

Interactive comment on "Impact of improved Sea Surface Temperature representation on the forecast of small Mediterranean catchments hydrological response to heavy precipitation" by Alfonso Senatore et al.

Anonymous Referee #2

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Review of: Impact of improved Sea Surface Temperature representation on the forecast of small Mediterranean catchments hydrological response to heavy precipitation (Alfonso Senatore1, Luca Furnari1, Giuseppe Mendicino1)

General comments

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This paper addresses the sensitivity of precipitation, and thereby river flow, prediction in WRF-Hydro to the choice of SST boundary forcing, with specific focus on 2 case study

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periods of heavy precipitation in southern Italy. The work is of general interest, particularly given the increasing effort on developing more integrated hydro-meteorological prediction systems. Overall the paper is well written and contains some original insights. The Introduction is well considered and clear in setting the context for this work, and I found Table 4 to be a very useful and well considered summary, along with most of the discussion presented in Section 5. In particular, the exploration of sensitivity of hydrological application to the regional atmosphere configuration is an important link between regional atmosphere and hydrological communities.

At times I found the presentation difficult to follow, particularly given the large number (6) of different experiments considered and effectively 3 different case study periods discussed. I expand on some specific examples below. My recommendation would therefore be for the authors to aim to undertake a thorough review of the materials and discussions, with a view to reducing the length of the paper, and number of figures, in order to provide a more concise and impactful treatment of the material. I also raise some more specific but equally fundamental concerns below which should be addressed. I encourage the authors to resubmit to HESS after these major revisions.

Specific comments

A) There are a large number of figures presented, and some (e.g. Fig 1, Fig 2) have a number of disparate sub-plots, which was more overwhelming than adding clarity. I encourage the authors to reduce the number of figures and sub-plots in general. As an example, the synoptic situation could be more usefully highlighted through Fig 1(b) and (e) only, and in Fig. 2(b) only perhaps.

B) Similarly, the other materials of Fig. 1 might be better placed alongside relevant comparison, such as in Fig. 4 (gauge rainfall comparing with gauged flows), and Fig. 6 (maps of precip accumulation).

C) P5, I30: Consider omitting this paragraph as not relevant to the paper. Perhaps instead it is worth highlighting the potential for higher temporal frequency updating products in the discussion section, alongside highlighting the potential for use of dynamically coupled ocean-atmosphere systems in place of the global-scale boundary conditions. A relevant recent reference here includes the work of: https://www.ocean-sci.net/15/761/2019/os-15-761-2019.html

D) Section 2.2.2: Calibration methodology. I find it strange that a calibration methodology is applied using data from the period of interest, rather than using some external data outside either case study 1 or case study 2 periods. This approach should be defended more clearly, or replaced with a more considered approach. Further, it is not clear which model data were used in the calibration – were all experiments based on the same calibration data, and thereby favouring one of the experiments over others presumably? Given that the study aims to assess the sensitivity of simulations to different configurations, this does not seem a valid approach here. The authors might clarify what effect the calibration had on the predicted flows, or perhaps simply be consistent and use uncalibrated settings for both case study 1 and case study 2.

E) Improved SST: The paper discusses the impact of "different accuracy levels of SST", and makes reference to "improved SST" in several places (e.g. title, abstract, section 3.1 from p8,I5, section 3.2 from p10,I6, Section 4, p15). However, the paper is critically lacking in Fig 4 and Fig 11 for example from lack of SST observations against which to conclude which systems "overestimate", "underestimate" or have "improved" SST. At best, it seems that the authors can illustrate how much the various SST products differ, but a more conclusive treatment might be possible through comparison with insitu observations (e.g. as provided through CMEMS). The authors should review the language of the argument here and throughout, but ideally provide some comparison to available in-situ SST observations.

F) Case Study 1: p9, l24 – I have some issue with the conclusions to Case Study 1, and the lack of treatment of hydrological flows. Given the original nature of going from SST,

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to precip, to river flows within the paper, this appears to be an omission, even if the result is that there is no sensitivity in this case. Without the full 'end-to-end' treatment here, it seems confusing to justify the exploration of both Case Study 1 and Case Study 2, such that for brevity I wonder what is lost from the paper by only concentrating on Case Study 2 period. This seems to be supported by more limited assessment (e.g. no categorical statistic assessment) for Case Study 1 period. The authors should clarify this final paragraph, and better justify the need for treatment of both period 1 and 2.

G) Case Study 2: I was not clear on the need for separate discussions of both 4-day and 3-day duration simulations, and again this added to the length and more confused presentation. To what extent are the authors conclusions altered by focussing only on discussion of 3-day (or 4-day) results for example? The authors should better justify the treatment of both lead times, and preferably condense the material into a more concise manuscript, perhaps highlighting the key messages of impact of lead time in the discussion. There are opportunities to consolidate some of this information within the Figures, e.g. by presenting the Taylor diagram summaries of 3-day and 4-day results alongside each other (using filled/unfilled symbols to represent different lead times perhaps).

H) It would be useful to highlight some discussion of the relative sensitivity to precipitation to SST shown here (e.g. Fig 6, Fig 12) relative to say the typical ensemble spread expected through other approaches to represent simulation uncertainty.

I) Figure 4 – given above discussion about calibration approach, please include uncalibrated flow results here. Please also clarify the difference between Fig 4 and Fig 17 b) and d). This is another example where a more concise and consolidated presentation of results would improve the paper. For example, which experiment does the "calibrated flow" in Fig 4 show, and why are results from all experiments not included here to provide a more immediately conclusive discussion?

J) Figure 6 – it is hard to distinguish the gauge site information in the shaded circles.

Consider making these larger, perhaps as unshaded circle outlines, or providing in a separate sub-plot of gauge-only observations. Again, some overlap of information with Fig 1g here.

K) Figure 8 – consider removing, given information contained in Fig 9.

L) Figure 12 – it is confusing to have different treatments of the observed precip for different cases and across different figures, e.g. radar and gauge plots in Fig 1 and Fig 2 (quite hard to see gauge sites in small panels), gauge points only in Fig 6, merged gauge/radar in Fig 12a. Authors should aim for more consistent treatment, perhaps across fewer figures, to provide a clearer discussion.

M) Figure 16 – could this be omitted (or provided in Supplementary) without any loss of understanding?

N) Table 2 – this seems to have repeated information between the top section and 'case study 2'. I would just leave the top part for brevity and clarity.

Technical corrections

a) While overall the paper is well written, there are occasional places where some improvement to the English style would improve readability. As a specific example, the first sentence of Section 2.1 should be reviewed. Others might be picked up through further edit by the authors and again in the paper production process.

b) Figure 2 - caption has incorrect dates and time for this case I think.

c) P2, I5-6: Please provide specific references to the initiatives listed.

d) P5, I22: Useful to translate 0.25deg. GFs resolution into an approximate grid scale length for region of interest here (i.e. how does this compare to the 16 km IFS?).

e) P8, I5: I did not follow this point - consider rewording

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f) Figure 12 - typo in 'observed' on panel a) âĂć P14, l6: Consider rewording

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