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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Global partitioning of runoff generation mechanisms using remote sensing data" *by* Joseph T. D. Lucey et al.

Anonymous Referee #1

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Review of 'Global partitioning of runoff generation mechanisms using remote sensing data' by Lucey, Reager and Lopez, hess-2019-292

Lucey et al. provide a method to assess contributions of precipitation and water storage to runoff generation at the global scale. The scientific significance is their use of global satellite data to study the two contributions at global scale, the paper therefore fits within the scope of HESS. The methods themselves seem valid, but the paper needs revisions mainly for clarification purposes. For instance, the abstract, intro ('our goal is') and conclusion can be more in line, and I do not understand why two time-lag thresholds are applied. Detailed comments are given below. I would therefore say the scientific significance is good-excellent, while the scientific and presentation quality is fair.

P1 l16-18: This last line of the abstract is not a conclusion that I got from reading the paper. Now it seems that this is one of your main points, whereas in the text you find that most areas have mixed time lags and are driven by both precipitation and water storage.

P2 l56-58. Here you clearly state 3 research questions. These are not reflected well in the abstract. Also, I feel that the characterization of general behavior is limited.

P5 l87: you mention the common period amongst the 3 satellite products is April 2002-October 2015, but the previous paragraph mention that all satellite products are available until 2017 or present. So why October 2015?

P5 l 96: is each dataset's climatology based on the 2002-2015 period? Then I would not call it a climatology as it only spans 13 years, rather 'the 2002-2015 average'.

P5 l105-107: I don't understand the concept of the two time-lag thresholds. Do you check the cross-correlations for up to 5 months lag as well as up to 11? Also, if you are using a climatology (i.e. 12 months), going up to 11 months lag makes little sense to me to start with.

P5 l 114: please explain what coverage means. E.g. grid cells covered by swamps? (I'm also not very familiar with GIS so the term polygon is also unfamiliar to me).

P6 eq. 4-6: abbreviation LTA is not explained here. Also does 'slope' refer to m1, m2 in Equation 3? I thought those were determined using the climatologies, but then determining the standardized values (Eq 5) would not make sense, hence I'm confused at the methods here. Or are these equations only used for the four highly studied basins, i.e. Eq 5 shows the standardized value determined by the gridcells in the basins? Also, Eq 6 could be re-written to make it clearer that the control variable is determined by the slopes, e.g. slopegpcp - slopegrace.

P8 l150: "we no longer consider all 0-11 month models", yet it is shown in Fig 9.

P8 Fig4: just a small suggestion to make the figures easier to read, at least in my

opinion. Maybe place a small text within the figures, rather than having the reader disentangle which figure represents which dataset and which lag threshold (also goes for other figures).

P9 l 164: “we can see” . . . “we know”. Actually I can’t see or know because so far I’ve only seen the results of the multi-linear regression (Fig 5) and not the single regression models. The comparison for single and multi-linear regression only shows up in Fig 10 (without time lags). So I would rephrase or re-order, as you mention in this paragraph that a ‘multi-linear regression model with a time lag correction between 0 and 5 months is the most rigorous for further analysis’, so I was a bit surprised to see Fig 10 discussed later on.

P10 l 173: I assume you mean August 2007 rather than specifically August 15th 2007.

P10 l 177-179: I got a bit confused which dataset has which limitations in which locations. SWAMPS data has limitations over desert and mountainous areas shown in Fig 1, but modeled SWAMPS (maybe better to call it modeled inundation rather than SWAMPS?) has limitations in areas with snow and ice or seasonal monsoon areas, so that is related to limitations in either GPCP or GRACE?

P11 l198-199: related to the above, is the inadequate data related to GPCP or GRACE?

P12 Fig 7: if there are no available measurements in winter, then the scatter plots reflect only the summer / fall months?

P13 l210-216: refer back to Eq 4-6 to help the reader remember how you determined your error.

P14 l224: “white areas represent no values” this is repeatedly mentioned in the text but not in the captions. It would be OK to mention this clearly once (white areas are masked using SWAMPS quality map).

P15 Fig 9: refer to Eq 6

P16 l241: bracket goes before ‘comparing’ instead of ‘Fig’.

P16 l252-257: Regression coefficient refers to Eq 3 (m1, m2, called slope elsewhere)? Furthermore, you use a scale from -1 to 1 whereas in lines 99-107 you mention that negative regression coefficients should be impossible, and therefore you introduce time lags. So why are there still negative values in Figure 11? Should results for those grid cells not be trusted? The color scale is also a bit misleading, as grey values (towards -1) do not reflect small values (around 0), but the orange colors do.

Overall: I felt it was a bit confusing that the terms GRACE / TWSA / water storage, GPCP / precipitation, SWAMPS / runoff generation / inundation are used interchangeably.

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