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# *Interactive comment on* "Snow density retrieval using SAR data: algorithm validation and applications in part of North Western Himalaya" *by* P. K. Thakur et al.

**Anonymous Referee #1**

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General comments The subject of the paper (i.e. the retrieval of snow density) is quite interesting and is one of the open issues on the table of the scientific community. Also, it is very interesting to look at the performances of the retrieval approaches used in the work since, up to now, the methods have been used in very few works. In particular: the tested approaches make use of backscattering data at frequencies which are poorly affected by the presence of dry snow. In fact, a common assumption is that dry snow can be considered transparent for frequencies below C-band (this is one of the reason because the proposed ESA mission CoReH2O was designed at X and Ku-band). This because the volume scattering of dry snow at C- and L-band is negligible. Overall the work seems to have been done in a good way, however, there are at least a couple



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of strong weakness in the paper, hereinafter listed, which must be addressed. Unless, the paper will result in a mere application of a method, without any real and appreciable insight.

SpeciïňĄc comments 1- In section 2.3, the retrieval approaches are generally sketched and some formulae given. However, nothing has been said about the physics over which rely the two approaches. Basically, it seems that Shi and Dozier (2000) approach relies on the Snell law at the air/snow and snow/ground interfaces. But nothing is said. In my opinion, a scientific paper should clearly expose the physic of the "tools" used, even if they are only applied. The same applies to the Snehmani et al. (2010) approach. Only a formula is provided in the text which generally says "Eq.4 can be used to estimate the dielectric constant of snow". Nothing more. Since the used approaches are not well established and not used in an operational way by the community, it would be nice if the authors describe accurately the methods that they used, and why they choose them. 2- Section 3.2 is supposed to provide the validation of the approaches. To my knowledge, the applied methods are still in a development phase and they need to be accurately validated before they could be used as they are (if any method could be used in this way). The validation reported in the paper is based just on nine points. Also the range of the investigated snow density is approx. 100-250 Kg/m3 (see fig.6), while the authors use it in the range 0 - >500 Kg/m3 (see tab.2). In my opinion, this part of the work cannot be claimed as "validation", it is just a first control that the methods work correctly. I know that it is difficult to build up a good dataset of snow ground measurements, and that this problem is common to other researchers, however the validation of the approaches is lacking and this makes the paper very weak.

Other issues related to the paper are: 3- Section 1.1. There is a great confusion in the use of the word "surface". The term "snow surface" can be used in lieu of "snow cover" or "snowpack" generally speaking. But when it comes to describe the scattering mechanisms, "snow surface" become a geometrical surface and a synonym of "interface". For instance: a snowpack is characterized by a certain dielectric constant but



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the snow surface is characterized by a certain roughness. This is particularly true in describing the models (as in pag. 1929 line 23). The section should be revised. 4- Pag. 1929 line 23. The models of surface scattering are, for instance, the Small Perturbation Model, the Small Slope Approximation, the Oh model. The citations reported here are very general. Has the word “surface” been used correctly here? 5- Pag. 1929 lines 27-28. “Snow surface” should be changed in “air-snow interface” and “the contribution from the ground” should be changed in “snow-ground contribution” since the ground does not give a volume contribution to the total sigma nought and the scattering take place just at the interface. 6- Pag. 1930 line 1. The sentence “snow-ground surface scatter dominates in the dry snow pack.” is false. At Ku-band the volume scattering of snowpack could be of the same magnitude of the snow-ground surface scattering. The sentence is true only at low frequencies (usually below X-band). 7- Section 2.3. Given that the approaches used in the retrieval are used only by a small part of the community, it would be really helpful to report at list some figures about the robustness of the methods in the retrieval of snow density. E.g. which is the error expected in the retrieval? 8- Pag. 1935 line 4-8. The sentence starting with “The coefficients. . .” has already been stated several times. Here can be deleted. 9- Pag. 1935 line 10-12. Why the accuracy of the inversion model is higher at L-band than at C-band? 10- Pag. 1935 eq 4. What the symbol sigmaAP(vvhh) represents? 11- Pag. 1936 sec. 3.2. The validation of a method should always be presented before the use of the method itself. It has no meaning to carry out a work and then realize that the method used do not works. 12- Pag. 1946 table 1. How many images have been used in the work? 13- Pag. 1948 Fig. 1. The caption is not clear. What “SAR based backscattering” means? 14- Pag. 1950 Fig.3a. Just a curiosity: usually, in the processing of SAR images, the mask for layover, shadowing, etc. is applied just after the calibration. In fact, it has no meaning to waste time in elaborating SAR images where the data cannot be used. Is there a specific reason because the masking is applied at the end of the retrieval chain? 15- The English of paper should be revised by a UK or American mother tongue reviewer since there are several errors, especially in the first sections.



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Interactive comment on The Cryosphere Discuss., 7, 1927, 2013.