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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Implementation of WRF-Hydro at two drainage basins in the region of Attica, Greece" *by* Elissavet Galanaki et al.

Anonymous Referee #1

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With their paper, Galanaki et al. perform a calibration and validation exercise of the fully coupled WRF-Hydro modelling system over the Attica Region, the most densely populated of Greece, considering 7 high rainfall events from 2011 to 2014. Even though the topic addressed is undoubtedly very interesting (an attempt to perform a complete meteo-hydrological forecast over small catchments in a densely urbanized area), my opinion is that, at least at this stage, the paper does not provide new insights, neither concerning methodology (for which I have some concerns) nor regarding results. The most important novelty, according to authors' words, is that "this outcome is important because WRF-Hydro is implemented under calibration with ground-truth observations for the first time in Greece", but in my opinion, it's not enough (otherwise, any first application of WRF-Hydro around the world should deserve publication). I've some

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major comments and several minor comments listed below. My general opinion is that the paper should be strengthened significantly before being ready to publication, even though I acknowledge that some results if presented better and with more details, could be useful and add information to the topic of fully coupled atmospheric-hydrological modelling and its operational application over small catchments. I hope my comments can help with strengthening the study.

Main comments

Introduction: a lot of work made on meteo-hydrological forecasting chains in the Mediterranean area (and in Greece), even using the WRF-Hydro modelling system, has been not considered, but it should. Please find at the end of the review only a partial list of possible references to be considered.

Calibration methods: I've several concerns. Mainly, it's not clear what is the input precipitation for the calibration of the hydrological model (I wonder if the whole fully coupled system was calibrated upon observed discharge). Furthermore, I've doubts about the final choice of the parameters, which not seldom are equal to one of the limits of the range of scaling factors. I also have other doubts for which I ask the authors to refer to my specific comments. Furthermore, I allow myself to suggest authors read the recently accepted paper of Fersch et al. (2020) dealing in the detail with WRF-Hydro calibration issues.

Results: I wonder about the differences between precipitation results with and without fully coupling. Several studies show that for short simulations such as those performed in this study it is very difficult that differences emerge in the precipitation fields due to the differences in soil moisture conditions. Among them, Avolio et al. (2019), which for a case study rather similar to those analyzed by the authors found that correct SST representation is much more impacting. Therefore, more details should be provided by the authors about how they reached their results, and they should try to explain the reasons they got these results.

Furthermore, concerning the presentation of the results themselves, much more details should be given (please refer to specific comments).

Concerning the utility of the study for "operational forecasting purposes", the authors should at least discuss: 1) why they use in their study reanalyses instead of operational GCM forecasts, which makes their study not completely indicative for operational purposes in terms of forecasts performance; 2) what is the additional computational burden of fully coupled simulations and if it's worth it.

Finally, I suggest a general review of the text concerning English grammar and style (some comments, as examples, are provided below).

Specific and minor comments:

L53: Wagner

Fig. 1a: the hydrological features are not clear. I suggest separate panels where the analyzed catchments (including their borders) are represented better. I guess that, given the high urbanization level, land cover is also an important piece of information to highlight. Finally, all the toponyms cited in the text (e.g., Cithaeron mountain range, Halandri's stream, etc.) should be reported in the map

L78: increased concerning what? To the past? What period? Please specify, otherwise, I suggest another term (e.g., high?). Anyway, the sentence looks a bit redundant.

L95: by the Ymittos Mountain

L100: I guess "were provided". This term "provide" is used 4 times in 5 consecutive lines. Probably the text could be revised

L106: I would organize Table 1 from the oldest to the most recent event. Furthermore, I suggest dealing with events #5 and #6 merging them, I guess they depend on the same synoptic situation

L114: "were occurred" not correct

L128: D04

L137: please revise the text

LL139-147: this information should be included in Table 2, possibly along with the corresponding WRF options

L145: it would be useful to explain why the Noah LSM scheme is preferred to the more recent Noah-MP

L157: “The simulation periods for each event are presented in Table 1.” Not clear: do the simulations include always the whole days (i.e., from 00:00 to 00:00)? Anyway, what spin-up times were selected?

Section 2.2.2. Even if it is already specified in the title of Section 2.2, I would specify here that WRF-Hydro is used in fully coupled (i.e., two way) mode.

L167: 605/95 = circa 7. So, the disaggregation factor is 7? Please highlight more this feature and explain your choice.

L183: I’m not aware that the stepwise approach is somehow recommended. There are many examples of mixed or automated calibration approaches. Among the others, I suggest a very recent one by Fersch et al. (2020). The cited work of Cuntz et al. refers to Noah-MP, not to WRF-Hydro.

L196: I guess “when a parameter was calibrated”

L196: I understand that there’s a kind of hierarchy in parameters calibration, but it’s not clear which is the parameter calibrated first and which later

Section 3.1.1: the fundamental information about the initial value of all the calibrated parameters is missing. Furthermore, other information is missing: e.g., what precipitation values were used for the calibration?

L217: the value is at the border of the calibration range. This means that probably the

authors should explore other lower values for REFKDT, relaxing their constraints. The same for RETDEPRTFAC

L219: it's even more unclear what precipitation was used for calibration. I hope observed, not simulated (in Fig. 2 there are two simulated precipitation series)

L224: no displacement would have been necessary if observations were considered.

Figs.2, 5, 6, etc. show both WRF-Hydro and WRF precipitations, but they are not introduced and the difference is not explained in due time into the text.

L245: Figs. 5a and 6a refer to precipitation

L248: time of maximum occurrence?

L251: "time of maximum values": not much better definition than before

Section 3.2: for Rafina catchment, same problems as for the previous calibration procedure (please refer to my comments above)

Section 3.3: what stations are considered? All? Only Vilia and N. Makri? Not clear. If it's only Vilia and N. Makri, how were the other stations shown in fig. 1 used?

L321: Anyah et al.'s work does not regard WRF-Hydro

Conclusions: it looks more like a summary. It should be enriched highlighting the strong points of the study.

References:

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