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Geoscientific
Model Development
Discussions

Interactive comment on "The NASA Eulerian Snow on Sea Ice Model (NESOSIM): Initial model development and analysis" *by* Alek A. Petty et al.

Anonymous Referee #2

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This clearly written manuscripts provides a detailed description and exploration of the new NESOSIM snow model. NESOSIM produces gridded, daily snow thicknesses and densities for Arctic ocean sea ice during the accumulation season (defined as mid-August through April) given daily inputs of Arctic wide snowfall, sea ice concentration, ice drift, and near surface winds. Although the Arctic melt season may extend well into September, the model does not include thermodynamic or radiative processes, and this certainly limits its utility. Rather, the emphasis here is on the impacts of wind via wind packing and blowing snow loss to leads/open water. The parametrizations are fairly simple – winds exceeding a threshold can only decrease snow thickness and increase snow density. There are no snow drifts, for example, or sub-grid regions of bare ice which are present in other models. In addition, there are no snow-aging

processes that may contribute to density changes. Still, the authors do a commendable job validating their model against observations and do a thorough evaluation of model sensitivity to the various snowfall reanalysis, ice drift and ice concentration products. This latter analysis highlights the true utility of the model – a simple framework for the inter-comparison of reanalysis-derived snow on sea ice data products.

Some specific scientific comments: The authors need to better place the work in scientific context and show how the work is unique. How is this an improvement over the simple models of snow depth forced from reanalyses? There are more complex snow on sea ice models (Lecomte-LIM, Liston-SnowModel, Hunke-CICE) which include some of the same processes (ice drift, dynamics, precipitation) yet rather than develop wind loss and compaction include some distinctly different processes (thermodynamics, radiative properties, snow ice formation, dune formation, ridge accumulation…). Are these models missing the "key sources and sinks"? There is also Dery and Tremblay (2004, JPO) that specifically looks at the effects of wind redistribution with an explicit mass flux into leads. Is your approach better? More useful? Consistent?

What is the impact of excluding thermodynamic processes on your results? Does this change your conclusions about the impact of wind processes?

Some misleading statements: First sentence of the abstract. … "produces daily estimates of depth and density of snow across the polar oceans". Not yet because of some important missing processes. Qualify with Arctic only and during the accumulation season. Using old vs new snow in the text and figures. It's clear that there is intention to one day include snow aging, but for now there is only fresh vs compaction. The depth hoar densities of 150-250 is never used in the model even though paragraph 10 seems to suggest that it is. The old snow value is 350 kg/m3 which is not the average of the higher end of wind slab and depth hoar (325 kg/m3) but rather the average of the wind slab bounds.

Perhaps future developments could be kept to a specific section to better clarify what the model does and doesn't do.

Snow density in NEOSIM is bounded by the two chosen snow density parameters (200 and 350 kg /m3) even though the observations referenced give values for dry snow of 150 and wind slab ~400 kg /m3 on average. Why exclude these possibilities at the outset? Instead of using an average value, doesn't it make more sense to use the upper and lower bounds given the nature of the parameterization? How sensitive is the model to these values?

The late summer initial conditions integrate all the missing snow melt processes and for that reason, they are rather important. The paragraph on page 14 does a fairly good job motivating your approach, but it would be clearer if you showed the equations for hs(0) and hs(1) after summer melt. Also better explain how snowfall events factor into this parameterization and explain why keeping the same fraction for fresh/compacted snow is the right approach (or clarify if you do something different). It would also be informative to see the Aug 15 values in your figures 3 and 4. Are there Aug observations to help validate the IC parameterization and fig 2 in particular?

Why absorb the timestep in the model equations? In (2) the parameter alpha has a timestep dependence that isn't explicitly called out and as a result, 0.05 is less meaningful. Better to define an alpha with units of per second.

Are the differences between simulations with different snowfall estimates larger than the differences between time periods? Are the time period differences significant?

Fig 14 seems to suggest that ice drift is actually quite important but masked by basin or large regional averaging. Magnitudes of the differences are similar to the snowfall sensitivity. Impacts are near the ice edge (increase ice retreat?) and add to smaller (but still > 100 km) scale variability (potentially impacting melt-pond formation).

Technical corrections:

Table1. add the model variable in the table.

Define U in Eq. (5).

Missing) in Eq. (6)

Why is the bs term in Eq. (7) positive?

Missing t dependence in some terms in Eq. (7) and the next equation Line 26 missing "of" in "one the better.."

Add W99 to upper panels of fig3

Add W99 to fig 6 (a)

In explanation for fig 8, NA region shows a small "decrease" due to convergence, not increase. CA shows a small increase but is not mentioned.

Fig (9). What is (b) Ocean? Change (d) wind/leads to wind loss to leads. Is there an ice area cutoff for snow depth in (k)?

Table 4 could be improved by adding the 1981-1991 time period for comparison, identifying the boxed regions as "reanalysis sensitivity", "ice drift sensitivity", "ice concentration sensitivity" and including in the description that the default configuration is MEDIAN-SF, NSIDCv3 ice drift and Bootstrap ice concentration

End of p 30. Comment on the NA region in table 4 when ice drift is included. Seems to be important here too.

Fig 13, (d) NA does not appear to be consistent with table 4. The NODRIFT value on May 1 is around 34 cm in table 4 but seems to be much higher in this figure. Explain.

Last comment before 5.3 does not seem correct. There does appear to be bias in the real-time products with respect to the peripheral seas .

P 37. What are uncertainties in the OIB observations?

Mention the results from sensitivity of ice concentration in the summary. These were interesting and significant.

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