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Comment

Interactive comment on “Trends of solar ultraviolet irradiance at Barrow, Alaska, and the effect of measurement uncertainties on trend detection” by G. Bernhard

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The paper is well-written and the statistics and uncertainty analysis appear to be very thorough considering the need to address data gaps etc.

My interest/concern relates to the analysis of "snow-in" date in autumn at Barrow. I would agree that this is an important factor and the correlation with UV in Oct. is significant and supports well the hypothesis put forth by the author. This is an important finding and has implications for a number of biogeochemical cycles beyond only UV irradiance.

I suggest inclusion of another analysis and revision of Fig. 6. Actually cross-correlate
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the UV parameters with the date of snow onset and plot this on Fig. 6 to show the anti-correlation more clearly. This will bolster the conclusions graphically and you can present the correlation coefficients as well.

It should also be pointed out that the period of record discussed is rather short and such trends are not representative of the longer term and have not, nor cannot be sustained. Caveats should be included in the conclusions or discussion of this phenomenon. Such a trend, 13.6+/-9.7 days per decade, cannot be sustained, nor is it historically sustained in the longer time series.

Fig. 6 begins in 1992 and ends in 2009, with 2006 missing. In the longer time series you find this choice might result in "end-point" issues when performing the regression because 1992 was the second earliest onset and 2009 the latest in the time series under consideration. I have gauged this against the 1974-2010 record I have at my disposal, although my analysis is preliminary and unpublished. Further, I understand why 2006 was omitted due to ambiguity. It should be stated why somewhere. A more sophisticated algorithm is probably required to determine "onset" for years when the signal is ambiguous; that is, albedo exceeds 60% for a day or more but then the snow melts and it drops to < 60%. Refinement of the criteria for these ambiguous years I realize is beyond the scope of this study, however. The interested reader can access yearly time series of daily average albedo at Barrow at: <http://cmdl1.cmdl.noaa.gov:8000/www/albedo/>

that will illustrate the value and also pitfalls of determining onset of snow definitively.

I have verified independently the trend of onset date stated in the paper for the same period of time. I also have performed an analysis (unpublished) of the 1974-2010 time series and find the following: the overall "trend" is 2.7 (+/-3.3) d/dec, and broken at 1991. I find a distinct negative trend from '74-'91 and positive trend from '91-2010 of -6.7(+/-9.0) and +10.3(+/-7.6), respectively; noting that my value for the recent decades is lower than that reported by the author (by 3 d/dec) but I also include my best estimate

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for 2006 and 2010. This illustrates the issue of endpoint influence and selection of a rather short time series for this sort of analysis, although it does not negate the value of correlation with the UV parameters in any way in my opinion.

My concern is that caveats be made pertaining to the record of snow onset at Barrow (as presented), with recognition the trends are not sustainable and past records suggest a more cyclical behavior. In turn, it might be speculated that these variations occur in response to dynamical forcing due to changes in both atmospheric circulation and also the distribution of sea ice in the Beaufort Sea at the time of snow onset. If the sea remains open as is the case for large retreat in recent years, the snow may come later than earlier??

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26617, 2011.

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