Response to Reviewer 2

I thank the reviewer for his / her valuable comments on the manuscript. My response to the comments and the changes to be made in the revised manuscript are detailed below. For clarity, the comments are in blue font, while my response is in black. In some cases, I have included the text planned to appear in the revised manuscript in red font.

The author compares how three contributing factors that influence March historical snow water equivalent trends are represented in two reanalysis products and in 22 CMIP6 models. The factors contributing to total SWE change are changes related to the total precipitation, the fraction of precipitation occurring as snowfall, and the fraction of accumulated snowfall which remains on the ground. The author determines that there is broad agreement in the subcomponents over the long analysis period assessed (1951-2022) but that internal variability and model biases reduce the agreement between historical trend estimates and the CMIP6 multi-model mean over the shorter 1981-2022 period analyzed.

General Comments: Overall this paper is well-written and provides logically argued results. The framework used to attribute SWE changes to changes in precip, snowfall fraction, and snow-on-ground seems to works well and provide reasonable results. I only have a few issues I'd like to see addressed or commented on.

Specific comments:

The snow-on-ground term G absorbs a wide range of processes that will vary substantially from model to model. While inter-model difference in snow melt will be a key component of this term, differences in sublimation and any other structural differences in how a particular model treats snow will also affect it (e.g. vegetation-snow interactions, differences in how well snow water mass is locally conserved in a given model). Thus, it isn't surprising that the models disagree the most on the changes in this term. I think it's worth pointing this out in the text when you first discuss the equations and also at line 270 where you attribute the differences in G primarily to snow melt.

This is an excellent comment. The snow-on-ground fraction is indeed affected by a large number of factors. In response to this comment and related comments from Reviewer 3, the following paragraph will be added to the end of Section 4 in the revised manuscript:

Table 4 also shows that the inter-model differences in G are in relative terms larger than those in P* and F*. This is perhaps unsurprising, since G may be affected by a multitude of factors. As defined by Eq. (1), G reflects the balance between the source (accumulated snowfall) and sinks (snowmelt plus sublimation) of snow. The accumulated snowfall depends on both the amount and phase of precipitation, whereas snowmelt and sublimation are ultimately determined by the amount of energy that the land surface model allocates to them. The latter, in turn, depends on the radiative energy input from the atmosphere, the exchange of sensible and latent heat between the land surface and the atmospheric models, the description of the surface albedo and emissivity, and the use or release of energy associated with temperature changes within the snow-ground-vegetation system. As many of these processes are described differently in different land surface models, it is perhaps unsurprising that the simulated SWE may vary substantially even between land surface models that share the same atmospheric forcing (Mudryk et al., 2015). A more detailed understanding of the causes of variation of G within the CMIP6 ensemble is an important target for future research.

Line 186: I believe you are defining X1=X_clim and X2 = X_clim + dx where dX is the monthly anomaly. Using these definitions x-bar is the monthly mean value plus half the monthly anomaly. If you actually used G_bar = G_clim + 0.5dG, F_bar = F_clim + 0.5dF, P_bar+0.5dP in equation (2) please state this more explicitly.

Yes, this is exactly what was done. I tried to clarify this with the last, bolded sentence in the refined description of Equation (2):

The monthly mean values of X = *SWE*, *G*, *F* and *P* over the whole analysis period are denoted as X_1, whereas their values in an individual winter are denoted as X_2. By further defining $\bar{X} = (X_1 + X_2)/2$ and $\Delta X = X_2 - X_1$, one obtains

$$\Delta SWE(t) = \underbrace{\bar{G}\int_{t_0}^{t} \bar{F}\Delta P dt'}_{\Delta SWE(\Delta P)} + \underbrace{\bar{G}\int_{t_0}^{t} \Delta F \bar{P} dt'}_{\Delta SWE(\Delta F)} + \underbrace{\Delta G \int_{t_0}^{t} \bar{F}\bar{P} dt}_{\Delta SWE(\Delta G)} + \underbrace{\frac{1}{4}\Delta G \int_{t_0}^{t} \Delta F \Delta P dt'}_{\Delta SWE(NL)} \quad (2)$$

Thus, the anomaly in SWE is decomposed to contributions from the total precipitation (ΔP), snowfall fraction (ΔF) and snow-on-ground fraction anomalies (ΔG), plus a non-linear term that is typically two orders of magnitude smaller than the others. **However, there is an implicit non-linearity in the coefficients $\bar{G}$, $\bar{F}$ and $\bar{P}$ in (2) since, for example, $\bar{G} = G_1 + \Delta G/2 \neq G_1$.**

If you assumed the additional ½ dG/dF/dP components of G_bar/F_bar/P_bar would be absorbed into the non-linear term as second-order terms and approximated G_bar = G_clim, F_bar=F_clim, etc, please state this instead.

No, I did not do it this way.

I think the MMM trends you show are probably a good representation of the ensemble average given the range of models. But it would still be good to check that the figures look similar if you plot the median trend value at each location in case there is any undue influence in the mean from outlier trends.

I followed this suggestion. The trend maps are very similar; for example, the Total Snow Area spatial correlation between the CMIP6 mean and median trends in SWE is 0.97 in both the 1951-to-2022 and 1981-to-2022 periods. I chose to not include this analysis in the manuscript but will add the CMIP6 median values to Table 4 about the area mean statistics. These are indeed very similar with the multi-model mean values.

Minor comments:

L541: Please rephrase. I don't think snow melt occurs more efficiently as the climate warms. There's just increased snowmelt in a warmer climate.

"Increased snowmelt" is also a slightly problematic formulation because, as integrated over the whole winter, the total amount of snowmelt is the same as the accumulated snowfall (which may either increase or decrease). Therefore, this will be reformulated as "enhanced snowmelt in a warmer climate".