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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Unveiling aerosol-cloud interactions Part 1: Cloud contamination in satellite products enhances the aerosol indirect forcing estimate" *by* Matthew W. Christensen et al.

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Responses to reviews of the original submission

Review Comments in black; responses in blue

Anonymous Referee #1

Christensen et al. present a new technique of relating aerosol- and cloud retrievals from satellite data. They created an algorithm that searches for the nearest aerosol retrieval for each cloud retrieval. Different from previous approaches (Bréon et al., 2002), no backtrajectories are computed, but the nearest pixel, independent on whether or

not the aerosol might actually be advected to the cloudy region. Despite this, it is an innovative approach and may indeed help overcome some issues with the aerosol- and cloud retrievals. The authors analyse statistical relationships between the aerosol index and cloud albedo computed on the basis of satellite cloud retrievals using a radiative transfer code, as well as between AI and cloud fraction. They proceed to compute implied radiative forcings.

The manuscript is astonishingly superficial in many of the explanations. Many statements are very difficult to follow, or not at all reproducible from the information provided.

The authors are imprecise in their language. It seems they in general want to assess the effective radiative forcing due to aerosol cloud interactions, i.e. the overall cloud response to the aerosol, including cloud water path and cloud fraction changes.

Nevertheless, it is a useful paper and should eventually be published. However, I have numerous specific points the authors should address.

» Thank you for the excellent and thoughtful feedback. In writing the manuscript, we were aiming for brevity since this is a two-part paper. But you rightly point out that some crucial details are needed in order to reproduce the methodology. Therefore, we have revised the manuscript based on your suggestions and the other reviewers so that this research is understandable and reproducible.

P1 L17 Not so much in satellite estimates

» The relative spread in the effective radiative forcing estimates of aerosol radiation interactions and aerosol cloud interactions (ERFari+aci) based on the "expert judgement" in the IPCC report chapter 7 figure 7.19 has a similar range in both satellite data[-0.93 to -0.45 W/m2 with median value of -0.85 W/m2; (range/median) = 56%] and GCM's [-1.68 to -0.81 with median value of -1.38 W/m2; (range/median) = 63%].

L20 the "buffering" isn't precisely defined. A better more specific explanation on what is missing is necessary

» This sentence has been modified to include "cloud-top entrainment feedbacks in aerosol-cloud interactions" in the description.

P2 L1 given the large range of GCM estimates, it needs to be clarified which publications the authors refer to

» We now reference the appropriate IPCC 5th Assessment Report reference including the specific chapter in which these forcing estimates are published.

L4 remains

» Done

L4-10 the order is awkward. If one had proper CCN retrievals (in the order the authors impose item 5), items 1-3, perhaps even 4, wouldn't matter. Also not all problems are pertinent to all aerosol-cloud interactions. The authors need to be specific about what exactly they want to study and where which of the issues arises.

» Fundamentally, from a satellite perspective we are aiming to obtain a suitable proxy for CCN. It could be argued that points (1-3) hamper attempts to retrieve this accurately from space. I agree that points 4, 5 & 6 are different problems and have therefore deleted them here to make the point clear that we are referring to aerosol retrievals – not aerosol-cloud interaction processes.

L11 the authors need to clarify what they mean by "contamination" (do they mean problem 1, 2, or 3?) 3, to some extent 2, cannot really be called "contamination" since these are plausible physical processes. It is also important that the authors shouldn't forget to mention that clouds are also an actual source of aerosol. Sulfate predominantly nucleates via the aqueous phase.

» We agree that the general word "contamination" is being used too loosely here and throughout the text. Therefore, unless specifically referring to "cloud contamination" in the aerosol product the retrieval is described as being an "artefact" caused by other processes besides humidification which is a real process that enhances the size of the

aerosols near clouds. In this particular sentence we have simply removed the word contamination and replaced it with "retrieval artefacts."

L19 "larger" than what? And do the authors really refer to a forcing here, or rather to an effect?

» Thank you for flagging this the end of the sentence seemed to be missing. The results from the Twohy et al. (2009) refer to the "aerosol direct radiative effect."

L29 that hold of course only if one analyses one grid box over one season. Experience shows that in such attempts, very rarely 90 data points would be available.

» True, when the cloud cover fraction is 100% over a grid-box there is no availale aerosol data and under these conditions this would decrease the number of level-3 pre-averaged samples. Therefore, we have added the words "at most 90-pairs..."

L31 of course also the problem of spurious clouds in pixels labelled cloud-free

» Yes, thank you for pointing this out. This process is now included and discussed.

P3 L1 this statement needs further explanation to be understandable.

» The main point was missed here and restated. Wet deposition processes would be unconstrained in cloudy areas where the satellite cannot retrieve aerosol that is used for assimilation in these reanalysis products.

L3 While the authors call their method "new" they should acknowledge at the presumably first aerosol-cloud interaction study from satellites (Bréon et al., 2002) already applied such a method.

» Thank you for pointing out the method described in Bréon et al. (2002). Their approach is arguably different but indeed another way to pair satellite retrieved aerosols and clouds together. For completeness, we have added a sentence describing the back-trajectory approach and reference their work. Although, it is not clear from Bréon et al. (2002) whether the assumptions are valid, namely, whether errors in the wind field for which the back-trajectory model relies upon may confound the aerosol-cloud pairing scheme. A deeper examination of this method combined with the screening selection CAPA algorithm, we believe, would be a powerful approach to studying aerosol-cloud interactions. We have therefore included this important step in the manuscript and discuss in the conclusion section how a back trajectory method could used here.

L7 the theoretical maximum for the 1km MODIS retrievals of clouds is about $110 \times 110 \times 90 = 11 \times 10^5$. Is the reduction by a factor of 3-4 an empirical result?

» No, this was a mistake. The calculation was initially based on an estimate using monthly data (30 days) and I forgot to update the value for CAPA based on three months of continuously sampled data. The text has been updated to the correct value of 11×10^5.

L8 Can the authors clarify what the scale of the MODIS retrievals is? I believe it is 1 km for the cloud product, but is it also 1 km for the aerosol product?

» The MOD04 product is sampled at 10 km spatial resolution but the data has been resampled to 1-km resolution in this study to match the corresponding cloud product which is at 1 km. We have added this pertinent information to the method section of the manuscript.

L16 It would be good to report the overpass time

» This information has now been included for both instruments.

L18 "seconds" should be abbreviated ("s")

» Done

l24 the authors should explain their statement "this consistency is essential". The conclusion is not straightforward, but obviously using the same cloud mask for aerosol and cloud retrievals also introduces issues.

» It could also be argued that by processing visible and IR data simultaneously and

using a single meteorological state, the outputs of ORAC are physically consistent with all radiances observed at TOA. Many other types of algorithms (e.g. MODIS collection 6) process VIS and IR separately and integrating the results implies a physical state that is "inconsistent" with what was observed. ORAC uses the same radiative transfer forward model and particle scattering calculation framework, so systematic errors resulting from those are shared between aerosol and cloud retrievals providing tighter consistency than other retrieval algorithms.

L26 bracket awkward

» This sentence was split into two for increased clarity.

p4 l7 The appropriate reference for MODIS collection 6 cloud products is Platnick et al. (2017, doi:10.1109/TGRS.2016.2610522)

» Thank you for referring us to this recently published paper. It is now cited accordingly to properly reference MODIS C6 products.

l22 "lower" than what?

» This sentence was deleted since active sensing retrievals are not the focus of this work.

L27: $F^{\downarrow}_{\text{clr}}$: why the index "clr", this is just the incident solar radiation, it seems? Why not operate in Eq. 1 simply with reflected fluxes that are actually observed? Also, at a pixel level, CRE is not defined from observations (cloud fraction is either one or zero). At which scale do the authors compute the CRE?

» $F^{\downarrow}_{\text{clr}}$ is the incoming incident solar radiation. This quantity is shown in the equation merely for completeness. We do not operate equation 1 simply with the reflected fluxes because this relationship is strongly influenced by the aerosol effect on cloud fraction. In this sense it is better to decompose it into intrinsic and extrinsic forcings to avoid bias in the strong correlation with cloud fraction Feingold et al. (2016). To clarify, we are not computing CRE at the pixel scale resolution. The derivatives forming equation 4 stem from two separate populations. 1) the 1-km pixels forming the clear-sky pixels used to compute ($dA_{\text{clr}}/d\ln AI$) and 2) those forming the cloudy-sky pixels ($dA_{\text{cld}}/d\ln AI$). We have added this key piece of information to the text. In a separate paper (Christensen et al. 2016, JGR) we examined these derivatives using pixels containing only cloudy observations but processing the flux algorithm a second time but assuming "clear-sky" conditions and found that the results were equivalent two processing using two separate populations with cloudy and clear-sky pixels. In general, the $dA_{\text{clr}}/d\ln AI$ term is much smaller than $dA_{\text{cld}}/d\ln AI$.

L28: It is a bit misleading to call F_{obs} "observed". It obviously rather is the flux computed on the basis of the aerosol and cloud retrievals. Why not "all-sky" as usually defined? Are the clear-sky thermodynamic profiles from reanalysis for the appropriate grid cell? Is the humidity for these the all-sky or the clear-sky humidity? In which sense is "clr" less observed than "obs"? Isn't that applying the retrieved aerosols?

» The F_{obs} term is misleading since it isn't actually "observed" like what CERES, for example provides. Therefore, this term has been changed to F_{allsky}. The thermodynamic profiles used in the broadband flux calculations are interpolated to each 1-km imager pixel (cloudy or clear) from the N256 spatial resolution of the ECMWF (European Centre for Medium range Weather Forecasting) Interim Reanalysis product. The typical cloud fraction global distribution derived from the allsky composite of low-level warm liquid clouds is provided in Figure 1 of this response, this forms the c_m climatology term for JJA used in Equation 4.

P5 l4 This is a very loose definition of an "indirect effect". The authors can of course define such a quantify. Usually one would call the definition in Eq. 4 something like a "cloud radiative effect sensitivity", and if one multiplies this with the anthropogenic ΔAI, one would obtain a proxy for the effective forcing due to aerosol-cloud interactions (proxy since it only accounts for column physics).

» Good point. The definition now includes the term "cloud radiative effect sensitivity"

L8 it is peculiar that the authors make use here of the annual-mean incoming solar radiation. The factor in the brackets presumably has a strong diurnal and annual cycle. Co-variation of this factor with the incoming solar radiation then leads to possibly substantial differences in the radiative effects compared to the ones proposed by the authors.

» This is a typo/mistake. The incoming solar radiation is not actually based on the annual-mean value. All of the fluxes are instantaneous with the satellite observations but corrected by the daily-mean solar insolation value. To avoid sampling biases we then compute the mean insolation over each season along with the aerosol-cloud sensitivities, first, to limit the co-variation between the annual cloud cycle and the radiative fluxes and then average the seasonal sensitivities together to form the annual mean. We have clarified this point in the text. We follow a similar process of that used by Grandey and Stier (2010)

L9 "is called", I propose the authors rather specify "can be called" or "is called here" (or provide a reference if they use this term from a definition elsewhere).

» The intrinsic/extrinsic forcing concept was first defined in Chen et al. (2014) and now referenced accordingly.

L13 The authors should provide the reference of where this "has been shown"

» Besides the seminal paper by Nakajima et al. (2001) another more recent example describing the use of "Aerosol Index" as a better proxy for CCN than AOD is described in Gryspeerdt et al. (2017).

l21 What is assumed about the anthropogenic fraction of the Ångström exponent?

» In this calculation we have assumed that the fractional change in the anthropogenic AOD is equivalent to the fractional change in anthropogenic AI. However, as pointed out by reviewer 3 this assumption may lead to an underestimation of the aerosol indirect forcing.

L28 "Square" in terms of pixels? » Yes, the word "pixels" has replaced the word "square".

L29 Is the 250x250 pixel square moving with the cloud retrieval? If not, couldn't it easily appear that the nearest aerosol retrieval is in the next, not analysed, square? Maybe the authors can provide a sketch to clarify what exactly they are doing. » For clarity we have added the following: "for pixels near the edge of the square region (within 125 km) the search radius is extended into the nearest adjacent square region."

L31 Again, it is necessary that the authors define the scale at which they determine a cloud fraction. So far, I understood from the text that they work at the pixel level (1x1 km^2). At this scale, cloud fraction is simply zero or one.

» The cloud fraction for each subsection consisting of 250 x 250 pixels is computed using the 1-km cloud mask data (in MOD06 or ORAC). This clarifying detail has been added.

P6 l12: ", whereas" » Done

l14: This statement is inconsistent with the "methods" section where it was stated that the aerosol is retrieved at 1 km resolution.

» The ORAC aerosol product is retrieved at 1 km resolution. However, the version we use here is averaged over 10 x 10 km^2 regions. We have clarified this point in the method section.

L19: why not describe what actually is found, namely that the Ångström exponent decreases for pixels nearer to the clouds? Of course it is possible to interpret this in terms of particle size, but this cannot be quantified.

» ORAC actually retrieves aerosol particle size and we see an increase in particle size in closer proximity to clouds, however, this is not a standard output for the MODIS product so we use Ångström exponent to make a comparison for this plot. To avoid confusion the words "particle size" were removed this statement.

L21: this is hard to see from Fig. 2. Could the authors help the reader with a more readable figure, e.g. by horizontal lines?

» The plot has been modified to plot error bars every other data point to increase visibility.

L23: It should again be made clear what is meant by "contamination". Is the limitation to the inner half of the points in terms of brightness authors the only thing done in the MODIS retrieval to address such issues? Don't they also use different cloud fractions for cloud and aerosol retrievals?

» The MODIS cloud and aerosol products do in fact use different cloud flags, and this in part could be an explanation for why we observe artefacts in collection 6 aerosol near clouds but this would need to be explored further to determine whether it is related to contamination or other processes.

L25: Of course AOD is also large near clouds due to swelling, but this is not "artificial".

» It is "artificial" in the sense that, near clouds, AOD is a less reliable proxy for CCN. Although, this could be caused by many factors so the word "artificial" was removed and we have also included swelling as another mechanism to explain this possible increase.

L28: Once more "contamination" - it could be cloud contamination, but could also be 3D effects or swelling, one cannot tell these apart from the analysis.

» This is a good point which is not being expressed in this paper. The increase in AOD could also be due to 3D effects or swelling near clouds and these processes confound our ability to use the aerosol retrieval as a proxy for CCN.

P7 L28: Obviously the standard error decreases with the sample size as n$^{-1/2}$. But didn't the authors discuss standard deviation?

» This statement is rather obvious from a statistical analysis point of view and does not actually contribute to the main point in the paragraph so it has been deleted.

P8 L8: To me it seems that the differences are mainly due to the result that for CAPA-L2, the regression coefficients seem to be mostly positive, while for CAPA-2L_15km, there are very large areas where the regression coefficients turn positive.

» Your observation is correct. I have examined this further and found that 11% of the grid-boxes have a negative cloud albedo effect sensitivity in CAPA-L2, whereas 31% are negative in CAPA-L2_15km (see below histogram values for each 1x1 degree region using these composites). This important observation is now stated in the manuscript.

L12: A better approach would be to show joint histograms. It would be advisable to use the method of Gryspeerdt et al. (2016; doi:10.1002/2015JD023744). It is astonishing that this analysis yields no relationship between cloud albedo and AI, while Fig. 3 and 4 show a substantially positive relationship in the same region. Or do clouds in this region usually have COD < 5 in the AATSR retrievals?

» Thank you for recommending the approach used by Gryspeerdt et al. (2016). While we have not tested this approach here we have tested the approach used by Quaas et al. (2008) that is based on changes in cloud droplet number concentration. In general, the relationships yield similar results for the effective aerosol indirect forcing estimate in each of these composites of our dataset (see full response to reviewer #3).

Close inspection of the comparison between Figure 3 and Figure 5 does indeed show that the mean cloud albedo susceptibilities are roughly the same, i.e. providing values of approximately $dA_{\text{cld}}/d\ln AI = 0.1$ for the region off the coast of California. Your point regarding, "it is astonishing that this analysis yields no relationship between cloud albedo and AI" is not accurate. The linear least squares fit regressions for the CAPA-L2 data show a strong relationship and are statistically significant at the 99% confidence interval using a two-tailed t-test. The CAPA-L2_15km shows a very weak sensitivity by comparison, and this is one of the main points of the paper. For reference, the low-level

warm maritime clouds sampled in this region have annual mean COT's roughly around a value of 10 (see plot below).

L23: "Independently derived" seems exaggerated. After all, as I understand, the retrieval algorithm is the same in both cases, as is the way to compute cloud albedo using the radiative transfer model?

» The words "independently derived" were removed. In general, I agree with you that this seems a bit exaggerated. However, the broadband fluxes are computed based on ORAC-AATSR inputs and C6-MODIS inputs so the datasets are independent but the retrievals are not.

P9 l5: Does not Fig. 2 suggest that swelling is negligible at scales > 15 km away from cloud edges?

» I have added to the statement: may be exaggerated due to retrieval artefacts "caused by contamination of the aerosol retrieval, 3D effects and swelling" to this sentence since it is not possible with the analysis tools to attribute a specific mechanism to this response.

L18: Which references used in assessment reports do the authors refer to? I'm not aware of many estimates that also include what is called here "extrinsic" forcing.

» You are correct. The IPCC report does not include the "extrinsic" cloud fraction forcing term. The intrinsic aerosol indirect radiative forcing computed here is consistent (Amiri-Farahani et al., 2016) with the radiative forcing concept used to describe the effective radiative forcing for aerosol-cloud interactions. We therefore specifically point to the "intrinsic" forcing for comparison.

L26: Is this really the standard error, or not rather the standard deviation of the spatial distribution?

» It is the standard deviation of the spatial distribution in Fig 3, this has been clarified in the caption, and the standard one-sigma error propagated from the linear least squares fit in Figs 7/9.

L32: This is only true for some GCMs.

» In general I would argue this is true for "most" GCMs that were represented in Figure 7.19 in the IPCC report. I appreciate this point, particularly because the GCMs used in the last IPCC report are now more than 5 years old now, so we now reference "most GCMs" instead of the assumed "all GCMs" as previously written.

P11, References General comment on references: The authors should consistently show or not show dois and URLs. Journal names should be abbreviated

» Reference section has been formatted accordingly.

P14, Caption Fig. 1: 512×100 km^2 » Done

P15, Figure 2: It would be useful to show in addition the product of Ångström exponent and aerosol optical depth. Is AI actually approximately constant with distance from cloud?

» Thank you for this suggestion. The relationship between AI and the distance to nearest cloud has been added to this plot which now includes a 3rd row for AI. The relationship is not constant, AI increases nearer to the clouds because the relative changes in AOD are greater than the relative changes in angstrom exponent.

P16, Caption Figure 3: It seems what is provided as "mean" is the global mean values? And "Standard deviation" the standard deviation of the spatial variability of the regression coefficients?

» Yes, this is true. "mean" and "standard deviation of the spatial variability of the regression coefficients" have been added to the caption.

P20, Table 1: "CAPA_L2": this seems to correspond to all aerosol retrievals, not just the green ones in Fig.1 "CAPA-L2_15km" I believe these are the green pixels aren't they?

» Nice catch! This is typo, it should be red and green pixels (not just red).

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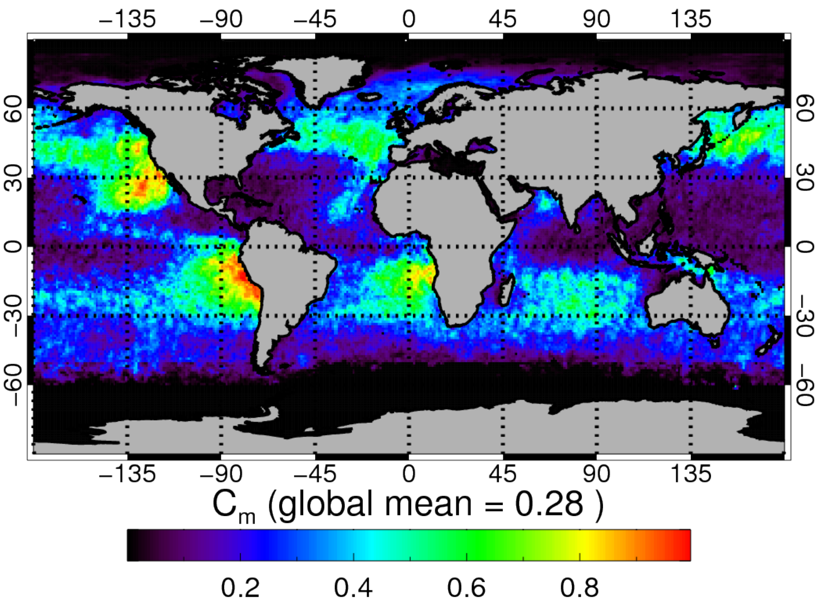

Fig. 1. Low-level liquid cloud fraction climatology for June, July, August using ORAC AATSR observations over 2008.

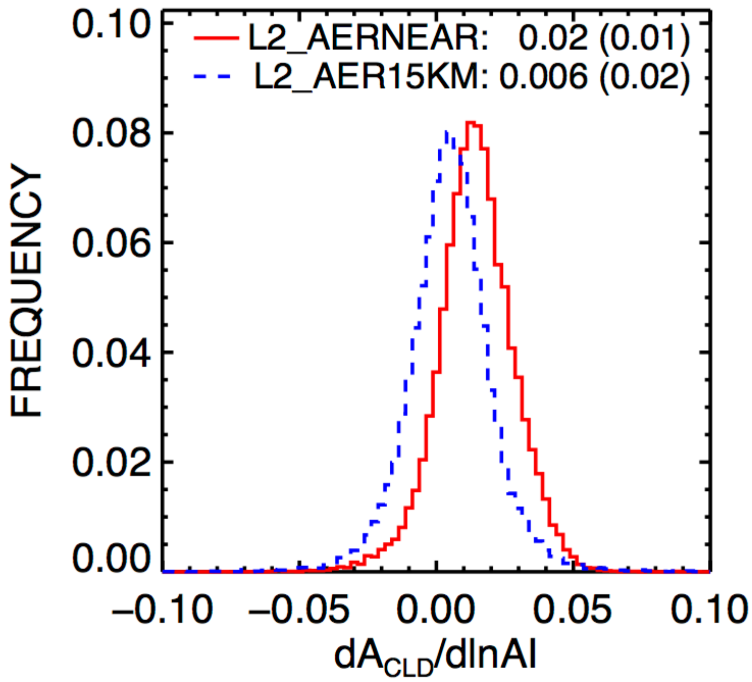

Fig. 2. Histogram of the cloud albedo susceptibilities derived over 1x1 degree regions using AATSR ORAC observations annual average over 2002 - 2012 for the aerosol-cloud pairs using the nearest and 15 km pai

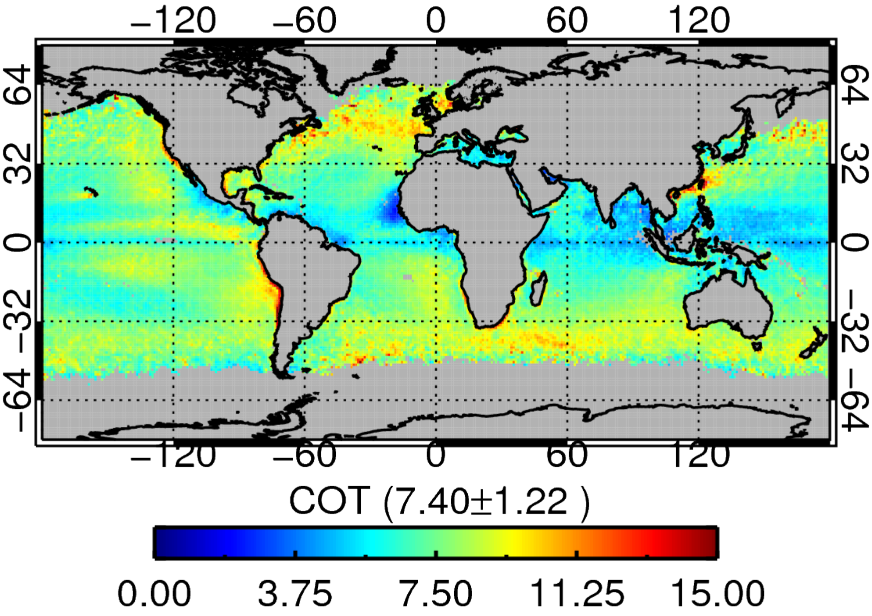

Fig. 3. Mean cloud optical thickness of low-level liquid phase clouds for ORAC AATSR observations annual mean over 2002 - 2012.