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# *Interactive comment on* "A parallel workflow implementation for PEST version 13.6 in high-performance computing for WRF-Hydro version 5.0: a case study over the Midwestern United States" *by* Jiali Wang et al.

**Anonymous Referee #2**

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The paper of Wang et al. deals with a potentially interesting implementation of the parallel version of the PEST software. PEST is a powerful and very useful tool for hydrologists, helping them during long and "exhausting" calibration sessions. Therefore, introducing the portability of parallel PEST to HPCs is good news and, specifically for the present paper, the main theme to highlight. Nevertheless, in my opinion the way the paper is structured mainly highlights, instead of the advantages of the novelty, the performances of the PEST calibration, which is something widely and well assessed by the hydrology research community. Almost all figures and tables deal with PEST



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results. Furthermore, the calibration procedure presented is questionable from different points of view (some of which are exposed later). The most interesting/innovative Section of the paper is Section 5.1, but the analysis of scale-up capabilities should be described with much more detail. Concerning the main outcomes of the paper highlighted in the summary, points from 2 to 5 are quite obvious (they deal with the recognized skills of the PEST software), while point 1 should be expanded: what does a factor of 30 "with respect to a serial calibration" exactly mean? In my opinion it's not a rigorous statement. What do the authors exactly mean with "serial"? Even though PEST calibration is serial, WRF-Hydro can run in a parallel fashion, and the speed of the calibration process would depend on the number of nodes used for the hydrological simulation. A possible idea is to provide hints about the trade-off between the number of nodes/CPUs used for running the parallel model (i.e., WRF-Hydro in this case) and the number of nodes/CPUs used for running PEST in a parallel fashion. I guess it depends somehow also on the dimensions of the domain (and no information is given here about the number of cells in which the basin is discretized, so the reader has no idea about the actual computational burden).

Another important point, that should be better discussed, is the missed capability of the implemented version of PEST to deal with the calibration of spatially distributed parameters. This is important because it's reasonable to expect parallel PEST executions with WRF-Hydro for wide domains, and wide domains often need spatial differentiation of spatially distributed parameters, like, e.g., OVROUGHRTFAC, RETDEPRTFAC or other spatially distributed parameters available with WRF-Hydro v5.0. By the way, another limitation is that, at least as I understand, the calibration is available only against observed streamflow. Of course, this is the first option but not the unique one (one can decide to calibrate also against, e.g., soil moisture or latent heat flux data).

Finally, another important point is to (at least) discuss the problem of equifinality, which is incidentally (but not explicitly) dealt with in P11 L29 – P12 L5.

Summarizing, though I acknowledge that the research presented is potentially inter-





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esting and innovative, I suggest to re-think the paper highlighting much more the computational benefits provided and reviewing the calibration performed in the case study. Following, a (not comprehensive) list of doubts regarding the calibration procedure and other minor comments and typos. I hope my comments can help improving the research.

Doubts about the calibration procedure:

Even though I acknowledge that authors decided to "focus less on extensively assessing the performance of the WRF-Hydro model", several aspects of the calibration procedure are very questionable.

1. no information about spin-up. This is extremely important, especially for such a short range calibration (only few days). The model should be run in advance (at least one month, I would say) in order to let several variables (e.g., moisture fields) have a realistic spatial distribution.

2. the authors state that: April 8-11 moderate rain, April 12-14 no rain, April 15-18 rain, peak flow April 19. 3-day calibration is: April 9-11 (to be precise, April 12 at midnight), then validation is April 13-23 (April 12 is missed). 7-days calibration is April 9-15, validation is April 17-23. To me, it does not make too much sense that 4 more days are added when only the last one is rainy. It would be much better to calibrate the model with respect to a previous flood event, as it is usual. After all, observing graphs in figures 3 and 5 one after another just shows that increasing the number of days used for calibration improves the performances (but this is rather obvious), even though not yet enough.

3. In order to deal with the observed streamflow in Section 1, it is fundamental to work with weights.

Minor comments, grammar and typos

P6 LL6-17: not clear if in this case overland flow is switched on. It should.

P6 L19: probably “tools”

P7 L18: GENPARM.TBL

P8 LL11-12: master, not mater. The full stop is missing.

P8 L30: As it is a common problem, it is usually solved ‘simply’ reallocating manually the stations. It’s a pity to miss streamflow data for this reason

P9 L24 and following: I suggest to explicitly declare also the meaning of the ovn parameter

P12 L17: 50%, maybe

Figs.2 and 3: April 12 is missing. It should be the first validation day, I guess.

Figs. 4 and 5: the same for April 16

Table 3: the note is incorrect, it refers to information about the 3-day calibration

Section 4.3: this is a purely “hydrological” analysis that could be skipped, given the numerous limitations of the calibration procedure and the focus on the implementation of the PEST software

P16 LL9-10: please check the sentence

P16 L18: to investigate

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