

## ***Interactive comment on “Assimilation of space-based passive microwave soil moisture retrievals and the correction for a dynamic open water fraction” by B. T. Gouweleeuw et al.***

### **Anonymous Referee #2**

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Near-surface satellite soil moisture retrievals are complicated by low spatiotemporal frequency/footprint of passive microwave emissions. The manuscript tackles the further complexity of open water bodies causing a strong positive bias in these retrievals (this is a crucial topic of hydrologic interest). This bias occurs from temporal changes in smaller water bodies (i.e., small percent of the sensor footprint size), as against the typical retrieval assumption of static and large continental lakes and coastal areas. Using multiple sources comprising ground in-situ, model and retrievals, the authors demonstrate that seasonally varying biases of up to 30 vol.% soil water content can result from relatively small areas (<5 %) of open water. I see referee # 1 has already highlighted important points about the South-Central area analysis and the soil moisture range of

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the stations etc. One point I am concerned about was the comparison between different depth values: Mesonet seems average over a 5 cm profile, AMSR-E and CLM are over 2 cm, and Noah is 10 cm. If I understand correct, this study uses real AMSR-E observations rather than synthetic ones analyzed in earlier ones like Davenport et al (2008), Loew (2008) etc. and this is a positive point that should be brought out well, maybe in the abstract. Somehow I feel that maybe a major revision is not required, and the required revision to be done is minor to moderate. For comments given below, p is pdf page, followed by actual page # in paranthese (one pdf page is 2 actual pages), and L is line #.

Specific comments: =====

p2 (1015), L25: It seems Davenport et al (2008) and Loew (2008) probably uses absolute error and this study does relative error? Is it possible to have a few sentences towards the end in the discussion section that compare the error between this and the synthetic studies?

p4 (1020), L13: Both images do show a distinct gradient.

p5 (1021), L1: Why should it be "Despite these differences": maybe remove the phrase? If I understand correct, AMSR-E VUA in Fig. 3 has an assumed static open water fraction, and it is shows pronounced seasonality. So for AMSR-E UoM with the dynamic open water fraction can show even more pronounced seasonality, or maybe some reduction of the seasonality which is what the figures seem to be showing. I think this "Despite these differences" phrase should be used only after the VOD-based discussion on p.1023.

p5 (1021), L3-5: South-Central Mesonet also seems higher (though statistically insignificant). I think the sentence should be elaborated upon more in terms of higher vs. lower soil moisture areas as seen from the Figure 2 map.

p5 (1021), L5-7: But then the CLM series does not match the AMSR-Es. Can you

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speculate why in terms of any known forcing, radiative model, open water sensitivity etc. biases?

p5 (1021), L9-11: Now this seems to imply an absence of average forcing bias, something that seems contrary to implied as referenced in comments above for p5 (1021), L5-7. Please reconcile and/or speculate about this apparent discrepancy.

p5 (1021), L11-13: So this means a static open water fraction is better (i.e., if I understand correct that AMSR-E VUA has a static open water fraction)?

Figure 4: Is there supposed to be a colorbar here?

Technical comments: =====

Title: Remove the phrase "Assimilation of" from the title, there is not assimilation here.

p1 (1014), L13: maybe "to those based on dynamic estimates of open water fraction" instead of "to dynamic estimates of open water fraction"?

p3 (1017), L27: maybe replace "dynamic variable" by "dynamic model state"?

p3 (1018), L19-23: Perhaps explain the 2 sentences more clearly for the benefit of readers. e.g., what resampling method? how/why 37 km?

p5 (1021), L8: Why cannot it be changed? Please provide an appropriate citation.

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