changes in convergence of moisture are not needed to explain why the LW cloud effects dominate over the SW effects in AMJ. Regardless of any change in moisture convergence, cloud LW effects dominate SW effects in this season because the surface albedo is still high.

Line 372-373: This first statement is not true. The statement in question is speaking to why there is a trend towards decreasing net CRF. Indeed, there is more insolation in JAS compared to AMJ but that is true of every year of the analysis and it does not explain a temporal long-term trend in CRF. The second statement is true and is a direct result of the Shupe and Intrieri figure 7. However, this mechanism would lead to a decreased net CRF (more cooling) even if the COT and LWP did NOT change. In reading through this paper, overall, it appears that the clouds have changed somewhat, although it is difficult to discern exactly how from the information provided. However, it is quite certain (although not shown here) that the surface albedo itself has decreased over areas where sea ice has retreated. This surface albedo change itself will lead to the observed trend in total CRF, and the potential change in clouds might serve to enhance or diminish this direct impact of the decreasing albedo.

Line 376-384: Here again is a reference to changing CFC over Greenland. I suggest that the authors either remove all discussion of Greenland processes or spend the time to sort through the different perspectives. In this case Hofer et al. have drawn some conclusions, and these tend to be in opposition to the conclusions drawn by Bennartz et al. This is in large part because Hofer et al. are talking about processes around the periphery of Greenland where decreased CFC will enhance the net surface SW and lead to enhanced melt. Bennartz et al. are talking about the middle of the ice sheet where clouds warm the surface nearly all of the time. A change in LWP there will perhaps warm the surface slightly more or less, but there will still be a warming effect. Much of the explanation in this paragraph does not make sense.

Line 412: This statement is entirely dependent on season. The basic point is that, for a given amount of condensed mass, liquid clouds have a stronger interaction with atmospheric radiation than ice clouds. Thus, more liquid clouds at the expense of ice clouds in mid-summer will likely have an increased cloud cooling effect (particularly over low albedo surfaces) >>> this is the negative feedback. However, for the non-summer months (little to no insolation and generally higher albedo) that comprise most of the year, this change will simply enhance the cloud warming effect >>> positive feedback.

Line 412-414: First, mixed-phase clouds are supercooled liquid clouds, they just also have some ice. Both have a similar impact on atmospheric radiation. Second, the statement about reversing the sign of the net cloud feedback lacks a lot of context. The seasonally-varying feedback described in the previous point exists now and into the future. It won't really be "reversed" because two of the primary determinants of the sign of the feedback are sun angle and surface albedo. Cloud properties might play some role in that, although this point has not been thoroughly demonstrated or quantified in the current paper.

Line 454-455: There are substantial differences in the standard deviation between GOME and SCIAMACHY. What does this say about the data and how does it impact the ability to detect the very small trends that are reported in the data?

Overall: Based on my full assessment of the paper it is essential here to use the existing data to determine the answers to a couple of important questions that will help with interpreting the results:

- Is there a net change in condensed cloud mass (LWP+IWP) or is there simply a conversion of mass from ice to liquid?
- Is there a net change in CFC, or is there simply a conversion of ice clouds to liquid clouds?
- Is there a net change in total COT, or is there simply a conversion of ice COT to liquid COT?
- What is the impact on CRF (SW and total) of the observed changes in surface albedo, independent of any changes in cloud properties? The magnitude of this effect is important to understand as a context for any possible cloud changes that might also impact CRF.

Technical corrections:

Line 25: Change "which" to "that"

Line 41: Change "waters" to "water"

Line 118: Change "gas" to "gases"

Line 131: Change "parallel" to "latitude"

Line 152: Change "methodology" to "methodologies"

Line 159: Add "The" before "CFC and optical properties...."

Line 185: Change "observed" by models to perhaps "simulated" or "calculated."

Figure 3 caption: "over the Arctic Circle" is not correct. Simply state the latitude range instead.

Line 203: Do you mean "three" instead of "four"?

Line 204: I believe you mean "Western" instead of "Eastern"

Line 230:has "the" opposite sign....

Line 266: Change "darker" to "less reflective"

Line 268: Should be a semi-colon after water.

Line 271: Add "the" after "with"

Line 284: "as a result of LW effects offsetting SW effects"

Line 295: "Over the Arctic Ocean...."

Line 310: "Otherwise, the CRF trend...."

Line 319: "exhibiting"

Line 320: Change "a1" to "at"

Line 324" "...over the Canadian Archipelago,...."

Line 418: Change "expenses" to "expense"

Line 427: Change "increase" to "increased"

Line 436: "... as a function..."

Line 452: "...not only do the two..."