

Response to Reviewers, egusphere-2026-712: “Arctic Weather Satellite Assessment and Assimilation at ECMWF”

D. Duncan et al.

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Thank you to the editor and the three reviewers for the constructive feedback offered. In the following, reviewers’ comments are given in *italics* with the authors’ responses in normal text. Updated or new text is copied here where appropriate in **bold**. The full set of differences in the manuscript are attached in the tracked-changes document.

One note that is not covered below. A small error was discovered in the plotting code for Fig. 5 that caused the data from some channels on non-AWS instruments to be ordered incorrectly. This has now been rectified (see end of document for comparison), and the *y*-axis range increased to see all FY-3D channels on the plot.

1 Reviewer 1

General comments:

It is a pleasure to read this article. The presentation is logically structured and the text reads easily. It is useful to developers at other NWP centres to learn how ECMWF has adapted their all-sky radiance assimilation framework to deal with the new technology of the AWS satellite and its microwave radiometer. I appreciate the quick timeline from the satellite launch to operational assimilation and further to comprehensive documentation of the setup and impact in scientific literature. The authors provide sufficient, but not excessive, detail on almost all aspects. I have only a couple of minor suggestions how the article could potentially be improved.

Thank you very much for the positive feedback.

Specific comments:

I wish the authors could unify the way they refer to the primary temperature-sounding spectral region in the microwave. Most often in the text, they say either “50 GHz band” or “50 GHz channels”, so I would suggest a slight rephrasing on lines 5 and 47 to be consistent throughout the text.

Thanks, these two instances have been adjusted to 50 GHz to create better consistency in the text.

Lines 32-33: I believe some more accuracy is needed to define the CGMS baseline orbits. I will leave it up to the authors to decide, but the text should made it explicitly clear that we are talking about sun-

synchronous orbits and that 1:30, 5:30, and 9:30 refer to local times of overpasses at the equator.

This has been amended for clarity as suggested: "...1:30, 5:30, and 9:30 (**local solar time at the equator for sun-synchronous orbits**) agreed by..."

Given that the 183 GHz channel definitions in the AWS spectrum are not exactly identical to those in ATMS and MWHS-2 spectra, the concept of "equivalent channels" sounds a bit vague. It would be useful to discuss in more detail how the authors have determined which ATMS / MWHS-2 channel is equivalent to which AWS channel.

Thanks, this is a point worth clarifying and also raised by Reviewer 3. Two sentences have been added in Sec. 2.1 to address this: "**Third, AWS 183 GHz channels feature single-sidebands, whereas on heritage sounders these channels typically feature dual-sidebands. This instrument design choice does not significantly impact the channels' geophysical characteristics, and herein we consider them equivalent for sake of comparison, e.g. 183.31±3 GHz on ATMS and 180.31 GHz on AWS.**"

Line 138: I suppose "TB space" here refers to brightness temperature observations? More clarity is needed here.

Now clarified thus: "...define cloudiness in **terms of brightness temperatures (TBs).**"

Technical corrections:

On lines 145-146, it would be useful to include a reference to middle

panels of Fig. 3 .

Now added: "...at 165.5 GHz (see middle panels in Fig. 3)."

There is a reference to Table 3 on line 187, which comes before the first reference to Table 2 (line 197). I would suggest removing the reference on line 187.

Thanks, now removed.

There is unnecessary repetition on lines 356-357. I suggest removing "spanning the first three months of AWS active use in the operational system" (essentially the same is told on lines 352-353).

Thanks, removed as suggested.

2 Reviewer 2

General Comments This paper discusses the bias characteristics and noise features of AWS observations and presents assimilation test results. I believe the study can serve as an important reference for future applications of AWS data. The manuscript is generally well written, and the methodology and primary conclusions appear reasonable. However, several issues require clarification before the paper can be considered for publication.

Specific Comments

1. *Use of Quasioptics in AWS The statement: "to simplify the design and save weight, the AWS radiometer forgoes quasioptics that many*

heritage instruments rely upon to co-locate antenna beam patterns on the ground (Albers et al., 2023)” may cause confusion. AWS does employ quasioptics, but it does not include a flat reflector. The current wording could be interpreted as implying that AWS does not use quasi-optical components at all. The authors are encouraged to revise this statement to more accurately describe the instrument configuration.

Thanks for catching this. To ensure that this is properly written, the authors reached out to Roland Albers (whose study on beam patterns is cited here). We have revised this statement in line with his suggestions: **”to enable a compact design** and save weight, the AWS radiometer **does not use a quasi-optical network with beam co-alignment, which many heritage instruments rely upon to co-locate all channels** (Albers et al. 2023).”

2. Scan Geometry Description The description of the AWS scan geometry appears to be inaccurate: “AWS rotates at 50.4 rpm and produces 145 observations per scan line from 2.5 ms integration time.” According to Eriksson et al. (2025), during a 1.19-second constant scan period, a total of 185 samples are collected: 145 Earth-view samples, 15 OBCT samples, and 25 cold-sky samples. Please double-check and revise this description to ensure consistency with the referenced literature.

The authors believe that this description is accurate, though perhaps the use of 'observations' rather than 'Earth view observations' here was misleading. In one scan of 145 Earth observations, these occur during ± 55 scan angle, so 110 degrees. This is 30.56% of the total rotation, and $(1.19 * 0.3056) = 145 * 0.0025$. To clarify for readers the text has been amended to include the 110 degrees of scan angle covering the Earth: **”AWS rotates at 50.4 rpm, producing 145 Earth-view sam-**

ples per scan line in 110 degrees of scan angle from a 2.5 ms integration time, and the near-nadir cross-track samples are separated by about 8 km.”

3. *CI Screening Results for Channel 4* The screening results for AWS Channel 4 differ significantly between land and ocean surfaces. Based on Figure 4, the CI screening algorithm appears to perform better over ocean than over land, particularly for the low V-band channels. For example, over land, some of the removed samples appear to include not only cloud-contaminated pixels but also pixels that resemble clear-sky conditions. Additionally, the screening results exhibit some artificial structures and nearly all of the land samples have been screened out except for the scan-edge, which is actually not affected by the screen algorithm. Please provide further explanation for these differences and the potential causes of the apparent artifacts.

These differences are mainly a consequence of how AWS-4 typically has greater surface sensitivity than a pure sounding channel. The screening criteria are thus quite conservative over land, where we don't want surface emissivity errors to enter our cal/val sample. This is perhaps too conservative, in that many clear-sky cases are removed as you point out. The reason that scan edges are more likely to be included in the cal/val sample is because here the surface to space transmittance (τ) is typically small enough to meet the screening criteria. It is good to clarify this for readers, and so we have added a sentence to Sec. 3.3.2: **”Most channel 4 scenes over land are removed, as the screening is conservative regarding surface sensitivity for sounding channels, though higher zenith angles at the scan edges typically make it past the screening due to low enough τ values.**

4. *Scene-Dependent Bias in Channel 4* From the original O-B results for AWS Channel 4 shown in Figure 4, a scene-dependent bias is observed, particularly a warm bias over high-latitude regions. Is this bias primarily caused by calibration-related errors, background model errors, or other factors? Please discuss the possible root causes.

This is a very good question, and one that we struggle to answer at the moment. To be brief, this type of signal exists at the level where disentangling model bias, instrument bias, and radiative transfer / spectroscopy bias is very difficult. If we compare to references like AMSU-A or ATMS, the patterns of zonal bias vary to different degrees, so it is not clear that AWS is an outlier. For instance, the ECMWF operational monitoring plots for ATMS show that NOAA-20 has a bias in the opposite direction as AWS for this channel, being warmer in the tropics, whereas NOAA-21 ATMS does not (https://charts.ecmwf.int/catalogue/packages/obstat/products/map_0001LWDAod_ATMS?base_time=202603310000&data_type=fgdep_bcor&flag=Used&label=map_from_atms_0001_LWDA_tovs_tb_225_short&level=6&stream=All_datastreams). As such, the authors would prefer not to speculate on the cause of this pattern here.

5. *Equation (3): Zenith-Angle-Dependent Noise Term* In Equation (3), it is unclear how the zenith-angle-dependent noise term is calculated. Additional details would be helpful for readers. Since the scaling factor equals 1 at nadir, the scaled noise is identical to the noise at nadir. It would therefore be helpful to explicitly state that this term is defined relative to the noise at the nadir position. Furthermore, the authors should provide a clearer explanation of all terms in the equation. As mentioned, the second term is intended to remove the scan-angle-dependent noise feature after superobbing. Clarifying the formulation would help readers

better understand the results presented in subsequent sections.

Thanks, we have added a sentence to the end of Sec. 3.2 to clarify exactly this: **"As formulated, the additional term causes no inflation at nadir and produces enough inflation at scan edges to account for greater effective noise in superobs with fewer observations."** And also clarified that both the values and function form of this additional term were empirically derived: **"The values for σ_{zen} and the functional form for f_{scale} were determined empirically..."**

6. Scan-Angle-Dependent Noise After Superobbing (Figure 9) In Figure 9, the scan-angle-dependent noise appears to increase after superobbing, which is opposite to the expected behavior described by the authors. Please provide an explanation for this result.

It is not clear what the reviewer refers to here? In Fig. 9, the un-superobbed data has a relatively flat line of $std(O - B)$ across the scan, whereas the superobbed data exhibits an increase in $std(O - B)$ at higher zenith angles. This is as expected, due to fewer observations per superob in the outer scan positions. The only exception in the figure is channel 4, which exhibits slightly greater $std(O - B)$ near nadir. This is due to background errors being larger near the surface, so this term dominates the effective instrument noise.

7. White vs. Non-White Noise Characteristics The manuscript states: "For a purely white noise distribution, effective noise should decrease as a function of $1/\sqrt{N}$." What does the non-white noise component look like for AWS observations? If significant non-white noise (e.g., $1/f$ noise) exists in the original samples, striping noise could become more prominent after superobbing due to the suppression of white

noise. It would be beneficial to include a discussion of potential non-white noise characteristics in AWS and their implications for the superobbing results.

This is an important topic for MW sounders, but unfortunately not a topic that we looked into quantitatively. In the earlier AWS report that we cite in this paper (<https://www.ecmwf.int/en/e-library/81681-assessment-arctic-weather-satellite-nwp>), we discussed striping ($1/f$) noise briefly at the end of Sec. 7.2. To summarise what was said there, striping noise was not analysed in detail because it was not a primary concern in early evaluations of AWS (but we also did not say that it was non-existent). Furthermore, the topic was touched on by Eriksson et al. (2025): "Initial assessment gives that the striping in AWS data is modest. It is the highest for channel groups 2 and 3..." From a data assimilation perspective, it was also not a primary concern in the same way that it was for say ATMS on SNPP. You are entirely correct that striping noise can in some cases become much more obvious after superobbing, as we have seen for Metop-C MHS. The question for us is then whether to characterise striping noise at the level of superobs or original observations. We would be happy to discuss methods for this with you in the future, as it is indeed a key topic for NWP use of sounder observations.

3 Reviewer 3

General comments:

This paper is a very enjoyable and informative read. The structure is clear and logical and the scientific approach is rigorous. The authors

provide a well-documented analysis of the assessment of data quality, pre-processing methods and the assimilation of AWS brightness temperature data within the ECMWF NWP model. The figures are clear, relevant and well-designed. They contribute to the understanding of the methodologies and results. This study will be a good resource for researchers on the assimilation of AWS data. I have a few minor suggestions which could improve precision in certain sections.

Thank you for the positive feedback.

Specific comments:

Table 1: For humidity-channels, AWS channels 10-15 are presented to be equivalent to 17-22 ATMS channels and 10-15 MWHS-2 channels but their definitions are a little different. For example, the channel 11 of AWS has a frequency of 176.311 GHz whereas the “equivalent” channels of ATMS and MWHS-2 frequency is defined as 183.31 ± 7 GHz. Can considering only one side of the absorption band influence the comparisons of the data of these instruments?

Thanks this has been clarified in the text as seen in response to Reviewer 1 above.

Figure 3: For feedhorns 3 and 4, there is a peak in the number of observations in the zero-bin of SI or CI (particularly on land). It could be interesting to have some explanations about what these observations represent and why they do not appear in the feedhorn 1.

This is an interesting point, but with a mundane explanation. For feedhorn 1, the bin size shown in the histogram of counts is quite small (0.1 K), and thus inside the noise of the instrument, so there is not such

a clear peak near $CI = 0$. Noise and some imperfect bias correction combine to smear out true clear-sky observations into several bins as shown. For the middle and right panels (SI and CI for feedhorn 4), the bin size is 0.5 K, emphasising the peak much more.

Figure 4: On cal/val sample of the channel 4, most observations are screened out over land but some remain, particularly at the edges of the swath. It could be interesting to investigate or explain this behaviour.

Please see response to Reviewer 2 (their comment 3), above.

Figure 6: To better assess the Gaussianity of the cal/val samples, overlaying a Gaussian curve (with the mean and standard deviation of the samples) on the histogram would provide a visual confirmation of the distribution's fit.

Thank you for this suggestion. The figure has been amended by adding a Gaussian curve for the cal/val sample. The text has also been changed accordingly: "This figure illustrates how much of the PDF "tails" are removed by the cal/val screening, **more in line with the Gaussian curves shown in black.**" "A dotted black curve provides a Gaussian with mean and standard deviation of the cal/val sample for comparison." (figure caption)

l.250: When discussing the across-scan biases for the feedhorn 1, a reference to Figure 4 would help to illustrate the negative bias observed in channel 4 at the first scan positions.

Thanks, done: "...show slightly larger biases near scan edges (**Fig. 4**)

Technical corrections:

l. 138: The abbreviation “TB” is not defined yet, it could be clearer to write “brightness temperature”.

Thanks, done.

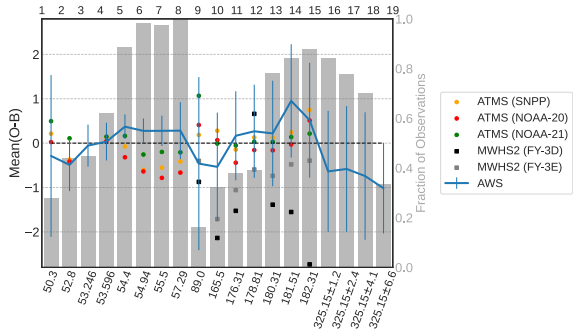


Figure 1: Updated Fig. 5.

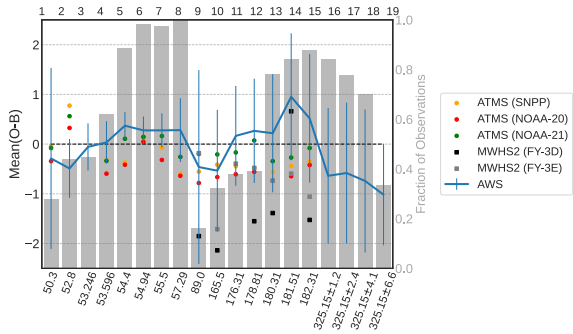


Figure 2: Previous Fig. 5.