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Hydrology and Earth System Sciences Discussions

Interactive comment on "An application of GLEAM to estimating global evaporation" *by* D. G. Miralles et al.

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The review from Dr. C. Jimenez is addressed in this response. We would like to thank him for his valuable comments on our manuscript. The text is being revised to accommodate all the changes.

Referee #2

GLEAM is a modelling framework, so the results obtained will depend on the choices made for the driving datasets and the model formulations, as pointed out by the authors at different occasions in the paper. Therefore the importance of obtaining an idea about the uncertainty in the estimates, and comparing the obtained results and derived conclusions with reported from other methodologies, as pointed out also by other

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reviewers.

The paper already presents a significant amount of work, but it would undoubtedly benefit from addressing some of the issues raised by the previous reviewers, in particular the need to somehow evaluate the model uncertainty and further discussing some of the obtained results/insights in the context of other reported studies.

Reply

Correct. We are currently performing an error analyses that will be included in the corrected paper. We will then also be able show the effect of the choice of precipitation input in the final uncertainties of the evaporation product. The results presented in the corrected manuscript will be put in perspective by comparing them to more previous studies.

Referee #2

Being the main objective of the paper the application of the methodology, the abstract may benefit by making the model description shorter and expanding the summary of the results presented.

Reply

The abstract will be modified giving less weight to the methodology. The error analyses will be mentioned and more emphasis will be put on the results presented in the manuscript.

Referee #2

P2.L25. Judging by the reference given, I guess the reviewer meant land surface models, instead of GCMs. A very appropriate reference to add for the same modelling exercise, specifically focusing in evaporation, is Schlosser, 2010. Regarding GCMs, a good reference is also the compilation of IPCC AR4 GCM estimates in Lim and Roderick, 2009.

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Reply

It should indeed be LSM instead of GCM. These lines will be modified to accommodate the work by Lim and Roderick (2009). The volume of global evaporation estimated by Schlosser and Gao (2010) will be included in the first paragraph of the discussion.

Referee #2

P5.L14. It may be a matter of personal preference, but I would rather call this types of exercises as an evaluation (rather than validation). As pointed out by the authors in the section, all estimates involved (P, E, runoff) are subject to uncertainty, and I would argue that it is difficult to say that one estimate is validating the other.

Reply

We understand the point the reviewer is making. However, once we include the error analyses, the content of the manuscript may be more easily followed if we refer to 'validation' for the comparison to in situ measurements (of discharge), and to 'evaluation' when we refer to the test of model performance by the error analyses.

Referee #2

P6.L14. By methodology, it is meant GLEAM or the $P-E$? It is not clear to me whether the change of P to obtain $P-E$ also involves the change of P going into the E model.

Reply

We mean GLEAM. Actually the previous line states: "Note that the choice of precipitation product implicitly affects the calculation of E, despite the fact that the sensitivity of E to values of P is much lower than the sensitivity of $P-E$ estimates (this can be noted in Table 1)."

From the last two comments, we recognize that it is not easy to interpret the scatterplot of the validation study, as it is hard to distinguish whether the scatter responds to errors in the precipitation or to those in evaporation. We could however consider $P-E$

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as one single variable (instead of a substraction of two variables). This variable will be highly sensitive to errors in the precipitation and – to a smaller extent – to errors in the evaporation. This sensitivity will be higher to errors in P because of the higher magnitude of the flux (compared to E) but also because of the intrinsic sensitivity of the E estimates to P. Table 1 shows the different sensitivity of both $P{-}E$ and E to the change in the precipitation product quite clearly.

This part will benefit from a more clear explanation of the uncertainties of $P{-}E$ in the corrected manuscript.

Referee

P6.L20. The scatter may also be placed in perspective by comparison with other $P{-}E$ versus Q figures published (e.g. Vinukollu et al, 2011).

Reply

We thank the reviewer for the suggestion. However, Vinukollu et al. (2011) performed the analyses in terms of E versus $P{-}Q$ making the comparison of the results from both papers not straightforward.

We would also like to take this opportunity to clarify an apparent misunderstanding of our work in Vinukollu et al. (2011) and reiterate that: a) GLEAM interception is daily, b) it is driven by CMORPH precipitation, and c) it is applied globally over the fraction tall vegetation per pixel.

Referee

P6.L28. Simpler to say $P{-}E$ volumes (instead of volumes in the vertical axes)?

Reply

'Error in the vertical axes' gives a mental picture of the vertical error bar, but in any case it will be changed if that way the sentence reads more easily.

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Referee

P7.L13. I was wondering whether the fact that satellite soil moisture is assimilated into the model may capture the fact that the land is irrigated, with a possible impact into the evaporation estimates.

Reply

This is indeed a very good comment. The skill of the soil moisture product capturing irrigation will be softened because of the scaling of the satellite observations during the data assimilation. At every pixel the mean and variance of the satellite soil moisture annual time series are scaled to the mean and variance of the modelled soil moisture (without data assimilation). This means that the soil moisture will present the same annual mean, but the seasonal changes in soil moisture due to irrigation will be captured. Therefore irrigation will be accounted in the model to a certain degree. However, independently of to what degree irrigation is captured, the maximum removal of water through evaporation will be determined by the value of potential Evaporation - which is not (directly) dependent on the volume of irrigation.

We also note that the paragraph the reviewer is referring unfortunately contains a miscalculation. We assume that the increase from E to E_p (potential evaporation) due to irrigation (R) should cause a reduced $P-E$. This is because we assume that discharge is $Q=P-Ep$. This assumption is not correct. Under optimal irrigation, $P=E+Q$ should become $P+R=Ep+Q$. Therefore the discharge will be $Q=P-Ep+R$. In the computation explained this paragraph the flux of irrigation (R) should have been added to the $P-E_p$ estimates.

The flux (R) should be at least the volume of water required by the soil to stay at critical soil moisture constantly (to evaporate at potential rate). In reality the volume of irrigation could be much larger and that volume cannot be estimated with GLEAM. The computation explained in this paragraph is thus inadequate. Those few sentences will be deleted from the manuscript.

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Referee #2

P7.L19. Could MBE be defined? If an error, it may be better to use the term difference.

Reply

'MBE' stands here for Mean Bias Error (it is true that 'error' is not the most appropriate term considering the level of uncertainty in the x axis). The word 'bias' (to the one-to-one line) will be used instead; we believe with the referee that this way it will be more intuitive and more easily understood.

Referee #2

P10.L5. When discussing Figure 5, it would have been useful to also have the P and net radiation maps (though at the price of reducing the level of detail in a necessarily smaller E maps).

Reply

These maps will be included in this Fig. 5 to show the different sensitivity for JJA and DJF of the product to these two inputs.

Referee #2

P11.L10. I was wondering if the Table 2 estimates have been compared with something else. For instance, as the paper claims the importance of the satellite estimates to benchmark GCMs, I compared Table 2 $P-E$ with the IPCC AR4 GCM multi-model $P-E$ in Lim and Roderick, 2009. Even taking into account that the time periods are different (IPCC 1970-1990, GLEAM 2003-2007), for some continents the differences are very large (e.g., for South America GLEAM reports 742 mm, while ISCCP reports nearly half, with closer attention showing that in this case the E agrees well, with the $P-E$ difference coming from the P differences), for others there are not (e.g., for North America IPCC and GLEAM P and E are relatively closer).

Reply

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We thank the reviewer for the suggestion. This study will be used to put in perspective and further discuss the results presented in Table 2.

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P13.L15. I noticed in Figures 6c and 6d that over the South West Sahara there seems to be a sharp gradient (sort of a straight line separating blues and reds in Figure 6c, light and stronger reds in 6d). I was wondering where that may be coming from, an artifact (or a real feature, e.g., related to aerosol presence) in the radiation data that may shift the balance between prec/radiation control of E in that area?

Reply

We believe that the reviewer is referring to the change in land cover from savannah to desert. This is also present in Fig. 6a and 6b. This different cover will be reflected in the input of the model in different ways, but the most crucial parameter determining the change in output will be fraction of bare land per pixel. It will also cause a change in albedo that will be reflected in the radiation input and a change in the way soil moisture is estimated by GLEAM. These changes show up in the correlations presented in this Fig. 6 as a rather sharp boundary.

Referee #2

P14.L15. Not specifically picking on the authors, but here there is a claim indicating that the constituent parts of GLEAM have been successfully validated by comparison with different in situ data. This claim has also been made in other publications reporting estimates by other methodologies/drivers. Nevertheless, the authors are actively participating in a comparison of his product with other global E estimates in the framework of the GEWEX initiative LandFlux and are fully aware that, at the global scale, sometimes and in some regions the differences are relatively large. I was wondering if the authors would like to comment on that.

Reply

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The reviewer is correct. The fact that there are large differences among the existing (validated) products will be mentioned in this paragraph.

References

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