Relevant changes made to the manuscript after the second review phase

1. The title has been changed to the original version and shortened to account for the comments of both referees

2. The full manuscript, and especially the introduction, has been streamlined with the support of an external technical editor

3. New results have been computed for the time of emergence of statistical significance of the trends in cloud radiative forcing

4. A new figure with seasonal sea ice concentration maps has been added to the main text

5. The “Data and Methods” and "Discussions” sections have been subset into thematic subsections to increase in-topic granularity and readability

6. A paragraph on the limitations of the present study has been added to the “Conclusions” section

Answer to Anonymous Referee #1, January 17, 2023

Structure of the document:

1. The remarks by the referee are black and labelled as $MR_{1...n}$ if in the head of the review (major remark) or $R_{1...n}$ in the specific comment section
2. Our answers are red and labelled as $AMR_{1...n}$ answering to the MR, $A_{1...n}$ otherwise. For each answer, we explicitly say how the text will be updated together with new figures, where appropriate.
3. Relevant non-trivial changes are verbatim reported in blue and labelled $C_{1...n}$ with the line numbers of the revised manuscript.

This is the revised version of a paper using extensive analysis of satellite data to explore the radiation balance at the surface and top of the atmosphere as it pertains to climate change and the Arctic amplification, and especially the interplay with melting sea ice and changes in clouds. Like I commented in the first review, the study is highly timely and very important and the results should be published. It is very welcome and I do encourage the authors to revise and resubmit this paper.
While the original paper was rather poorly put together, it pleases me to note that the revised version is much better, and I enjoyed reading it. In think it shows that the reflectivity in the Arctic is not decreasing as fast as it should, given the melting sea ice, and snow, that this is due to a concurrent increasing in optical thickness of the clouds and that this is due to more liquid and less ice. But sometimes I do feel confused by the text and, hence, we're still not at the point where I can recommend publication. The language is sometimes a problem and there are also other issues with the manuscript as such that needs a second revision. I don't think much more analysis is required, although some statistical measures could be refined, but I will still recommend major revision, just to make sure this revision will happen.

We appreciate the time devoted by the referee to scrutinze the manuscript. In the following we will provide general answers to the major concerns raised by the referee. We will then delve into the review answering point-by-point to his specific comments related to these major concerns.

Major concerns:

MR1

While the clarity of the text is increased substantially, there are still problems with the message. It is unclear to this reviewer if the CRE – which incidentally is the accepted vocabulary and not CRF – is positive or negative at the pan-Arctic scale. On line 632 RTOA is increasing but on line 604 there are "decreasing pan-Arctic trends of reflectance" – unclear if at TOA or surface. And these are not the only times contradictory, or at least seemingly contradictory messages are delivered. Hence the whole text needs a work-over to make sure that the message is clearly conveyed in an understandable manner. I think I get it, but also I am sometimes confused.

AMR1

(1) We understand the comment by the referee. The concept of "cloud forcing" can be interchangeably described by cloud radiative forcing (CRF) or cloud radiative effect (CRE). CRF is widely used in spaceborne, airborne and ground-based literature as well. We adopt the same terminology of the following papers (listing the three most cited):

Ramanathan, V., Cess, R, D., Harrison, E. F., Minnis, P., Barkstrom, B. R., Ahmad, E., and Hartmann, D.: Cloud-radiative forcing and climate: Results from the Earth Radiation Budget Experiment, Science, 243, 57–63, 1989 (Citations 2301 as of November 17, 2022)

Harrison, E. F., Minnis, P., Barkstrom, B. R., Ramanathan, V., Cess, R. D., & Gibson, G. G. (1990). Seasonal variation of cloud radiative forcing derived from the Earth Radiation Budget Experiment. Journal of Geophysical Research: Atmospheres, 95(D11), 18687-18703.(Citations 879)

Shupe, M.D. and Intrieri, J.M., 2004. Cloud radiative forcing of the Arctic surface: The influence of cloud properties, surface albedo, and solar zenith angle. Journal of climate, 17(3), pp.616 - 628. (Citations 622)

We also adhere to our established project-internal naming of variables (CRF and not CRE), which can be seen in the following two summary publications:

Wendisch, M, et al. "The Arctic cloud puzzle: Using ACLOUD/PASCAL multiplatform observations to unravel the role of clouds and aerosol particles in Arctic amplification." Bulletin of the American Meteorological Society 100.5 (2019): 841-871.`https://doi.org/10.1175/BAMS-D-18-0072.1`

Wendisch, M., et al. "Atmospheric and Surface Processes, and Feedback Mechanisms Determining Arctic Amplification: A Review of First Results and Prospects of the (AC) 3 Project." Bulletin of the American Meteorological Society (2022).`https://doi.org/10.1175/BAMS-D-21-0218.1`

(2) For the contradictory messages, we anticipate that we have been careful to specify the geometry of observation in the text: we refer to TOA only when discussing changes in reflectance, whereas CRF is discussed only at the surface (bottom-of-atmosphere, BOA).

(3) With respect to CRF, we resort to Fig. 10 of the manuscript, where we juxtapose the multiyear mean CRF at the surface (i.e. the *"climatological CRF"* mentioned at line 631) to the CRF trend at the surface. While the maps show that CRF trends are regionally partitioned, the pan-Arctic CRF change is negative.

(4) The sentence at line 604 refers to the (decreasing) trend of reflectance at pan-Arctic scale. The line 632 refers to the increase in reflectance explained by the change in phase of clouds at regional scale.

MR2

The issue of what is measured and what is coming from other sources, be that a priori model data (e.g. reanalysis or operational models) or from radiative transfer modeling (or both) needs to be much clearer. I appreciate the discussion on taking as much as possible from direct observations, but no satellite in the world can observe downwelling radiation at the Earth's surface; that just has to be a model product, although it may be constrained by observations. And very likely there is a (fallible) model temperature profile in there somewhere, am I right?

AMR2

There must be some misunderstanding. We never claimed that satellite measure fluxes at the surface. We use the terminology *"direct observations"* only referring to reflectances at the top-of-atmosphere measured by spaceborne spectrometers (see lines 596–598: *"Another advantage of this record of reflectances is that they are direct measurements, realization of basic physical processes, and are not dependent on algorithmic assumptions"*).

Section 2.2 of the manuscript ("Cloud and flux data products"), and more precisely the paragraph starting at line 212 throughout line 248, contains the information required by the referee that lead to the computation of fluxes. There it is described how fluxes are computed solving radiative transfer (general RT setup lines 212–216; lines 229–234 for the all-sky and clear-sky state), ingesting retrieved cloud properties (l. 216–217), surface albedo (l. 217–219) and sea ice extent (l. 243) as observational constraint, with additional inputs from reanalysis for temperature profiles (l. 219) and cloud parameterization (l. 222–229).

The description is necessarily compact, but all references of interest to the reader are given in this section.

MR3

The issue of what is and what is not significant is much better handled in this revision than in the original manuscript. But it only goes as far as it does; several of the maps still lack stippling for what is considered statistically significant, and the text sometimes ignore this and discusses significant and insignificant trends in the same sentence. I note that very few of the results over perennial sea ice are ever significant, which is something the authors need to comment on. I also see many references to trends at or near the North Pole, where there are no observations at all. I also see significance coming and going between optical thickness for liquid, ice and total clouds water; presumably if one of these are not significant, none of the others can be and there is very little discussion about how the significance relates to accuracy; a rends can in fact be statistically significant and still meaningless if for example it is so small it doesn't matter or if the accuracy of the observations is so poor that it can't possibly be resolved.

AMR3

In the referee's comment, we distinguish three points: (1) accuracy of the basic quantities (reflectances and cloud properties) (2) accuracy of the trend and (3) statistical significance. Note that have put all adopted solutions for trend and significance assessment in the Appendix because we did not want to jam the main text with technicalities.

(1) Accuracy of reflectances and cloud properties.

We have ensured that all time series (i.e., reflectances, cloud properties and fluxes) from which we draw the trends are unbiased (see Appendix A). To the best of our knowledge we have applied state-of-the-art corrections to handle intra-sensors inhomogeneities both for radiometry (different spectral resolutions and drifts) and platform-dependent design (i.e. different local overpass times and spatial resolutions). We also make use of the pixel-level uncertainties of cloud properties, which we propagate to quantify the error of the mean in cloud properties upon aggregation into the final time series. This requires the notion of the *correlation length*, which we estimate with the approach sketched in Appendix C. This gives us a sense of whether a trend (statistically significant or not) is, at the same time, also accurate.

(2) Accuracy of the trend.

We have ensured that the sample populations from which we draw the trends are gaussian, so that a linear trend model (relying on the randomness of the sample) can be applied.
We also have ensured that the trend model is not pre-conditioned and is unaware of the sample population (this happens when one has to choose a-priori functional parameters in the objective function of the trend model). This is the content of Appendix B.

(3) Statistical significance.

Significance is identified computing the standard deviation of the trend and looking at those locations whose measured trend is twice as great as its standard deviation. The CRF trends are not significant within the time frame of this dataset (20 years). Therefore, for this revision, we have computed the time of emergence (ToE) of the CRF trends, so that we can quantify how many years of observations are still needed for the trend to exceed natural variability in the time series. See answer A38.

MR4
Finally, there is a debate in the scientific community as to if aerosol indirect effects are responsible for any of this or if it is all thermodynamics, or maybe even dynamics since clouds tend to form where the dynamics dictates they should form; dominating clouds are different in different climate regimes because of the general circulation more than anything else. The authors discuss changes in optical depth and water paths for quite a while but it isn't until end of page 24 that aerosol concentrations come in via the effective radius. This is followed by a confusing discussion on trends in this parameter that seems to be there to satisfy someone that wants this to a factor. Statistical significance is not discussed and there's a lot of handwaving. I suggest that the whole thing about whether the changes in optical thickness are due to changes in water paths or effusive radius is either given its own section and is based on what measurements are available, or left entirely to another paper, where this can be properly addressed.

AMR4
We agree that the indirect effect of aerosols is one of the basic unresolved issues in the Arctic. We also agree with the consideration that it is premature and inconsistent to write about it if pan-Arctic spaceborne mature data sets are not available. As specified below, we leave the scientific question open, both in the mani text and in the conclusions.

Minor comments:

R1
Page 1: Title is clunky and awkward and reads like a part of the text. I suggest a much short and

snappier title would enhance the chances the paper will be read!

A1

We agree with the referee. The title change seemed to us justified by the confusion caused by the comparative of "liquid": wetter. As referee #2 also suggested (see his comment R1), we adopt the original title, using "more liquid" this time. This way we think the reference to a specific thermodynamic phase of the clouds and not to its integrated water path is unambiguous.

C1

The title now reads: "Regional and seasonal Arctic cooling by brighter and more liquid clouds from satellite remote sensing".

R2

Page 1, lines 13: Temperature doesn't have a "size" as such; you never say "it's hot today, the temperature must be large".

A2

We have restructured the introduction taking into account this comment. This specific sentence has been deleted.

R3

P1, l13-20: No need to go back to the "old Greeks" here. Just state that the globe is warming because of anthropogenic emissions of greenhouse gases, that the Arctic warming more then the globe on average and that this set a number of feedbacks into play where radiation is a key to some. Neither Arrhenius nor Keelings work is necessary as a background for this paper.

A3

We have removed these references.

R4

P2, l21: A recent paper in Nature (I think) has the Arctic amplification to a factor of four, so it should at least be "larger than twice".

A4

We assume that the referee is referring to this paper:

Rantanen, M., Karpechko, A.Y., Lipponen, A. et al. The Arctic has warmed nearly four times faster than the globe since 1979. Nat Commun Earth Environ 3, 168 (2022). `https://doi.org/10.1038/s43247-022-00498-3`.

We will cite this paper in the introduction.

R5

P2, l37-44: This section is a bit awkward. It starts by telling the reader what the satellites measure, but only for SW radiation (l37). Then at l41, LW slides in but not as something that satellites measure, but as something that "also" modified by clouds. Satellites measure radiation, plain and simple, across the whole spectrum and everything else is inferred from this, quite often using a priori information that is hardly ever discussed.

A5

We will make the language in the introduction more accurate.
Section 2.2 briefly describes the methodology deployed to derive the fluxes we use in the rest of the work.

C5

(lines 26–30)
Instruments aboard satellites measure radiation at the top-of-atmosphere (TOA) across the whole electromagnetic spectrum, both SW and LW. The former is scattered back to space by the Arctic surface as well as from atmospheric constituents, such as trace gases, aerosols, and clouds (Serreze and Barry, 2014; Kokhanovsky and Tomasi, 2020). LW radiation (> 4 m) is emitted from both the Earth's surface and atmospheric gases and clouds (Kiehl and Trenberth, 1997; Stamnes et al., 2017).

R6

P2l49-50: Sea ice is also water, so if you mean open (= ice free) ocean you need to say that. Come to think of this is slightly tautological – what else would and ocean be made of if it wasn't water?

A6

Yes, indeed. With "open" we mean "ice free" and also in absence of melt ponds. We have removed this sentence.

R7

P2, l49-51: This is confusing; given that the annual cycle of CFC from many studies goes from 40-70% in winter to 70-95% in summer across several Arctic stations, it is difficult to see i) how the in summer is "located in the north Atlantic ..." and the second – wherever that is – is only 40%.

A7

In the text we cite the relevant references where the reader can observe the relative CFC maxima locations on the Arctic maps. A possible source of confusion might be the wording "maxima". We mean also the negative ones, that is in absolute terms. In the previous version of the manuscript, the terminology "extrema" was criticised and we replaced it.

We have removed this sentence as attempt to streamline the introduction.

R8

P2, l53-p3, l58: I think what the author is referring to hear is the fact that the same observations from AVHRR when run through different retrievals give different results. But the question is, if different retrievals are used, are the results then the "same data sets" ?

A8

At line 58 we use the following verbatim wording: *"... even though all three research groups use the same data"*. With "data" we refer to the set of L1 radiances and not the derived geophysical L2 products. We will specify this in the text.

R9
P3, l64: "by changes in atmospheric". If the reflection by the atmosphere is not changing it cannot offset another change.

A9
Correct. We rephrase the sentence to be more accurate.

R10
P3, l67-68: You need a reference for this if you are to use this argument here. You cannot start using the results in this paper in the introduction to it.

A10
The reference exists and can be found at the end of the ensuing sentence. Basically, the reasoning by Hofer et al. (2017) spans both sentences. We will rephrase to avoid confusion.

R11
P3, l70-72: This is trivial and is true for all clouds everywhere on Earth. Without understanding dynamics, understanding the clouds is futile! Cloud formation needs water and temperature and aerosols, but without dynamics (advection, surface cooling, evaporation, lifting, subsidence etc., etc.), it still won't happen 99% of the time. It is not an accident that we find subtropical stratocumulus in the subtropics and deep convection along the ITCZ!

A11
This is correct. We intend to report two results in the literature that have recently shown that Arctic cloudiness is closely correlated with both the underlying surface type (He et al. 2019) and the dynamics of air masses (Hofer et al. 2017).

He, M., Hu, Y., Chen, N., Wang, D., Huang, J., and Stamnes, K.: High cloud coverage over melted areas dominates the impact of clouds on the albedo feedback in the Arctic, Scientific Reports, 9, https://doi.org/10.1038/s41598-019-44155-w, 2019.

Hofer, S., Tedstone, A. J., Fettweis, X., and Bamber, J. L.: Decreasing cloud cover drives the recent mass loss on the Greenland Ice Sheet, Science Advances, 3, https://doi.org/10.1126/sciadv.1700584, 2017.

R12
P3, 73-74: One step to many here; logically, trends in albedo cannot be "compensated" by CFC. This is only one component and you are taking too many logical steps at once here.

A12
Yes, indeed. We will make the logical steps explicit, adding that an increase in CFC leads to an increase in reflectivity. We have moved this sentence to the section of R_λ^{TOA} results, as it reads more consistent to explain these concepts while commenting the results of this work.

C12

(lines 292–294)

In Pistone et al (2014) a downward trend of all-sky albedo across the Arctic is reported. This is not compensated by an opposite trend in albedo as a result of increased cloudiness, which thus levels the recent pan-Arctic reflectance trend.

R13

P3, l87-P4, l92: The Pithan and Mauritsen paper lists cloud feedbacks as a minor third in importance, after albedo feedback and lapse-rate feedback. In fact when they say temperature, the do not mean thermodynamics in general; they do mean "temperature" plain and simple.

A13

To avoid misunderstanding, we remove the concepts on feedbacks from the introduction. We keep, however, the paragraph describing how an increase in temperatures can influence the Arctic cloud state through changes in the thermodynamic processes. We will be more precise in citing that paper while introducing thermodynamics.

R14

P4, l100: What are "sea ice edge shelves"?

A14

The edges of ice floes. We will rephrase the sentence.

R15

P4, l105: ""modulate" is better than "regulate".

A15

We replace it.

R16

P4, l108-109: First, this is true at the surface as well as at TOA, so drop the last part. Second, here and throughout, the accepted terminology is Cloud Radiative Effect (CRE).

A16

We delete "at the surface". For the terminology, see our answer AMR1 above.

R17

P5, line 142: "Two exceptions to the latter . . . "?

Figure 1. Is BOA an accepted abbreviation? Else, if you mean the "surface", then say write "surface".

A17

We add "to the latter" in the sentence at line 142.

BOA is accepted terminology in the realm of spaceborne remote sensing of surface properties. The terminology appears also in the glossary of the American Meteorological Society at the page "Atmospheric Radiation" (https://glossary.ametsoc.org/wiki/Atmospheric_radiation).

Please note that we label the secondary y-axes TOA in the upper plot and BOA in the lower plot for consistency. The individual plot insets read *"(a) Atmosphere"* and *"(b) Surface"*. The information about the surface is already present.

R18

P7, l174-P8, l183: Apparently someone criticized how the seasons were divided and the authors feel a need to defend themselves. I would recommend that you don't, you just looked at the data and use the delineation that made most sense. None of the arguments you give in this paragraph are very good. I don't understand why meteorological seasons are no good because you are using a long data set, and looking carefully at the figure, the transition from June to July is structurally different comparing the beginning and the end of the time series.

A18

Meteorological seasons are not suitable for the study of long-term changes (and trends) in reflectance at high latitudes because in May and June (i.e., respectively the last month of meteorological spring and the first of summer) multiple scattering between the surface and the atmosphere prevails (thus coupling both radiatively).

This effect can be seen in Fig. 1. MODIS/Terra RGB overpasses of NSA Barrow are shown for a single mid-month day of each month between April and September. The TOA reflectance in June is still largely determined by the surface. Thus any reflectance trend assuming summer as Jun-Jul-Aug (meteorological seasons) contains changes in albedo of both the surface and the atmosphere.

In recent Arctic literature, the grouping Apr-May-Jun as Arctic spring and Jul-Aug-Sep as Arctic summer is increasingly adopted, although without any sort of justification. See, for example, He et al. (2019) and Philipp et al. (2020).

We will remove the above references and we explain the concept underlying Figure 1.

Regarding the remark of the referee that "*the transition from June to July is structurally different comparing the beginning and the end of the time series*", this is the result of Letterly et al. (2018) meaningful to our purposes.

Letterly, A., Key, J., and Liu, Y.: Arctic climate: changes in sea ice extent outweigh changes in snow cover, The Cryosphere, 12, 3373–3382, https://doi.org/10.5194/tc-12-3373-2018, 2018.

He, M., Hu, Y., Chen, N., Wang, D., Huang, J., and Stamnes, K.: High cloud coverage over melted areas dominates the impact of clouds on the albedo feedback in the Arctic, Scientific Reports, 9, https://doi.org/10.1038/s41598-019-44155-w, 2019.

Philipp, D., Stengel, M., and Ahrens, B.: Analyzing the Arctic Feedback Mechanism between Sea

Ice and Low-Level Clouds Using 34 Years of Satellite Observation, Journal of Climate, 33, 7479–7501, https://doi.org/10.1175/JCLI-D-19-0895.1, 2020.

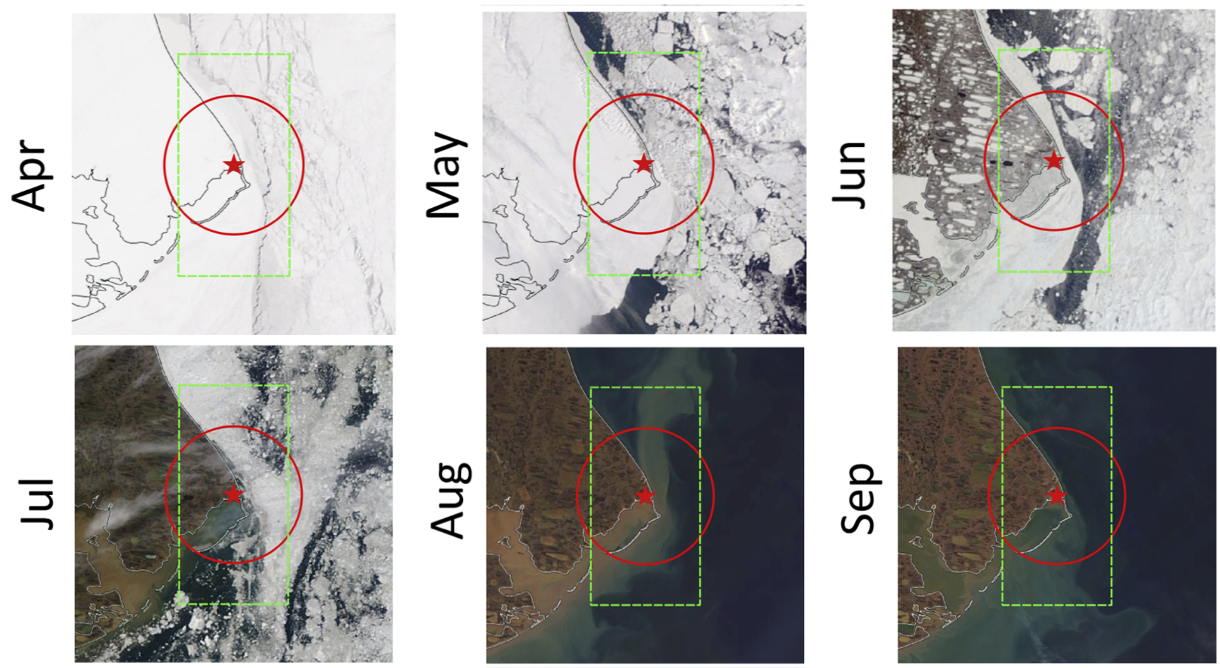

Fig. 1: True color images, taken by MODIS on Terra, for one mid-month day of each month between April and September over the NSA Barrrow site (project internal).

C18

(lines 145–149)

Ignoring the astronomical definition, the meteorological seasons are not suitable for our purposes because in May and June (respectively the last month of meteorological spring and the first of summer) multiple scattering between the surface and the atmosphere still prevails, thus coupling both radiatively. The definition of ad-hoc Arctic seasons ensures that the computed trends describe only those changes of R_λ^{TOA} caused by distinct underlying processes, which in turn determine the breakpoints in the time series of R_λ^{TOA} shown in Fig. 2.

R19

P8, l204: This is not only lower, it is differently much lower in different clouds at different times.

A19

We agree, of course. However, the context of the sentence is merely technical and not geophysical. We are pointing the reader to the general consideration that photons of different wavelengths penetrate a cloud at different depths. See, for instance, Platnick (2000) and Rozanov and Kokhanvosky (2006).

Platnick, S. (2000). Vertical photon transport in cloud remote sensing problems. Journal of Geophysical

Research: Atmospheres, 105(D18), 22919-22935. https://doi.org/10.1029/2000JD900333

Rozanov, V. V., and A. A. Kokhanovsky. The average number of photon scattering events in vertically inhomogeneous atmospheres. Journal of Quantitative Spectroscopy and Radiative Transfer 96.1 (2005): 11-33. https://doi.org/10.1016/j.jqsrt.2004.12.026

C19
(line 173)
The two references are added to the text.

R20
P9. L228: Really!! 369 and not 368 or 370? How accurate is this observation?

A20
We point the referee to the paper where relevant information is given.

Meerkoetter, R. and Zinner, T.: Satellite remote sensing of cloud base height for convective cloud fields: A case study, Geophysical Research Letters, 34, https://doi.org/10.1029/2007GL030347, 2007.

R21
P9, l239-241: How large are these biases and why the different signs. Why should surface heterogeneity generate a bias with different signs for small and large values?

A21
For SWup $<$ 100 Wm^{-2}, the average bias amount to $\simeq$ 20 Wm^{-2}, while for SWup $>$ 250 Wm^{-2} it can be up to $\simeq -50$ Wm^{-2}. In both ranges the average relative bias amounts to $\simeq$ 20%.

Surface heterogeneity is the cause of discrepancy because the surface area encompassed within a satellite pixel is always greater (and more heterogeneous) than that in proximity of the in-situ instrumentation.

The change in sign of the bias boils down to the value of surface albedo assumed in the satellite algorithm, which can overestimate or underestimate the actual surface albedo. We explain this at lines 245–248.

C21
(lines 224–226)
The average AVHRR-based estimates tend to be biased high of $\approx$ 20 Wm^{-2} for $SW^{+} < 100$ W m^{-2}while the opposite holds for $SW^{+} > 250$ W m^{-2} with an average underestimation up to -50 Wm^{-2}. In both ranges the average relative bias amounts to $\approx$ 20% (Stengel et al, 2020).

R22
P9, l244: Is the sea ice albedo in these calculations always the same? Is that what you say? Have you been to the Arctic? What about snow or bare ice, what about melt ponds. This is gross!

A22
(see also A37 in this document)

We are not claiming that the real surface albedo is always the same. Of course we know this fact, let alone Fig. 1 in the manuscript or our activities in aerosol and cloud retrieval algorithm development.

We had been asked by referee#2 to point out possible sources of inaccuracy in the data set, which was not produced by us. And a single value of surface albedo has been assumed above sea ice throughout the record. This value is, however, spectrally and spatially weighted in the algorithm, as reported in Sus et. al (2018) as follows:

> *"The albedo of snow- or ice-covered pixels is set to globally constant values of 0.958 (Ch1, CC4CL ID as in Table 1), 0.868 (Ch2), 0.0364 (Ch3), and 0.0 (Ch4) and is area-weighted in the event of fractional sea or ice cover."*

Sus, O., Stengel, M., Stapelberg, S., McGarragh, G., Poulsen, C., Povey, A. C., Schlundt, C., Thomas, G., Christensen, M., Proud, S., Jerg, M., Grainger, R., and Hollmann, R.: The Community Cloud retrieval for CLimate (CC4CL) - Part 1: A framework applied to multiple satellite imaging sensors, Atmospheric Measurement Techniques, 11, 3373–3396, https://doi.org/10.5194/amt-11-3373-2018, 2018.

C22
(lines 231–233)
The albedo of snow- and ice-covered surfaces is set to 0.958 at wavelength 630 nm, 0.868 (910 nm), 0.0364 (1.6 µm), and 0.0 (3.74 µm). The albedo is additionally area-weighted for fractional sea ice or snow cover scenes (Sus et al., 2018).

R23
P9, l246: The sentence "cloud radiative . . . surface" is somehow meaningless or at least trivial. Maybe the authors think about the surface albedo, which often determines the sign of the CRE.

A23
Yes, indeed, we mean this. We remove the sentence because it reads redundant at this point.

R24
P10, l263: Do not understand the meaning of "seasons of our paper". Do you refer to the time period or the choice of seasonal boundaries or what?

A24
Yes, correct. We refer to the choice of grouping April May June as Arctic spring and July August September as Arctic summer. We will clarify this.

R25
P12, l301: Why do you expect that a warming Arctic would feature a "statistically significant" decrease in reflectance? Why a decrease and why significant? If you mean that the loss of sea ice should lead

to a lower albedo, then say substantial and leave the statistics out of this.

A25

We will use "substantial" in the revised text.

R26

P12, l305-207: Isn't it also possible that the CFC in summer, when the ice is melting, is so high and the cloud thickness sufficient, that there would not have to be an increase in anything for the surface albedo decrease to go unnoticed at TOA?

A26

This is not the case.

The onset of ice melt occurs between June and July, while sea ice retreat (and loss of corresponding albedo) accelerates during summer months to peak in September. It is therefore logical to assume that the albedo decrease associated with the sea ice retreat is noticeable at TOA.

To prove this, we provide the following qualitative reasoning. Please note that this is not intended to be a fully quantitative assessment, but rather to act as qualitative tool to understand the sign and magnitude of the reflectance changes at TOA in the presence of clouds above a bright surface.

For an average Arctic sea ice decline of 12.6% decade^{-1} (-25.2% for the 20 years period of our study, `https://climate.nasa.gov/vital-signs/arctic-sea-ice/`) and sea ice cover (SIC) of 20% for all latitudes north of 60° in 1996, assuming an average albedo of sea ice, snow and ponds together (SIA) of 0.6, an average albedo of land masses and open ocean together of 0.15, we obtain the following values for the full Arctic albedo (FAA, defined as the spatially integrated albedo for all latitudes north of 60°) in 1996 and 2016:

$$\mathrm{FAA}_{1996} = \mathrm{SIC} \times 0.6 + (1 - \mathrm{SIC}) \times 0.15 = 0.45$$
$$\mathrm{FAA}_{2016} = 74.8\% \times \mathrm{SIC} \times 0.6 + (1 - 74.8\% \times \mathrm{SIC}) \times 0.15 = 0.217$$

Table B1 of the manuscript reports the mean pan-Arctic and regional values of CFC and COT. For CFC at 72%, an average COT of $\approx$14 is still not sufficient to effectively and completely shield a surface albedo change from 0.45 to 0.217.

This can be seen in Fig. 2. Based on our RT computations, for a water cloud of fixed geometrical thickness of 1 km, and top altitude of 3.5 km, COT in range 5–70 is on the x-axis and the TOA reflectance at 560 nm for the FAA in 1996 and 2016 is on the y-axis. We compute the reflectance for a fully cloudy pixel (Fig. 2 left) and a fractional cloudy pixel (Fig. 2 right). The reflectance at TOA (R_{toa}) for the second case is calculated with the independent pixel approximation, assuming that the surface reflectance R_{surface} equals the FAA:

$$R_{\mathrm{toa}} = \mathrm{CFC} \times \mathrm{R}_{\mathrm{toa}}^{\mathrm{CFC}=100\%} + (1 - \mathrm{CFC}) \times \mathrm{R}_{\mathrm{surface}}$$

It can be seen that for actual optical thickness values of Arctic clouds the TOA reflectances already

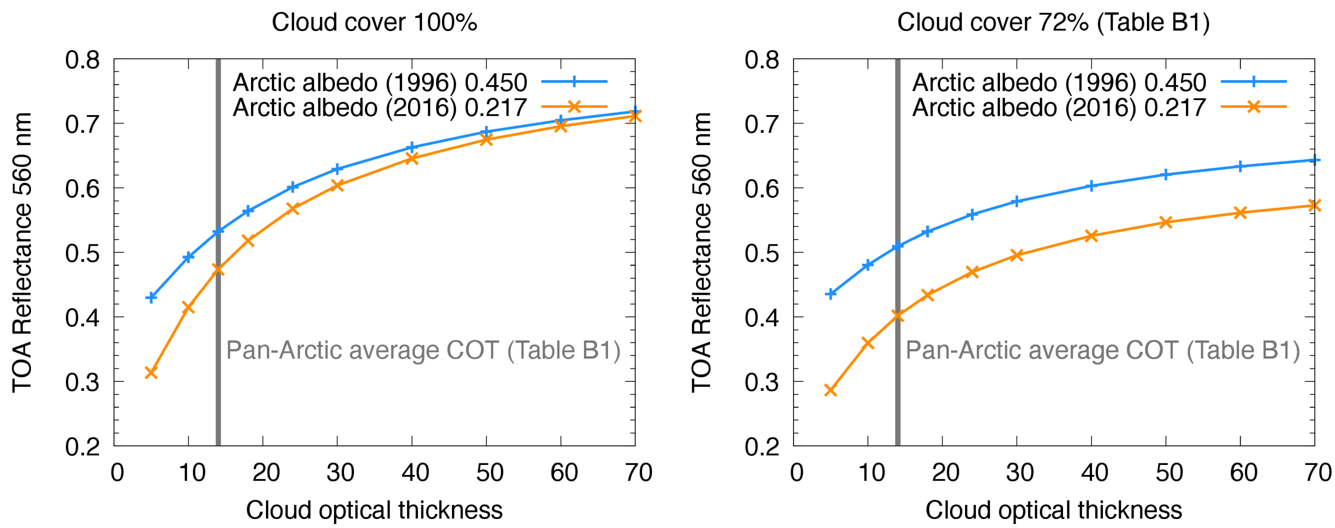

Fig. 2: Sensitivity of TOA reflectance at 560 nm for varying cloud optical thicknesses, two full Arctic albedos (Lambertian, 0.45 in 1996 and 0.217 in 2016) and observational geometry nadir view, SZA 60°, RAA 80°. The change in reflectance for COT = 14 (average pan-Arctic value for AMJ and JAS) amounts to 11% for CFC 100% and 21% for CFC 72%.

change by 11% as function of sea ice retreat for a fully cloudy pixel. The surface starts being effectively masked for COT values greater than 14. In the case of broken cloudiness, the change at TOA increases to 21% and the curves do not converge.

R27

P12, l310-311: Drop "local". Why 75%; SIE is usually defined at 15%...

A27

We have chosen the 75% SIE threshold for two reasons.

The first reason is to be consistent with Figure A1 (p 7498) in Philipp et al (JCLIM) 2020. In the section of that paper, the authors assess the accuracy of CRF as function of the misclassification of satellite-derived CFC, which is in turn related to SIE. The authors identify the 75% threshold in SIE as the demarcation between two distinct regimes of CRF accuracy. Because in our paper we relate TOA reflectances to CRF, the reader would find direct consistency between our results for TOA reflectance and those in Philipp et al 2020.
The second is that the geographical contours of sea ice are fundamentally different from the contours identified by means of (gridded) TOA reflectance, let alone the high SIE variability within a grid cell for the full time series.
To avoid any confusion, we propose to add Fig. 3 to the manuscript and remove the SIE outlines from the maps of spectral reflectance and let the reader compare the map himself.

Philipp, D., Stengel, M., & Ahrens, B. (2020). Analyzing the Arctic Feedback Mechanism between Sea Ice and Low-Level Clouds Using 34 Years of Satellite Observations, Journal of Climate, 33(17),

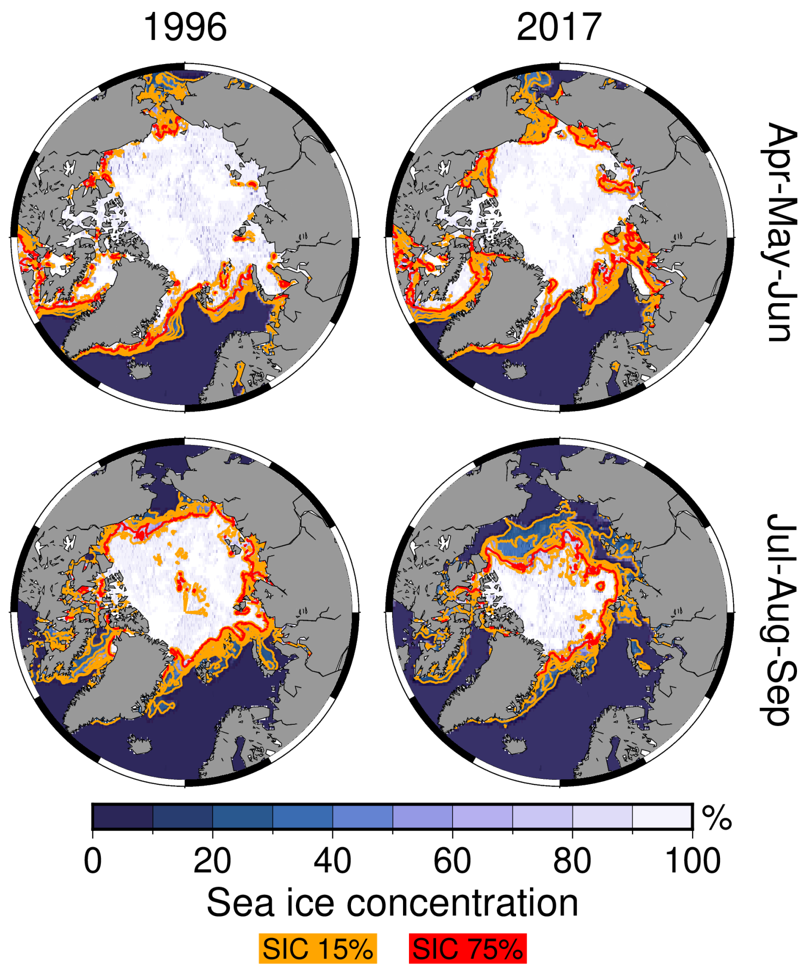

Fig. 3: Sea ice concentration (SIC) for Arctic spring (top row) and summer (bottom row) for 1996 and 2017. Data from Welsh et al. (2016). The orange and red contours indicate a local SIC concentration of 15% and 75%.

7479-7501 `https://doi.org/10.1175/JCLI-D-19-0895.1`

C27

(lines 303–308)

To answer these questions in the following, we show the Arctic sea ice concentration (SIC) in 1996 and 2017 for AMJ and JAS in Fig. 5 and the R_λ^{TOA} trends for the wavelengths 510, 560, and 620 nm in Fig. 6. The mean seasonal sea ice extent (SIE) at 15% and 75% SIC is respectively coded in orange and red contours. While SIE is usually identified by a SIC threshold of 15%, a value of 75% better represent the geographical contours identified by means of R_λ^{TOA}. Moreover, Philipp et al (2020) identify the 75% SIE threshold as the demarcation between two distinct regimes of accuracy in broadband fluxes as function of the misclassification of satellite-derived CFC above bright surfaces.

R28

P12, l316 and onwards: Very few results are statistically significant anywhere over perennial sea ice.

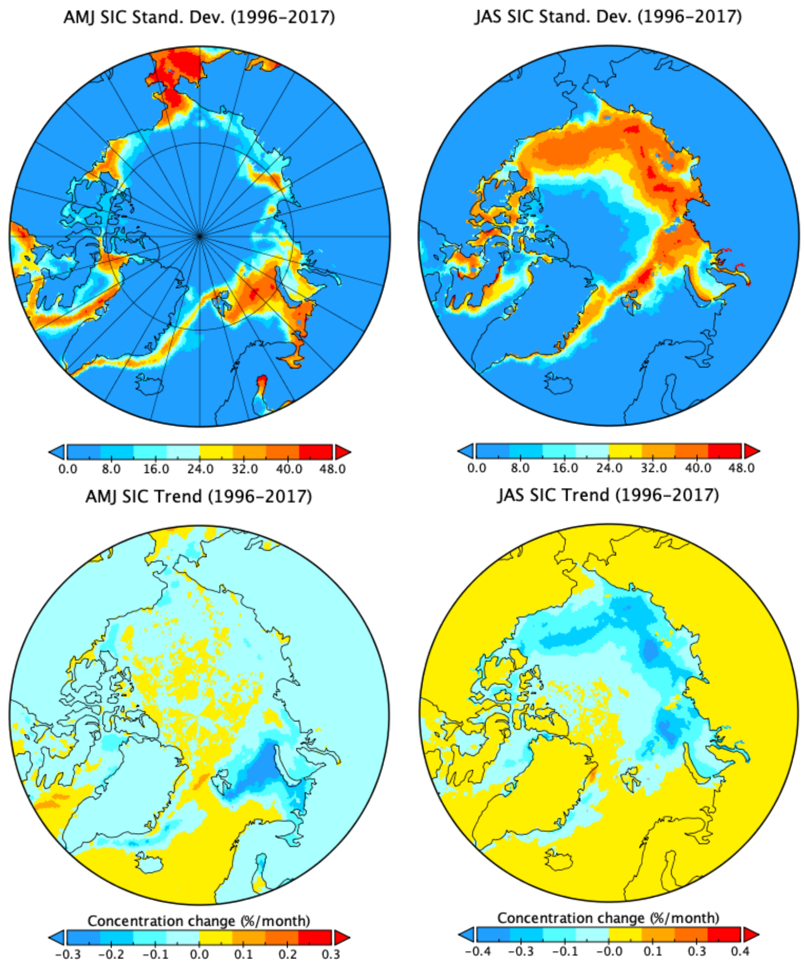

Fig. 4: Top row: standard deviation of sea ice concentration for AMJ (left) and JAS (right). The same as top row but for the trend in sea ice concentration. Data from Welsh et al. (2016).

That needs to come out in this whole discussion!

A28

We will add the following concepts resorting to the standard deviation and trend sign and magnitude of sea ice concentration (SIC), plotted in Fig. 4.

C28

(lines 321-324)

In both cases, open ocean areas and freshwater lower the albedo of the scene sensed by the satellites, as can be seen comparing the 15% and 75% SIC contours in Fig. 5. The areas that do not show statistical significance are generally above the perennial sea ice during AMJ. These months are characterised by a small standard deviation and by a non-existent SIC trend (not shown).

R28-1

P12, l317: In what sense do you mean that a trend in one location can be"compensated" by another trend at another location?

A28-1
In the sense that if the trend increases in one region, it must decrease by a similar amount in another region to result in a constant (negligible) pan-Arctic trend. We add this concept to the text at line 326.

R29
P13, l323: You do not have any observations around the North Pole!

A29
Yes. For this reason we do not write that the trends are valid at the North Pole (or above the North Pole), but "around the North Pole". We believe this information is readily inferred by the reader by simply looking at the figures.

R30
Figure 6: What are the black bars in the figure? They are much larger than anything else...

A30
They represent the 2-σ standard deviation of the respective trend, as in Figure 9. We will add the information in the caption.

R31
P15, l348-349: Why mention this at all if it's not significant? These changes are so small they are well within the measurement uncertainty; CTH cannot even be defined this accurately!

A31
We mention this because we have included the oxygen A-band (mostly affected by changes in CTH) in the set of analyzed reflectances.
Also because CTH is an important cloud parameter, possibly influencing the relationship $\tau = 3/2 \times \mathrm{LWP}/(\rho\,\mathrm{r_{eff}})$ through changes in ρ (assuming that the cloud bases are unchanged).
Therefore, we think that CTH has to be shown and commented on together with changes in optical thicknesses and water paths for consistency purposes. Moreover, the absent pan-Arctic trend in CTH reinforces the conclusion that Arctic climate change must be studied regionally.

R32
P15, l351: Suggest: "... that the temporal trend over two decades for τ of liquid clouds ..."

A32
We rephrase it.

R33
P17, l368-376: Again, most of the ice area lacks significance and there are still no observations at the North Pole; also at line 376.

A33

We will specify that the area of investigation is not the "North Pole" but close to it or poleward.

R34

P18, l398: So which is it -"and","or" or both? Or can't you tell. Why do you even state this here, when no results have been shown for Reff yet?

A34

We state this because of the nature of the formula relating τ to LWP and Reff. Given that the role of Reff cannot be unequivocally ascertained within the scope of this study, we have phrased "and/or". We believe it is a balanced formulation.

R35

P19, l405-407: And yet almost all data from in-situ studies suggest that CRE is positive; that clouds warm the surface almost always, especially over sea ice, except briefly in summer when surface albedo drops enough!

A35

We agree. We note however that in-situ studies are, by definition, limited in coverage and time, whereas satellite-based studies are not. We also encompass an enlarged Arctic region (north of 60°N), such that areas of lower surface albedo might be overrepresented. This can be seen in the new Tables 2 and 3 in which we report total CRF and its standard deviation. Where the climatological mean total CRF is negative, the standard deviation is the greatest and exceeds the mean, except for the North Atlantic and the Barents Sea. This holds for AMJ and We will specify this in the text.

Second, we clearly state throughout the text that a cooling tendency by clouds is superimposed on the top of the (climatological) warming. This information can be found in the abstract, in section 3.3 (on cloud radiative forcing), in the discussion section and in the conclusions.

Third, as most important remark: while true as general reasoning, recent results (Stapf et al., 2020) suggest that we might underestimate cooling by clouds.

This happens because the actual cloud-mediated interaction between surface and atmosphere makes the radiative field spectrally more broadband. As a consequence, even with the use of a realistic albedo parameterization of the surface including snow as well as sea ice instead of a constant albedo, the CRF becomes more markedly negative. Keeping the LW component unchanged, the (negative) SW component of the CRF doubles in the presence of clouds. We verbatim report here one relevant conclusion by Stapf et al.:

> *"The spectral weighting effect of downward irradiance appears to be dominant for snow surfaces and enhances the cooling effect of clouds at the surface ... For the ACLOUD campaign, characterized by snow on sea ice in the beginning melting season, the averaged shortwave CRF estimate over homogeneous sea ice of -32 Wm^{-2} (cooling) almost doubles to -62 Wm^{-2} when surface-albedo-cloud interactions are taken into account by using the proposed retrieval of cloud-free albedo from cloudy observations. Due to this consideration, the campaign-averaged total (shortwave plus longwave) CRF is shifted from a mainly warm-*

ing effect of clouds over sea ice to an almost neutral effect for the ACLOUD observations with relatively small SZA."

The results presented by Stapf et al. (2020), obtained during the ACLOUD airborne campaign, further corroborate our thesis that the optical thickness of clouds plays a major role in determining the overall sign of CRF. Not only because of a more effective reflectivity (SW shielding effect), but also because of the modulation of the radiation field between the surface and the clouds themselves.

Another important finding of their study is quoted verbatim (page 9906, second column last paragraph):

"The impact of the surface-albedo-cloud interaction becomes evident in the distribution of total (shortwave plus longwave) CRF (Fig.10c), which shifts for cloudy conditions from a significant total warming effect of 37 W m^{-2}over sea ice to an on average almost neutral effect (6 W m^{-2}) by applying α_{cf}. Also, the distribution of the $\Delta F(\alpha_{cf})$ indicates that already when the α_{cf} dropped approximately below 0.75 (mid of June) the cooling effect was dominant; meanwhile, the $\Delta F(\alpha_{cf})$ was positive throughout the campaign. Considering that the predominant surface type of the campaign was still sea ice covered by snow, the transition from a warming to a cooling effect of clouds could already start early in the season, even before the formation of melt ponds ..."

We will report relevant results of this study in the conclusions as outlook for a better assessment of CRF.

Stapf, J., Ehrlich, A., Jaekel, E., Luepkes, C., and Wendisch, M.: Reassessment of shortwave surface cloud radiative forcing in the Arctic: consideration of surface-albedo-cloud interactions, Atmos. Chem. Phys., 20, 9895–9914, https://doi.org/10.5194/acp-20-9895-2020, 2020.

C35

(lines 422 and ff)
Having defined the Arctic as all those areas north of 60°N encompassing also low-latitude areas of relatively dark surface, at a pan-Arctic scale clouds exert ...

(Conclusions lines 657–679)
Last, a better estimation of the cloud-free surface albedo would enable to pinpoint the broadband radiative interactions between the surface and the clouds. Recent results suggest that the SW effects of clouds at the surface almost double even in the presence of sea ice and snow. As a result, the total cloud radiative forcing shifts from warming to neutral values already at the beginning of the melt season in mid June (Stapf et al. 2020). This would imply that the results presented in this study underestimate the cooling effect by clouds.

R36

P19, ll417-418: Such low albedos basically mean open water; very few land surfaces and no sea ice has an albedo as low as 10%.

A36

We agree. We are citing here a result by Shupe and Intrieri (2004) to support our reasoning toward the influence of cloud τ on CRF.

R37

P19, l420: This sentence seems to be contradicting what was stated earlier. You need to be very careful here; are the clouds warming or are they cooling?

A37

In earlier statements, and throughout the main text, spring and summer months are discussed. This sentence is not contradicting earlier statements because we report the pan-Arctic annual climatological mean. It comprises also autumn and winter months, for which the SW reflection is almost absent.

Basically, we want to provide the reader with a broader context for our computations, as requested by referee#2, first review round. Moreover, we want to compare our CRF derivation with those found in literature, which employ a similar approach (e.g. Kay and L'Ecouyer, 2013).

We have restructured the beginning of Section 3.3 as follows, taking also into account R20 and R21 of referee#2, second review round.

C37

(lines 404–407)
The multi-year mean and trends of SW^{boa}, LW^{boa} and total CRF^{boa} for AMJ and JAS are plotted in Fig. 11. The pan-Arctic and regional values are reported in Tab. 2 for AMJ and in Tab. 3 for JAS. Although not the focus of the current study because of the observational limitations of R_λ^{TOA} during the polar night, an annual perspective on mean CRF can be found in Fig. D1 and CRF trends in D2, both at the surface and TOA.

(lines 408–411)
The climatological annual pan-Arctic total CRF (see Fig. D1) is positive at the surface with the sole exception of the Greenland Sea. Minimum values are found over Baffin Bay and the Barents Sea. Over the Arctic ocean, the total CRF is positive and amounts to ~7.0 W m^{-2}, which is lower than the 10 W m^{-2} reported by Kay and L'Ecuyer (2013, KE-13 hereinafter), while over land masses clouds warm the surface by ~11 W m^{-2}.

(lines 411–421)
Our results are directly comparable to those of KE-13. In general, the algorithm computing the broad-band fluxes is based on the same radiative transfer (Henderson et al, 2013) and the CRF is inferred from the difference between the all-sky and clear-sky atmospheric state, as in Eq. 2. Among the differences that may explain the bias in CRF between our results and those in KE-13 we count differences in spatial coverage of the Arctic and in the spectral albedo of ice- and snow-covered surfaces. KE-13 define the Arctic as the region between 70°and 82°N, while in this study the Arctic is defined between 60°and 85°N. The spectral surface albedo in this AVHRR record is 6% higher for wavelengths in the visibile and NIR (0.958 at 630 nm and 0.868 at 910 nm vs. 0.9/0.85 for the dry/melt months in KE-13), while it is lower for wavelengths in the SWIR (0.036 at 1.6 µm and 0.0 at 3.7 µm vs. 0.15/0.05 and 0.05/0.05 in KE-13). This means that the Arctic albedo in our record is more indicative of dry and bright surfaces at shorter wavelengths but more appropriate for melt and darker surfaces toward the infrared. This would

lead to an overall underestimation of the (negative) CRF in the SW.

R38
P19, l431: Is this result statistically significant?
Figure 10: Statistical significance please!

A38
Within the 20 years of our data set, none of the seasonal trends in total CRF, SW or LW, was statistically significant. We will comment Figure 10 with the aid of the following result, computed for this revision, which will be added to the Appendix B, after the paragraphs introducing the derivation of statistical significance.

The following table lists the first year of seasonal trend emergence at 95% for each of the 12 Arctic regions. The ToE values are added to the main text in the table of CRF trends.

C38
(lines 757-766)
The CRF trends of Fig. 11 are not statistically significant within the 20 years of the record. Therefore, we estimate the time of trend emergence (ToE) by finding the time T (in years) needed for the measured trend $\widehat{\omega}$ to become as twice as great than its standard deviation $\sigma_{\widehat{\omega}}$. The results are plotted in Fig. 5 and the first year of ToE is reported in Tab. 1 for the 12 Arctic regions of Fig. C1. The $\sigma_{\widehat{\omega}}$ is related to the standard deviation of the respective CRF time series σ_N , which can be regarded as the natural CRF variability, as follows (Weatherhead et al. 1998)

$$\sigma_{\widehat{\omega}} \approx \sigma_N \left[\frac{12\,dt}{T^3} \frac{1+\phi}{1-\phi} \right]^{\frac{1}{2}}. \tag{1}$$

In Eq. 1, we set $dt = 1$ because ToE is expressed in years and the autocorrelation $\phi = 0$ because we have measured the trend $\widehat{\omega}$ from the independent sample length of the time series (see App.B). In this case autocorrelative effects vanish already at the first lag of the monthly-sampled original time series.
The following table lists the first year of seasonal trend emergence at 95% for each of the 12 Arctic regions.

Weatherhead, E. C., Reinsel, G. C., Tiao, G. C., Meng, X.-L., Choi, D., Cheang, W.-K., et al. (1998). Factors Affecting the Detection of Trends: Statistical Considerations and Applications to Environmental Data. J. Geophys. Res. 103, 17149–17161. https://doi.org/10.1029/98JD00995

R39
P20, l445-446: This is the question isn't it? How do you know this as a fact?

A39 (see also A26 above)

This is a fact according to the following physical reasoning.

The atmosphere is made of gas, aerosols and clouds. In Section 2 (and Figure 1) we introduce the 10

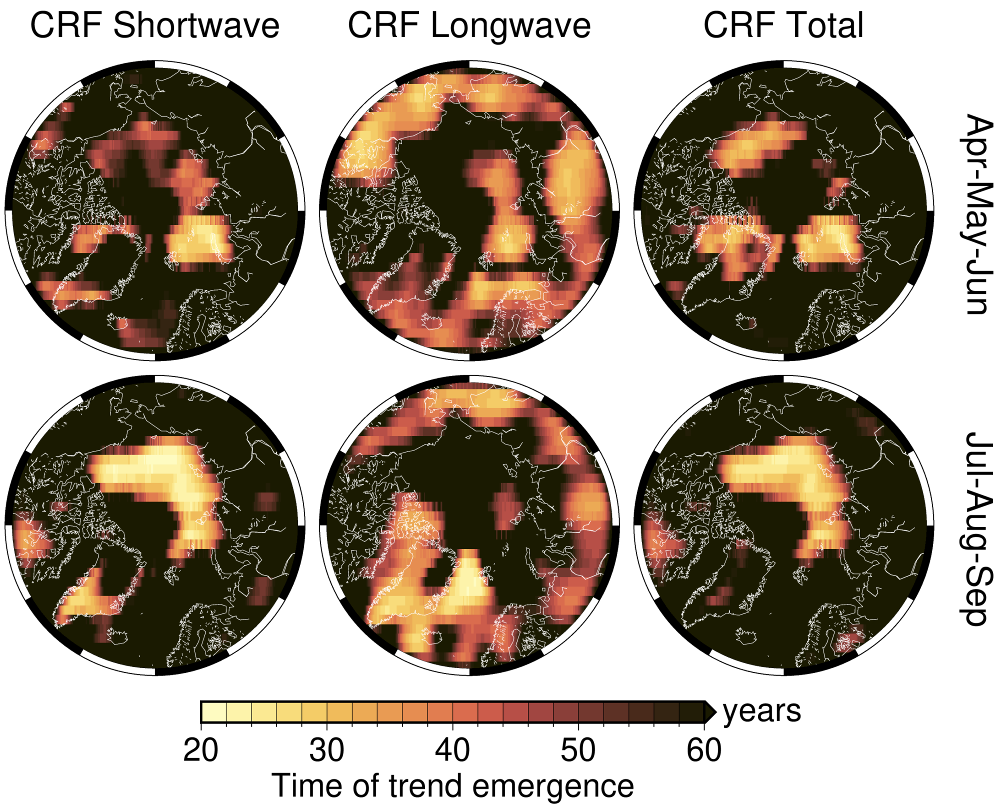

Fig. 5: Time of emergence of the trend to become statistically significant at 95%. The first year of trend emergence for each Arctic region is listed in Tab. 1.

Table 1: Time of emergence (ToE), in years, of the CRF seasonal trends for 12 Arctic regions. For each spectral window, the shortest ToE is boldface.

Region	CRF SW		CRF LW		CRF Total	
	AMJ	JAS	AMJ	JAS	AMJ	JAS
1. Beaufort Sea	42	**22**	48	35	29	24
2. Chuckchi Sea	23	**21**	27	22	24	24
3. East Siberian Sea	38	**21**	35	54	37	24
4. Laptev Sea	37	**22**	35	44	38	25
6. Kara Sea	**23**	**23**	31	45	25	25
7. Barents Sea	**23**	32	27	46	24	33
8. Greenland Sea	41	45	28	**22**	36	70
9. Greenland	34	**26**	42	**26**	**26**	46
10. Baffin Bay	35	60	45	34	**30**	61
11. Hudson Bay	64	**34**	59	66	48	38
12. Canadian Arch.	58	46	53	**32**	37	50

wavelength bands for the analysis of reflectance. Except for the oxygen A-band in the NIR, the other wavelengths in the visible are atmospheric windows only.

The average optical depth of the absorbing aerosols is ≈three orders of magnitude smaller than that of clouds (Chen et al. 2022).
Water absorption in the visible is largely negligible, hence an atmosphere with clouds can only increase TOA reflectance. We conclude that the atmosphere increases the TOA signal through reflection of light and it does not decrease the TOA signal through absorption of light, when measured at a wavelength inside an atmospheric window.
As the decrease in sea ice extent is common knowledge, without a (cloudy) atmosphere, the corresponding decrease in albedo would necessarily translate into a decrease of the TOA signal.

Chen, C., Dubovik, O., Schuster, G.L. et al. Multi-angular polarimetric remote sensing to pinpoint global aerosol absorption and direct radiative forcing. Nature Communications, 13, 7459 (2022). https://doi.org/10.1038/s41467-022-35147-y.

R40

P20, l447: Is it more liquid in the clouds or are there more liquid clouds? Or is it Reff ...?

A40

From a spaceborne perspective, for a given thermodynamic phase, a change in cloud optical thickness generates the same reflectance as a change in cloud fractional cover. The two cloud parameters are correlated. Fractional cover itself is a measure of occurrence of clouds inside a grid cell. Therefore, the two statements are intrinsically linked through cloud cover.
Where the liquid component in the clouds increases and the ice component decreases, we expect more liquid clouds for cloud cover being unchanged or increased. Thus, the occurrence of liquid clouds increases. Where cloud cover has decreased, the reader has to resort to Figures 7 and 9, in which we show that the portion of liquid in the clouds also systematically increases together with the decrease of the ice component. In this case, we infer that there is more liquid in the clouds.

We insert here Fig. 6, created for response A30 to referee #2 (see https://acp.copernicus.org/preprints/acp-2022-28/acp-2022-28-AC2-supplement.pdf).

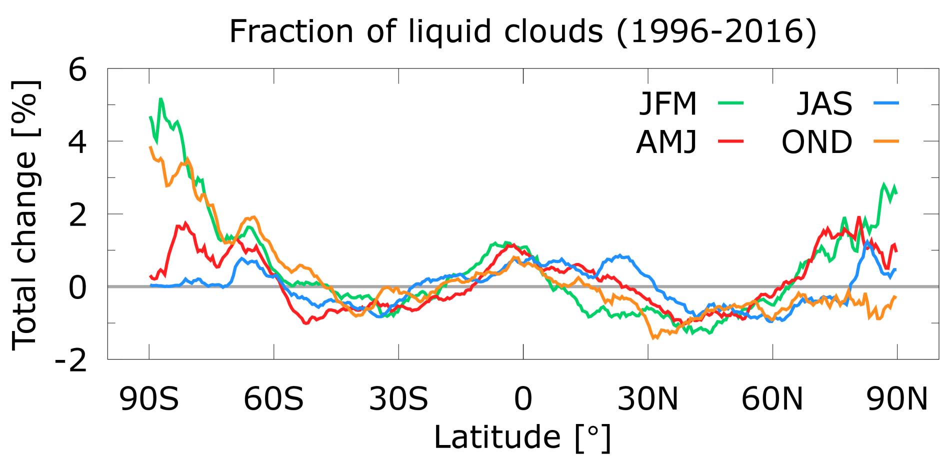

Fig. 6: Seasonal total change of fraction of clouds in the liquid phase.

R41
P21, l449: Is this a statically significant result? Just because the trends in both is doesn't mean the trend in the differences is.

A41
Correct. The difference in LWP and IWP trends shows up in the CWP map. There we see that both LWP and IWP trends must be significant for the CWP trend to be significant as well. We will comment on this in the revised manuscript as follows.

C41
(lines 485–489)
Additionally, from Fig. 11 it can be seen that only those CWP trends in both seasons are statistically significant where the LWP and IWP trends are statistically significant too. This holds for the Fram Strait, the northernmost area of the Canadian Archipelago, the Bering Strait, and the coastal area of the Siberian continent. Only in AMJ, more statistically significant patterns of CWP trend emerge, these comprising areas from the Laptev, Kara and throughout the northernmost part of the Barents Seas.

R42
P23, l503: Exactly what is it that is "the case"?

A42
The words *"This is the case . . ."* at line 503 refer to the previous statement at line 502. Resorting to Figure 12 of the manuscript, lines 500-503 explain that CRF is increasingly determined by changes in τ and LWP over darker surfaces rather than brighter surfaces. This is the case if one compares the plots for the Arctic spring with those of the Arctic summer.

R43
P23, l513: Do you mean "absorption" ? Sounds like a contradiction otherwise . . .

A43
Thanks for pointing this out. Here we mean a SW reflection by the clouds relative to that of the surface, and not an absorption of SW radiation by the latter. We will clarify this in the text.

C43
(line 534–535)
Those regions characterised by a darkening surface undergo a relative increase in SW reflection by more liquid clouds . . .

R44
P23, l522: This has nothing to do with "midsummer", which here is actually in spring. Rather it is late summer when the surface albedo is at a minimum; mid-September . . .

A44
We agree with the comment. The sentence refers to the results of Shupe and Intrieri (2004). They

write in the first paragraph, left column, at page 601, of their paper (and in the caption of their Figure 6):

> *"At SHEBA, $\partial CF_{LW}/\partial A_c$ was larger than $\partial CF_{SW}/\partial A_c$ for the majority of the year; thus, increases in cloudiness from current conditions would lead to a surface warming effect. Only in midsummer when the sun was highest in the sky did $\partial CF_{SW}/\partial A_c$ surpass $\partial CF_{SW}/\partial A_c$, indicating that increases in summer cloudiness would cool the surface."*

We clarify this point citing Shupe and Intrieri (2004) at the end of the sentence.

Shupe, M. D., & Intrieri, J. M. (2004). Cloud Radiative Forcing of the Arctic Surface: The Influence of Cloud Properties, Surface Albedo, and Solar Zenith Angle, Journal of Climate, 17(3), 616-628. `https://doi.org/10.1175/1520-0442(2004)017<0616:CRFOTA>2.0.CO;2`

C44

(line 561–563)

. . . thereby warming the surface while cloud cooling took place only in midsummer months with highest sun illumination and lowest surface albedo in late summer (Shupe and Intrieri, 2004).

R45

P23, l526-528: Awkward and confusing; if the ocean is not the surface of the water how can there be a convergence of it?

A45

Kapsch et al (2013) make the case that the ocean in Arctic spring can not be locally an appreciable source of water vapour in the boundary layer. The long-range transport of moisture (and local flux convergence) is held responsible for the increase in atmospheric opacity, then leading to an increase in downwelling LW fluxes.

Kapsch, M.-L., Graversen, R. G., and Tjernstrom, M.: Springtime atmospheric energy transport and the control of Arctic summer sea-ice extent, Nature Climate Change, 3, 744-748, `https://doi.org/10.1038/NCLIMATE1884`, 2013.

R46

Figure 12: Interesting figure but complicated. Try and modify so its easier to understand. What is on the y-axes?

A46

The figure has been redone adding labels to the y-axes, decluttering the individual plots by extracting the common scales of LWP and CFC and aligning the coefficients of determination for easier reading.

R47

P24, l543-P25, 547: Confusing sentence at the start: what is a "decreasing trend", is that the second derivative or do you mean a "downward trend"? Else, this is the question isn't it, so why wait until here? Either get to the bottom of the Reff problem or leave it open! Referring the reader to an Appendix

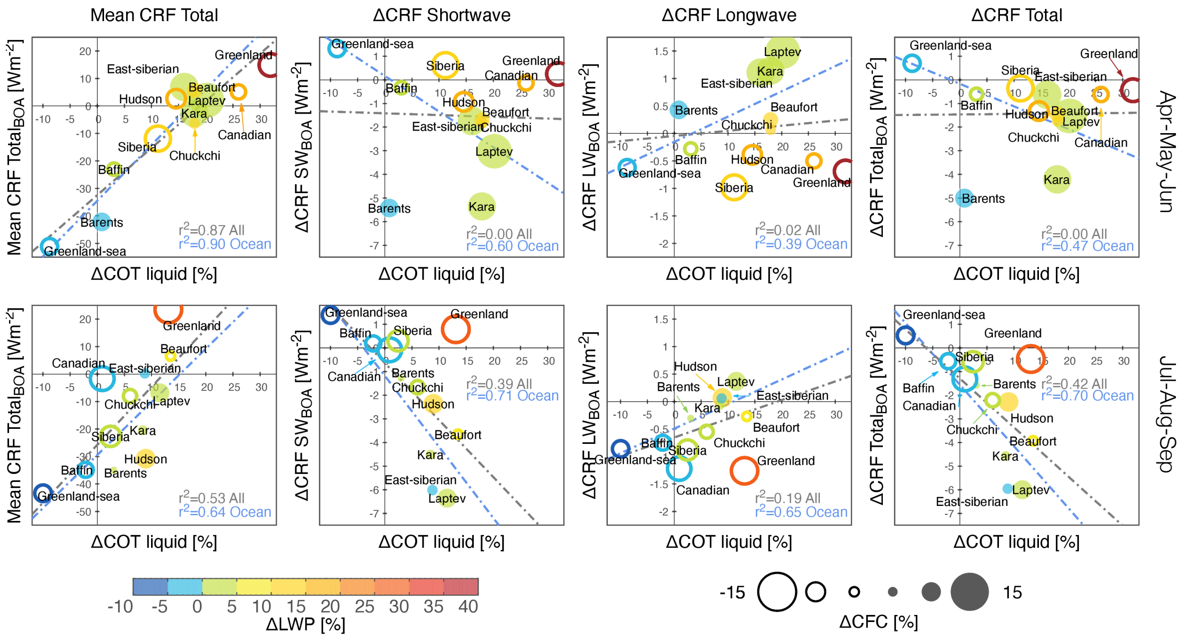

Fig. 7: New Figure 13 in the manuscript.

isn't good enough; either you do it you don't!

A47
For the first remark, we mean "downward trend".

Regarding the second remark, in the first version of the manuscript we left the map of Reff in the Appendix because we were unable to address the problem of aerosol-cloud-interactions (ACI) in the Arctic. The reasons are listed precisely at lines 547–560. There are currently no robust pan-Arctic aerosol data sets covering high latitudes nor data sets profiling the radius of liquid droplets or ice crystals (or mixed-phase) in clouds. Even if there were, we believe it is topic for a separate study.

As such, we agree with the referee that at this stage showing Reff is premature and not consistent. We remove Figure C1 and we rephrase the text to leave the Reff problem open.

See also A23 to referee#2, where we explain that spaceborne Reff values are representative of the clouds tops and the frequent mixed-phase occurs mostly in the middle of the clouds (results based on four airborne campaigns, totalling 18 flights).

R48
P25, l547: I don't see the "mostly decreasing" trends in Reff in Figure C1; the opposite I would say. It seems to be more increasing than decreasing, especially over the high Arctic.

A48
In fact our language was inaccurate at this point. We were referring to the Greenland trend and not

the pan-Arctic one. However, as said in the previous answer, we remove the figure and all mentions to Reff, leaving the ACI problem open throughout the text and mentioning it in the conclusion as outlook.

C47-48 together

(line 610 and 619–620)

To this end, the role of $\mathrm{r_{eff}}$ remains the unexplained factor in the relationship between τ and water path.

...

Satellite-derived single $\mathrm{r_{eff}}$ values, such those in the record analysed in this work, are only representative of the droplet/crystal population at a level of $\approx$1-τ from the cloud top (Platnick 2000).

(Conclusions: lines 669–673)

From an observational perspective, three aspects were not considered in this study. First, it was not possible to ascertain the role that variations in the effective radius of cloud droplets or ice crystals ($\mathrm{r_{eff}}$) has in determining changes in optical thickness. This was due to both the lack of extensive validation of single-valued $\mathrm{r_{eff}}$ and the absence of spaceborne datasets of aerosol components in the Arctic. These are needed to better characterise both the long-term direct (Chen et al. 2022) and indirect radiative effects specific to the Arctic (Curry, 1995).

Chen, C., Dubovik, O., Schuster, G. L., Chin, M., Henze, D. K., Lapyonok, T., Li, Z., Derimian, Y., and Zhang, Y.: Multi-angular polarimetric remote sensing to pinpoint global aerosol absorption and direct radiative forcing, Nature communications, 13, 1–11, https://doi.org/10.1038/s41467-022-35147-y, 2022.

Curry, J. A.: Interactions among aerosols, clouds, and climate of the Arctic Ocean, Science of the total environment, 160, 777–791, https://doi.org/10.1016/0048-9697(95)04411-S, 1995.

R49

P27, l632: I though RTOA was decreasing but not as much as you expect it to?

A49

Yes, correct. We replace "increase in R_λ^{TOA}" with "trends in R_λ^{TOA}".

Answer to Anonymous Referee #2, January 17, 2023

Structure of the document:

1. The remarks by the referee are black and labelled as $R_{1...n}$
2. Our answers are red and labelled as $A_{1...n}$. For each answer, we explicitly say how the text will be updated together with new figures, where appropriate.
3. Relevant non-trivial changes are verbatim reported in blue and labelled $C_{1...n}$ with the line numbers of the revised manuscript.

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

This is a second review of the paper in question. I continue to believe this is an important contribution. I also continue to believe the paper is not publishable in its current form. The writing is simply not to the standard that is necessary. In my specific comments below I started to note some of the wording issues, but at some point, I simply stopped doing this. The paper absolutely requires a technical editor. Beyond the editorial issues, I have additional concerns with the writing style, which were shared by the other reviewer of the first draft. There are often too many numbers presented, in oddly worded sentences, that make the reading very difficult. Overall, the paper was exceedingly hard to read and digest, even for a person such as myself with very extensive knowledge of the topic at hand. The presentation of material must be simplified, clarified, and in other ways cleaned up in order for a standard reader to have a successful interaction with this paper. Lastly, I still believe there are a number of mis-interpretations by the authors. These also need to be addressed, typically along with the following text that often builds on those mis-interpretations. Overall, I believe this is a very important study, but it simply cannot be published in this form. I would suggest some "distillation" of the manuscript to remove superfluous details and focus on the important points. And as noted above, after seeing two versions of this paper, I cannot imagine this paper ever being in publishable form without an external technical editor's involvement. I hope the authors are willing to take the steps to get this paper into proper form.

We appreciate the time devoted by the referee to scrutinize the manuscript. In the following we will provide answers to the general and specific concerns raised by the referee, providing a point-to-point answer and suggested changes.

General comments

R1

Title: The title has been changed to an unacceptable form. To clean up the English, the title could be written as: "Regional and seasonal changes in solar spectral reflectance and cloud radiative forcing by brighter, liquid water clouds in the Arctic from satellite remote sensing". But even this option just seems like a meandering title. The original title was much better, other than the use of "wetter" which is a loaded word that may or may not be accurate. I do not intend to write the title for this paper, but it should be carefully considered and re-written to be concise, clear, and true.

A1

After the first review round, we changed the title because of the discussion about the comparative of the word "liquid", i.e. wetter. Since this remark was also made by the first referee, we adopt the original title with "more liquid" instead of wetter and we replace "Satellite-based" with "from satellite remote sensing"

C1

The new title reads: "Regional and seasonal Arctic cooling by brighter and more liquid clouds from satellite remote sensing".

R2

Line 34: "are" should be "is" Line 41: "scatter" should be "scatters" Line 44: "lead" should be "leads" Line 103: "in" should be "of" Line 113: "provides" should be "provide" Line 188: "trends" should be "trend" Line 110: "Chukchi". This correction needs to be made elsewhere in the text; do a global search and replace. Line 200: perhaps "improved upon" Line 319: Another incorrect spelling of Chukchi. Line 412: Remove "optical"

A2

We grouped here all technical improvements, which are updated if the corresponding text has not been removed from the revised manuscript.

R3

Line 49: "And is located in the North Atlantic and circumpolar ocean waters" While that might be where the highest CFC values are in summer, there is also CFC elsewhere in the Arctic at this time, and the literature also suggests a maximum CFC in many of these other areas at that time of year (i.e., this is not solely a phenomena at the locations indicated).

A3

This sentence has been removed in the revised version.

R4

Line 84 – 101: This paragraph contains information that appears to be true, but is generally oddly worded. As a person who has studied these processes for multiple decades, I had to read many of these sentences multiple times to make sense of them. The writing of this paragraph is indicative of the overall challenging writing of this introductory section. There are missing uses of the word "the", or sometimes "the" is used when it should not be. There is confusing use of plural vs. singular, etc. I'm only mentioning some of these issues in my comments here as there are really too many for a reviewer to manage. I suggest the use of a proper scientific editor to address these issues.

A4

We will restructure the introduction, simplifying the language and we will resort to an editor for copy-editing.

R5

Line 114: What goal? No goal has been outlined.

A5

The goal is to collect insights into the evolution of the Arctic, as written at line 113. We will make the goal of the study more explicit.

R6

Section 2.2: I'm missing a clear definition of CRF. CRF can be calculated in multiple ways, with various corrections and/or adjustments. I do not see where CRF is actually defined, nor any discussions of the implications of defining it that way.

A6

Section 2.2 is not about cloud radiative forcing (CRF) yet. This section describes the basic optical and physical properties of clouds and the flux components (in clear-sky and all-sky state) that will be used later on to calculate CRF. Consistently, the definition of CRF is introduced in Section 3.3 where we make the first use of it. This is because the first part of the result section (3) deals with reflectances at TOA (Section 3.1) and the second with cloud properties (Section 3.2).

R7

Line 188-190: The sentence starting "Inspection ..." Needs to be re-written as it appears to be missing a few words and had incorrect grammar.

A7

The sentence has been corrected.

C7

(lines 154– 156)
Inspection of the time series of cloud properties and fluxes for the AM series showed that the drifts in local overpass time of the NOAA-12 platform before 2003 lead to calibration offsets and that the scan motor errors of the NOAA-15 platform lead to data gaps.

R8

Line 197-204: I do not believe this list of i), ii), and iii) is done correctly. Periods embedded into individual points that are linked via semi-colons is not the proper form. Some other form of making the list is needed.

A8

We will double check with an editor for the correct form.

R9

Line 221: In spite of a reference to other work, I have a very hard time believing that OLR can be estimated with an accuracy of 0.3 W/m2 given the uncertainty in atmospheric profiles and especially clouds, as is discussed in the following sentences (height issues, adiabatic assumptions, etc). This is also true given that the surface (or BOA) upwelling LW radiative flux bias is given as 3 W/m2 in line

238. This implies that the atmosphere (with its many uncertainties) improves the representation of OLR relative to upwelling LW at the surface. It is entirely possible that I'm missing something here as this whole paragraph on uncertainties is written in a very confusing way. I have to read some of these sentences over and over to try and figure out what they mean, and I'm not always sure I get it right in the end.

A9

Thanks for pointing this out. The referee is correct. There was a typo in the text. The bias is 3 Wm^{-2} and not 0.3 Wm^{-2}, as compared with measurements by the broadband radiometer GERB (Geostationary Earth Radiation Budget) onboard the MSG-2 (Meteosat Second Generation) platform (see page 5 in Christensen et al. 2006). We will update the sentence.

C9

(lines 199–203)

The combination of the above factors yields an accuracy of 3 Wm^{-2} in outgoing LW radiation when compared with observations by the broadband radiometer GERB (Geostationary Earth Radiation Budget) onboard the MSG-2 (Meteosat Second Generation) platform. This value is line with the radiometric accuracy of GERB, which is 1% for clear-sky fluxes at TOA (Clerbaux et al., 2008)

Clerbaux, N., Russell, J., Dewitte, S., Bertrand, C., Caprion, D., De Paepe, B., Gonzalez Sotelino, L., Ipe, A., Bantges, R., and Brindley, H. (2009). Comparison of GERB instantaneous radiance and flux products with CERES edition-2 data. Remote Sensing of Environment, 15:102–114. `doi:10.1016/j.rse.2008.08.016`.

R10

Line 237-239: I don't know what information is being conveyed here. Brackets, parentheses, "in range", . . . It might be preferable to try an convey all of this information in a small table instead of in sentences that are hard to understand.

A10

This is the standard mathematical notation that represents the set of all real numbers x greater or equal to a and less or equal to b.

$$[a, b] \Rightarrow \{x \in \mathbb{R} : a \leqslant x \leqslant b\}$$

R11

Line 240-242: This is an important statement. The authors, both here in the text and in their response to reviewer comments, seem to be strongly confirming the accuracy of these measurements. One part of that claim is the "validation with BSRN measurements". However, it is very hard to "validate" satellite measurements with those made at 2m above a single location on the surface. I've been involved in multiple studies of this nature and the comparisons reveal all kinds of issues, especially when attempting to consider upwelling SW at the surface.

A11

(see also A21 to the first referee where we give more precise figures on this topic)

We agree with the referee that the validation of spaceborne fluxes with ground-based stations is a delicate exercise. We recall the relevant papers cited in this section and we report for convenience the most relevant result (i.e., Fig. 5, p 48, in `https://doi.org/10.5194/essd-12-41-2020`). Our summary in Section 2.2 briefly describes the features of the next Figure 1.

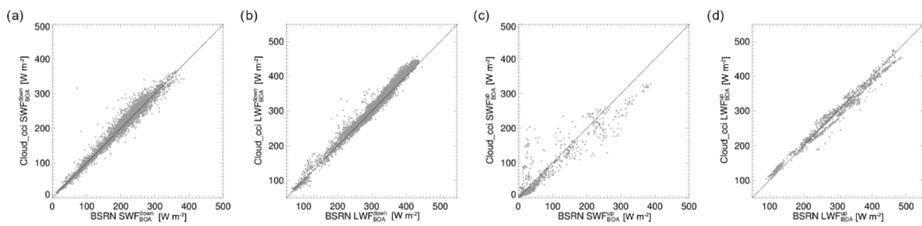

Fig. 1: Comparison of bottom of atmosphere (BOA) shortwave (SW; panel a) and longwave (LW; panel b) downwelling fluxes with ground-based reference measurements taken at globally distributed Baseline Surface Radiation Network (BSRN) sites for which equivalent reference data were available. Panels (c) and (d) are as in (a) and (b) but for upwelling fluxes. Period 2003–2016. (From `https://doi.org/10.5194/essd-12-41-2020`).

R12
Line 244-248: This assumption of a fixed surface albedo is troubling since the study is focused on the notion of changing reflectivity of the atmos-surface system in the Arctic. It is clear that surface albedo is not the same everywhere and all of the time. The authors are correct that uncertainties in albedo will reflect themselves into uncertainties in CRF. But they do not appear to attempt any amount of quantification of that issue here, but it actually matters because these differences in surface albedo can, for example, determine the balance of SW and LW CRF and ultimately determine the sign of the overall CRF.

R13
Line 247-248: If your assumed surface albedo is underestimated (i.e., too small), then the SW CRF would be a larger cooling of the surface. Assuming LW CRF is the same in either case, then the overall effect is a larger cooling of the surface by the clouds than if you had the correct surface albedo.

A12-13 *(We group the two answers into a single one, because the topic is the same. See also A35 to referee#1).*

While true as general reasoning, recent results suggest that our estimation of cooling by clouds is underestimated and can be considered as a conservative estimate.

This happens because actually the cloud-mediated interaction between surface and atmosphere makes the radiative field more broadband. As a consequence, even with the use of a realistic albedo parameterization of the surface including snow as well as sea ice, the CRF becomes even more markedly negative. Keeping the LW component unchanged (as rightly suggested by the referee), the (negative) SW component of the CRF doubles in the presence of clouds. The results presented by Stapf et al.

(2020), obtained during the ACLOUD airborne campaign, further corroborate our thesis that the optical thickness of clouds plays a major role in determining the overall sign of CRF. Not only because of a more effective reflectivity (shielding effect), but also because of the modulation of the radiation field between the surface and the clouds themselves.

We point the referee to the main conclusion of the following study.

We will report relevant results of this study in the conclusions as outlook for a better assessment of CRF.
Stapf, J., Ehrlich, A., Jaekel, E., Luepkes, C., and Wendisch, M.: Reassessment of shortwave surface cloud radiative forcing in the Arctic: consideration of surface-albedo-cloud interactions, Atmos. Chem. Phys., 20, 9895–9914, https://doi.org/10.5194/acp-20-9895-2020, 2020.

C13

(Conclusions lines 675 – 679)
Last, a better estimation of the cloud-free surface albedo would enable to pinpoint the broadband radiative interactions between the surface and the clouds. Recent results suggest that the SW effects of clouds at the surface almost double even in the presence of sea ice and snow. As a result, the total cloud radiative forcing shifts from warming to neutral values already at the beginning of the melt season in mid June (Stapf et al. 2020). This would imply that the results presented in this study underestimate the cooling effect by clouds.

R14

Line 317: "compensated" is not correct here.

A14

We will rephrase as follows, taking into account also A28 to referee#1.

C14

(lines 313 – 315)
For AMJ a significant negative trend over the Barents Sea is balanced by a positive RTOA trend at all three wavelength bands over Greenland, the Canadian Archipelago, and Western Arctic Seas, such that the pan-Arctic trend remains almost unchanged.

R15

Line 349-350: I don't understand the last part of the sentence after "or"

A15

The second part of the sentence (after "or") represents the following reasoning: changes in measured reflectance at TOA may be due to an increase in cloud cover (which simultaneously masks more surface area) or, in the case of decreasing cloud cover, to a larger surface area becoming visible to the satellite. It is implicit in the reasoning that the spectral response of clouds and Arctic surface (sea ice or snow) at these wavelengths is similar (see Fig. 1 of the manuscript).

R16

Line 373-384: The paper distinguishes ice from liquid clouds when discussing the optical depth. From a simple phase perspective there are ice, liquid, and mixed-phase clouds. The paper does not discuss this distinction, nor does it clarify what is actually meant by the liquid and ice properties that are presented. For example, do mixed-phase clouds (which are very frequent in the Arctic) contribute to the statistics that are presented for both liquid and ice? Does the cloud algorithm distinguish the contributions from each phase such that they each contribute to their respective statistics?

A16

See also A23 below.

The algorithm does not distinguish clouds in thermodynamic mixed-phase because it is trained with the CALIOP phase classification. CALIOP, at the moment, does not deliver information on the mixed-phase of clouds.

R17

Line 409: More than "offsetting" this is "dominating" or "being larger than".

A17

We will update the text accordingly.

R18

Line 415-416: SW is larger than LW CRF, such that total CRF is negative, largely because of the low albedo surface while the cloud optical depth is likely secondary. The same clouds over the Greenland Ice Sheet would have a net positive CRF. This point is kind of alluded to in the following sentence, but the interpretation in this sentence is wrong.

R19

Line 417-419: Speaking of the following sentence. The first part is correct. However the second part following "whereas" is incorrect. SW CRF typically does NOT offset LW CRF over high surface albedos.

A18-19 together

For the remark on the second sentence, we will specify that the liquid water content in the clouds must be less than 30 g m^{-2} at SZA greater 50 °for SW CRF to be greater than LW CRF. We note also that at higher surface albedos, the balance between SW and LW CRF becomes more sensitive to changes in LWP and τ-liquid and SZA. This is a more precise citation of Shupe and Intrieri, Fig. 7., in view also of the average SZA values for the seasons of our paper plotted in Fig. 2.

In general, while the comments by the referee are true, newest results collected during the ACLOUD campaign indicate that changes in cloud optical thickness increasingly determine the sign of CRF (i.e., the balance between SW and LW), even in the presence of highly reflective surfaces. We point the referee to the relevant conclusions in Stapf et al. (2020):

> *"The spectral weighting effect of downward irradiance appears to be dominant for snow surfaces and enhances the cooling effect of clouds at the surface . . . For the ACLOUD campaign, characterized by snow on sea ice in the beginning melting season, the averaged*

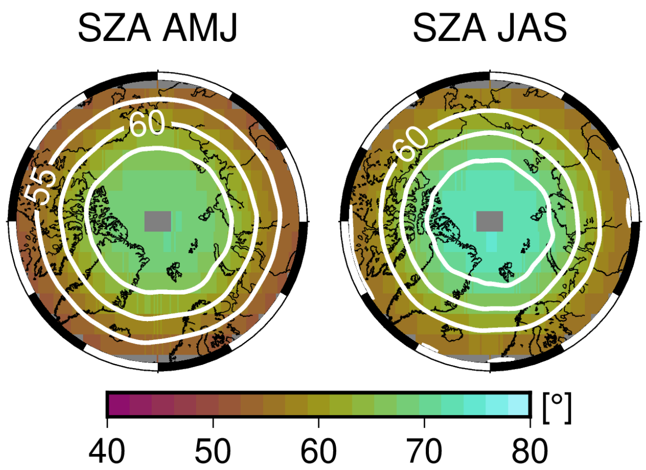

Fig. 2: Seasonal solar zenith angles, with 5° isolines, supporting the interpretation of Fig. 7 in Shupe and Intrieri (2002).

> *shortwave CRF estimate over homogeneous sea ice of −32 Wm^{-2} (cooling) almost doubles to −62 Wm^{-2} when surface-albedo-cloud interactions are taken into account by using the proposed retrieval of cloud-free albedo from cloudy observations. Due to this consideration, the campaign-averaged total (shortwave plus longwave) CRF is shifted from a mainly warming effect of clouds over sea ice to an almost neutral effect for the ACLOUD observations with relatively small SZA."*

and that even for a surface albedo of 0.75 Stapf, J., Ehrlich, A., Jaekel, E., Luepkes, C., and Wendisch, M.: Reassessment of shortwave surface cloud radiative forcing in the Arctic: consideration of surface-albedo-cloud interactions, Atmos. Chem. Phys., 20, 9895–9914, `https://doi.org/10.5194/acp-20-9895-2020`, 2020.

We will add this information to the text.

C18-19

(lines 435 – 440)
At low surface albedos, typically less than 0.2, SW CRF outweighs LW CRF for the great majority of clouds, irrespective of their water content, τ-liquid and sun illumination. Typical values of solar zenith >65°correspond to latitudes north of 75°N, encompassing the Arctic ocean both in AMJ and JAS. Resorting to Fig.7 in Shupe and Intrieri (2002), we obtain a lowest LWP threshold of ∼20 g m^{-2}at surface albedo 0.5 and ∼250 g m^{-2}at albedo 0.8. This means that with increasing surface albedo, SW radiative effects may offset those by LW only at specific values of LWP and sun illumination angles, thus making CRF more sensitive to changes in cloud τ-liquid.
...

(Conclusions, lines 675 – 679)
Last, a better estimation of the cloud-free surface albedo would enable to pinpoint the broadband radiative interactions between the surface and the clouds. Recent results suggest that the SW effects of

clouds at the surface almost double even in the presence of sea ice and snow. As a result, the total cloud radiative forcing shifts from warming to neutral values already at the beginning of the melt season in mid June (Stapf et al. 2020). This would imply that the results presented in this study underestimate the cooling effect by clouds.

R20

Line 420: I appreciate having the annual perspective here, but in the response to reviews that authors argued strongly that they were NOT including the winter season for a variety of reasons including problematic observations. So why include annual statistics here that must be based, in part, on those problematic measurements?

A20

These numbers are not based on problematic measurements because we make use of two groups of instruments in our study. The first group comprises spectrometers that measure reflected sunlight in the UV-NIR range. For this reason, the winter seasons over the Arctic cannot be measured by these instruments due to the obvious lack of sunlight and, therefore, reflection. The second group of instruments (used to derive both cloud properties and fluxes) measure emitted radiation in the TIR. Thus they can measure LW even in the absence of reflection.
In the main text we limit the analysis to the months between April and September to make use at the same time of all information from both groups of instruments. Coherently, we have put maps and statistics of all the seasons to the Appendix. Second, we want to provide the reader with a broader reference context to evaluate our seasonal results, as requested by a referee during the first review.

R21

Line 422: Without a clear definition for how CRF has been calculated, and a discussion of how that method is similar to or different from Kay and L'Ecuyer, it is hard to assess whether or not one would expect these values to be the same or not.

A21

The definition of our CRF calculations is given right at the beginning of Section 3.3. Our method is similar to a great extent to that of Kay and L'Ecuyer (2013), in that our data set not only uses same inputs (see Table 1, p 7220, in Kay and L'Ecuyer (2013)) but also computes CRF as difference between the all-sky and clear-sky states of the atmosphere. As such the quantities reported in our work and in the mentioned paper are directly comparable.
We will describe the differences as follows.

Kay, J. E., and T. L'Ecuyer (2013), Observational constraints on Arctic Ocean clouds and radiative fluxes during the early 21st century, J. Geophys. Res. Atmos.,118, 7219–7236, https://doi.org/10.1002/jgrd.50489

C21

(lines 410 – 421)
... which is lower than the 10 $\mathrm{W\,m^{-2}}$ reported by Kay and L'Ecouyer (2013, KE-13 hereinafter), while over land masses clouds warm the surface by $\sim$11 $\mathrm{W\,m^{-2}}$. Our results are directly comparable to those of KE-13. In general, the algorithm computing the broadband fluxes is based on the same radiative

transfer (Henderson et al, 2013) and the CRF is inferred from the difference between the all-sky and clear-sky atmospheric state, as in Eq. 2. Among the differences that may explain the bias in CRF between our results and those in KE-13 we count differences in spatial coverage of the Arctic and in the spectral albedo of ice- and snow-covered surfaces. KE-13 define the Arctic as the region between 70°and 82°N, while in this study the Arctic is defined between 60°and 85°N. The spectral surface albedo in this AVHRR record is 6% higher for wavelengths in the visibile and NIR (0.958 at 630 nm and 0.868 at 910 nm vs. 0.9/0.85 for the dry/melt months in KE-13), while it is lower for wavelengths in the SWIR (0.036 at 1.6 μm and 0.0 at 3.7 μm vs. 0.15/0.05 and 0.05/0.05 in KE-13). This means that the Arctic albedo in our record is more indicative of dry and bright surfaces at shorter wavelengths but more appropriate for melt and darker surfaces toward the infrared. This would lead to an overall underestimation of the (negative) CRF in the SW.

R22

Line 448-449: I don't understand the point of this sentence. Obviously thermodynamic phase processes are physical.

A22

Indeed. We intend to say that the phase separation manifests itself not only at the physical scale of thermodynamics, but also in the integral optical quantities as inferred from the satellites, in this case optical depth. We change the sentence as follows.

C22

(lines 466–467)
Therefore, the thermodynamic phase separation of clouds manifests itself not only in the integral optical quantities but also in the water mass amount.

R23

Line 450-451: These statistics are interesting, but without a clear definition of ice vs liquid clouds (which also takes into account the frequent mixed-phase clouds), it is hard to know what these statistics even mean.

A23

In the AVHRR satellite record we use, the cloud phase can be only ice or liquid and the mixed-phase is not identified. This is because the data set we use is neural-network trained on the CALIOP cloud phase itself. CALIOP does not natively provide information on the mixed-phase in clouds. The input signal for AVHRR comes from the reflectances measured at 0.6, 0.8 μm and 3.7 μm. Given the different complex refractive index between water and ice phase across the SWIR wavelengths, the method is effective in separating the two phases.

We note that in our data set the thermodynamic phase is representative for the top of the clouds. This is because passive sensors (such as AVHRR) are not directly designed to derive in-cloud extinction profiles. For this reason we suggest the adoption of more advanced techniques to profile clouds, which will eventually provide a picture of the mixed-phase even with passive sensors (see lines 557-563).

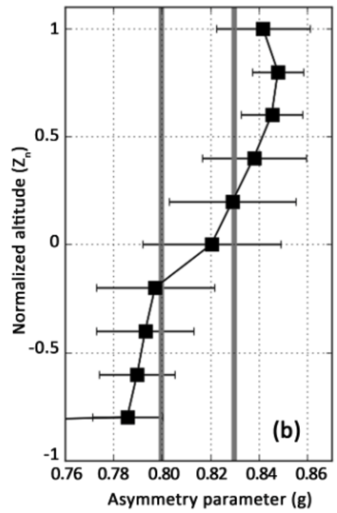

Fig. 3: Mean vertical profile of asymmetry parameter (for all the campaigns). The grey bars indicate the threshold g values for the assessment of ice ($g < 0.80$), mixed ($0.80 < g < 0.83$) and liquid ($g > 0.83$) cloud phases (from Mioche et al., 2017)

It is worth noting that Arctic cloud tops are predominantly in the liquid phase, whereas the mixed-phase occurs in the middle of the clouds. This is the outcome of four airborne measurement campaigns (18 flights in total), reported by Mioche et al. (2017). For convenience, we report here the relevant figure (Fig. 2-b in the aforementioned paper).

Mioche, G., Jourdan, O., Delanoe, J., Gourbeyre, C., Febvre, G., Dupuy, R., Monier, M., Szczap, F., Schwarzenboeck, A., and Gayet, J.-F.: Vertical distribution of microphysical properties of Arctic springtime low-level mixed-phase clouds over the Greenland and Norwegian seas, Atmos. Chem. Phys., 17, 12845–12869, `https://doi.org/10.5194/acp-17-12845-2017`, 2017

We add to Section 2.2 ("Cloud products") a paragraph describing the limitations of the algorithm in detecting mixed-phase clouds and the results of Mioche et al. (2017) as follows

C23
(this changes apply to R16 as well)
(lines 174–181)
In this AVHRR satellite record, the cloud phase can be only liquid or ice. The input signal for AVHRR comes from the reflectances measured at 0.6, 0.8 and 3.7 micron. Given the different complex refractive index of water and ice phase across the SWIR wavelengths, the method is effective in separating the two phases. It is worth noting that Arctic cloud tops are predominantly in the liquid phase, whereas the mixed-phase occurs in the middle of the clouds. This is the outcome of four airborne measurement campaigns, totalling 18 flights, reported in Mioche et al (2017). Nonetheless, the mixed-phase is not identified, despite its all-season occurrence (Morrison et al, 2019) and role in the Arctic climate (Tay and Stovrelmo, 2018). This is because the data set is neural-network trained on the CALIOP cloud

phase, which does not natively provide information on the mixed-phase in clouds.

(Conclusions, lines 673–675)
Second, it was also not possible to single out the occurrence and radiative forcing of mixed-phase clouds, because the algorithm used to generate the record of cloud properties is not capable to effectively detect them.

R24
Line 454: I don't understand the statistics. It says -0.51 +/- 11.01%. So does this mean from +10.5 to -11.52?

A24
Yes. CWP trends are highly variable across the Arctic. It can be seen from Fig. 11 that the trends are strongly regional. This information can be directly gathered from Fig. 11 in the manuscript.

R25
Line 455: "over areas of sea ice melting". Perhaps you mean over areas that have lost sea ice? "Areas of sea ice melting" = everywhere there is see ice in summer, and I don't believe you mean this.

A25
Yes. We are not precise in the language here. We will replace "sea ice melting" with "sea ice loss".

R26
Line 506-507: Yes, this is simply a rather obvious statement of the definition of surface cooling.

A26
It is true. The sentence reads redundant and we delete it, harmonizing the explanation of Figure 12 throughout the paragraph.

R27
Line 509-511: This sentence is indicative of a writing style that is not very effective in my opinion, and apparently the other original reviewer as well. Lots of numbers with fragments of sentences. It simply is to hard to read and comprehend. If the authors insist on including so many specific numbers they must do so in a way that is clear and straightforward to the reader, otherwise the paper will just be too hard to read.

A27
Following the suggestions of both referees, we will remove from the text the numbers, keeping the most significative ones, and report them as a separate table. In the table we will list regional and seasonal trends in CRF together with relevant statistics.

R28
Line 531-532: How can tau-liquid increase when there is less liquid water content, and also apparently an increase in effective radius? For the same amount of liquid, and increase in effective radius would lead to a lower optical depth. Thus, for a decrease in liquid water AND an increase in effective radius,

one would definitely expect the optical depth to decrease. What is the explanation?

A28

Following the suggestion of referee#1 we remove any mention of effective radius from the discussion because the Reff dataset is, in our view, not consolidated enough to draw any sound scientific conclusion. For this reason, we suggest to advance algorithmic techniques to derive in-cloud profiles of liquid droplets or ice crystals (lines 557–560).

We do not have an explanation yet. We note that in JAS the East Siberian Sea has experienced a decrease in cloud altitude (which is a well behaved parameter in the Cloud_cci record over most of the considered Arctic stations, Vinjamuri et al. 2022). For a sub-adiabatic cloud, a decrease in cloud altitude implies a decrease in ρ, which could translate into an increase in τ.
Another possibility is that with the concurrent decrease of IWP and an increase in cloudiness, the relative occurrence of liquid clouds increases. As such, aggregated values of τ would see a corresponding increase. In this regard, see also A31.

Vinjamuri K.S., Vountas M, Lelli L., Stengel M., Shupe M.D., Ebell K., Burrows J.P., Validation of the Cloud_CCI cloud products in the Arctic, Atmos. Meas. Tech., submitted, 2022

C28

(lines 547–551)
One exception is the East Siberian Sea in JAS where τ-liquid of clouds grows in spite of a lower content of liquid water. Notwithstanding the unexplained contribution of $\mathrm{r_{eff}}$, we note that in JAS the East Siberian Sea has experienced a decrease in cloud altitude (see Fig. 11), which is a well behaved parameter in the AVHRR record over most of the Arctic (Vinjamuri et al. 2022). Assuming that the cloud bases are unchanged, any change in CTH can possibly influence the relationship $\tau = 3/2 \times \mathrm{LWP}/(\rho\, \mathrm{r_{eff}})$ through changes in ρ.

R29

Line 604: This is again an incorrect use of "melting ice". Perhaps it is best to say, "In spite of the retreating ice coverage ..." Or something of that nature.

A29

We thank the referee for noticing inaccurate wording. We rephrase accordingly.

R30

Line 608-609: ???? This sentence is apparently missing some words?

A30

(lines)
We update the sentence as follows

C30

(lines 644–645)
The periennal and marginal sea ice zones (from the Beaufort Sea until the Laptev Sea) have increasingly

reflected less light in both seasons, while in JAS a generally greater R_λ^{TOA} decrease is observed.

R31

Line 615-616: is this a statistically significant increase in the "occurrence" of liquid phase clouds or the "condensed mass" of these clouds (and same for the ice)?

A31

From the spaceborne perspective, for a given thermodynamic phase, a change in cloud optical thickness generates the same reflectance as a change in cloud cover. Fractional cover itself is a measure of occurrence of clouds inside a grid cell. Therefore, the two statements are intrinsically linked through cloud cover. Where the liquid component in the clouds increases and the ice component decreases, we expect more liquid clouds for cloud cover unchanged or increased. Thus, the occurrence of liquid clouds increases. Where cloud cover has decreased, the reader has to resort to Figures 7 and 9, in which we show that the portion of liquid in the clouds also systematically increases together with the decrease of the ice component. In this case, we infer that there is more liquid in the clouds.

R32

Line 618: What is radiative decoupling from the surface? Sounds interesting but I'm not sure how radiation becomes decoupled.

A32

In this context, radiative decoupling means that the atmosphere contributes the most to the signal at TOA and the surface the least. This is because the downwelling irradiance of the atmosphere is not itself effectively reflected by a surface with low albedo and multiple scattering below the clouds does not prevail.

C32

(lines 654–657)

This especially holds in summer months when the atmosphere is radiatively decoupled from a relatively dark surface, i.e. multiple scattering between the atmosphere and the surface is not substantial.

R33

Line 620: The language is getting sloppy. The prior sentence talked about the change of mass from ice to liquid. But then here in this sentence is states that the "net change to more liquid clouds", which again suggests fractional occurrence. It is imperative that all references to these processes either refer to mass or occurrence explicitly because these are two entirely different concepts with different implications. Without those explicit references, the reader will not understand which is being discussed.

A33 Agreed. We will harmonize the reference to this change in thermodynamic phase throughout the text, while making explicit when we refer to optical properties of clouds instead.

C33

(lines 466–467)

Therefore, the thermodynamic phase separation of clouds manifests itself not only in the integral optical quantities (Fig. 8) but also in the water mass amount, considering Fig. 12.

R34

Line 623: Another incorrect use of “melting” sea ice.

A34

Thanks for pointing this out. We remove “melting”.

R35

Line 633: I believe this implies an “increasing amount of liquid cloud droplets”. i..e, they could be supercooled or not (and might be increasingly not supercooled over regions where the sea ice has retreated.

A35

Correct. In this way the sentence is more accurate. We reword it.

C35

(lines 682–683)

. . . that implies an increasing amount of supercooled cloud droplets. At the same time, also the occurrence of cloud droplets at temperatures above the freezing point might increase, especially over regions where sea ice has retreated.

R36

Line 633-634: “The higher reflectance of clouds results in a more negative radiative forcing at the surface” is only true over certain surfaces and at certain sun angles (i.e. times of year).

A36

We will specify it modifying the sentence as follows.

C36

(line 685)

. . . especially where sea ice retreats and most notably in summer.

R37

Line 636-638: This line of reasoning is opposite to what one would expect based on the Francis and Vavrus type mechanism. Rather, if the meridional temperature gradient is strengthened, that increases the speed of the jet stream and diminishes the north-south exchange. I'm not saying that this mechanism is true (there is clearly a lot of debate about it in the research community), but the statement made here is directly opposite the one being discussed by the community.

A37

In general the comment is correct and we agree with the referee. At this point we believe it is advantageous to add to the conclusions an article reviewing the Francis-Vavrus mechanism, its rebuttal by Barnes and the ensuing debate, that is Coumou et al. (2018). That said, one must distinguish the time and spatial scales at which these phenomena occur. Temporally, our study deals with changes on time scales close to the 30-year time window that defines the climate normal. Interannual variability is not investigated. Spatially, it has been shown that the meridional inflow of energy into the Arctic is connected to the gradient of surface temperatures and it occurs along the North Atlantic pathway but not along the North Pacific or the Siberian pathways (Mewes and Jacobi, 2019).

Coumou, D., Di Capua, G., Vavrus, S. et al. The influence of Arctic amplification on mid-latitude summer circulation. Nat Commun 9, 2959 (2018). https://doi.org/10.1038/s41467-018-05256-8

Mewes, D. and Jacobi, C.: Heat transport pathways into the Arctic and their connections to surface air temperatures, Atmos. Chem. Phys., 19, 3927-3937, https://doi.org/10.5194/acp-19-3927-2019, 2019.

C37

(lines 687–694)

However, cooling by clouds implies the strengthening of the meridional temperature gradient. This might lead to increase the inflow of warmer and moister air masses from the lower latitudes into the Arctic climate. Even so, this has been shown to occur only along the North Atlantic pathway but not along the North Pacific or the Siberian pathways (Mewes and Jacobi, 2019). Conversely, the strengthening of the jet streams as a result of an increased temperature gradient could also slow the meridional exchange of air masses (Comou et al, 2018). The combination of such mechanisms may then either further decrease Arctic Amplification by generating more liquid water cloud following the retreat of sea ice or possibly enhance Arctic Amplification by the increased input of warmer air. Future model projections of the Arctic climate must take into account these effects to accurately predict the impact of anthropogenic emissions of greenhouse gases and short-lived climate pollutants.