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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Forecasting terrestrial water storage changes in the Amazon Basin using Atlantic and Pacific sea surface temperatures" *by* C. de Linage et al.

Anonymous Referee #2

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Review of HESSD, 10, 12453-12483 — First review — November 2013

"Forecasting terrestrial water storage changes in the Amazon Basin using Atlantic and Pcific sea surface temperatures"

by C. de Linage, J. S. Famiglietti and J. T. Randerson

GENERAL

The authors propose to establish the proof of climate influence of Pacific and Atlantic ocean temperatures (and thus precipitation) onto water mass storage variations in South America. For this purpose, they use a first-order dynamical model to predict

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long term variations including extreme floods and droughts by introducing (individual or combined) known climatic indexes as forcings. The empirical parameters of the model are adjusted for each grid cell using the 10 years of 10-day GRGS GRACE solutions. They come to the conclusion that an optimal linear combination of dual climate forcing climates is more appropriate to explain water mass variations in the Amazon basin, than considering only one index. The numerical demonstration is relatively well presented and the basic idea and results remain significant enough to be published in HESS. I have no comment about the figures since they are of quality.

MAIN COMMENT

However, I would suggest to reorganize a little bit and rewrite to make parts of the text more fluent, especially Discussion. I was surprised that : (1) this part is divided in individual subtitles without clear a guiding principle; (2) there are generalities in 5.3 : "the northeastern Amazon is characterized by...", "farmers and fishermen may be more vulnerable...", "...reducing economic losses...". One would expect these general information at the begining of an introduction (or possibly end of conclusion to offer perspectives). The main point of the article is the use and validation of a predicting dynamical model, isn't it ?

OTHER COMMENTS

* I easily guess its meaning but please define the acronym "RMSE" and the term "R^2" in the body text, even mathematically.

* Paragraph 2.1 : please add "Spatiale" when defining the acronym "GRGS". In one or two sentences : what are the motivations of using this unofficial GRACE solution set in particular, instead of monthly CSR, JPL or GFZ solutions ?

* Computing the "uncertainty". I don't see the connection between signals on the oceans and "uncertainty". I consider the amplitudes over oceanic area just as residuals that remain after substracting unperfect dealiasing models of oceanic and atmospheric

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mass... plus probably aliasing related to the difference of sampling. Residual signals on oceans may have geophysical meanings of oceanic processes we did not successed to model accurately, with no relation with pure global error.

* "The increase in the dynamical range... (Prigent et al., 2007)", this sentence is unclear. Please reformulate.

* Paragraph 2.2 : "We chose Nino 4... as shown by de Linage (2013)". Is there a physical reason ? Does it mean Nino 3 is finally not connected to the climatology of South America ?

* Paragraph 4.2 : "The shortest lead time... Sect.4.4". It simply means your searching domain of parameters is not large enough at the begining. For clarity, it would have been more logical to start and present the search in the largest parameter space, and then to focus on reduced parameter intervals.

* Paragraph 5.3 : "Faster processing...". Another critical problem for forecasting is that GRACE mission ends. This will limit the analysis in time. Moreover, be careful that the forecasting of hydrological variations is made if the condition of signals stationary inside the analysis window is proved. If not no accurate prediction is possible. Is it really the case ? Please discuss this point in one or two sentences.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 12453, 2013.