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The Cryosphere
Discussions

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

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Dear Dr. Marsh and the editorial board,

I would like to thank the anonymous reviewers for their insightful comments. Below we have outline how the reviewers' comments have been addressed.

Regards, Steven R. Fassnacht

Anonymous Referee #1 > 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. ..An additional has been made to the abstract to clarify the possible impacts of a changing climate on the snowpack. Further, we have added text in the Introduction to explain why albedo changes and differences are important.

> 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. ..As per the previous comment, several references have been made to work by Qu and Hall (2006) and Painter et al., (2007, 2010) to put albedo into context. Additional comments have been added to the discussion.

> 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. ..Several references have been added to present the snow cover changes and possible albedo feedback changes across both the Northern Hemisphere and the Northern Great Plains area. We feel it is more important to focus on snow-based changes than global climate change.

> 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. 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. ..ok.

> 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. ..A paragraph has been added earlier in the Introduction to explain why we care about snow albedo. We have previously assumed that this was

obvious and we needed to state this early in the paper.

> 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. ..The CLASS model was not used, only the albedo decay model presented by Verseghy (1991). The above was summarized in the text. The text relating to resetting the albedo for fresh snow has been modified to read: “Fresh snow was considered to reset the albedo to 0.84 when at least 2.54 cm of snowfall was observed over a day, since this was the resolution of fresh snow depth measurements (1 inch).”

> 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. ..This is now presented in the discussion: “The constant soil albedo of 0.2 can create problems when snow free conditions persist in the winter months. In the Community Land

Model version 4.0, the albedo of soil is a function of color, wetness, and wavelength, such that it can vary between 0.04 in the visible for saturated dark soil to 0.61 in the near infrared for dry, light soil (Oleson et al., 2013). The identification of soils and vegetation at the 20 stations was not undertaken.”

> 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). ..This is possible and a discussion has been added: “For the Northern Great Plains, the winter albedo of snow free areas will vary over space, but not necessarily over time due to the dormant nature of the vegetation. In the prairie regions, the grasses can be up to 50 cm tall yet will lie down during snow accumulation yielding 10 to 20 cm high vegetation. Thus 10 to 20 cm of snow is required to completely cover such vegetation. However, the dataset used herein are collected at airports and near towns with grass areas that are landscaped, thus the vegetation is only 3 to 5 cm high allow it to be buried much quicker than native, non-landscaped prairie vegetation. Formulations do exist to consider the burial of vegetation by snow (e.g., Wang and Zeng, 2009), but this is beyond the scope of this paper.”

..We have added the NARR albedo dataset which was thought to be a good comparison, but the problem is scaling from the point (station) to spatial (32 km resolution) dataset with imhomogeneities in the latter. Also, the latter uses a simple version of the albedo equation used in this work.

>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? The abstract was changed to state that warming could rather than would result in less snowfall. ..What is observed is actually the opposite; with warming, especially Tmin, precipitation as snow and the number of days with snow both increase.

>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. ..We are not sure what the reviewer is asking here. There are some elevation differences but those are small, at least regionally.

>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. ..As per the detailed comments from reviewer 2, we have added information about the specific stations used, in particular a description of station moves.

>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. ..Good point. This has been added.

>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. ..Not necessarily. It depends on when the snow falls during the day.

>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. ..Throughout the document the phrase "days with snow" was changed to "days with snowfall." Both snowfall and snow on the ground amounts are measured. When there was snow on the ground, days with observed snowfall reset the albedo (see above), using the amount of precipitation of snow distinction defined by Huntington et al. (2004).

>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? ..This should have said "too large," and thus albedo decayed too quickly.

Minor comments and errors: >P3332 - Line 10: "There was substantial variability. . .." ..changed.

>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 (C/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. ..This sentence should have read: "In some locations temperatures rates of change are increasing faster warming much more than the global average, ..."

>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 C may also be useful. ..Good point. However, here we used annual temperatures as they can

be compared to other studies.

>P3332 - Line 21: ". . ., a shallower snowpack. . ." ..Changed.

>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 including the region of interest. .."In non-polar regions" was added, and it was further stated that "an overall warmer climate could yield ..." rather than "would yield ..."

>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. ..Some of the snow could be mixed phase precipitation, but if it was still snow in the morning when the daily measurement was made, it was all considered snow. Conversely if it melted between when it fell and when the measurement was made, it was considered rain. This should not be systematically incorrect; not other data were available.

>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." ..changed

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

>P3338 – Line 26: 'to understand better' .."for further study" was deleted.

>P3339 – Line 2: '. . . saw significant increases in both Tmax and Tmin' ..changed.

>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? .."The results presented herein include an additional 10 years of data and illustrate less consistency in changes to the amount of snowfall."

>P3339 – Line 13: '. . . such a decrease. . .' .."s" deleted in "as"

>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. ..I will work with the copy-editor on this.

Review #2 comments – "Snow and albedo climate change impacts across the United States Northern Great Plains by Fassnacht et al.

> This paper analyzes trends in physical climate variables as well as estimated (derived) albedo for 20 stations across the Northern Great Plains with (nearly) serially complete records for the 60-year period 1951 through 2010. The most robust trends were increases in daily minimum temperature and days with precipitation. Other variables, including albedo, had less consistent trends. ..Trends in albedo were computed at 11 stations, 7 decreasing and 4 increasing.

> While the paper does not represent breakthrough science, it does make a modest contribution to the regional understanding of climate change in the cold regions of the U.S., and eventually should be publishable. However, I think the authors need to be sent back to do more analysis, and improve the presentation. With respect to the latter, I find the figures very hard to follow. While their attempt to include multiple variables and the spatial location of stations on single plots is clever, I also find it nearly impossible to digest – information overload. The way many authors have presented this kind of information that works well is with bubble diagrams, which provides a much better sense of the combination of spatial structure, trend direction, and magnitude. It does require separate plots for each variable. My suggestion is to replace Figures 1-3 with such plots (multi-panel of course). There are various ways of doing this; one is to use the size of the bubble to reflect the strength of the trend, with color (typically red and blue) indicating the trend magnitude, and solid vs open circles for statistically

significant vs not. ..We agree that Figure 1 has much information, but feel that it puts the climatological similarities and differences into spatial perspective. Figure 2 and 3 each show the same information to illustrate the magnitude and direction of change (Figure 2) and the spatial distribution of change (Figure 3).

> My major technical concern is that the paper doesn't investigate the cause for spatial anomalies. The Sterling vs. Kimball comparison is interesting, but the authors don't offer any explanation as to why the trends are so different. My suspicion is that changes in station location, conditions, and/or instrumentation may have played a role. Presumably the 20 stations are in the NCDC Cooperative Observer network. There is a metadata archive for these stations, which the authors should review carefully. It is not necessarily the case that stations have been in the same location even if the station number hasn't changed. I have seen presentations by Kelly Redmond that have highlighted horrors in these station records where the station has moved, but the same station ID was retained (his examples are in the West, where station moves often mean changes in elevation, and hence spurious temperature trends – that won't be so much the case in the Great Plains, but other local factors may well be responsible for some of the apparent trends). In the case of precipitation, and the snow part in particular, minor changes in station location can easily change wind patterns, and hence snow undercatch, and even if the station location is unchanged, construction of buildings, growing or removal of trees, and so on can have a major effect. Also, there is the issue of time of observation. Nothing is said in the paper about observation time, which has changed in many cases and can introduce spurious trends. NCDC has a time of observation file for all of these stations (the authors may want to talk to Pasha Groisman who is an expert on these matters). ..We have explored the metadata for all 20 stations. These stations were selected since they had a long continuous record from 1951 through 2010 with few missing years of data. The metadata for all stations was explored and the distance that stations were moved has been summarized. This is also presented in the discussion. It should also be noted that the Northern Great Plains are relatively flat and thus any change in elevation from a station move is negligible. For

example, the Goodland station was moved about 500 meters, but it remained at the airport so the change in elevation could be assumed to be only in the order of meters.

> Conspicuously missing from the methods section is any discussion of how the observations were taken. In part, this should include a summary of gauge type and time of observation (and any changes therein), but also how solid precipitation was recorded. Many (perhaps all) of these stations are manually observed once per day, and with snow in particular, how is this collected? Is the presence of snow via manual observation of snow depth, which then is annotated and the total (liquid) precipitation total ascribed to snow rather than rain over the previous 24 hours? And whatever the protocol is, has it remained the same over the 60-year period? ..The presence of snow was determined as fresh snowfall. This was used to reset the albedo of snow. The presence of snow on the ground was used to confine albedo to snow-based or soil.

> Again, I think a discussion with Pasha Groisman would be worthwhile. Incidentally, I am surprised that I don't see any reference to his work on similar topics. ..Good point. We have added citations to Groisman talking about station moves and their implications.

> I also have to wonder why the authors didn't use stations in the Hydroclimatic Network (HCN), for which some quality control has been done. Some of these stations no doubt are in HCN, but others may well not be, and if so why not? Were they not included (in HCN) because of quality control issues. ..Nine of the 20 stations are part of the US HCN. The metadata for all stations was explored and the distance that stations were moved has been summarized. This is also presented in the discussion.

> Finally, although not conclusive, changes in snow albedo, if present, would be important. The authors note the limitations of the USACE decay algorithm, and I understand that there is no viable alternative over such a long period. Given that the computed albedos are a function of several measured variables, I think the authors should analyze trends in the contributing variables, and then show which trends are most respon-

sible for the observed trends (this is different from the partitioning of variance, which they do discuss). It may also be that in the case of stations with no significant trend, this is resulting from cancellation of trends in the driving variables, and this should be noted as well. ..This has been added to the discussion.

Interactive comment on The Cryosphere Discuss., 9, 3331, 2015.