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Comment

## ***Interactive comment on “Snow and albedo climate change impacts across the United States Northern Great Plains” by S. R. Fassnacht et al.***

### **Anonymous Referee #1**

Received and published: 13 August 2015

This paper was interesting to read, and the content is appropriate for The Cryosphere. However, before I would recommend acceptance for publication, I suggest that some substantial changes be made. The authors are interested in assessing whether there have been changes in precipitation falling as snow and the number of days with snow. They include the impact on the modeled albedo in the objectives, but there isn't really sufficient justification provided. The paper has not been set up to show why there would be an interest in the modeled albedo without tying it back to climate change or regional resource interests. I suggest that the authors start by citing some studies that have looked at climate or temperature changes in the region, and include a short reference to global trends. The connection between snowfall and water storage has been well made. Albedo could be brought in through the fact that changes in albedo contribute to the snow albedo feedback and act as a positive feedback in the climate system.

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Without regional albedo observations during the period of record employed here, a practical albedo model was chosen to estimate the changes that may be associated with the changes in temperature and snowfall.

P3333 – Line 22-23: As stated above, it is not immediately obvious why the modeled albedo is a concern. Up to this point, the authors are discussing effects of climate change, namely trends in temperature, precipitation and specifically snowfall on the size of the winter snowpack, and the possible implications of reductions in the winter snowpack on water availability.

P3336 – Line13: On what was the snowpack albedo refreshment threshold based? Verseghy (1991) does not specify a value of new snow that is required to refresh the snowpack albedo to 0.84. Until CLASS (Canadian Land Surface Scheme) version 3.0, the albedo refreshment threshold was time-step dependent, ( $1.4E-6$  based on the snowfall rate in m/s (of depth, not SWE), which works out to 2.52 mm depth with a 30 minute time step and 1.26 mm depth with a 15 minute time step; see Langlois et al.,2014). The albedo refreshment threshold in CLASS was changed to 5 mm in a time step (see Langlois et al., 2014 who used CLASS 3.5) and was no longer dependent on the length of the time step. In CLASS version 3.6 the albedo refreshment threshold was reduced to 0.1 mm depth in a time step. The authors should not be required to follow the albedo refreshment threshold used in any version of CLASS, but their text implies that Verseghy (1991) is the basis of the value used. In fact, the value employed by the authors is larger than the largest value ever employed in CLASS. I acknowledge that one is based on a model time step and the other on a daily total. There should be some explanation or justification for the source of the threshold employed, and possibly a sensitivity test if the value is uncertain.

P3336 – Line 14: A ‘soil albedo’ of 0.20 is going to remove any spatial variability in albedo that is not snowpack dependent, and will bias the effect of assessed snowpack related trends on albedo, depending on the bias of 0.2 compared with the average snow-free albedo at each location. This should be discussed or acknowledged.

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P3337 – Line 10: I suspect that the variability in winter albedo has been underestimated significantly. While the landscape is prairie, and most vegetation types still present in winter (e.g. crop stubble, dead grass [ignoring trees]), would be buried by snowpacks of say 10-20 cm, there would be variability in how efficiently various species are buried, depending in part on whether they are bent flat or remain upright. I don't see this as a fatal flaw in the paper, but there should be some discussion about the applicability to bare ground and easily buried surfaces (i.e. true regional trends are not being simulated, but rather trends at sites likely represented by weather stations).

P3337 – Lines 18-20: The abstract indicates that a warming would result in less snowfall, but either this is not the case, or stations with cooling have obscured this. Can the authors test this using only stations showing warming?

P3338 – Lines 20-26: It may be difficult to assess some trends without looking at whether spatial differences in actual albedo, changes in albedo, elevation differences and possibly changing weather patterns had any effect. I acknowledge that doing this may be too cumbersome here but the authors may want to add to their discussion of the difficulty. Also, were the instrument histories of the stations assessed to determine whether any instrument types were changed or locations moved slightly. Such actions can have a large effect on the ability to detect trends. A simple statement of the latter will do.

P3339 – Lines 17-25: The two periods of analysis meet relatively close to the well documented change in global temperature trends, which showed cooling from the 1940s – 1970s and warming thereafter. The authors may be seeing a global trend on top of the local or regional trend. This should be mentioned. These trends offer much to explain the changes in days with snow, precipitation as snow and albedo (Figure 5). In broad not necessarily consistent terms, there was more cooling from 1951-1980 and more warming from 1981-2010, and 1951-1980 showed more precipitation as snow, more snow days and a higher albedo.

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P3339 - Lines 26-29: Also, a bare surface with one large snowfall late in a given time period will have a very low average albedo. The ‘days with snow’ appears to be calculated as days with snowfall. If days with snow on the ground has not been calculated, could this be added? It may be more significant as it integrates the effects of snowfall and melting.

P3340 - Lines 10-12: The meaning of ‘stringent’ is not clear to me. Do the authors mean too static or not flexible enough? Was a slower albedo decay and a higher albedo desired? If so, why?

Minor comments and errors: P3332 - Line 10: “There was substantial variability. . . .”

P3332 - Line 16: Did the authors mean to state that “In some locations rates of change are increasing faster than the global average”? A temperature increase can be expressed as a rate of change ( $^{\circ}\text{C}/\text{Time}$ ), or one can state that in a given location, temperatures are increasing faster than the global average. An increase in the rate of change implies the rate of change of the slope or the derivative, but I don’t think this was what the authors intended to express.

P3332 - Line 18 and P3334 – Lines 19-20: I suspect that trends in daily maximum and minimum temperatures would be important for snowpack properties, as these would affect phases changes and metamorphism. I am curious about why average annual minimum and maximum temperatures were used rather than the average maximum and minimum temperatures over the defined winter period of each year. Other indices, such as the number of days with the maximum or minimum temperature below  $0^{\circ}\text{C}$  may also be useful.

P3332 - Line 21: “. . . , a shallower snowpack. . . .”

P3332 - Line 21-22: I agree with this statement as applied to the US northern great plains where there is a seasonal snowpack, but in a colder environment with a longer winter, a warming climate may bring more snowfall. I would qualify the sentence by

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including the region of interest.

P3334 – Line 22-23: This method of calculating snow would overestimate the snow amount. If the number of days with air temperatures just above 0°C (days likely to have mixed or transitional precipitation) shows a trend, then this will bias the fractional snow calculation. (i.e. if the number of days with air temperatures say in the range of 0-5°C increased, then this method will bias the trend in snowfall high, whereas if the number of such days has decreased, it will bias the trend in snowfall low. The authors do a decent job of explaining their rationale for this choice of method so no change in methodology is requested.

P3335 – Line 1: This sentence is awkward. I would reword it as: ‘We did not attempt to correct for snowfall that melted before being measured.’

P3338 – Lines 5-8: This sentence is awkward.

P3338 – Line 26: ‘to understand better’

P3339 – Line 2: ‘... saw significant increases in both Tmax and Tmin’

P3339 - Lines 7-9: Studies have shown that the NGP is experiencing increasing temperatures and decreases in annual snowfall, but this study shows more increases in snowfall. Sample bias?

P3339 – Line 13: ‘... such a decrease...’

Some figures are hard to read, If a figure is wider than a single column, I suggest that it be increased to the usable width on the page within the margins.

Langlois et al. 2014. Evaluation of CLASS 2.7 and 3.5 simulations of snow properties from the Canadian Regional Climate Model (CRCM4) over Québec, Canada. Journal of Hydrometeorology, 15: 1325-1343, DOI: 10.1175/JHM-D-13-055.1.

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