Reply to Anonymous Referee #2

We thank the anonymous referee for his review and his detailed comments.

Below, the referee's comments can be found in bold, followed by the authors' replies.

Simulators are useful for predicting the performance of instruments and helpful in understanding various error sources and in devising algorithms to extract the maximum amount of information. In this paper a simulator for the proposed WIVERN W-band radar is used to examine the behavior of the reflectivity and Doppler profiles over mountainous terrain.

Although I have several questions on the details, I found the paper informative. Since the WIVERN radar will be used primarily for cloud sensing, I expected to see some results on the atmospheric effects on surface cross section and Doppler but perhaps that will be dealt with in a separate paper. I recommend publication after the authors address the comments below.

Table 1: It's not clear to me whether the radar will transmit H and receive H and V (and transmit V and receive H and V) or whether it will transmit H and receive H only and transmit V and receive V only.

The radar will transmit and receive both H and V signals in pairs. Each pair has a repetition frequency of 4 KHz, with a delay between the H and V pulses of 20 microseconds. This value was missing and has been added to Table 1 as T_{HV} in the revised manuscript. Figure 1 of Rizik et al., 2023, shown below, offers a good representation of the pulse sequence for the WIVERN polarization diversity radar. A reference to this figure has been added to the revised manuscript.

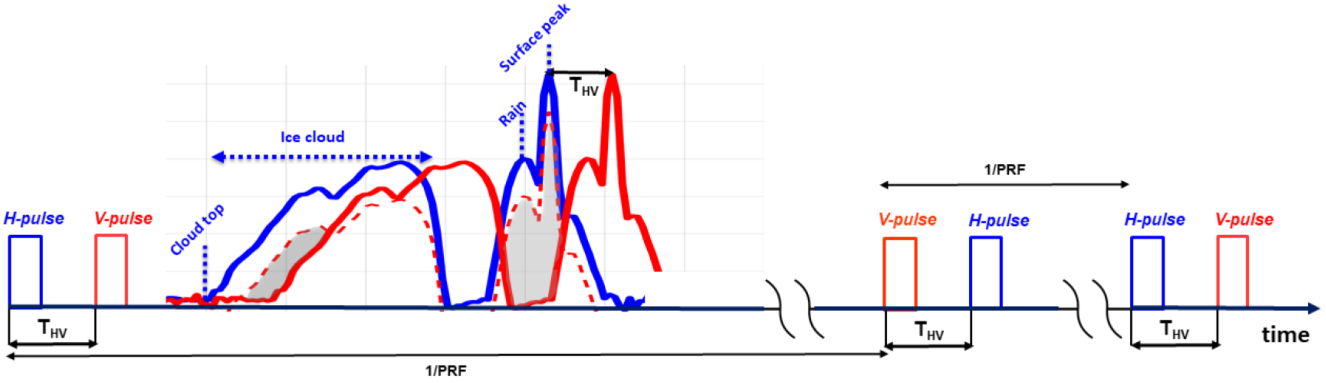


Figure 1: Two pulses with different linear orthogonal polarizations are sent with a short pulse-pair interval, T_{HV}. The dashed line corresponds to the cross-talk of the leading pulse which interferes with the trailing pulse. The shaded regions correspond to ranges where in the V-receiver the cross-talk signal exceeds the co-polar signal. The sequence is repeated with a much longer separation equal to the inverse of the Pair Repetition Frequency (PRF). The order of the polarization state of each pulse pair is switched from pair to pair.

More generally, I'm not clear about the meaning of the 'ghost pulse' and how this affects the Doppler processing. This issue comes up later in the paper where the rho(HV) parameter, which implies that the cross-pol will be measured, is used to categorize the results. A few more sentences would be helpful to

explain how this parameter is related to the scattering properties of the surface.

Ghost pulses originating from scattering of H or V pulses in the other channel can be used to calculate the Doppler, using the cross-talk signals. This sacrifices SNR in the return power to obtain a less noisy Doppler signal.

The issue is explored more in depth in a later response (regarding lines 148-155), with an explanation and a graphical representation of the process and the cases adopted for computation of the Doppler signal.

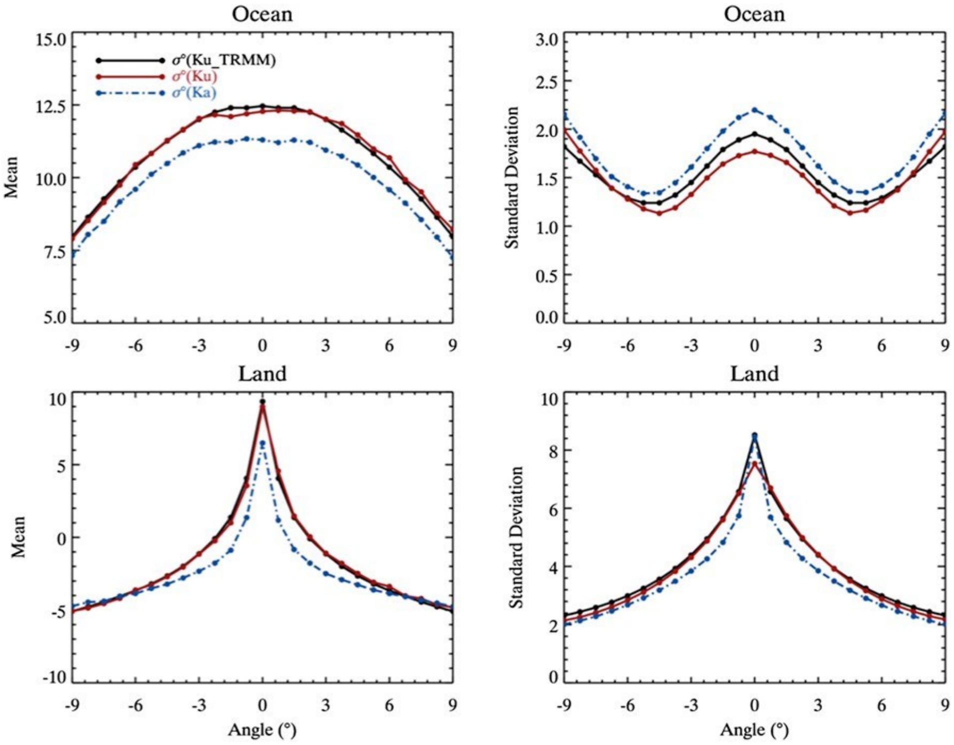
In the revised manuscript, Section 2.3 has been reworked to better address this aspect. A figure will also be added (see Figure 2 in the following discussion).

Fig. 4. The DPR (dual-frequency precipitation radar on the GPM satellite) surface cross-section data (Ku/Ka-band) over land shows a sharper decrease with angle in moving off nadir than the results shown here. The DPR data covers the angle range from nadir to 18 deg so airborne data are needed to fill in at the higher incidence angles.

I couldn't remember how sharp the drop-off with angle was but found the figure below. The land data are not categorized by surface type – all surface types are included.

The radar frequency is not mentioned in Fig. 4 and while the DPR database shows the Ku & Ka-band are similar in their angle dependencies over both land and ocean, the W-band data might depart significantly from the lower frequency data.

Unfortunately, there doesn't seem to be much off-nadir sigma-zero data at W-band, at least that I'm aware of. One advantage of the simulator is that the surface scattering model can be updated as new information becomes available.



Mean values of DPR sigma-zero from 1-month of data are shown on the left for ocean (top) and land (bottom). These data were taken from 35° S to 35° N to match the TRMM coverage. The Ka-band cross section over land decreases by about 12 dB going from nadir to 9°. (Since the data used here were measured early in the mission, the Ka-band data extended only to 9°.)

The data adopted to develop the look-up tables comes from experimental campaign in the 1980s. For consistency, the data was taken from the same source, which included extensive tables for different types of surfaces and frequencies. Unfortunately, data for W band was not available for all classes, so data for Ka or Ku band was adopted for some of them as a best approximation.

We are aware of the GPM measurements. Apart from the routine measurements *K. Yamamoto et al., "A Feasibility Study on Wide Swath Observation by Spaceborne Precipitation Radar," IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 2020* describes results for a wide swath experiment with GPM performed in Ka band, confirming the drop of sigma-zero up to +/-18 deg, for large footprints (in the order of 5 km). The experiment extended the sigma-zero characterization up to 35 deg scan angles. For the ocean the drop is consistent and for land there seems to be a slight decrease too. However, land classification is not present in the above-mentioned paper, and the data may be unreliable due to the fact that the GPM electronically scanning antenna was not designed to look at those scan angles (up to 35 deg).

In general, given WIVERN scanning geometry, better coverage at around 40 deg incidence is crucial. We are aware of the limitations in our NRCS model, and it is indeed true that it can be easily updated with newer data as soon as it is available.

Lines 148-155. I had trouble following this discussion. First, it would be clearer to say something like: 'Land surfaces are generally categorized by large values of depolarization (-10 to -3 dB) and low values of rho(HV) (0.4 to 0.8).'

Lines 148-150 have been modified in the revised manuscript following this recommendation.

My confusion comes in the next sentence. 'While there is not much correlation for the co-polar surface signals ..' *(does this mean between rho(HH) and rho(VV))* '...there is an excellent correlation between the cross-polar signals ...'. –italics mine. But the previous sentence stated that these values are low so I must be missing something.

Two different methods to calculate the Doppler signal return. The first originates by correlating the H and V signals from the actual surface range. Unfortunately for land surfaces the correlation between the H and V reflection signals is low (of the order of 0.5 as observed from airborne observations, Wolde et al., 2019) . The second originates from correlating the surface ghosts (Rizik et al., 2023), which have a much higher correlation but lower signal-to-noise ratio (but still much larger than 0 dB) because of the large sigma-zero of land surfaces. The figure below better explains these two different methods to derive the estimates of the surface Doppler.

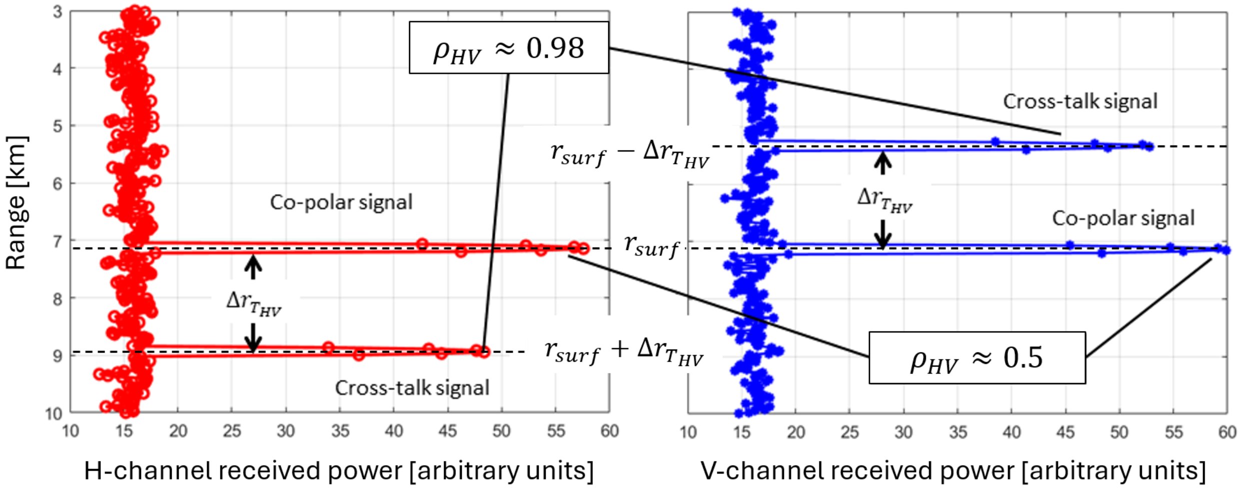


Figure 2: Schematic describing the two possible methods for deriving surface Doppler based on pulse pair estimates.

In the first case co-polar signals at the same range (the surface in figure, at r_{surf}) in the H and V channels are used: this offers higher SNR compared to the other method (by 5 dB, 10 at maximum) but the correlation is lower (around 0.5) due to the scattering characteristics of land surfaces. In the other case, the two cross-talk signals are used at different ranges, in particular separated by a range equal to $2\Delta r_{T_{HV}} = 4T_{HV}\,c$, where T_{HV} is the interval between H and V pulses, 20 μs (this value was missing from Table 1, it has been added in the revised manuscript). The two co-polar signals at the surface range correspond to the H (first) and V (second) separate pulses sent T_{HV} one after the other bouncing back from the surface without changing polarization. The two cross-talk signals originate from the same H or V pulses which are backscattered in the cross polarization and therefore appearing at different ranges (higher above or below the surface). For these signals the return power is lower but the correlation is much higher because of reciprocity.

In the revised manuscript, Section 2.3 has been reworked to better address this aspect.

Is 'rho' in lines 157 and 159 the same as 'rho(HV)' in the previous paragraph? If so, the same notation should be used.

Correct, thank you for pointing out the mistake. The notation has been fixed in the revised version.

Fig. 6 caption: ‘wo’→‘two’.

The mistake has been fixed in the revised version.

Line 166: I see a blue ‘X’ but not a black cross.

The mistake has been fixed in the revised version.

I'm a bit confused by Fig. 7 and the associated discussion. It is assumed that the antenna is pointed in a direction orthogonal to the satellite velocity vector so if the sigma-zero were uniform over the footprint, then the Doppler would be zero - as shown by the red line. The variation in range in the Doppler is presumably caused by NUBF so a positive Doppler (assuming positive is toward the radar) would be caused by the return power from the forward portion of the beam being larger than that from the backward portion of the beam. Is this correct?

Yes, it is correct. Return power is directly related to the w_{ij}^{v} quantities plotted in fig. 8, as shown in Eq. (6). This equation fundamentally says that the Doppler velocity at a given range is the result of the weighted average for any given surface domain corresponding to a given range of the satellite velocity projection along the line-of-sight (value in the red iso-lines in Fig. 8) with the weights w_{ij}^{v} (color coded in Fig. 8). These weights indicate the return power reflected to the radar by a given surface pixel, which results from combination of NRCS and the square of the antenna gain.

A revision has been made at line 179 to better illustrate this aspect pointed out in the comment.

I would have expected the reflectivity profile to be much more variable in range than the blue line shown in Fig. 7 left panel. How typical is this; how much does it change when a field of view over the mountains is taken?

In this case, the radar illumination comes from the right of the scene. Although in the radar footprint there is a huge inhomogeneity in NRCS, this is not reflected in a strong difference between the reflectivity profile (blue line in Fig. 8) and the reflectivity profile of a flat homogenous surface (red dashed line in Fig. 7) because the iso-range lines are almost parallel to the gradient of sigma-zero. A profile much more variable in range can be obtained for this scene by changing the illumination geometry, e.g. aligning the boresight with the sigma-zero gradient. Otherwise, this scene is rather flat in terms of orography, compared to much more mountainous parts of the Piedmont regions, e.g. the Alps. For the latter conditions, it is also possible to find case studies where vertical profile of reflectivity strongly departs from the flat homogenous refence shape (see Fig. 9, top right panel, in the orographic segments at the beginning and the end of the scan).

An idea of how the reflectivity profiles are deformed in mountainous regions is given by the two top panels of Fig. 11 (though these are CFADs). On the left the profiles are close to the flat homogenous case, while on the right cases with high elevation standard deviation are considered.

These observations have been added to the revised manuscript right after line 193 following the referee's comments.

Not sure if side lobes are included in the antenna pattern but these would add to the Z variability, especially in the mountains.

Yes, sidelobes are considered in the computation of the return power, up to -30 dB (normalized gain). Fig. 8 and the right panel of Fig. 6 show gain contours in black, with the presence of the first sidelobes which is 0.17 deg from the boresight peaking to a normalized gain of -24 dB.

As can be seen by the weight magnitudes in Figure 8, the contributions of the region illuminated by the antenna sidelobes are much smaller than those from the region illuminated by the main lobe.

A note has been added to the revised manuscript at line 167 to remark this aspect.

Fig 9 caption on explanation of the bottom two panels, right-hand side. Presumably, means are given by the blue lines and std dev's are given by the red lines. This should be mentioned.

In the revised version this information is pointed out as recommended.

Does '1 km averaging region of elevation' mean that for calculation of the surface cross section the radar return power is used over a 1 km range window to compute mean and std dev? For example, if the mean & std dev at a particular point are (15, 5) dB, does this mean that about 66% of the data falls within 10 to 20 dB?

For computation of the mean and standard deviation values, a square grid of 1km×1km centered on the point hit by the boresight axis on the DEM surface is used. This was done in order to account for the region that contributes more to the computation of the Z and v_D profiles (outside from this region the antenna gain is very small). Using this window, which is running along the boresight track, the mean and standard deviation of sigma-zero in dB and elevation in metres is obtained computed and attributed to the central pixel of the window domain.

Does the phrase 'with the antenna scanning at the side of the satellite ground track' mean that the data are taken at an azimuthal angle at 270 deg?

The azimuthal angle is around 90 deg, and the satellite is moving northbound. We agree that the phrase is rather confusing, in the revised manuscript it has been substituted with “in side configuration, with a scanning azimuthal angle of about 90 deg”.

Line 209: correspondence

The mistake has been fixed in the revised version of the manuscript.

line 213: '.. this value is expected to be zero (*for fields of view orthogonal to the direction of spacecraft motion*)..' . - italics mine. Although this was noted earlier, I think it's important to emphasize that the direction along the incidence angle is perpendicular to the spacecraft motion.

Regardless of the direction of the boresight during its rotation, once the satellite orbital velocity projected along the boresight is subtracted from the Doppler profile, the value corresponding to the surface range along the boresight (assumed to be at the peak of reflectivity) is expected to always be zero for flat and homogenous terrain. Departures from the zero value can be attributed to noise and NUBF.

Line 218: should the fourth category be: 7 dB < std(s0) < 25 dB ?

Correct, the mistake has been fixed in the revised manuscript.

Use of 'dB' here and use of 'meter' for the std dev of height might make this more readable and remind the reader of the units.

Yes, this recommendation has been followed in the revised manuscript.

Line 220: 'A few' rather than 'Few'. 'Few' implies 'Only a few'.

Corrected in the revised version.

Lines 221-222: point 1 is difficult to understand and should be rewritten.

Point 1 has been rewritten in the revised manuscript.

One possibility: The classes have been defined to include a significant number of cases in each. Those classes where the standard deviation in elevation is small have a high-count number because much of the terrain in the segment chosen is relatively flat.

Yes, this is correct. Even though the standard deviation classes have been spaced so a reasonable number of occurrences are present for each class, the ones at lower elevation std have a higher count due to the region chosen as case study being predominantly flat.

(The unwritten assumptions are that low std dev in elevation implies relatively flat terrain which implies a small standard deviation in sigma-zero. But I think these assumptions are OK.)

Orography does impact the variability of σ_0 due to the variability in incidence angle, but σ_0 variability is mainly impacted by the terrain class diversity. In Section 3.1 it is demonstrated that even slight orography induces NUBF and therefore departure from 0 m/s due to changes in range and antenna gain.

Fig. 11: I'm having trouble understanding the behavior of the Doppler in the middle figures. From the title of the left middle figure, it seems that phi(A) is being varied from -15 to 15 deg but wouldn't the Doppler be the same regardless of a change of sign in phi(A)? What parameter is being changed to produce the positive and negative Doppler.

The Doppler profiles at around 0 deg and 180 deg (forward and backward, respectively) in theory have the same shape (approximately), but the sign of the slope is opposite. The Doppler profiles with ϕ_A in the [165, 185] deg range have been inverted in sign in order to group them together as done with the two side configurations (two bottom panels).

For the forward-looking case, the large Doppler shift induced by the satellite motion has been subtracted off, correct?

Yes, correct.

Line 242: renormalised

The mistake has been fixed in the revised manuscript.