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Geoscientific Model Development Discussions
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Interactive comment on "GLEAM v3: satellite-based land evaporation and root-zone soil moisture" *by* Brecht Martens et al.

Anonymous Referee #2

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This is a well-written manuscript whose conclusions are derived via a methodologically evaluation analysis. I think that the (clear) description and evaluation of the version-to-version differences constitutes a significant enough contribution to warrant publication. However – prior to that point – the following two major points should be addressed.

Major points:

1) In the introduction, the author's argue that GLEAM is unique in that it is "primarily driven by microwave remote sensing observations." So the novelty here really seems to spring from 1) the assimilation of microwave-based soil moisture and 2) the use of microwave-based vegetation optical depth in the canopy stress formulation. If you take away these two aspects, the approach really just collapses down into a basic rain-driven soil water balance approach (which is relatively simple compared to the com-

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bined water/energy balance land surface already being run globally in e.g. GLDAS).

So it would strengthen the paper if there were more support for the assertion that GLEAM is driven "primarily" by surface microwave observations.

Figure 5 and 6 are clearly an attempt to do this. . .but the results are not very compelling. The second and third columns of Figure 5 show that the background water balance model is generally superior to the assimilated observations. So naturally, more weight is (generally) placed on the water balance model background. This is ok. . .but it is really consistent with GLEAM being "primarily" driven by the microwave surface observations? Instead, it seems more accurate to say that GLEAM is being "primarily" driven by water balance considerations and these balance considerations are being nudged by "secondary" considerations derived from microwave DA.

No comparable results are shown for either root-zone soil moisture or ET. . .presumably because the impact of microwave DA is even less for these outputs.

I realize that some of this is just semantics (i.e. what constitutes "primary" versus "secondary"). . .but I do think that the authors should either: 1) present better evidence for the "primary" role of the microwave observations in GLEAM or 2) be more objective in describing the novelty of their approach. . .particularly the impact of their novel methodological elements relative to approaches (like a classical soil water balance model) which have been around for quite some time.

2) Some type of statistical significance analysis is needed to assess the noted version-to-version differences. I do not think that "statistically-significant" differences should be a requirement for publication. Nevertheless, the reader should be given a sense as to how large the stated performance differences are relative to expected levels of sampling noise.

Minor points:

1) Page 1, Line 7. . .I'd stay away from subjective statements like "most of these vari-

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ables can be relatively easily observed at different spatial scales”. . .it is a stretch to call the remote estimation of rainfall (for example) “easy”. . .much safer to say from the remote retrieval of ET is difficult relative to other water balance components.

2) Figure 6 does not seem to be references in the manuscript. Also, unclear why case 3c is dropped when moving from Fig. 5 to Fig. 6.

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