This paper presents an analysis of reservoir storage trends across the contiguous United States (CONUS), based on a new dataset of historical storage that authors put together recently (ResOpsUS). I agree with authors that conclusions based on this dataset would be a useful complement and even an upgrade on existing studies of reservoir behaviour that are based on other data sources. I also broadly agree with the main findings. Yet, this study has several weaknesses, detailed below:

- It is not clear what the direct connection is with the special issue on the representation of reservoirs in hydrological models. In reality, conclusions are not related to this topic. Literature on the topic is introduced at times, and the methodological assumptions made when modelling reservoirs are discussed (e.g. lines 104-110) but with never a mention of how this study actually relates to any of these assumptions.

- More generally, the scope of the paper is unclear. Authors (rightly) realise that there are many ways an analysis of this data might contribute to the literature, but they seem to not choose one (or more) focal point(s). As a result, the literature review is the introduction is disjointed, and several themes are broached without a clear focus.

- A major selling point of the paper is the use of new data, but in fact, this is not exploited by the paper. They do not rely on a comprehensive review of existing knowledge on reservoir storage / operations in the CONUS to propose a systematic view of how this dataset completes or updates this existing knowledge (and this would be a low hanging fruit for a useful paper!).

- The analysis has two weaknesses in leveraging the dataset to derive insights. (i) Authors aggregate by regions without ever looking at individual reservoirs. (ii) A consequence of this aggregation is to drop key regions full of reservoir such as the northwestern U.S.. With these choices, it is not clear they take full advantage of the fine grain data offered by their new dataset.

- As a consequence of unclear scope and methodological choices, findings are all unsurprising and it is never clear how they compare in relation with the literature. Besides, many of the findings on drying at the national level seem to be related to drying in one particular region: the southwestern U.S and in particular the Colorado river basin. It would be great for authors to quantify this contribution from a particular region to national conclusions, or abstain to make these national conclusions.
The resilience angle is interesting, but the concept is not clarified until it is clarified as a recovery metric, and no definition is proposed until the discussion (lines 602-605). If authors want to use their dataset to look at the drought recovery ability throughout the U.S., that is potentially a great idea (if they can show there is a gap in the literature there), but I’d suggest they leverage the dataset by looking at the data from individual reservoirs.

Last but not least, the writing is unequal (see e.g. the abstract) and suggests a paper hastily put together. This impression is reinforced by the unclear scope and disjointed references to the literature (e.g., resilience definitions proposed in the discussion).

For these reasons, my take on this work is that it is probably not ready for publication at this stage. There is clear potential though, and I’d like to suggest that rather than trying to push preliminary work through, authors have much to gain by electing to do a deeper analysis before publishing their findings, probably over several excellent papers. I am also not sure they need to focus on this special issue until and unless they can use the data to address some of the assumptions made in representing reservoirs in hydrological models.

A few detailed comments (given the stage the paper is at I do not provide this for the whole text):

The text would benefit from a rigorous round of edits. Just in the abstract I found a series of imprecisions that would have been fixed by proofreading:

Line 7: as soon as they introduce the phrase, authors need to explain what “reservoir resilience” means in their context

Line 13: “dominated” instead of “dominate”

Line 15: “reservoir storage has decreased”, is it average annual storage?

“our national fraction filled decreases are 50% less than those shown previously”, please rephrase to make this clearer to the reader.

Lines 16-17: the definition of operational range is great to see but should occur the first time the term is introduced.
The idea that the lack of data has been a limiting factor (lines 8 and 27) deserves clarification: what do you see thanks to the new dataset that studies with less data could not?

Line 38: "there are large dams are spread out"

Line 39: provide reference for the standard.

Line 100: repetition of the idea expressed in lines 28-30.

Lines 104-110: is the aside on reservoirs in hydrological models part of the paper's scope?

Methods: this is a collection of indicators. Many of them are interesting to look at, but it is not clear how they complement each other, and what question(s) they are meant to answer when taken together.