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Interactive comment on "Increasing the spatial resolution of cloud property retrievals from Meteosat SEVIRI by use of its high–resolution visible channel: Evaluation of candidate approaches with MODIS observations" *by* Frank Werner and Hartwig Deneke

Anonymous Referee #1

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Review of "Increasing the spatial resolution of cloud property retrievals from Meteosat SEVIRI by use of its high–resolution visible channel: Evaluation of candidate approaches with MODIS observations" by Werner and Deneke.

The manuscript discusses a topic relevant for the scientific community. Various downscaling techniques are presented and analysed in order to derive high resolution (1km) cloud properties from low resolution (3km) Meteosat SEVIRI data. Methods and mo-

tivations are layed out in detail, but the presentation suffers from the dizzying number of approaches used for downscaling, used for verification, of data spatial resolutions and of acronyms used. While I have full confidence in the authors scientific rigour, I was close to giving up reading through all the details offered. At the same time, I'm missing general interpretation of the large variety of results in some places. This leads to the unsatisfying point that the support of final and most important conclusions is not easy to find for the reader. I have the impression that this manuscript could be much improved by a major revision and tightening of the presentation, especially of the comparison results section 6.

Major points:

Especially section 6 is confusing. I would suggest to reduce the number of downscaling code versions. Especially the results section 6 even has versions not discussed anywhere. I would also suggest to reduce the number of error quantities discussed, maybe to the set shown in the tables Fig 10 and 12. Do not discuss other additional numbers in the text. Please see details below.

Is Deneke and Roebeling 2010 the basis of this paper? I had the impression down to page 8 that many things come from this older publication. If this is the case, it would be one possibility for shortening. You have to make the connection of the two clearer in the introduction.

Technical problem is that a companion paper (Deneke et al 2019) is obviosuly not submitted at this stage. I would recommend removal of all references to it or waiting for its puplication in discussion stage.

Specific points

p.1, l. 11 ff: Where do these numbers for tau, reff and WL and ND come from? I can not easily find these numbers in the manuscript and I hardly can find any discussion of them. Please extend discussion of these later on or remove them from the abstract.

p.1, l. 20: The whole abstract reads as if it does not work very well. Maybe apart from tau. This concluding sentence reverses these statements. Please revise.

p. 3, l. 11: Isn't a clear reference to Deneke and Roebeling 2010 missing here? Can you please make that clear in the introduction.

p. 3, l. 20: This way you will only get relative errors. All problems retrievals at 1km resolution still suffer from are not discussed or improved … e.g. Zhang et al, 2009, 2011. Can you please mention that.

p. 3, introduction in general: Can you please make clearer: What is the motivation for an improved resolution of products? What problem do you expect to improve?

p. 4, l. 30: "horizontal resolution of 3 x 3". This is the nominal sub-satellite resolution. Can you please make clear whether you consider the full spatial response function for each point. Much later it sounds like, but here you widely stick to the simplified "1 km", "3 km" without further explanation.

p. 5, l. 14/15 and 17: I do not understand the need for theses statements. These are purely technical, aren't they? First you mention a method not used in this study?! With modifications described in a study not published yet?! Then you are talking about a version control system development branch (?) to make clear that this algorithm is not perfectly the same as in the unpublished companion paper!? I doubt that the reader needs these documentation details.

p. 5, l. 28: Again the reader wants to know whether you consider the different spatial response function of normal and high resolution channels? Please comment why you think you do not need to consider this or how it is considered.

p. 6, l. 16: "Statistical downscaling": I do not see the "statistical" element? Are not all downscaling steps fully deterministic? No random element is in there? Please clarify.

p. 7, l. 2: IQR=0.03 of what? Daily values? Hourly?

C3

p. 7, l. 4, Fig 2: I can only see three colors for 14:00, 15:00 and 16:00 UTC?!

p. 8, l. 7: "Diurnally, the variability in the hourly derived ..." : You mean the IQR is derived over 18 or 19 hourly values over one day? Or over 16 days?

p. 10, eq. 9: Under which assumption does eq. line 2 follow from eq. line 1?

p. 10, l. 15, "R $\sim$ 1.0" : This proves that both approaches are rather equivalent, but suffer from the same core problem. the reff impact. Is this an important comparison or just a distracting sideshow?

p. 10, l. 22: I can see that the first approach could produce negatives, but the second?

p. 13, l. 1: Is this section including Fig 6 really needed? It confirms stuff that could be seen before and adds another side aspect.

p. 14, l. 11: You did never mention failed retrievals and reasons for it. Skip this sentence?

p. 14, l. 12, chapter: This section is confusing. I started reading with the understanding that you only use MODIS data in this chapter until I read the Fig. 8 caption which sounds like it shows SEVIRI data. Please make sure that this stays clear from the beginning and throughout the section. Do you use "SEVIRI data" or only "SEVIRI-resolution MODIS data" in this section?

p. 14, l. 18: You mention spatial response for the first time here I guess. What about spectral differences between MODIS and SEVIRI? Please discuss.

p. 14, l. 33, "interpolation of SEVIRI samples": Are you talking about SEVIRI or MODIS data here? See point above.

p. 15, l. 4-8: Phew! Now you add sub-experiments "a, b, c, d" on top of the new nomenclature "1, 2, 3 " ... I'm struggling, to keep reading ... At least, do not use "1,2,3" acronyms alone, but write out the experiments to make them more recognizable. Please do not introduce experiments you will not even discuss (1a, 2a).

C4

p. 15, l. 11-12: And now ... a few new products on top. You have to mention the relevanve of these right in the introduciton.

p. 15, l. 21: This is not the first time you use the exact spatial response functions, isn't it? This is rather late to mention the reference for the first time.

p. 15, l. 23, "3x3 block". This block is 333 m resolution here, right? Please make sure that this can not be confused with the other 3x3 blocks mentioned before.

p. 15, l. 25, "level 1b": Could be easily confused with your experiment notation. You did not use the term level 1b data before, you do not need it here.

p. 15, l. 28-29: What is the "modulation transfer function" good for? Why do you only mention it here, that late in the manuscript?

p. 16, l. 2, "spectral characteristics": This is again too late to mention such an obvious problem that late.

p. 17, l16ff: I'm missing this kind of more conclusive interpretation elements instead of adding number to number in the text.

p. 17, l.31: This sounds as if 2/3 and 5/9 are magic numbers found empirically. Approx. 2/3 follow from optical properties directly and in general. 5/9 contains an empirical element. I would prefer to say "WL=2/3 ro tau reff" and adiabatic clouds have a typical additional factor of factor=5/6 due to their vertical structure.

p. 18, l. 3, eq. 18: Again, this equation seems to contain magic, but is rather simple in its core. Maybe give some additional explanation: "Droplet number could simply be derived from LWC and a droplet size. Using empirical factors accounting for typical droplet size distributions and vertical cloud structure, the following can be derived:"

p. 18, l. 22, "3c overall performs worst": Why? Can you give a general explanation or guess?

p. 19, l. 25, "results provide strong evidence that simulateneous downscaling of the

SWIR reflectances is essential": Again, why? Can you give a general explanation or guess?

p. 20, chapter: The conclusions section also needs more of this kind of general explanations and interpretations instead of repeating "x is better than y, so use x".

p. 21, l. 5/6: Many studies show that going below the 1 km scale might introduce new problems with variability which are smoothed out at this scale. Please discuss this caveat when making such a suggestion.

Fig. 9, caption: Here "1b" is mentioned. Isn't it "2b" in the text?

Fig 10.: Please write out the experiments in words in addition to number codes.

Fig. 6 and 13.: Needed?