Answer to Anonymous Referee #2, 20 April 2022
https://doi.org/10.5194/acp-2022-28-RC2

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.

Before we delve into responses to the specific points raised by the referee, we want to thank him for taking the time to read our text in depth.

Summary:
This paper is an interesting assessment of satellite observations of trends in total albedo of the Arctic along with a number of other properties of the clouds that would impact that albedo. The topic is very relevant and timely, considering the rapid shifts occurring in the Arctic over the past couple of decades. Moreover, there is wide interest in understanding the potential contributions and feedbacks associated with clouds in the changing Arctic. The type of data used here is perhaps the only means for answering the questions that are posed. Thus, the topic and general approach are quite reasonable and worthy of publication. However, there are a number of major challenges with this paper that render it not publishable in its current form. The first of these is the lack of clarity in the writing. The introduction is a great example. It touches on important points and provides good references, but is generally not clearly written. As a person who has studied this field for many years, I was often confused by what was being stated. Some points are not entirely correct as stated and often the linking of one sentence to the next does not make sense. This lack of clarity is present through the rest of the manuscript (apart from the appendices, which happen to be quite clear). Heavy scientific editorial work is needed to clean up the writing, adjust sentence structure and wording choices, and to overall help make the points clear.

We inform the referee that the introduction has also been rewritten with referee#1's comments in mind, and the flow has been made clearer, logical and explicit. The section "Discussion and conclusions" has been split.

R1
Additionally, this work is focused on decadal trends that are often very small and, in most locations, not statistically significant. Thus, the whole analysis is really working on the margins of what is possible to conclude from the data sets that are employed. Yet there is very little effort put towards quantifying or characterizing the uncertainties that are inherent in the data used, as well as the implications of these uncertainties for the conclusions that are drawn.

A1
Certainly, some parameters do not show any trend in the time frame investigated. But this is not an handicap for the following three reasons:

1. We confirm that most of the non-significant or very small trends are at the pan-Arctic level. However, the breakdown in seasons and regions reveal concealed patterns and trends. Some of these trends are relatively large magnitudes and are statistically significant. In our analyses, we investigate pan-Arctic and regional trends. We consider that this enables the maximum information to be deduced from the spatial and temporal coverage of the space borne records available.

2. We are also convinced that observing a small trend, which is not statistically significant, is in itself an interesting result, provided that a deeper analysis is offered. Statistical significance implies that our null hypothesis is not verified. In our case, the null hypothesis is that the variation of a parameter is within natural variability.

3. With respect to the deficiency in quantifying the uncertainties for the three families of parameters used in the paper (i.e. reflectances, cloud properties, cloud radiative forcing), we provided respectively: **(A)** a detailed appendix explaining the methodology used to harmonize spatial (i.e. radiometry) and geometric sampling during the merging of three sensors on different platforms; **(B)** an appendix where we calculated, based on the pixel-level uncertainties, the error in averaging the cloud properties; **(C)** Comprehensive list of references to the verification and validation of satellite-derived broadband fluxes against in-situ stations.

Additionally, we have prepared a manuscript on the evaluation of the CCI cloud parameter data set, ready for submission, in which we validate retrievals of cloud properties (CTH, COT and LWP) by comparison with the measurements of these from ground-based measurements obtained from Arctic stations. We are happy to confidentially share this.

Lastly, the main point of the paper is cooling by "wetter clouds;" however, it has not been demonstrated that the clouds are actually wetter, and it might be that the clouds have simply shifted from more ice to more liquid water without a net change in mass. This point must be addressed.
Further, the decline in surface albedo alone will lead to a stronger effective "cooling" by the clouds, regardless of changes in the clouds. The importance of this point has not been appropriately addressed in the paper. Generally, there appear to be some misinterpretations of cloud radiative effects. Below there are many comments regarding these general critiques. I believe the core of the work is a good starting point, yet major revisions are needed to bring this manuscript up to the standards that are expected for publication.

Specific Comments:

R2

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

A2

Agreed. We have corrected the sentence as suggested.

R3

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

A3

The referee's comment is correct. The U-shaped distribution of cloud fractional cover is well-known. We are not referring to this but to the yearly cycle instead. We will clarify this aspect.

R4

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

A4

The referee's comment is indeed correct. We will change the sentence accordingly.

R5

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

A5

This is not the outcome of the study by Donohoe and Battisti (2011). We quote the relevant part of their conclusions:

> *The planetary albedo changes associated with global warming were found to be primarily due to changes in cloud albedo (93% of the intermodel spread). This result is unsurprising given that cloud albedo plays the dominant role in setting the planetary albedo in the unperturbed climate and that the surface albedo's impact on the TOA radiative budget is strongly attenuated (approximately threefold) by the atmosphere.*

Donohoe A. and Battisti D. S.: Atmospheric and Surface Contributions to Planetary Albedo, Journal of Climate, 24, 4402–4418, `https://doi.org/10.1175/2011JCLI3946.1`, 2011.

As for the contradicting message in lines 57-58, please refer to the next answer, where we combine the response to two similar comments by the referee.

R6

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

A6

The referee, in his comment addresses one of the purposes of our introduction, namely to summarise the scientific findings in the literature, and contrast them where necessary, to point out the contradictions, that our research tries to reconcile.

Note that the lines 57–58 are followed by a reasoning where we point to possible sources of inaccuracy in the methodology devised by Pistone et al (2014).

Our intention is to point out that in the presence of a loss of brightness from the surface, Pistone et al. (2014) should have seen a leveling off of recent reflectance, due to a simultaneous increase in cloudiness. But this is not the case, due to the missing land areas in their analysis and the conversion from clear-sky to all-sky at the beginning of the record.

We will clarify this in the text.

R7

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

A7

We will rephrase the results of He et al.

R8

Line 69-76: Again, it is hard for me to following the train of thought in this paragraph. Also, is it not obvious that "temperature-related processes dominate the Arctic warming"? Lastly, clouds only "amplify warming in the Arctic region" under certain conditions of sun angle and surface albedo; under other conditions they dampen the warming.

A8

There seems to be a misunderstanding about the sentence, as also pointed out by the first referee too.

We have not written "temperature dominates the Arctic warming". That would be trivial. We have written "Temperature-related processes dominate the Arctic warming" instead. The difference lies in the distinct response of different processes to an increase in temperatures, as described in Pithan and Mauritsen (2014) and to a large number of micro-physical processes in cloud physics, that can be of opposite sign and different magnitudes as function of temperature. We will correct and improve this sentence as follows, bearing in mind that it is an introductory paragraph.

"It is known that thermodynamics (i.e. temperature and pressure) of the environment, together with aerosol particles (CCN or INP), determines the phase of a newly formed cloud particle. Depending on the cloud phase, the radius of particles vary, being liquid droplets typically smaller than ice crystals. This in turn determines the average optical thickness of clouds and how the liquid and ice phase of water in the clouds interacts with radiation in the solar and in the terrestrial spectral range. Already early studies (Curry et al., 1996) stressed that the additional presence of an underlying cold, bright surface and frequent temperature inversions impact atmospheric radiation budget through processes involving water condensate in form of liquid and ice clouds as function of temperature profiles. For a warming environment, such as the Arctic, it is expected that clouds will shift more to the liquid phase, increasing their liquid water content and reflecting more SW radiation (Boisvert and Stroeve, 2015; Cesana and Storelvmo, 2017)."

Please see A15, A47 for the dependency of CRF on solar zenith angle and A37 for surface albedo.

Curry, J.A., Schramm, J.L., Rossow, W.B. and Randall, D. Overview of Arctic cloud and radiation characteristics. Journal of Climate, 9(8), pp.1731–1764, 1996

Cesana, G., and Storelvmo, T. (2017), Improving climate projections by understanding how cloud phase affects radiation, J. Geophys. Res. Atmos., 122, 4594–4599, `doi:10.1002/2017JD026927`.

Boisvert, L.N. and Stroeve, J.C.. The Arctic is becoming warmer and wetter as revealed by the Atmospheric Infrared Sounder. Geophysical Research Letters, 42(11), pp.4439–4446, 2015.

R9

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

A9

We plan to correct the sentence accordingly.

R10

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

A10

We will improve the sentence accordingly.

R11

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

A11

The referee is correct. We shall correct this accordingly.

R12

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

A12

The comment is correct and important. As it will also be part of the discussion section of this paper, we shall add the information in this part of the paper.

R13

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

A13

We agree and plan to update the punctuation marks accordingly.

R14

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

A14

Our study is limited to the months of April to September (both included) and the winter months are excluded.

Lines 87–90 of the introduction read: "*A key set of satellite sensors record backscattered radiation in the solar portion of the spectrum. Consequently, this study focuses on the months between April and September.*"

Features beyond the time window April - September are not discussed because **(1)** we do not have enough radiation at $\lambda < 4$ µm available (as pointed out by the referee) and **(2)** because the sensors do not have all the same spatial coverage closer to the North Pole. Figure 2-c in the manuscript shows that we have guaranteed homogeneous coverage by the three sensors (GOME, SCIAMACHY and GOME-2) only up to the 85 N parallel.

We set this common latitudinal threshold exactly to avoid uncertainties in the data stream. In summary, we do not have sufficient coverage with these sensors to deem as real the "brightening" of the Arctic in the winter months and we refrain from such inferences.

R15

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

A15

We will add the following text to the manuscript. We blend the part on solar zenith angle with the concepts provided later on in A47.

"The broadband fluxes for the solar and IR part are computed solving the radiative transfer combining the two-stream approximation by Stephens et al (2001) for the bulk bidirectional reflectance, transmission and source terms within a plane-parallel atmospheric slab and the spectral band model by Fu and Liou (1992) for gaseous absorption. Six bands in the SW and and 12 bands in the LW are calculated sequentially ingesting local properties of clouds retrieved with a Bayesian technique (Sus et al. 2018, McGarragh et al. 2018), which provides estimates of the individual uncertainty at pixel-level (see App. C). Specifically, effective radius and cloud optical thickness are the primary inputs for flux calculations together with solar zenith angle and ancillary data from MODIS climatologies of visible and near-infrared surface albedo, linearly interpolated to each spectral band centre. Local vertical atmospheric profiles from ERA-interim account for the p-T variations, while constant aerosol optical depth of 0.05 and concentrations of well-mixed gases are assumed, the latter being linearly interpolated for their time-dependent increase. The combination of the above factors yields an accuracy of $\pm\,0.3\ \mathrm{W\,m^{-2}}$in outgoing LW radiation (Christensen et al, 2016). The physical boundaries of clouds are additionally required to correctly compute scattering and absorption along the vertical. From the retrieved CTH and effective radius, the bottom cloud layer is calculated assuming a subadiabatic variation of cloud water path, separately for the liquid and ice phases. While this approach is appropriate for the shallow case (Merk et al. 2016), the thickness of deeper clouds is computed combining a variable increase of water content matching within-cloud temperature profiles. The nominal accuracy limit in this case is reached at temperatures less than 217 K (-56 °C), which exceeds the yearly climatological range for the Arctic (-25 °C February, $+2.5$ °C July, Hersbach et al, 2020), and AVHRR-derived CBH is found to be in good agreement within $\pm$ 369 m against ceilometer observations (Meerkotter and Zinner, 2007).
Radiative transfer is solved twice. First all-sky fluxes are calculated with retrieved cloud properties and then the clear-sky fluxes, assuming that the pixel is devoid of clouds. This approach is in contrast to that employed with the MODIS cloud record and the CERES-EBAF radiation measurements at TOA, by virtue of which the interpolation of the measured clear-sky pixels serves as gap filling of all-sky pixels for the monthly aggregation of fluxes at BOA (Kato et al. 2013). AVHRR-derived fluxes at BOA have been validated by comparison with BSRN stations and the CERES-EBAF product (Stengel et al, 2020, ESA PVIR, 2020).
While on average comparisons with BSRN measurements show a good agreement for all downward fluxes and LWup, in some locations AVHRR-based estimates tend to be biased high for SWup $<$ 100 $\mathrm{W\,m^{-2}}$while the opposite holds for SWup $>$ 250 $\mathrm{W\,m^{-2}}$. This bias of higher spread can be due to the surface heterogeneity around the validation site, which influences the comparison of SWup because of the difference in spatial scales between the satellite footprint and the BSRN effective point measurement. The surface treatment in the satellite record is also a potential source of error because SWup is equal to SWdn times the surface albedo. While the actual sea ice extent is taken from measurements in the microwave (Henderson el al. 2013), a fixed value of surface albedo is assumed throughout the record. Consequently, intra-annual variability and long-term changes of surface reflectivity are not accounted for. This would lead to underestimate actual surface albedos in those months having fresh snow and ice (spring) and to overestimate during months of melting surface upper layers (summer). Cloud radiative forcing is dependent on fluxes at the surface. In the case of underestimation of surface albedo (or sea ice extent) we expect an overestimation of CRF and thus warming by the clouds and viceversa.

We do not expect differences in BOA fluxes as function of solar zenith angles because the instantaneous fluxes are corrected for the diurnal cycle of solar illumination by adjusting the surface albedo and the atmospheric path lengths (Stengel et al, 2020). The LW fluxes have been also corrected by using a cosine function derived from measurements of the geostationary SEVIRI sensor. The final aggregation is a good approximation to a true 24h average, needed to determine the true climatological mean of SWand LW fluxes and thus CRF. Consequently, also the seasonal averages (i.e. AMJ and JAS) do not exhibit variation induced by solar zenith angle and directionality of surface reflection".

Stephens, G. L., Gabriel, P. M., and Partain, P. T.: Parameterization of Atmospheric Radiative Transfer. Part I: Validity of Simple Models, J. Atmos. Sci., 58, 3391–3409, https://doi.org/10.1175/1520-0469(2001)058<3391:POARTP>2.0.CO;2, 2001.

Fu, Q., and K-N. Liou, 1992: On the correlated k-distribution method for radiative transfer in nonhomogeneous atmospheres. J. Atmos. Sci, 49 , 2139–2156.

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, Atmos. Meas. Tech., 11, 3373–3396, https://doi.org/10.5194/amt-11-3373-2018, 2018.

McGarragh, G. R., Poulsen, C. A., Thomas, G. E., Povey, A. C., Sus, O., Stapelberg, S., Schlundt, C., Proud, S., Christensen, M. W., Stengel, M., Hollmann, R., and Grainger, R. G.: The Community Cloud retrieval for CLimate (CC4CL) – Part 2: The optimal estimation approach, Atmos. Meas. Tech., 11, 3397–3431, https://doi.org/10.5194/amt-11-3397-2018, 2018.

Christensen, M. W., Poulsen, C., McGarragh, G., and Grainger, R. G.: Algorithm Theoretical Basis Document (ATBD) of the Community Code for CLimate (CC4CL) Broadband Radiative Flux Retrieval (CC4CL-TOAFLUX) module, ESA Cloud CCI, available at: http://www.esa-cloud-cci.org, 2016

Meerkotter, R. and Zinner, T. (2007). Satellite remote sensing of cloud base height for convective cloud fields: A case study. Geophys. Res. Lett., 34, L17805. doi:10.1029/2007GL030347

Hersbach, H, Bell, B, Berrisford, P, et al. The ERA5 global reanalysis. Q J R Meteorol Soc. 2020; 146: 1999–2049. https://doi.org/10.1002/qj.3803, 2020

Stengel, M., Stapelberg, S., Sus, O., Finkensieper, S., Wuerzler, B., Philipp, D., Hollmann, R., Poulsen, C., Christensen, M., and McGarragh, G.: Cloud_cci Advanced Very High Resolution Radiometer post meridiem (AVHRR-PM) dataset version 3: 35-year climatology of global cloud and radiation properties, Earth System Science Data, 12, 41–60, https://doi.org/10.5194/essd-12-41-2020, 2020.

Kato, S., Loeb, N. G., Rose, F. G., Doelling, D. R., Rutan, D. A., Caldwell, T. E., Yu, L., and Weller, R. A.: Surface Irradiances Consistent with CERES-Derived Top-of-Atmosphere Shortwave and Longwave Irradiances, J. Climate, 26, 2719-2740, https://doi.org/10.1175/JCLI-D-12-00436.1, 2013.

Merk, D., Deneke, H., Pospichal, B., and Seifert, P.: Investigation of the adiabatic assumption for estimating cloud micro- and macrophysical properties from satellite and ground observations, Atmos. Chem. Phys., 16, 933–952, `https://doi.org/10.5194/acp-16-933-2016`, 2016

Henderson, D. S., L'Ecuyer, T., Stephens, G., Partain, P., and Sekiguchi, M. (2013). A multisensor perspective on the radiative impacts of clouds and aerosols. Journal of Applied Meteorology and Climatology, 52(4), 853-871.

Riihelä, A., J. R. Key, J. F. Meirink, P. Kuipers Munneke, T. Palo, and K.-G. Karlsson (2017), An intercomparison and validation of satellite-based surface radiative energy flux estimates over the Arctic, J. Geophys. Res. Atmos., 122, 4829–4848, `doi:10.1002/2016JD026443`.

R16
Line 164-167: I assume that these values for F (LW, SW, +, -) are at the surface since you cite comparisons with BSRN measurements.

A16
Yes. We will add the information.

R17
Line 173-174: This sentence needs to be rewritten for clarity.

A17
It is not clear to us how the sentence can be improved. The sentence at lines 173–174 (**boldface**) completes the information given at lines 170–173 :

> *The conversion of directional radiance, measured at TOA, to irradiance requires the knowledge of the angular light redistribution function of the surface and atmospheric components.* **If this is not accurately assumed, the irradiance and $\mathbf{F}_{SW}^{+/-,clr}$ above reflecting surfaces cannot optimally be calculated**.

We will start line 173 with "If this conversion is not accurately performed ..."

R18
Line 174-175: The text here has not really provided justification for the suitability of AVHRR cloud property retrievals for use in this paper. The prior statement simply says (I believe) that Philipp et al. showed that the trend in CRF has a low sensitivity to biases in cloud properties. This is different than saying that the cloud properties are suitable. The preceding statements also have not provided justification for how suitable the satellite retrieved cloud properties are.

A18
Philipp et al. (2020) do not only show that the "*CRF trend has a low sensitivity to biases in cloud properties*", as mentioned by the referee. If this were the case, we would certainly agree with him.

Philipp et al. investigate the biases in cloud radiative forcing (CRF) trends at the surface due to

potentially inaccurate cloud fractional cover (CFC, also termed cloud masking, which is the most important quantity and a key input to a cloud retrieval algorithm) as a function of sea ice concentrations (SIC) between 0% (e.g. open waters) and 100% (fully sea-ice covered satellite pixels)
In our paper, we introduce the topic with the sentence on lines 168–169: "*Misclassified cloudy scenes especially over dynamically bright surfaces (i.e. marginal and fractional sea ice zones) impact the calculation of broadband fluxes.*"

The assessment of whether CRF trends are affected by misclassified cloudy scenes above bright surfaces exactly addresses the question of whether the data record is suitable for Arctic studies. For a more in-depth justification about the concern raised by the referee, please refer to the bullet list in the next answer A19 (especially bullet 4).

As a side note, the paper by Philipp et al. is titled "Analyzing the **Arctic** Feedback Mechanism between Sea Ice and Low-Level Clouds Using 34 Years of Satellite Observation" and the authors use the very same cloud data record we analyze. In summary we conclude that the AVHRR cloud record is suitable for Arctic studies of cloud parameters.

R19

Moreover, there are numerous papers documenting how AVHRR derived cloud properties have large uncertainties. Thus, further justification for the utility of the derived cloud properties is needed here. Specifically, it is important to describe how the uncertainties in the cloud products impact the analysis conducted here.

A19

We will update the manuscript with a digest of the following text, which describes the features of the algorithm relevant to the aim and scope of our work.

The main factors hindering the generation of an accurate Arctic cloud record from AVHRR data and its exploitation for the assessment of cloud properties trends in the past were **(1)** a poor account of the surface signal; **(2)** a poorly radiometrically calibrated AVHRR channels; **(3)**-**(4)** the usage of AVHRR channels, whose radiometric response drifts in time. These are the customary culprits for any cloud retrieval algorithm exploiting radiometric sensors.

We address each of these points here below, providing the evidence that the cloud record we use is appropriate and an improved reprocessing compared with previous AVHRR cloud records.

(1) AVHRR cloud mask is NN-trained on CALIOP surface mask (which is the gold standard in spaceborne research), and in-cloud extinction profiles are corrected with CALIOP profiles.

(2) AVHRR channels in the solar spectral range have been cross-calibrated with SCIAMACHY channels. SCIAMACHY is recognised for its accurate radiometric and spectral calibration.

The first author (Luca Lelli) has personally helped the record creator (ESA-DWD) in this task (see `https://essd.copernicus.org/articles/12/41/2020/#section11`).
This strengthens the meaningfulness of our results, because the part of our study dealing with TOA reflectances is conceived in a way that the record is radiometrically coherent with SCIAMACHY

(see Appendix A). Even more important, note that in the AVHRR cloud retrieval algorithm the values of cloud optical thickness are retrieved from AVHRR in the same spectral range as SCIAMACHY. This implies that any reflectance change for SCIAMACHY visible channels is present in the AVHRR-calibrated radiance ingested in the cloud retrieval algorithm.

We make use of the Post-Meridiem (PM) AVHRR series and not the Ante-Meridiem (AM) AVHRR series. During the preparation of the manuscript we have quality-checked the time series of all cloud parameters and broadband fluxes above the Arctic used for the generation of our results. We found that the AVHRR PM dataset fulfilled the calibration stability requirements which allows trends' assessment to be made.

We share here below the respective time series of used cloud properties (Fig. 1) and broadband fluxes (Fig. 2) for both AM and PM AVHRR series. It is straightforward to see that the AM record (red lines, left column in the Figures) is prone to calibration offsets, whereas the PM series is not, for the NOAA-12 platform before 2003 due to continuous drifts and change of local overpass times and to data gaps for NOAA-15 due to scan motor errors. This is confirmed in the validation report by ESA (ESA Cloud_cci, 2020).

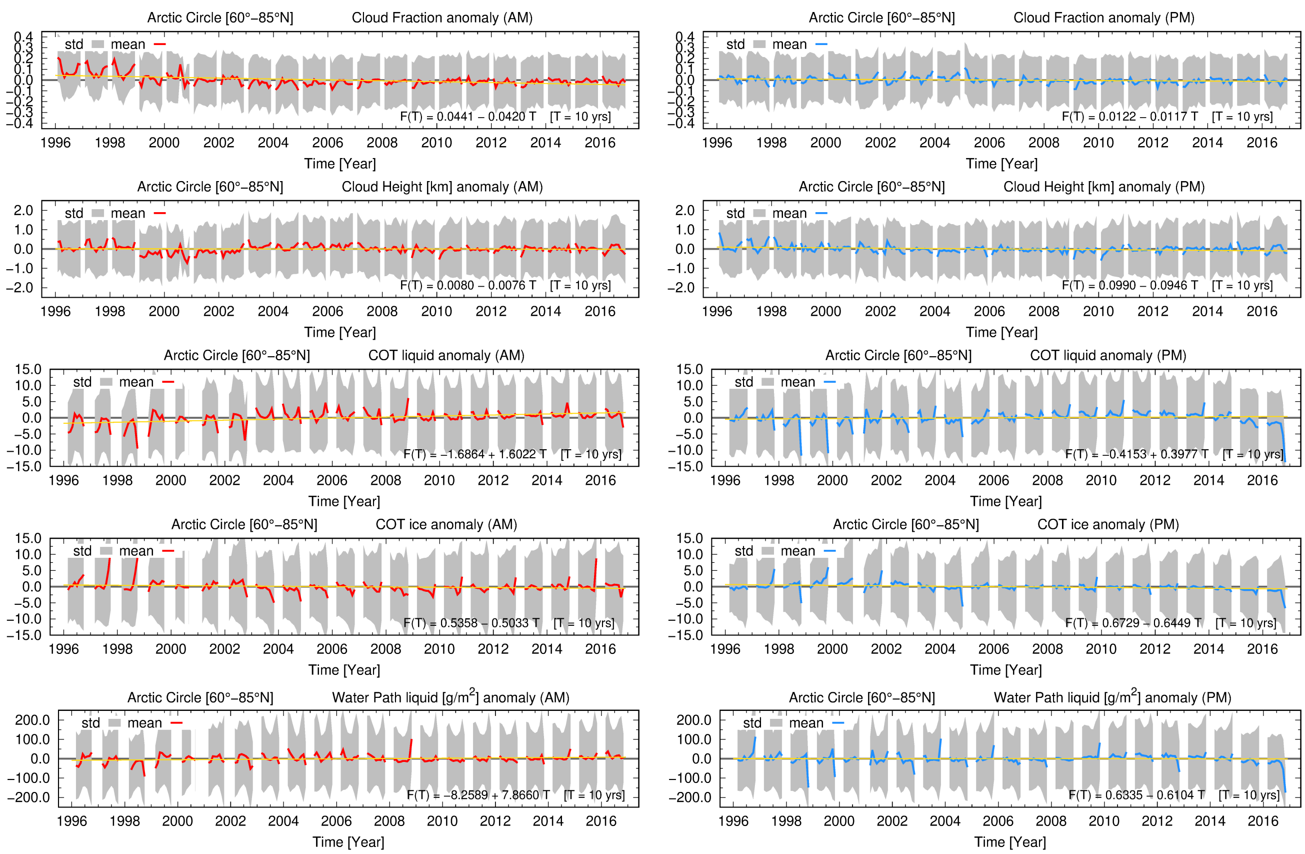

Fig. 1: Time series of those cloud properties used in our work for the AVHRR AM (left) and PM (right) series. From top to bottom: cloud fractional cover (CFC), cloud top height (CTH), cloud optical thickness of liquid clouds (COT liquid), cloud optical thickness of ice clouds (COT ice), liquid water path (LWP). Please note the suboptimal calibration of the AM series, especially at the beginning of the record (1996-2001), which impacts fluxes in the same time span (see Fig. 2).

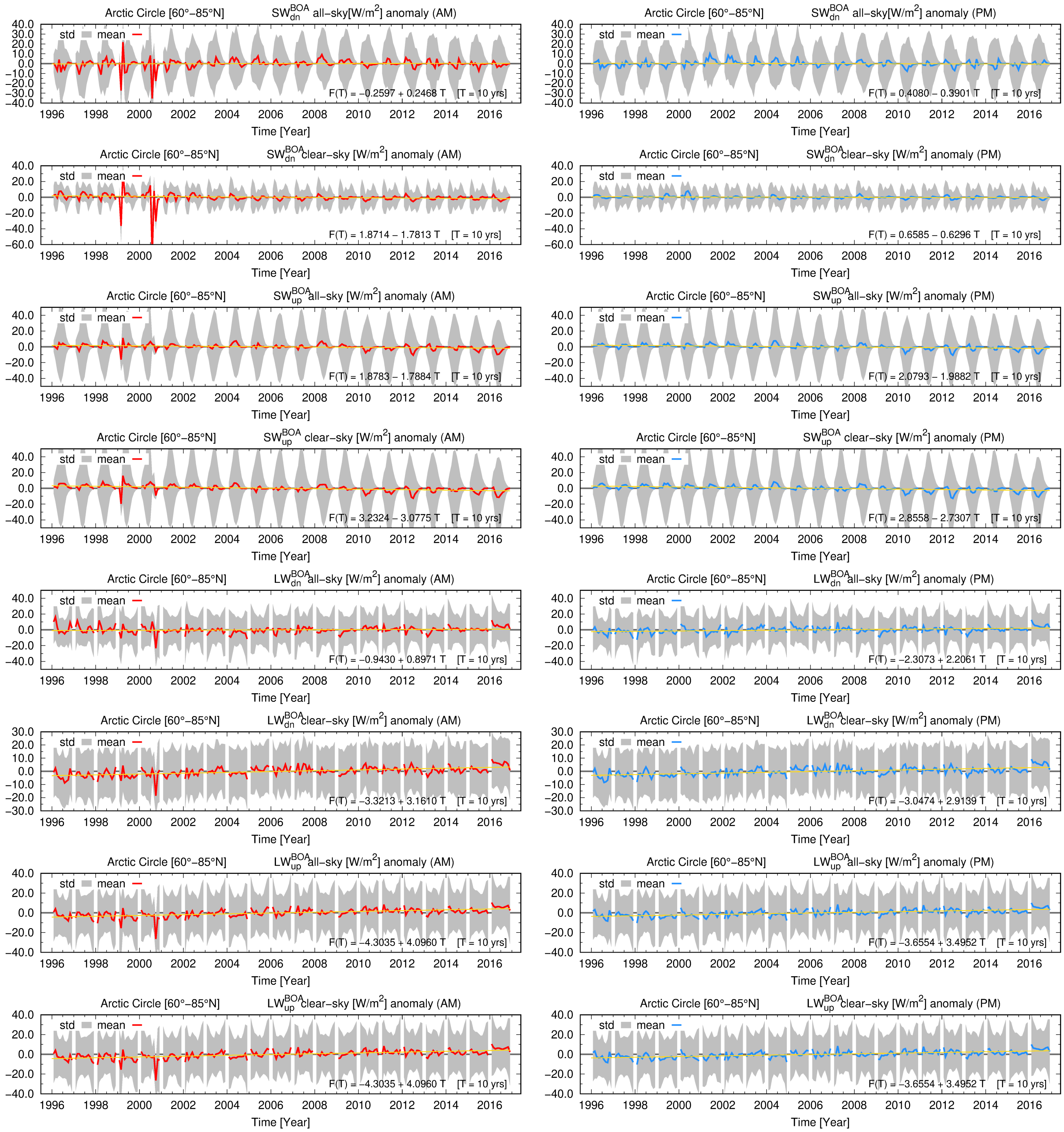

Fig. 2: Time series of broadband fluxes at the surface used in our work for the computation of cloud radiative forcing. For the AVHRR AM (left) and PM (right) series, we have (top to bottom): shortwave (SW) all-sky downwelling (DN), SW clear-sky DN, SW all-sky upwelling (UP), SW clear-sky UP, long-wave (LW) all-sky DN, LW clear-sky DN, LW all-sky UP, LW clear-sky UP. Please note the suboptimal calibration of the AM series, especially at the beginning of the record (1996-2001), corresponding to the ill-posed cloud properties of Figure 1 in the same time span.

(3) Irrespective of the verification and validation of the cloud record carried out by the data creators and documented throughout the traceable reports available at

1. `https://climate.esa.int/en/projects/cloud/key-documents/`
2. `https://climate.esa.int/media/documents/Cloud_Product-Validation-and-Intercompa`
   `v6.0.pdf`

we have independently validated the cloud record over the Arctic. The results are meant for a separate publication which is going to be submitted to AMT. In case of interest, we will be happy to share the draft with the handling editor and the referee.

(4) Figure A1, p7498, in Philip et al. (2020) shows CFC biases as function of sea-ice concentrations (SIC) for the seasons analyzed in our paper (i.e. AMJ is top right, JAS is bottom left).

For season AMJ, the bias is systematically flat from SIC 0% to SIC 100%.
Given that our trend model is based on anomalies and not absolute values, any additive component of the bias cancels out and the resulting trend is not affected by it.

For season JAS, the bias is not flat.
Because the SIC bins of Fig. A1 in Philipp et al. represent the SIC variance over one location in time, this effect is relevant only for those locations with a large dynamic in SIC (e.g. the marginal sea-ice zone).
Other said: the CRF trend is affected if the SIC anomalies in the record drift in time, e.g. their PDF is not Gaussian but skewed. This would be the multiplicative component of the bias in cloud cover adding up differently as we progress in time, thus propagating into the CRF trend.
Looking at Fig. 8, p 7489, of Philipp et al. (2020), the SIC anomalies for the marginal sea-ice zone of the illustrative enlarged Chuckchi Sea are plotted. We note that the SIC anomalies are normally distributed. If the SIC anomalies would drift in time, thereby adding the time-dependent component in the CRF trend via CFC, then the PDF at the bottom right of Figure 8 would be skewed. In this case the biases of Fig. A1 for JAS would definitively introduce a multiplicative component in CFC and the noise component would not be gaussian anymore (e.g. randomly distributed as white noise) but would contain a latency part. Our trend model would account for this computing the length of the effective independent sample in the record (see App. A).

The authors (Philipp et al.) conclude (and we quote):

> *However, TCRF (Total CRF) trends remain strongly positive over the whole ice pack and most of the melting zones. The correction demonstrates that observed ON warming trends are not vanishing - that is, that* **these trends are not the result of limited cloud detection capabilities of passive imagers** *when observing melting sea ice surfaces.*

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.

European Space Agency, Cloud_cci, Product Validation and Intercomparison Report (PVIR), Issue 6, Rev 1, https://climate.esa.int/media/documents/Cloud_Product-Validation-and-Intercomparison-R v6.0.pdf, 2020.

R20

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

A20

The sentence refers to the bias correction section in Appendix A. We are not making any claims of geophysical significance regarding possible errors as a function of the sun elevation. What is important for the quality of the reflectance measurement is that the portion of the solid angle covered by the sensors during the time windows used for averaging of data does not change with time (see Fig. 3). This ensures that the trends in reflectances are not contaminated by spurious trends due to extreme shifts in observation geometry, which would imply a different angular sampling of the phase function of surface and clouds (see Fig. 4).

We replace the sentence with a clearer explanation.

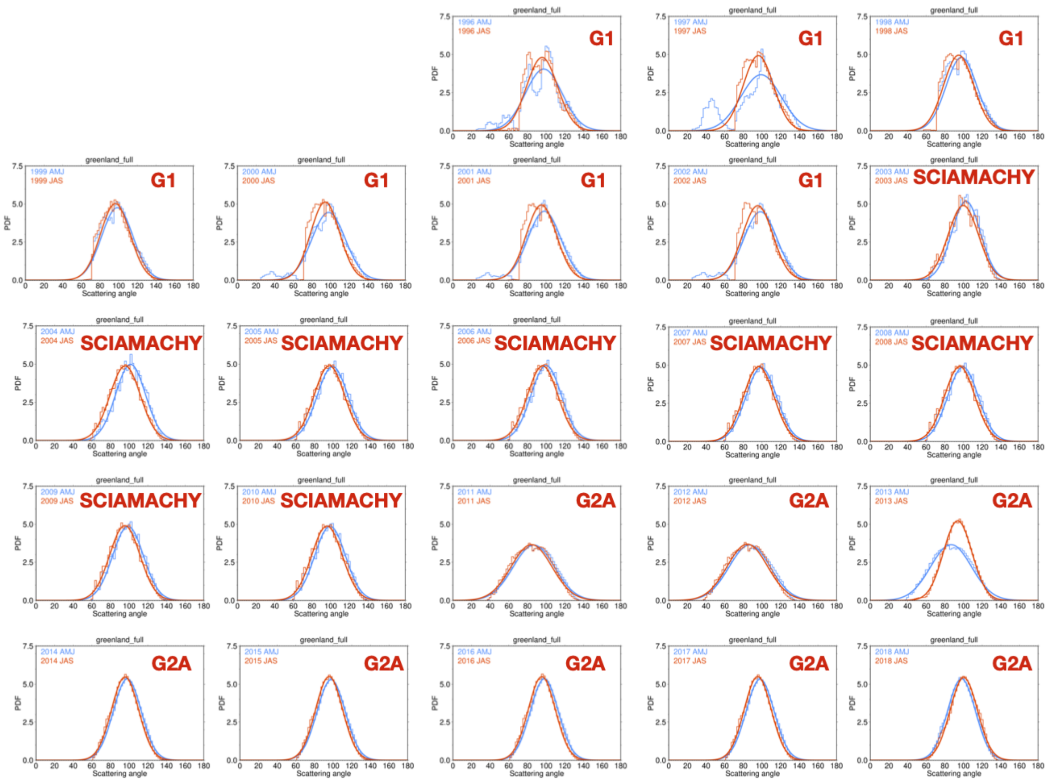

Fig. 3: Seasonal scattering angle distributions over Greenland for each year from 1996 to 2017.

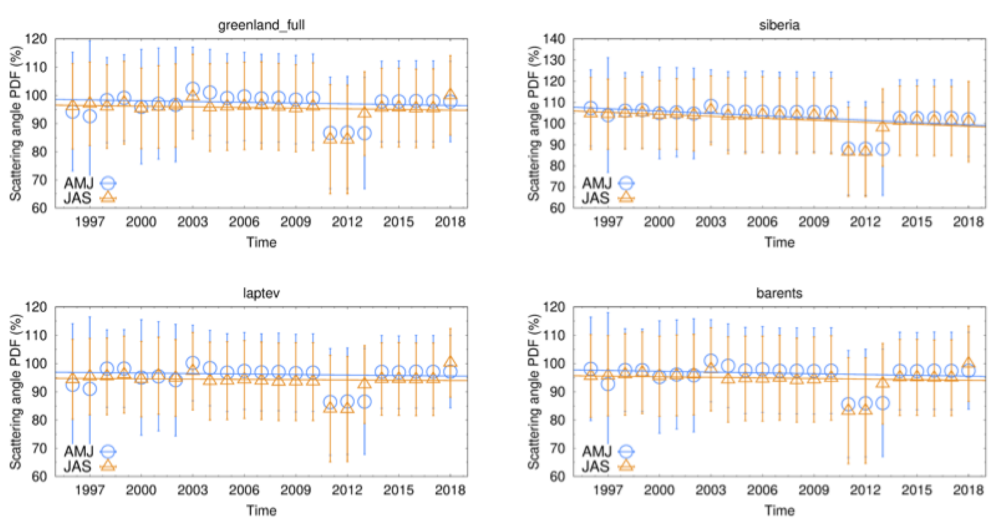

Fig. 4: The same quantities shown in Fig. 3 are plotted with 2 σ and the linear trends. See Appendix A in the manuscript for the assessment of the shift in scattering angle.

R21

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

A21

We will discuss it with the aid of the following text.

Please note that at line 434, Appendix A, the reader finds the following sentence: "Monthly aggregation leads to higher means for finer spatially-resolved instruments than otherwise".

The sentence indicates what the data say: GOME has a footprint of 320 $\times$ 40 km^2, considerably coarser than the follow-on sensors SCIAMACHY (60×40 km^2) and GOME-2 (80×40 km^2). This leads to different mean reflectances and standard deviations between sensors because the integration time of the acquiring on-board electronics for a coarser pixel is longer than for a finer pixel. This averages out sub-pixel heterogeneity of the sensed scene differently.

We account for this effect by assessing trends in reflectances not from mean values $Y(t,i)$ but from anomalies (see Eq. B1 in the manuscript) instead. The anomalies are customarily normalized with the standard deviation of the reflectances $s(t,i)$.
The standardized anomalies $y(t,i)$ are defined as

$$y(t,i) = \frac{Y(t,i) - \overline{Y}(t,i)}{s(t,i)}. \tag{1}$$

where $\overline{Y}(t,i)$ is the climatological mean of reflectance at time t and location i.

This is a common technique for the analysis of records which might be heterogenous in scale as in our case (see Section 3.4.3, pages 50-51 in Wilks, 2019).
Please note that normalization to the standard deviation of a sample is a linear transformation. Therefore, the underlying shape of the sample distribution is untouched and any non-Gaussian data sample is not made any more Gaussian. In other words, our approach does not precondition the sample statistics, hence the assessment of the trends' confidence intervals and the significance are not corrupted either in any way. Moreover, the bootstrap resampling technique has been chosen advisedly to avoid any ad-hoc assumption (i.e. parameterization) on the distribution of the sample.

In essence, each step of our data harmonization is similar to the ones carried out in Beirle et al. (2018), who deal with the same sensors and basic physical quantities (radiances and/or reflectances).

Wilks, D.S., Statistical Methods in the Atmospheric Sciences (Fourth Edition), Elsevier, `https://doi.org/10.1016/B978-0-12-815823-4.09987-9`, 2019.

Beirle, S., Lampel, J., Wang, Y., Mies, K., Doerner, S., Grossi, M., Loyola, D., Dehn, A., Danielczok, A., Schroeder, M., and Wagner, T.: The ESA GOME-Evolution "Climate" water vapor product: a homogenized time series of H2O columns from GOME, SCIAMACHY, and GOME-2, Earth Syst. Sci.

Data, 10, 449–468, https://doi.org/10.5194/essd-10-449-2018, 2018.

R22
Figure 3: There is a large discrepancy between SCIAMACHY and MERIS in the fall-winter. This discrepancy should be explained along with its implications for the results.

A22
This discrepancy has no implications for the results. We do not analyze fall and winter months, as stated at lines 87–90 of the introduction: "*A key set of satellite sensors record backscattered radiation in the solar portion of the spectrum. Consequently, this study focuses on the months between April and September.*

Nevertheless, please note that MERIS has a swath of 1150 km, whereas SCIAMACHY has a swath of 1000 km. This implies that with the onset of the polar night at high latitudes, the western part of the scan of both sensors (which are polar orbiters in descending node) will include increasingly dark Arctic areas, the MERIS scan being more northward leaning. Therefore, any averages of MERIS measurements will include more dark scenes than those in an average calculated from SCIAMACHY measurements. For this reason, the MERIS reflectances in fall and winter months are generally lower than those by SCIAMACHY.

What is important for our objectives is that during the months of full illumination (up to 85 °north latitude) the reflectances measured by both sensors are comparable and that there is no divergence between them over time. This is indeed the case.

We will add the above explanation to the manuscript.

R23
Line 188: "small downward trend". If I'm interpreting the numbers on Fig. 3 legend correctly, then the "trend" is actually within the 95% confidence interval of 0 (i.e., no trend). On line 190 you say that this is a "significant decrease." By this do you mean statistically significant? And if so, that should be stated clearly.

A23
Yes, both remarks are correct. We will clarify the text accordingly.

R24
Line 189: "at time t." I assume this means at the given time of the year, so that information should be included.

A24
Yes, correct. We update the text accordingly. See also response A24 to the first referee.

R25
Line 205-206: Yes, some of those negative anomaly areas are open ocean, but some of those are also over sea ice pack, and the year-to-year variability of sea ice extent is important here. Before attempting

to draw this type of conclusion, it seems imperative to use the actual sea ice extent, which is readily available, to confirm that the negative trends are indeed over open ocean as stated and/or to what degree that is true. I have the same comment for line 235 where changes are again related to regions of sea ice loss, but actual sea ice extent is not shown or discussed.

A25

We will update Figure 5 of the manuscript with Fig. 5 and corresponding explanation.

For Arctic spring (AMJ) and summer (JAS), the mean seasonal sea ice extent for year 1996 is coloured in green and for 2017 in purple. Sea ice extent is identified as those surfaces with at least local 75% sea ice concentration. Data of sea ice concentration are from Walsh et al. (2019).

From the panels in Fig. 5 we see that the trends are negative and statistically significant in both seasons where sea ice retreats, such as in the Barents Sea in JAS and in the perennial sea ice zone around the North Pole in AMJ. For the remaining areas that cannot be directly explained by the difference in sea ice extent, we assume patchy residual sea ice concentrations below 50% closer to Eurasia and occurrence of melt ponds on the sea ice pack. In both cases, open ocean areas and freshwater lower the albedo of the scene sensed by the satellites.

While areas with negative trends are spectrally neutral in both magnitude and statistical significance, areas of positive trends like the belt from the Canadian Archipelago, Beaufort and Chukchi Seas in AMJ and, to a smaller extent, Greenland in both seasons, show an increase in trend values and significance from λ 510 to λ 620 nm. While we cannot completely rule out the broadband influence of ozone trends (see Appendix D) on reflectances, the spectral patterns are coherent with an increase in some cloud properties conducive to snowfall and a brighter surface.

Walsh, J. E., W. L. Chapman, F. Fetterer, and J. S. Stewart. 2019. Gridded Monthly Sea Ice Extent and Concentration, 1850 Onward, Version 2. Boulder, Colorado USA. NSIDC: National Snow and Ice Data Center. doi: `https://doi.org/10.7265/jj4s-tq79` [last accessed: June 2021].

R26

Line 208: Can you explain the Greenland trend?

A26

In this section we describe only the mere results of reflectance. We defer the explanation to later sections when also cloud properties and radiative forcing are presented. It is an editorial choice.

R27

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

A27

We will swap the sentence. Our attention has been caught by the behaviour of reflectance trends and cloud properties in the Barents Sea. Namely the strong significant negative reflectance trends in AMJ and the ensuing build-up of reflectance, correlated with the increase of CFC and COT liquid in JAS.

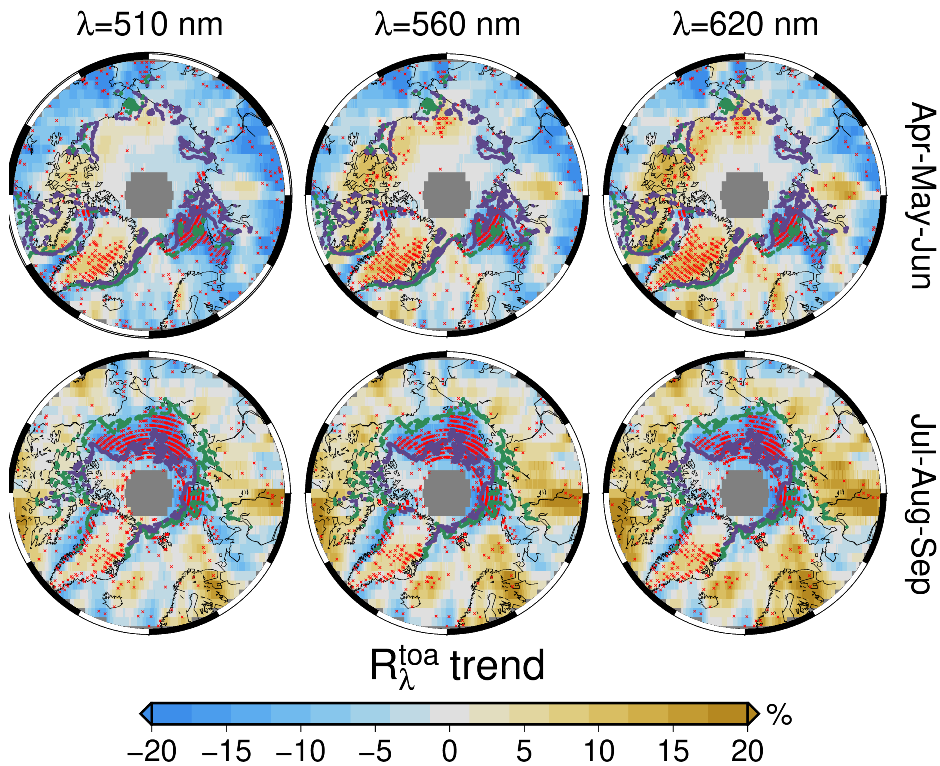

Fig. 5: Seasonal RTOA trends for 1996-2018 at selected λ for Arctic spring (AMJ, top) and summer (JAS, bottom). The values are relative to the first season of the record. Sea ice extent for 1996 in green and 2017 in purple. Stippling in red indicates significant trends at 95% confidence.

We remove the word "extraordinary".

R28

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

A28

Yes, indeed we can group this sentence together with the next paragraph. The reason for this statement is that 760 nm is the only channel with a very strong gaseous absorption and is not in the broadband continuum like all other channels. 760 nm bears more information on light scattering aloft than at the surface, because of the strong columnar absorption of atmospheric oxygen largely extinguishing photons before they impinge on the ground. No other Arctic region shows a similar behaviour except, to a lesser extent, Barents Sea.

R29

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

A29

We used the terminology to additionally characterise clouds as function of temperature (i.e. altitude), implying that clouds at higher temperatures should be lower placed than the cold clouds. However, we are aware that this general picture is not accurate for the Arctic, let alone the occurrence of inversion layers which may decouple the surface from the lower troposphere. We will remove "warm" and "cold".

R30

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

A30

Indeed, the comment is correct. Since no significant changes in cloudiness are detected at the pan-Arctic level (see Fig. 7 in the manuscript, topmost time series of CFC), we conclude that, as the cover fractional cover is unchanged within error, the optical thickness of clouds is changing as a result of changes in the amounts of liquid and ice particles in the cloud. Evidence for an increasing amount of liquid water cloud in the Arctic (and Antarctic) is provided in Fig. 6, which shows seasonal latitudinal changes of the fraction of liquid clouds in the time frame of our study.
We add a sentence explaining Fig. 6.

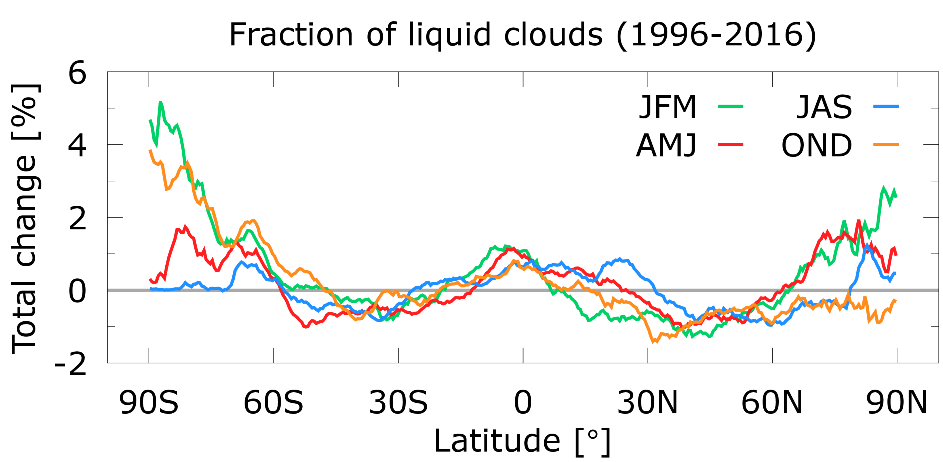

Fig. 6: Seasonal total change of fraction of clouds in the liquid phase.

R31

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

A31

Yes, correct. We will change the text accordingly.

R32
Line 252: I believe you mean Baffin Bay instead of Hudson Bay.

A32
Yes, correct. We update the text accordingly.

R33
Line 255-256: I do not understand how this sentence starting with "Conversely" is actual converse to the prior sentence, which discusses opposing trends of liquid COT and ice COT. This sentence discusses the fact that liquid COT increases in both AMJ and JAS.

A33
We agree that the sentence can be improved. We intend to explain that in some regions the increase in liquid COT does not necessarily result from a decrease in ice COT and vice versa.

R34
Figure 7: Make the colorbars the same for the liquid and ice COT fields so they can be readily intercompared.

A34
The colorbar has been updated. Please, note that we will add the panel of τ-total from Fig. 12 to this figure.

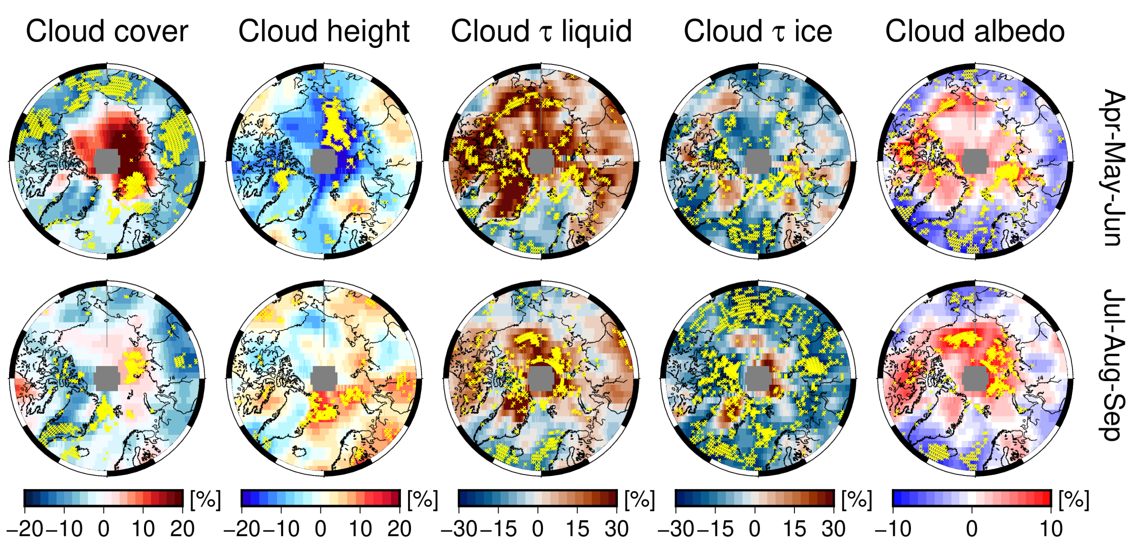

Fig. 7: Figure 7 of the manuscript with the updated color bar for τ-ice.

R35
Line 256-258: Do you mean "nearly unchanged" (which would be a trend of 0) or do you mean that

the trends remain similar for the different seasons?

A35

We mean that the trends are similar in the different seasons. We will clarify this in the text.

R36

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

A36

We intend to explain that the trends in CA do not appreciably change between seasons (temporal change) but have changes in their geographical (spatial) features. CA largely follows liquid COT values as a result of the well known relationship between the two cloud properties. However, CA over land masses at lower latitudes shows a different behaviour to that of liquid COT.

See also response A33 to the first referee. We will improve the sentence as follows: "The right hand side of the polar plots in the lower panel of Fig. 7 show seasonal trends in cloud albedo (CA) for AMJ and JAS. The magnitude of the positive trends in JAS is larger than those of AMJ but the spatial extent of the CA values are similar in both seasons."

R37

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

A37

We will add the definition of CRF and relative considerations about its dependency on surface albedo.

We compute CRF at the bottom-of-atmosphere (BOA, at the surface) from the differences between the downward and upward fluxes of SW and LW for all-sky and clear-sky conditions as follows

$$\mathrm{CRF}^{\mathrm{boa}} = (\mathrm{SW}_{dn} - \mathrm{SW}_{up} + \mathrm{LW}_{dn} - \mathrm{LW}_{up})^{\mathrm{boa}}_{\text{all-sky}} \tag{2}$$

$$- (\mathrm{SW}_{dn} - \mathrm{SW}_{up} + \mathrm{LW}_{dn} - \mathrm{LW}_{up})^{\mathrm{boa}}_{\text{clear-sky}} \tag{3}$$

Details of the derivation of monthly averages are given in A15. We repeat here the most important details. The instantaneous fluxes, at the basis of the monthly averages used in our study, have been computed with the approach by Henderson et al. (2013). Surface albedo at visible and near-infrared wavelengths are based on spatio-temporally resolved MODIS climatologies, which are used in the cloud retrieval algorithm. Sea ice extent is taken from measurement in the microwave.

During the preparation of the manuscript, we have generated also the results shown in Figures 8 and 9. Figure 8 shows histograms of SWE and LW CRF, for the different underlying surface type: all Arctic, sea ice covered areas (sic), marginal sea ice zone (msz), and open waters (oce) for the two periods 1982–1991 and 2005–2014. The first period is prior to the observation of Arctic Amplification and the second period covers the current Arctic Amplification. Sea ice areas have values of SW CRF can be larger than the LW CRF, leading to a positive CRF. With decreasing sea ice extent, SW CRF exceeds LW CRF, leading to a negative net CRF. The same data are plotted in the left column of Fig. 9 for a continuous SIC value range.
The right panels in Figure 9 show that the the (negative) SW CRF trend increases for a decreasing sea ice extent, while the LW CRF trends slightly increase. Inspecting qualitatively the crossing point between trends in SW and LW CRF as function of SIC, we see that already for SIC change of 0.05% month^{-1} suffices for the SW CRF to offset the LW CRF. In these results the CRF changes as a function of cloud property are combined.

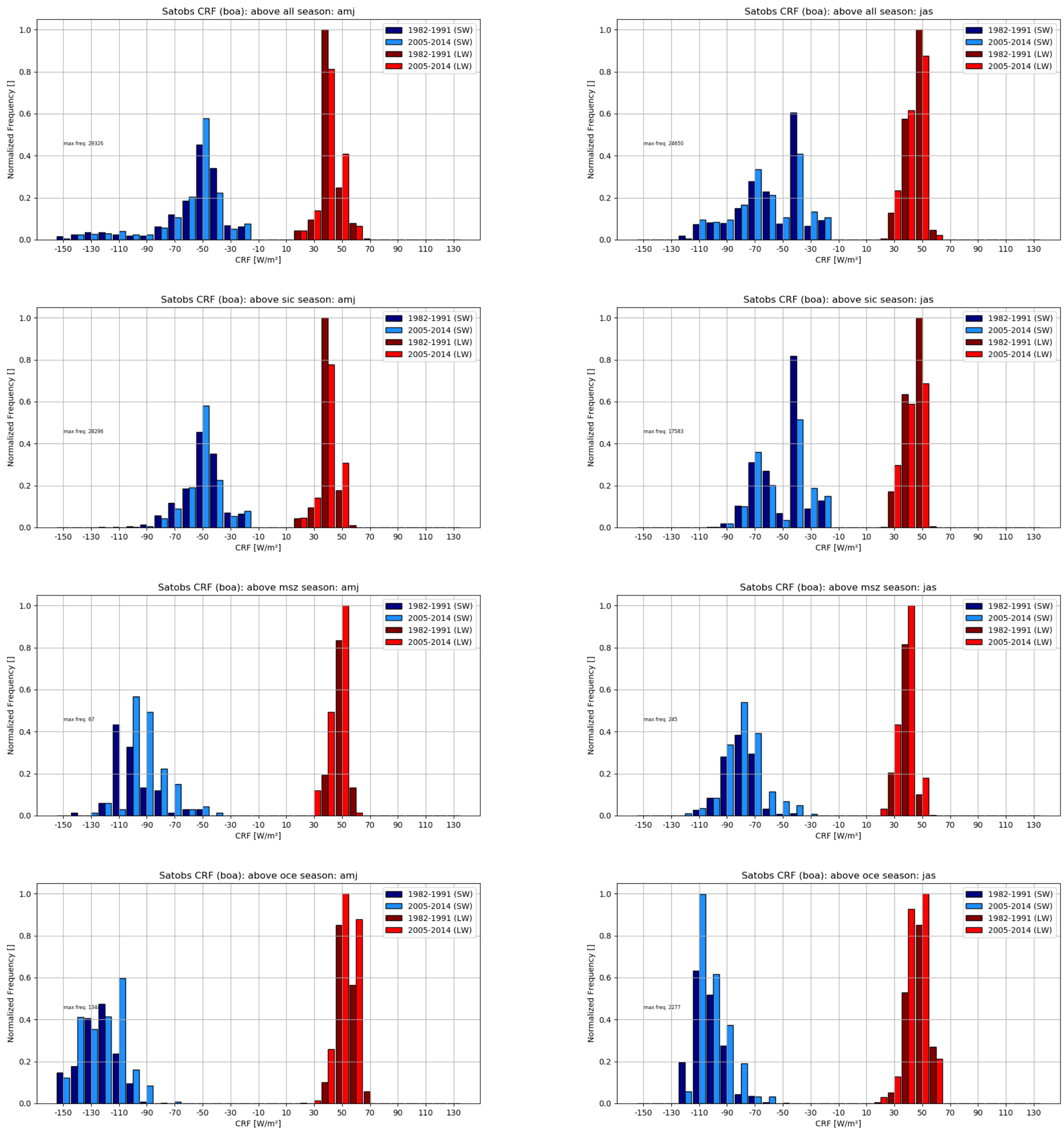

Fig. 8: The seasonal (left column AMJ, right column JAS) SW and LW CRF-components of cloud radiative forcing above all Arctic surfaces (top row), above sea ice covered areas (second row, "sic"), marginal sea ice zone (third row, "msz") and open waters (bottom row, "oce"). The CRF-SW is plotted in blue and the CRF-LW is plotted in red (for two different periods we use different shades of blue and red, respectively). Note that each individual distribution has been normalized to the value of highest occurrence given in each plot as "max freq". Data of sea ice concentration are from Walsh et al. (2019).

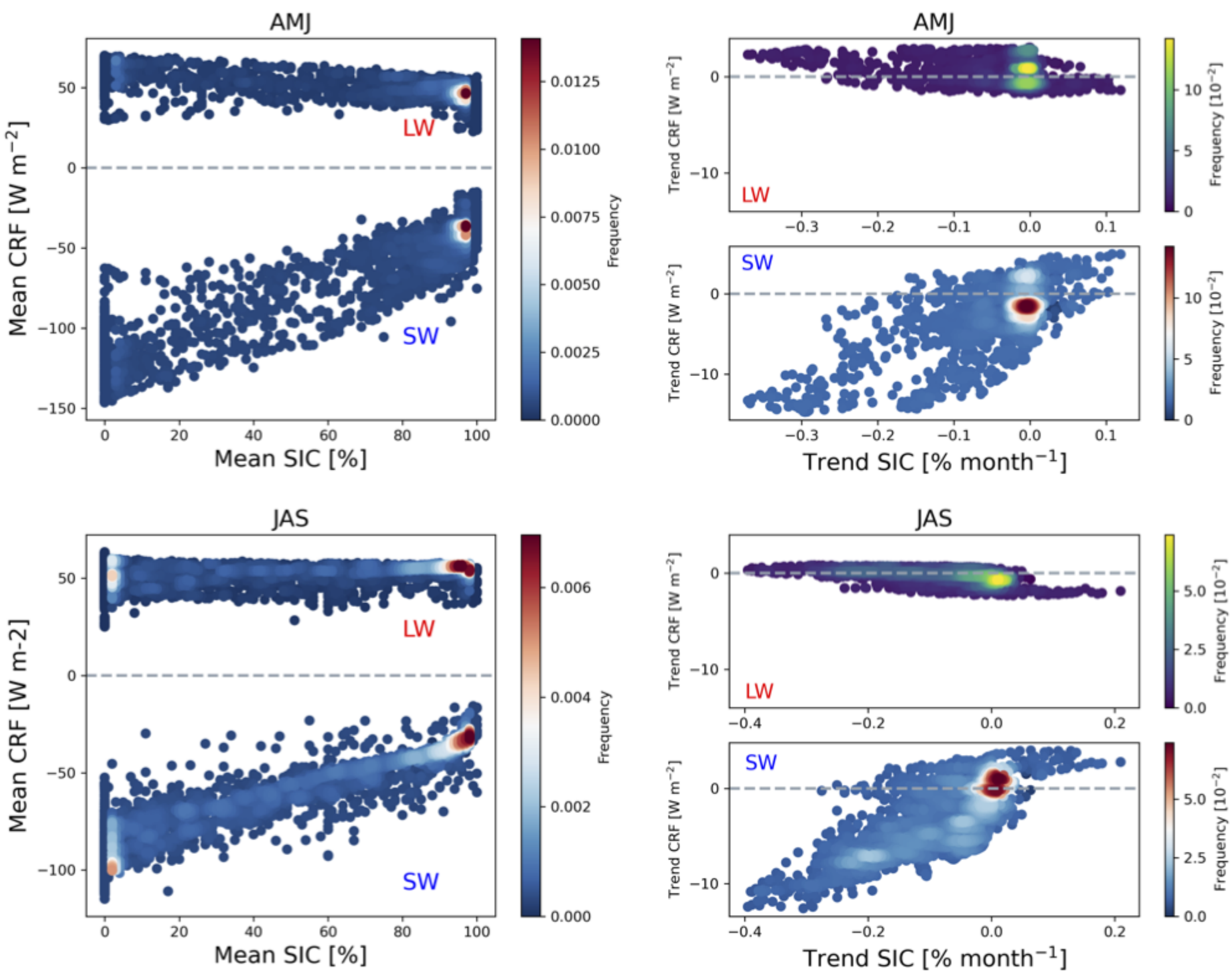

Fig. 9: Distribution of mean SW and LW CRF ($\mathrm{W\,m^{-2}}$) as function of SIC (left panels). Change in SW and LW CRF as function of SIC trends (right panels). The top row is Arctic spring (AMJ), bottom row is Arctic summer (JAS).

Walsh, J. E., W. L. Chapman, F. Fetterer, and J. S. Stewart. 2019. Gridded Monthly Sea Ice Extent and Concentration, 1850 Onward, Version 2. Boulder, Colorado USA. NSIDC: National Snow and Ice Data Center. doi: `https://doi.org/10.7265/jj4s-tq79` [last accessed: June 2021].

R38

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

A38

Thanks for pointing this out. We shall improve the text accordingly.

R39

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

A39

We thank the referee for pointing these issues out. The referee is correct that darker implies SW CRF component and is not suitable for the use with the LW CRF component. Indeed, our description of the dependence on the surface brightness did not mention temperature as driving factor of LW emission. We implicitly assumed it could have been inferred by the reader. We plan to explain better and clarify the issues raised in our text.

R40

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

A40

We agree with the referee. The numbers in the text had been computed without showing the maps of climatological annual and seasonal CRF and its components. We add the panels of Fig. 10 to the Appendix with the corresponding maps as companion of Figure D1. The figure is attached here below.

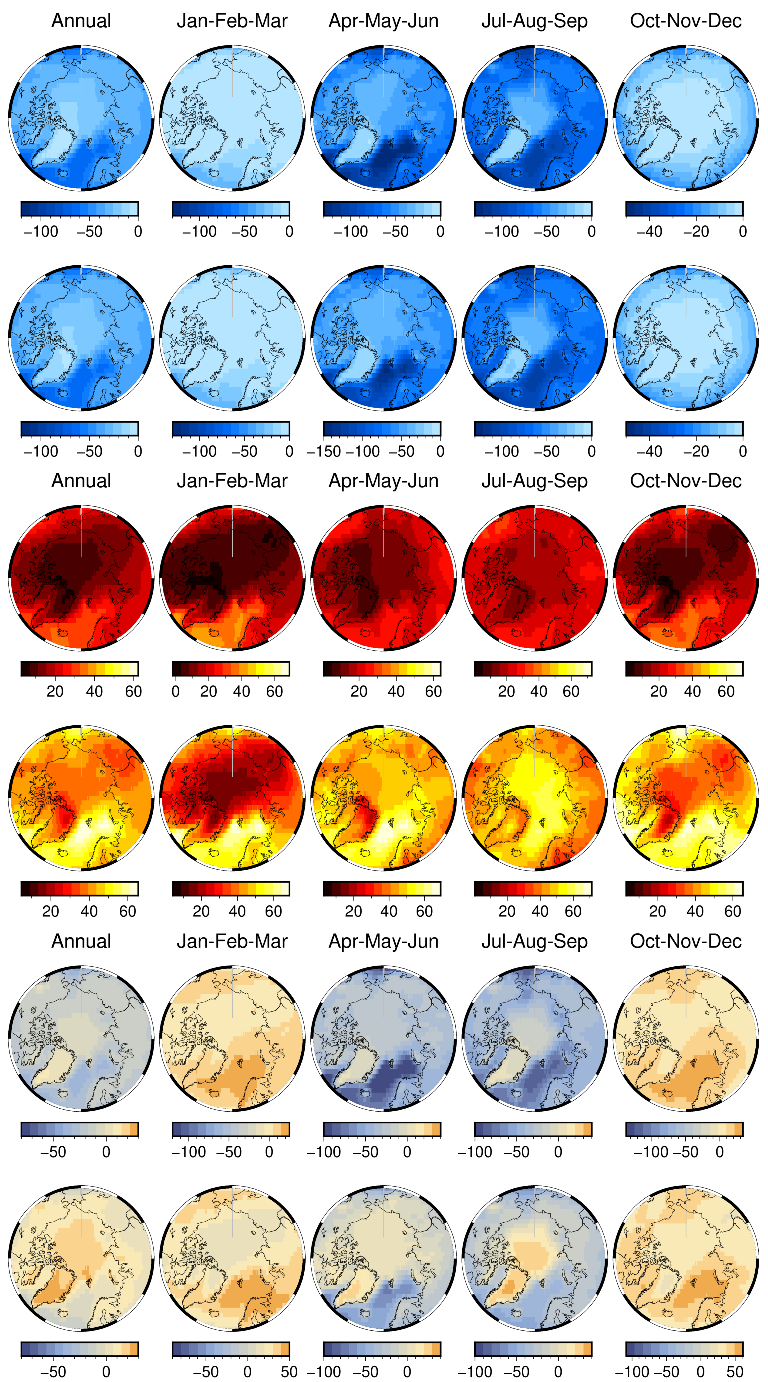

Fig. 10: From left to right, annual and seasonal average values of SW (rows 1-2), LW (3-4) and total (5-6) cloud radiative forcing (CRF, W m^{-2}) at TOA and BOA, respectively. Note the different color scales to match the CRF ranges.

R41
Line 296-298: "Consequently, the Arctic surface is warmed by clouds throughout…." It is not clear what this statement is referring to. Presumably this is referring to the annual total CRF (which is not shown anywhere).

A41
Yes, correct. We have added Fig. 10 to the Appendix.

R42
Line 304-312: These results are entirely consistent with the change in surface albedo. i.e., there is little change in the surface albedo over land surfaces, Greenland, and the N. Atlantic because there is no major shift in surface properties there. On the other hand, over the Barents/Kara in spring and over the whole Arctic Ocean domain in summer the declines in sea ice have led to a decrease in surface albedo. This leads to a larger negative SW CRF (independent of cloud changes), which increasingly outweighs the LW CRF.

A42 The comment is correct. See also A37. We update the text accordingly.

R43
Line 316: I don't know what "wetter Arctic clouds" means. In general, "wet" means water in any of its phases. But this paper has not established if there is in fact more water mass (LWP+IWP) in clouds. Rather, what has been show is that often the changes offset each other, and the percentage change (Fig. 8) is often larger on the IWP decrease than on the LWP increase. Nonetheless, the information has not been provided here to demonstrate if there is in fact more condensed mass in clouds or not (although it could and should be). Additionally, the paper has shown some changes in the COT of liquid vs ice clouds, but this is not changing how wet the clouds are but rather how optically thick they are. Lastly, it has not been demonstrated here that anything has actually changed with the amount of cloud or the condensed mass of the cloud. The changes in liquid vs ice could simply be a shifting from ice towards liquid (i.e., same mass), which would be expected as atmospheric temperatures slowly warm. The paper needs further work to disentangle these key points and to draw the appropriate conclusions as to what is actually changing. For this specific statement highlighted here, if anything, the conclusion would rather be "….compensated for by more reflective Arctic clouds" (although to truly make that statement would require spatial maps and temporal trends for total COT).

A43
By "wetter" we mean to express that the amount of liquid water in the clouds has increased. We will replace "wetter" with "more liquid" where appropriate, starting with the title. We will also add the new figures to the manuscript.

We provide results that answer the two questions posed by the referee: **Q(1)** has the total mass of condensed water in clouds increased? **Q(2)** How does the total optical thickness of clouds change?

A(1) Figure 11 shows seasonal total change (in %) from the first season in the record of LWP, IWP and cloud water path (LWP + IWP). Indeed, the loss of IWP is larger than the increase in LWP. The seasonal correlation between CWP and its water/ice component is respectively 0.79 / 0.75 in AMJ and

0.57 / 0.84 in JAS.

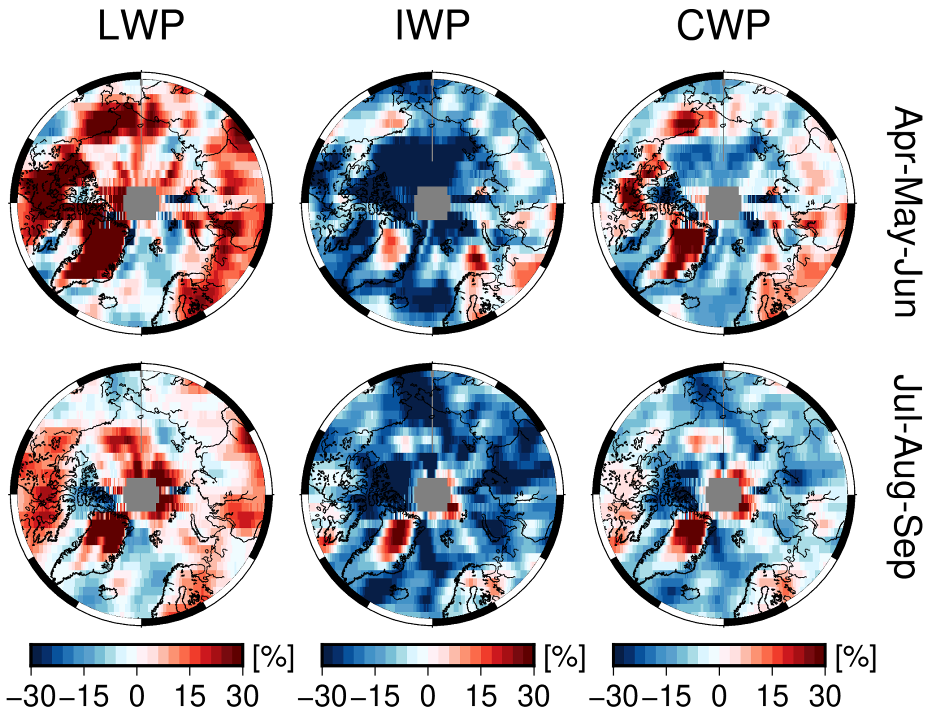

Fig. 11: Top: time series and trends of water path for liquid, ice and all clouds. Bottom: seasonal trends.

A(2) From Figure 12, total COT has remained approximately unchanged throughout the record. The seasonal maps reveal that trends in total COT are more correlated with trends in liquid COT than with those in ice COT. The seasonal correlation between total COT and liquid/ice is respectively 0.84 / 0.21 in AMJ and 0.63 / 0.50 in JAS.

The values of total change within the record, underlying Figures 11 and 12, are reported in Table 1.

We conclude that while the total mass of condensed water in both phases (i.e. liquid plus solid) has not considerably changed in AMJ and has moderately decreased in JAS. This is due to a faster loss of ice which is not compensated for by an increase of liquid water in the same amount. This implies a decrease of τ ice, for the concurrent increase in τ liquid, and the decrease of ice water path for the concurrent increase of liquid water path.

R44

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

A44

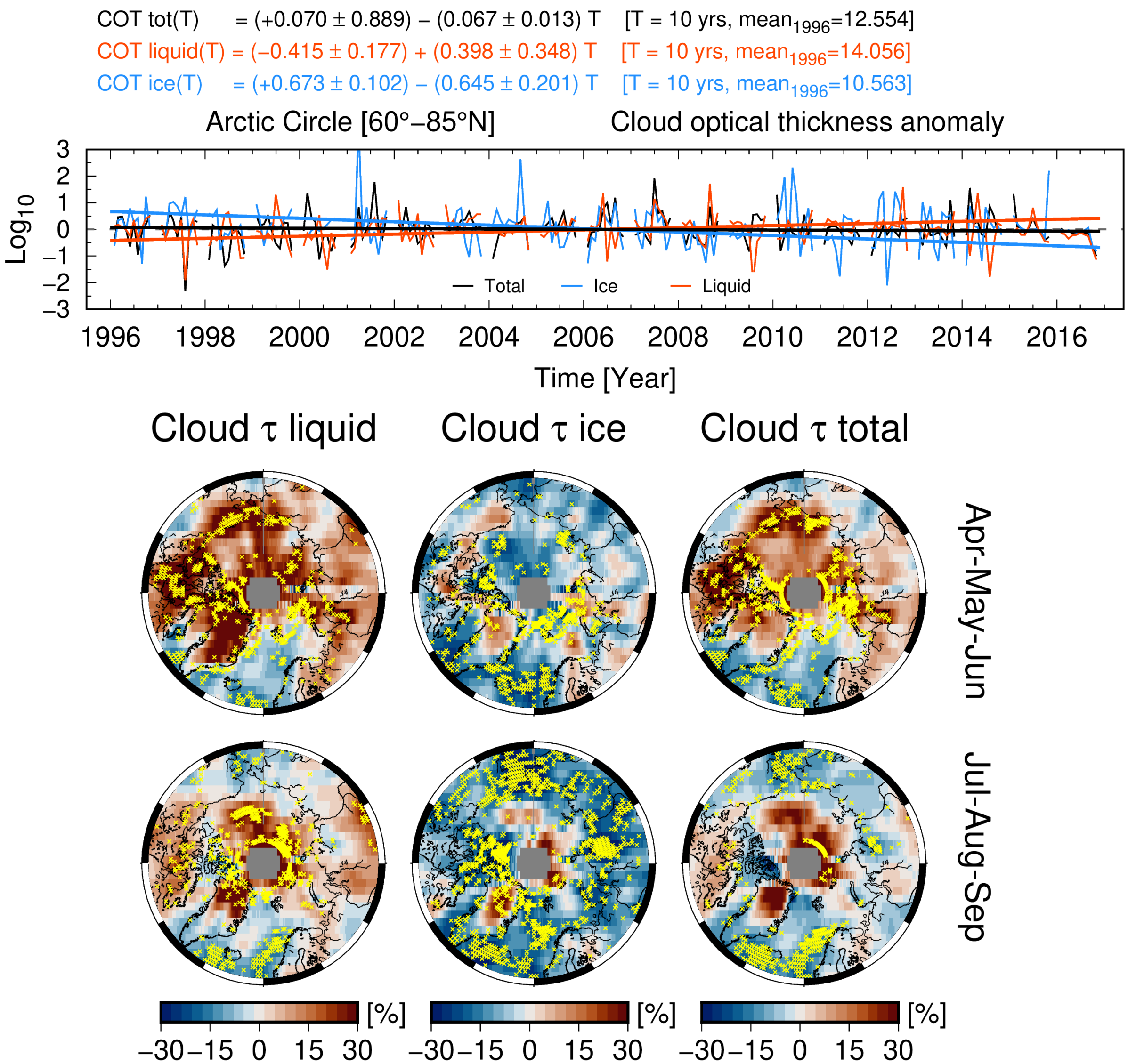

Fig. 12: Top: time series and trends of optical thickness for liquid, ice and all clouds. Bottom: seasonal trends.

With the results provided in the answer A43, we think that the increase of the liquid component of clouds is unambiguously identified in its optical and physical realization. The total optical thickness of clouds has not changed in the time considered.

R45

Line 326-327 vs Line 330-331 vs. Line 338-340. In the first sentence, it is stated that the CFC trend over Greenland turns "strongly negative" after 1995, explaining Hofer et al. But then a few sentences later it is stated that over Greenland there is an "insignificant CFC trend." Then in the third set of lines, it again mentions the Hofer et al. work and the decreasing CFC leading to more insolation. So which is it? If there is a decreasing CFC (ala Hofer et al) then there would be less clouds and more of

Table 1: Seasonal pan-Arctic total change ± one σ (%) from the first season in the record of physical and optical properties of clouds for all clouds and the liquid and ice phase. CWP and τ total values are weighted by the relative occurrence of liquid/ice clouds in AMJ (0.54/0.46%) and in JAS (0.63/0.37%).

Parameter	AMJ	JAS
LWP	+11.58 ± 3.10	+4.57 ± 6.64
IWP	−14.54 ± 23.50	−16.47 ± 20.47
CWP	−0.51 ± 11.01	−3.13 ± 8.59
τ liquid	+12.46 ± 5.43	+3.95 ± 2.82
τ ice	−7.12 ± 12.25	−12.67 ± 15.41
τ total	+3.40 ± 6.14	−3.66 ± 7.29

the Greenland Ice Sheet contributing to reflected irradiance. Assuming the surface is slightly more reflective than the clouds, this would explain the total change in TOA reflectance observed in this dataset.

A45

The sentence at lines 330-331 ("insignificant CFC trend") refers to the pan-Arctic CFC trends and not to Greenland. We acknowledge that having created Figure 7 for a pan-Arctic time series and seasonal maps makes the interpretation of the text difficult, when discussing specific Arctic regions, such as Greenland in this case. Our intention is to highlight that Greenland's reflectance trends are not fully explained by CFC changes. Consequently, we agree with the referee that the reduction in cloudiness over Greenland (confirming the results by Hofer et al.) contributed to higher surface exposure and, therefore, to a positive reflectance trend over the continent.

We will clarify this in the revised text.

R46

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

A46

We have provided evidence in A43 that the Arctic clouds are changing from the solid to the liquid phase, both in the optical and the physical domain.

R47

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

A47

We agree with the referee regarding the instantaneous fluxes. That said, we consider that the SW fluxes in the dataset we analysed have been corrected for the low-sampling rate of a single-polar orbiter as well as for their angular dependence of the solar illumination. This is achieved by computing the diurnal cycle of solar zenith angle, by adjusting the surface albedo and, eventually, the atmospheric path lengths.

The LW fluxes have been also corrected for with a cosine function derived by measurements of the geostationary SEVIRI sensor. The final average is a good approximation of a true 24h average, needed to determine the true climatological mean of fluxes. Consequently, also the average in seasons (i.e. AMJ and JAS) does not contain variations in solar zenith angle and directionality of surface reflection.

We point the referee to the relevant documentation.

Stengel, M., Stapelberg, S., Sus, O., Finkensieper, S., Wuerzler, B., Philipp, D., Hollmann, R., Poulsen, C., Christensen, M., and McGarragh, G.: Cloud_cci Advanced Very High Resolution Radiometer post meridiem (AVHRR-PM) dataset version 3: 35-year climatology of global cloud and radiation properties, Earth System Science Data, 12, 41–60, https://doi.org/10.5194/essd-12-41-2020, 2020.

ESA cloud cci - Algorithm Theoretical Basis Document (ATBD) of the Community Code for CLimate (CC4CL) Broadband Radiative Flux Retrieval (CC4CL-TOAFLUX) - REF: CC4CL-TOAFLUX ISSUE: 1.1 DATE: 14/10/2019. https://climate.esa.int/media/documents/Cloud_Algorithm-Theoretical-
v1.1.pdf

Shupe, M. D. and Intrieri, J. M.: Cloud radiative forcing of the Arctic surface: The influence of cloud properties, surface albedo, and solar zenith angle, Journal of Climate, 17, 616–628, https://doi.org/10.1175/1520-0442(2004)017<0616:CRFOTA>2.0.CO;2, 2004.

R48

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

A48

Agreed. We replace it accordingly.

R49

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

A49

With the previous answer in mind about how the fluxes were calculated (A47), we believe that the inference in the sentence ("SW CRF dominates over LW CRF") is essentially valid, in spite of the diurnal and seasonal variation in the solar zenith angle.

From the second column of Figure 10 in the manuscript, we note that in SW there is no robust correspondence between trend magnitudes in CRF and CFC.

For LW, a linear relationship holds both between the diameter and type (filled or open) of the circles and the sign of the LW trends:

1. The circle diameter is consistently proportional to the trend magnitude in CRF_LW (i.e. greater circles correspond to larger trends in ±CRF_LW)
2. Filled/open circles (i.e. increase/decrease in cloudiness) correspond to positive/negative CRF_LW trends.

For SW, the conditions of bullets 1 and 2 are not satisfied.

We substantiate it with Figure 13. They are the analogue of Fig. 10 in the manuscript, but now CRF_SW and CRF_LW are related with CFC and not COT-liquid anymore. For the shortwave part of CRF, the seasonal coefficients of determination R^2 by CFC is comparable to those of Fig. 10 (top right) in the manuscript, which show R^2 by COT-liquid. For the longwave part of CRF, R^2 by CFC is higher than R^2 by COT-liquid (CFC: AMJ 0.98 for both above ocean and all areas; JAS 0.87 above ocean 0.94 above all areas. COT-liquid: AMJ 0.39 / 0.02 above ocean / all areas; JAS 0.65 / 0.19 above ocean / all areas). We conclude that CFC modulates LW more than SW.

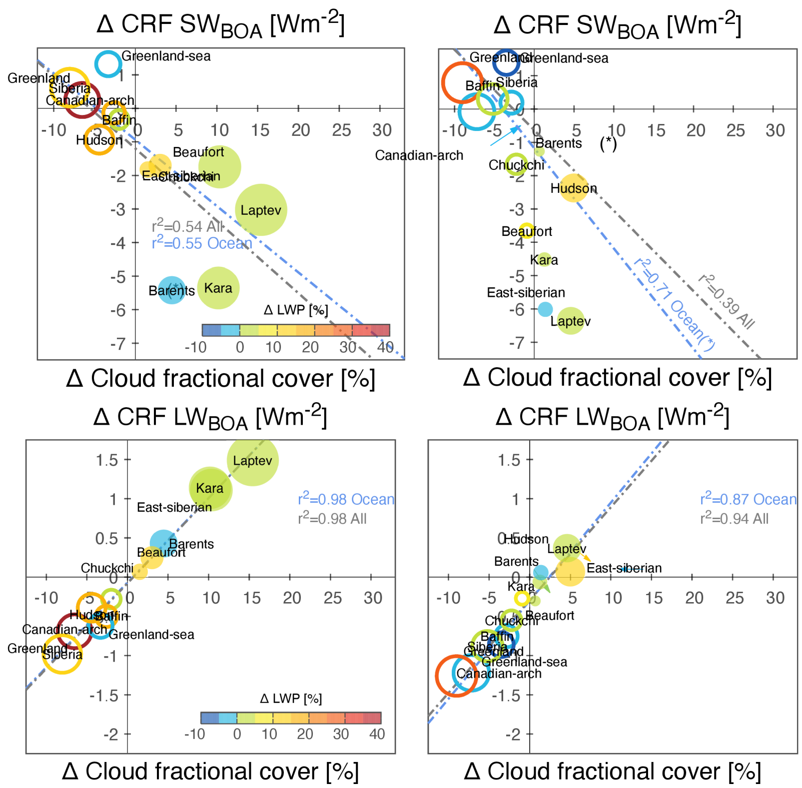

Fig. 13: Seasonal (left Apr-May-Jun ; Jul-Aug-Sep: right) of total changes [%] in cloud radiative forcing at the surface, cloud fractional cover and liquid water path.

The Fig. 13 and accompanying text will be added to the revised manuscript.

R50

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

A50

Agreed.

R51

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

A51

Indeed. Agreed.

R52

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

A52

We will clarify the relationship between reflectance, cloud properties and the radiative forcing keeping in mind also their changes.

R53

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

A53

It is true that in this paper, we link observed changes in reflectance primarily to changes in cloud optical thickness and thermodynamic phase of clouds. Nevertheless, the changes in CFC are not of minor importance. First, because CFC and COT are radiatively related. Second, it is our intention to provide the reader with evidence and confidence in our satellite-derived calculations of CRF. One way forward is to compare our results with those in the literature, especially if the latter have been derived from in-situ measurement campaigns. In this respect, the results by Shupe and Intrieri (JCLIM, 2004) from the SHEBA campaign are a standard reference.

R54

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

A54

We agree with this comment and we shall add this information to the text.

R55

Line 364-367: Enhanced convergence of moisture could be a possible mechanism to explain why there might be changes in cloud properties (although this paper has not shown that there is a change in the total condensed cloud mass). However, changes in convergence of moisture are not needed to explain why the LW cloud effects dominate over the SW effects in AMJ. Regardless of any change in moisture convergence, cloud LW effects dominate SW effects in this season because the surface albedo is still high.

A55

This is surely true and we agree with the referee. Above the East Siberian, Laptev and Kara Seas, the sea ice extent and its albedo are high enough to explain the LW CRF over those regions in AMJ (see Fig. 9 of our manuscript).
However, we concurrently see an increase in CFC in the last two decades. Provided that the ocean can not be an appreciable source of water vapour in the boundary layer, Kapsch et al (2013) attribute an increased downwelling LW flux to the increased atmospheric opacity as a result of convergence of moisture, in form of clouds and/or water vapour (see Fig. 3-a in Kapsch et al, 2013) as seen in reanalysis data for April and May (but not in June). The water vapour trends are confirmed by Rinke et al. (2019), who show that water vapour is increasing in all three months of the AMJ season (see Fig. 5 of their paper).
While it is true that in our dataset (see A43) the CWP is either unchanged in AMJ and slightly decreasing in JAS over the regions discussed in this section, we think also that long-range transport of moisture might be a concurrent cause of the increase in LW CRF trend in these regions.

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.

Rinke, A., et al. "Trends of vertically integrated water vapor over the arctic during 1979–2016: Consistent moistening all over?." Journal of Climate 32.18 (2019): 6097-6116.
https://doi.org/10.1175/JCLI-D-19-0092.1

R56

Line 372-373: This first statement is not true. The statement in question is speaking to why there is a trend towards decreasing net CRF. Indeed, there is more insolation in JAS compared to AMJ but that is true of every year of the analysis and it does not explain a temporal long-term trend in CRF.

A56

We agree with the referee that a long-term trend in CRF is not affected by changes in insolation in one individual season and claiming it makes no sense. We are putting into context the first statement at line 372 (" more insolation in the JAS months results in a more efficient SW scattering by cloud droplets") with the lines before (370-371) and namely we are comparing AMJ months with JAS months: "Although not surprising, we note that the AMJ changes in CRF do not correlate with either LWP or COT. In the JAS months, however, larger cloud optical densities and LWPs are matched by a decrease in CRF at the surface. This is the combined outcome of . . . ".

We will remove the first statement.

R57

The second statement is true and is a direct result of the Shupe and Intrieri figure 7. However, this mechanism would lead to a decreased net CRF (more cooling) even if the COT and LWP did NOT change. In reading through this paper, overall, it appears that the clouds have changed somewhat, although it is difficult to discern exactly how from the information provided. However, it is quite certain (although not shown here) that the surface albedo itself has decreased over areas where sea ice has retreated. This surface albedo change itself will lead to the observed trend in total CRF, and the potential change in clouds might serve to enhance or diminish this direct impact of the decreasing albedo.

A57

We hope that with the help of A37, A43 and A49, together with the trends shown in Figs. 7 and 8 of the original manuscript, it will clearer that cloud properties did change, and this has had an impact on the CRF at the surface.

R58

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

A58

We uphold our explanation, which is in fact not contrary to that given by the referee. Our intend to explain one of the most striking feature in our results, namely the increase in LWP and COT over Greenland and to relate it to CRF. We think that at least one paragraph should be devoted in the text to an explanation of the phenomenon. In the specifics, we explain the trends in our Fig. 8 and Fig. 10 with the results of Bennartz el al. (2013).

On the other hand, we agree with the referee that fragmented information on Greenland shall be consolidated for the sake of readability. We propose to move the paragraph to the end of the sentence at line 340 (after "(Hofer et al., 2017)").

Furthermore, Hofer et al. (2017) did not only deal with the periphery of Greenland but did consider the ice sheet in his investigations of the CFC. With the help of the reflectances and cloud data we can particularly clearly confirm Hofer et al: on the one hand we see a loss of CFC in summer in the southern part of Greenland and, at the same time, a loss of CWP (both LWP and IWP). On the other hand, we do confirm an increasing trend in optical thickness of relatively thin clouds in this region (see Table B1 in Appendix: τ-liquid AMJ 8.4 JAS 6.7, τ-ice AMJ 6.0 JAS 6.0), driven by the concurrent increase in LWP and IWP in both seasons.

The paragraph will be replaced by the following text:

"In addition to cloud loss (Hofer et al., 2017), extensive ice melt in Greenland is also known to be enhanced by low altitude liquid water clouds that have sufficient opacity to enhance downward LW flux, but are also optically thin enough to allow a significant amount of SW flux to pass through. This results in the surface being warmed (Bennartz et al., 2013). Such clouds occur in the LWP region between 10 $\mathrm{g\,m^{-2}}$and 60 $\mathrm{g\,m^{-2}}$. Figure 8 shows that the increase in τ-liquid of clouds and LWP over Greenland in spring and summer is among the largest in the entire Arctic (ΔLWP $>$ 20-40%). In both seasons, the cloud fraction decreases and τ-liquid (as well as the LWP) increases spatially on average. Both effects impact upon the downward SW flux at BOA, but in the opposite direction, resulting in a small net positive change in SW CRF. For decreasing CFC and in presence of an increase in near-surface temperatures, we expect a decreasing downward LW flux larger than the enhancement by more liquid water in the clouds (see Fig. 9, mid panel).
Overall, the radiative effect of CFC and τ is expected to be similar, provided that their changes in time agree in sign. Because CFC and τ change in opposite directions, the decreases in LW CRF and increases in SW CRF suggest a dominant influence of CFC rather than by water content in the clouds over Greenland. This CFC influence is still modulated, but not offset, by the changes in τ and CWP."

R59

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

A59

We agree with the referee about the distinction between sunlit months and those without SW radiation. We will clarify this issue, bringing also into context recent literature (e.g. Huang et al, 2021). The latter shows that prescribing in the CESM1-CAM5 a weaker scavenging of supercooled liquid droplets by ice crystals via deposition in spring months leads to an increase in available atmospheric liquid water and a concurrent increase in downwelling LW flux at the surface.

Huang, Y., Dong, X., Kay, J.E. et al. The climate response to increased cloud liquid water over the Arctic in CESM1: a sensitivity study of Wegener–Bergeron–Findeisen process. Clim Dyn 56, 3373–3394 (2021). `https://doi.org/10.1007/s00382-021-05648-5`

R60

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

A60

From our perspective, it remains important to distinguish supercooled liquid and mixed-phase clouds because of the microphysical interplay between the liquid and ice phases in the clouds. Moreover, the presence of some INP is needed to grow ice in the clouds and an accurate assessment of the Arctic aerosol loads and speciation is challenging. We briefly discuss this point in the context of future needs.

For the "reversal" of the cloud feedback, we agree with the referee that context is missing and the chosen wording is not optimal. Note that our intention in this paper is not to assess cloud feedbacks as a whole but to provide evidence-based support for modelling efforts. In this regard, we will provide more context with respect to the increased liquid content in the clouds and how this finding relates to changes in sea ice (Morrison et al., 2019), to Arctic cloud feedbacks (Sledd and L'Ecuyer, 2021b) and to the slowing of sea ice albedo feedbacks by clouds (Sledd and L'Ecuyer, 2021a).

Morrison, A. L., Kay, J. E., Chepfer, H., Guzman, R. and Yettella, V. (2018). Isolating the liquid cloud response to recent Arctic sea ice variability using spaceborne lidar observations. Journal of Geophysical Research: Atmospheres, 123, 473–490. https://doi.org/10.1002/2017JD027248

Sledd, A., and L'Ecuyer, T. S. (2021a). Emerging trends in Arctic solar absorption. Geophysical Research Letters, 48, e2021GL095813. https://doi.org/10.1029/2021GL095813

Sledd, A. and L'Ecuyer, T.S., 2021b. A Cloudier Picture of Ice-Albedo Feedback in CMIP6 Models. Frontiers in Earth Science, 9, p.1067. https://doi.org/10.3389/feart.2021.769844.

R61

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

A61

See A21.

Overall: Based on my full assessment of the paper it is essential here to use the existing data to determine the answers to a couple of important questions that will help with interpreting the results: Is there a net change in condensed cloud mass (LWP+IWP) or is there simply a conversion of mass from ice to liquid? Is there a net change in CFC, or is there simply a conversion of ice clouds to liquid clouds? Is there a net change in total COT, or is there simply a conversion of ice COT to liquid COT? What is the impact on CRF (SW and total) of the observed changes in surface albedo, independent of any changes in cloud properties? The magnitude of this effect is important to understand as a context for any possible cloud changes that might also impact CRF.

We thank the referee for the time devoted to the scrutiny of our work. We think that his contribution has pushed us to considerably improve the manuscript. We point the referee to those answers that address his remarks. We incorporate the answers in the conclusions for the sake of clarity.

1. *"Is there a net change in condensed cloud mass (LWP+IWP) or is there simply a conversion of mass from ice to liquid?"*

A43: a conversion of ice mass to liquid mass has taken place, without substantially changing the total mass of condensed water in the clouds.

2. *"Is there a net change in CFC, or is there simply a conversion of ice clouds to liquid clouds?"*

   A30: CFC has not changed at pan-Arctic scale. There has been a regional conversion from ice clouds to liquid clouds.

3. *"Is there a net change in total COT, or is there simply a conversion of ice COT to liquid COT?"*

   A43: Total COT has remained unchanged at pan-Arctic scale. COT liquid has increased while COT ice has decreased at pan-Arctic scale as well as at a regional scale.

4. *"What is the impact on CRF (SW and total) of the observed changes in surface albedo, independent of any changes in cloud properties?"*

   A37: The impact on CRF of the change in surface albedo (through sea ice concentration changes) is to enhance SW CRF (more cooling). From sea ice areas to open water, the relationship between SW CRF and sea ice concentration, thus to surface albedo, is close to linear (Fig. 9, left panels). We hypothesize that the spread around this relationship is mainly driven by changes in cloud properties.
   Based on Fig. 9, the total CRF turns from positive to negative (negative SW CRF > positive LW CRF) in the value range of sea ice concentration 60-80%.
   At this stage, we consider that the analysis of instantaneous sensitivities, supported by models, is required to quantitively and comprehensively address the comment by the referee. This would allow to decouple the surface impact on CRF from that of the changes in cloud properties. We consider this beyond the scope of the actual paper.

Technical corrections . . .:

Thanks. All updated.