Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-122-RC2, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Technical note: Precipitation phase partitioning at landscape-to-regional scales" *by* Elissa Lynn et al.

Anonymous Referee #2

Received and published: 11 May 2020

Review of "Technical note: Precipitation phase partitioning at landscape-to-regional scales" by Lynn et al. Submitted to Hydrology and Earth system sciences. 2020.

General Comments/Overview

This paper describes a new approach developed by the California Department of Water Resources to produce a long term (30+years), monthly, high-resolution (4km), rain/snow partitioning dataset over the Western US. The authors use this dataset/method to estimate long-term changes in rain/snow partitioning. With warmer temperatures, more precipitation is falling as rain rather than snow – which will impact snow water storage and water management practices. The authors argue that due to the paucity of snow observational datasets and the complex topography of the western US multiple datasets are needed to monitor and model hydrologic conditions over

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Discussion paper

the Western US. Therefore they combine high-resolution PRISM precipitation data with coarse resolution freezing level and fractional snowfall calculations from NCEP/NCAR reanalysis to generate high-resolution fractional snowfall over California (and the Western US). While I believe this is a novel approach and one that has scientific merits, I have deep concerns about the use of the NCEP/NCAR 1 reanalysis product used in this study. In particular, the fact that precipitation from NCEP/NCAR is used to estimate the fraction of precipitation falling as snow. I also do not think the methods used in this paper are adequately described. As this is a technical paper designed to describe a method I believe this paper could be accepted following major revisions.

Major Concerns:

1. The NCEP/NCAR reanalysis dataset used in this study is one of the oldest reanalysis products. At the time of its production/publication the authors (Kalany et al, 1996) state that "C" variables (such as precipitation) are completely determined by the model and should be used with caution. As the fraction of precipitation falling as snow is determined from precipitation in NCEP/NCAR reanalysis, I believe this will add significant uncertainties into the study. At a bare minimum this uncertainty/limitation needs to be discussed in section 5.2 "Primary Limitations" and making sure the reader knows this is a limitation of the study. However I suggest the authors consider performing a similar analysis with a new reanalysis product that adjusts model derived precipitation (e.g. MERRA2 or ERA5) and compare the results with NCEP/NCAR reanalysis.

2. PRISM also provides daily precipitation and surface temperature at 4km resolution. One could estimate daily snowfall using a surface temperature threshold (as in the UA University of Arizona 4km SWE product estimations (https://nsidc.org/data/nsidc-0719; Broxton et al, 2019 and as is done in many land-surface models). The authors need to better explain the science behind why it is more useful or credible to use coarse freezing level data from NCEP/NCAR reanalysis to estimate snowfall percentages, than directly calculating these from PRSIM data. (I don't disagree that this surface temperature is not a great indicator of snow level, its just not explained/justified in the paper).

An interesting comparison could be to look at snowfall estimated from daily T/P vs their method of approximating rain/snow partitioning.

3. The sections describing the NAFLT methods and the DWR approach to rain/snow partitioning are not clear.

a). It seems to me the lowest to the ground freezing level would matter most for snowfall and surface conditions. Why then does the NAFLT method use the uppermost level in areas where there may. be a temperature inversion? Please justify this method.

b) It needs to be made clear that with this method – the freezing level can be below the surface topography.

c) Is the freezing level calculated independently for each 2.5° grid box?

d) Most critical: This statement on Page 3 Line 20 “percent of precipitation that falls at elevations above the 0°C isotherm at 200m increments from 0-4000m” does not make sense to me. Why wouldn’t all elevations above the 0° isotherm also be below freezing, and therefore all precipitation would fall as snow? Is there an equation being applied to estimate the fraction of precip falling as snow? Also, does the method start at the freezing level elevation and work up from there (is the reference point for the 0-4000m) then from the freezing level. Or does it start from the surface? Does the method really does is estimate the % of snowfall below the 0° isotherm (where you could have mixed precipitation). Similar language is used in section 3.1 to describe how you apply this method to the high-resolution precip. data from prism (Page 3 Lines 29- 31).

e) In the Primarily Limitations section you state the assumption was made that %snow linearly relates to the NAFTL – but that was not actually stated in the discussion of the methods, it needs to be. My suggestion is to think about how to describe this method to someone who does not know what the freezing level is, or how %snow is calculated and really step through the process – if this is too much detail you might put some of this in supplemental (a diagram could be helpful as well!).

Minor Concerns

- One thing that is missing from the introduction and could be a nice addition is an understanding of how this indicator could actually be used for water resource planning. This is touched upon in the Discussion (Page 5), but I think some of this type of information needs be included in the introduction to further motivate the need for this method. You do say that changes in rain-snow partition are important for water storage – however it wasn't until the Discussion that I could really see why this indicator might actually be used for planning purposes.

- Page 1, Line 23: "components was (replace with: has been) used as a foundation"

- Page 1, Line 27: The use of the word "fate" is a little awkward here, do you mean "phase"?

- Page 1, Line 36: Unclear what you mean by "winter snow levels" here. Do you mean the freezing level in the atmosphere or do you mean increase snow pack (which would be counterintuitive). The jargon of 'winter snow levels" is confusing.

- Page 2, Lines 9-11: The first sentence of this paragraph is not complete. It is unclear what are you incorporating multiple data sources and model outputs into.

- You mention on Page 2, Line 14 that DWR developed a methodology to study historical rain/snow trends at fine spatial resolutions and then on Page 2, Line 16 that the purpose of the note is to provide an updated approach and detail the methods of this indicator. It is unclear to me what in this paper is from the original DWR method and what is the "updated" approach. Is the only difference between the DWR method and the method described in the paper the resolution of the PRISM model data? If the goal of the paper is just to outline the DWR approach – then state that and remove the confusing "updated approach" language. However if the DWR approach is documented elsewhere, and you are documenting changes here in this paper, those differences need to be more explicitly stated.

- Page 2, Line 16: You say “detail the methods of this indicator” but at this point in the paper it is not clear what “indicator” you are talking about. A sentence about the “indicator” before this one is needed. (e.g. Page 6, Line 4 could also be stated here in the text).

- Page 2, Line 19: “and be an important” a modal verb is need before “be” as in “may be” or “can be” etc.

- Page 3, Line 20: . . .the percent of precipitation that falls (as snow??) at elevations above the 0° isotherm . . .

- 2.1 the Study Area Did DWR create this method exclusively for studying trends in rain/snow partitioning, or is this data used in operational forecasts?

- Figure 4 has a number of problems: a). What season is being plotted? Entire water year? Cold season etc? b). You discuss Figure 4b before 4a, they should be flipped in the panel. c). It is unclear from the text how Figure 4b is calculated – what is the IQR and 90%CI based on (is this covering every grid point within that elevation band?). d). Does Figure A represent the values shown in Figure 3 but sorted by elevation?

- Page 5, Line 37 – what is a flood pool? This should be stated in a way that non-flood forecasters/water managers can understand.

- Page 6, Line 4 – this description of the goal of this paper needs to be moved up into the introduction.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2020-122/hess-2020-122-RC2-supplement.pdf

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