

Interactive comment on “A new stream and nested catchment framework for Australia” by J. L. Stein et al.

Anonymous Referee #1

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The manuscript “A new stream and nested catchment framework for Australia” by Stein et al. presents a comprehensive and highly informative description of the latest iteration and advancements in creating an interlinked scheme of stream and catchment entities that represent the hydrography of Australia in a consistent cartographic manner, including a coding scheme that allows for the multi-scale assessment of topological structure and connectivity. The presented approach successfully accommodates special characteristics of the Australian continent (e.g. a high number of endorheic catchments), and as a key novelty the authors manage to “derive a stream network from a DEM that explicitly includes complex distributary and anabranching drainage patterns” (page 15452, lines 16-18) at a continental scale. This is a significant improvement over previous similar approaches, and the methods and results should thus be of high interest

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to other researchers.

I am familiar with the previous work by Stein et al. and their highly advanced and commendable efforts to provide consistent, standardized hydrographic and hydrologic descriptions of the Australian continent. This manuscript is of the same high-level of quality that I am used to, and I have not many comments. I recommend publication more or less as is, maybe with some minor revisions and a few additional comments by the authors, if possible (see suggestions below). I believe that the strategic implementation of an approach following the described example would be highly beneficial in many other regions worldwide and would provide adequate source data in an appropriate structure to support a wide range of eco-hydrological applications. In that sense, I hope the work by Stein et al. can serve as a role model for others to follow.

Some detailed and technical comments and suggestions:

Page 15436, line 23 to page 15437, line 1: I find the description of the global databases HYDRO1k and HydroSHEDS to be somewhat misleading. It is true that both databases offer only certain pre-defined stream networks and basin boundaries. But the mentioned limitations, in particular the fact that the available products only include stream delineations for catchments larger than 1000 or 20 square kilometers, respectively, are simply due to arbitrary thresholds that were applied by the producers. It is a rather straight-forward task to extract other stream networks or catchment subdivisions as desired by a user. Furthermore, the HydroSHEDS database offers a seamless drainage direction map, i.e. the basis for stream delineation, at 3 arc-second resolution, not 15 arc-second as mentioned in the manuscript. I suggest that these explanations are clarified accordingly. I believe, however, that this comment does not contradict the authors’ general conclusion that the global databases cannot (and are not intended to) achieve the quality of the applied national data for the Australian continent.

Pg. 15437, l. 14, and elsewhere: The reference of “Stein and Hutchinson 2014” should really be “Stein and Hutchinson in prep.” and should be changed accordingly. Obvi-

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ously, it is rather unfortunate that this publication describing the applied new methodology is not yet available. The same reference is used in section 3.1, and here the unavailability of the publication is even more critical. From what I understand, the described processes are designed to derive a DEM-based stream network that matches (as good as possible) the stream network of 1:250K maps, and then these two sets of stream networks are interlinked (via IDs). This is all very interesting, but without the mentioned reference it is somewhat hard to follow or verify. Maybe a few more explanations would be useful here?

Pg. 15438, l. 3: The authors mention that the automated procedures were combined with manual editing, and in chapter 3.1 they state that corrections were applied "as far as possible" (pg. 15439, l. 19). These diversions from fully automated procedures deserve some special attention as they make the applied methodology difficult to replicate or interpret. Maybe some additional comments can be added in the discussion section? Are manual edits unavoidable?

Pg. 15440: The last sentence is not entirely clear to me (semantically).

Pg. 15442: This page provides quite detailed regional information and explanations that are very specific for Australia. I believe this level of detail is not necessary to understand the manuscript. Is it possible to condense this section a bit? The same comment goes for pg. 15450.

Pg. 15443, l. 15-16: The authors use "a modelled estimate of runoff volume rather than contributing area to discriminate the tributary and main stem" in their Pfafstetter coding. They also mention this method ("surrogate of river flow") as an advantage in the discussion section (page 15452, line 29). On the one hand, I agree that this is an elegant solution to avoid that dry rivers with large contributing areas are (incorrectly) coded as the main stem rivers. On the other hand, this method introduces an ambiguous threshold: if the modeled runoff has any errors and requires updates in the future, it would necessitate that the entire Australian stream network is recreated and recoded.

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Can the authors comment on this problem in the discussion section? Can they also clarify where these runoff estimates are coming from (also in the caption of Figure 7)?

Pg. 15444, l. 13: The authors acknowledge that the catchment size within Pfafstetter levels (below level 9) can vary significantly. This may lead to significant inconsistencies in subsequent applications. Can the authors briefly comment on this shortcoming? Do they think it is an intrinsic problem of the Pfafstetter coding that cannot be solved?

Pg. 15448, l. 8: I suggest "up- or down-stream" instead of "up or down stream"

Pg. 15451, Section 4.3 (Limitations and uncertainties): I would expect that the delineation and assignment of inland sinks is highly problematic due to their ephemeral nature and the potential of bifurcations (if flooded, the sinks may overflow in different directions). I understand that there is no real solution to this problem, but can the authors mention this issue as another source of uncertainty?

Pg. 15452, l.1-11: I am not sure I understand this argument correctly. Even if streams are burned into the DEMs of the US and European databases, this does not prevent the calculation of topographic descriptors from the original DEMs (i.e. before burning). So I do not really see an important difference or advantage here.

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