Comments on "Arctic sea ice drift-strength feedback modelled by NEMO-LIM3.6" by Docquier et al.

1. Summary

The authors conducted ocean-sea ice model NEMO-LIM3.6 numerical experiments to investigate interaction and/or feedback mechanisms between sea ice drift speed and ice strength, focusing on the Arctic Ocean. As measures of ice strength, the authors employ ice concentration and thickness, and then examined relation between ice drift speed and them. In order to assess the model performance, sea ice observation data derived from satellite and ice-tethered buoys (ice concentration, ice drift speed and ice thickness) were exploited. The authors introduced a systematic model validation method based on sea ice diagnostics (ice extent, ice concentration, ice thickness and ice drift speed) as well as process-based diagnostics (relation between different ice properties, e.g., ice drift speed and concentration). They also introduced metrics which quantify modeled or observed ice dynamics as scalar variables. Using these diagnostic and metric approaches, the author assessed model performance, and then conducted sensitivity experiments varying ice strength parameter to see the effect of ice strength change on the relation between drift speed and ice concentration (or ice thickness) simulated in the model.

2. General comments

The authors provided a good review regarding relation between ice drift speed and ice concentration (and thickness) in the Arctic Ocean, mainly focusing on observational studies. The diagnostics and metrics used to validate model performance and the thorough assessments on the simulated ice properties are very helpful to assess the capability and limitation of the model, although I have an additional suggestion regarding ice thickness evaluation (see comments below). The results of the sensitivity experiment, in which ice strength parameterization is changed, are interesting to me (and probably informative to many sea ice modellers), while the whole strategy used to assess the feedback between ice drift and other ice properties seems to me not always suitable nor convincing for the purpose of the study. As far as I know, "feedback" is a term originally introduced to describe an electric circuit which has a recursive input, and is widely used in other study area to explain similar concept. Also in geo-scientific studies, the term "feedback" is widely used to explain a recursive interaction between different processes or subsequent chain of phenomena, sometimes without clear definition nor quantification. Although the authors invoked the term "feedback", the strategy does not seems to be designed so as to extract a feedback mechanism from a complicated system. For this reason, I would recommend to change the focus of the study to more specific issues (e.g., sensitivity of ice strength parameterization to ice drift, concentration and thickness) or to reorganize experiment design so as to quantify the feedback between them. Even without feedback issue, the manuscript contains interesting model results.

3. Major point

- The strategy used to extract (and quantify) the drift-strength feedback is not always suitable nor convincing. I think more consideration is necessary about how to quantify a "feedback". Since ice drift, concentration and thickness are described by a set of simultaneous partial differential equations in numerical models, it is a matter of course that there is some sort of 'feedback' between them. An important point is how to extract and quantify a feedback in a simple formula so as to abstract the essence of the complicated system. Mathematically, a feedback between two variables (most simple case) can be described by a set of equations,

$$\frac{\mathrm{d}\overline{|V|}}{\mathrm{dt}} = F(\overline{T}) \quad (1)$$

$$\frac{\mathrm{d}\overline{T}}{\mathrm{dt}} = G(\overline{|V|}) \quad (2)$$

where $\overline{|V|}$ and $\overline{T}$ are respectively mean ice drift velocity and ice strength (or thickness) in the present case (e.g., $\overline{|V|} = \frac{1}{TS}\iint |V| dxdydt$, *T*: one month or one year period, *S*: SCICEX box). *F* and *G* are the functions describing a feedback. The equations mean that the temporal evolution of mean ice drift is controlled (or affected) by mean ice strength, while at the same time, the evolution of ice strength is also controlled by drift speed. The most simple solution is an exponential formula, $Ce^{\alpha t}$, and then temporal evolution of $\overline{|V|}$ and $\overline{T}$ depends on α . The system has a positive feedback if α > 0, while a negative feedback if α < 0. If the authors intend to clarify and quantify the feedback mechanism between ice drift and thickness, a definite formula (not necessarily means complicated formula) in such a simple theoretical framework should be provided. Otherwise, we cannot learn anything about the quantitative features of the feedback between ice drift and thickness (or concentration) at all. Note that the system also needs a higher order stabilizing term to prevent exponential growth of a positive feedback. (At least discussion for such a damping mechanism is necessary)

- The design of the sensitivity experiment seems to me not suitable for examining the effect of ice strength change on ice drift, ice thickness and concentration. Since the authors change the ice strength, *P*, by changing exponent of ice thickness *h*, the associated change of ice strength has an opposite sign between *h* > 1 and *h* < 1. This is easily confirmed by calculating *P* value for different λ used in the sensitivity experiment: if *h* > 1, $P_{\lambda=2}$ is larger than $P_{\lambda=1}$, while $P_{\lambda=2}$ is smaller than $P_{\lambda=1}$ if *h* < 1 (this is also confirmed by closely looking Fig. 2). Due to this experiment design, we cannot directly relate the change of parameter λ to the change of ice strength and therefore, to changes of the ice thickness and ice concentration, which makes interpretation of the results complicated and difficult (There are many mis-interpretation in sec. 3.3 due to this fact, see the specific points below). Generally speaking, changing exponent of *h* in the ice strength equation is not equivalent to changing ice strength, but to changing sensitivity of ice strength to *h*. My recommendation is to use more simple formula for the sensitivity experiment (e.g., $P = P^* \lambda h \exp[-C(1 - A)]$, which is equivalent to change *P**), or to redo analyses based on the actual ice strength *P* used in the model (i.e., calculate $\overline{P} = \frac{1}{TS}\iint P(x, y, t) dxdydt$ and examine relation between $\overline{P}$ and modeled ice properties; ice drift, ice concentration and thickness).

- I admire the systematic approach used in this study to assess the model performance using observational data, whereas I have a concern about the use of ice drift and thickness data derived from PIOMAS, instead of direct observations. The authors did not utilize two important dataset for ice drift and thickness, which have sufficient spatial and temporal coverage for the present study. One is ice drift data provided from Colorado University group (Tschudi et al., 2016), the other is the long-term ice thickness estimate by Lindsay and Schweiger (2016). Both estimates provide error of the estimates as well. Since these data were derived from in-situ and satellite measurements while PIOMAS did not assimilate ice drift and thickness, I recommend to use these two data instead of PIOMAS simulation. Note that the bias (or error) of ice drift field in Tschudi et al. (2016) reported by Szanyi et al. (2016) is a crucial issue, only if the data is used for divergence/convergence calculation. Since the divergence/convergence features reported by Szanyi et al. (2016) always appear as a divergence/convergence pair around buoy-data merged location, a spatial averaging (e.g., SCICEX box) can eliminate the error.

4. Specific points / additional comments

- Page 2, line 1-8: I would suggest the author to use the term 'feedback' more carefully, particularly when mentioning 'positive feedback'. Since a positive feedback has an exponential growth feature by its definition, it always needs damping or stabilizing mechanism in higher order, when the concept is used to explain things occurring in the nature.

- Page 2, line 9-22: I appreciate the good summary of the former studies here, which is useful to survey the current status of our understanding on this issue. On the other hand, I am a little bit skeptical to provide conceptual illustration like Fig. 1, since such an illustration sometimes goes out of authors' control if once published, even if each arrow in the figure is not really examined.

- Page 3, line 29-30: Why the authors conducted the sensitivity experiment by changing exponent of *h* in the ice strength equation, instead of changing *P** or *C*? If the authors intend to increase ice strength in the entire *h* range, changing *P** seems to be more suitable approach (as many modellers do). I request more explanation.

- Page 4, line 1-7: Since we cannot directly relate the increase of λ to increase of ice strength in the present experiment design, more careful explanation is needed. For example, ice thickness during the summer season in the SCICEX box may thinner than 1 m, at least part of the area. In this case, an increase of λ leads to decrease of ice strength.

- Page 4, line 26-34: Why the authors did apply ice drift data provided from PIOMAS instead of satellite- and buoy-based data as a long-term ice drift observation? Polar Pathfinder Daily 25 km EASE-Grid Sea Ice Motion Vectors (Tschudi et al., 2016, now version 3 is available via NSIDC website) provides Arctic-wide long time series from 1979 to present. Although PIOMAS reasonably reproduced sea ice extent and ice concentration field as a result of ice concentration assimilation, there is no reason to believe that ice drift and thickness from PIOMAS are consistent with observation, since these variables are not assimilated. I think the ice drift data from PIOMAS should be dealt with a great care (Since PIOMAS applies strong constraint on ice concentration by an optimal interpolation, the simulated ice field never breaks down, even if ice drift is totally unrealistic). Use of Polar Pathfinder data is much more reliable approach, since the uncertainty of the estimates are also provided. The problem of the Polar Pathfinder data reported by Szanyi et al. (2016) is not a critical issue for the present application (see my major point as well).

- Page 5, line 20-24: For long-term ice thickness estimates in the Arctic Ocean, I strongly recommend to use the estimate presented in Lindsay and Schweiger [2015]. They provided an empirical function describing the spatially and temporally varying ice thickness field over the Arctic Ocean, by exploiting all available in-situ and satellite measurements of ice thickness. As far as I know this is the most reliable long-term estimate of ice thickness field based on measurement for the time being (One can calculate seasonally varying long-term ice thickness field by the description in Lindsay and Schweiger).

- Page 6, line 1-4: How did the authors define the daily ice drift in Eq. (2)? $u_d = \frac{1}{T_{day}} \int_{t_1}^{t_2} v(t)\,dt$ (i.e., temporal average of instant velocity in x and y direction) or $u_d = \frac{|x_{t_2} - x_{t_1}|}{T_{day}}$ (zonal or meridional displacement for 24 hours)? The definition of satellite- or buoy-derived ice drift is the latter. I don't think the difference between the two definition is large, but it would be nice if the authors clarify their definition for comparison with other model results.

- Page 7, line 27-30, Page 8, line 1-5: I think the comparison with PIOMAS thickness data provides useful insights, while I am skeptical to regard PIOMAS thickness data as substitution for observation, since we cannot distinguish bias or error of the estimates, which are always provided for observational data.

- Page 8, line 1: Do the authors have an explanation why the peak shifts?

- Page 8, line 23-24: How the IABP ice drift data were processed to calculate spatial (and temporal) average over the SCICEX box? Since the spatial coverage of the buoy data is not sufficient, one needs to interpolate/extrapolate the data. How did the author define influential radius of the buoy data? How much uncertainty should we expect from the interpolation/extrapolation process? Since the IABP data averaged over the SCICEX box is an important measure to validate the model, a description is necessary.

- Page 9, line 2-3: Why the authors show the relationships in terms of mean seasonal cycle, not by scatter plots based on the relations in each month? I mean, for example, a scatter plot for drift-concentration relation in each month can more clearly show the validity of the regression line.

Page 9, line 16-17: Why the authors did not apply normalization by wind stress? Since the wind stress may differ between each month, it is difficult to derive a general relation between drift speed and other variables. If there is a reasonable explanation for not to apply normalization, please describe in the text.

Page 9, line 30-31: I think the use of 'feedback' in this sentence is not appropriate. The result in this section (sec. 3.2) shows that there is a (linear) relationship between drift speed and strength (thickness or concentration) as equilibrium states (A feedback system does not reach an equilibrium state unless $\alpha = 0$, or having oscillating solution. see my major point). I don't mean the analysis in this section is meaningless, but is not appropriate to show the existence of feedback (see also major point).

Page 10, line 9-20: There are a number of incorrect sentences here. Please keep in mind that higher λ does not directly correspond to larger ice strength, due to the current formulation, Eq. (1). Particularly, it is not true, when discussing relation between ice drift and thickness (or concentration) in summer season. The thickness may thinner than 1 m at least part of the SCICEX box.

Page 10, line 16-20: I think this interpretation is wrong, probably due to the fact which I described in major point. Since the ice thickness during summer season is close to (or even smaller than 1 m), the ice strength for larger λ becomes smaller than that for smaller λ. Therefore, the larger drift speed for larger λ can be simply the result of smaller ice strength.

Page 10, line 32-33: I would say the result is not counter-intuitive. Note that increase of λ leads to smaller ice strength in $h < 1$ area, it means ice can be easily deformed or compressed in $h < 1$ area, compared to small λ.

Page 10, line 4 - page 11, line 8: Section 3.3 needs additional figure showing the relation between λ and mean ice strength in SCICEX box (a seasonal cycle for each λ should be shown), otherwise, we cannot relate ice strength with ice thickness (and concentration) nor examine the relation between ice strength and ice drift.

Page 10, line 33 - page 11 line 6: I think the reason for the increase of modal thickness for larger λ (Fig. 13) can be simply explained. The experiment with increased λ has larger ice strength for $h > 1$ (winter season), which prevents ice thickening due to ridging, while it has smaller ice strength for $h < 1$ (summer season), which enhance ice thickening due to ridging. As a result, larger λ leads to larger peak at modal thickness.

Page 11, line 14-15: I would say that this study also did not 'quantify' the magnitude of the drift-strength feedback. To quantify a feedback, one should present growth rate of the feedback.

Page 11, line 23-24: Due to the analyses without normalization by wind stress, it is difficult to distinguish the reason for the hysteresis loop shown in Fig. 9b and Fig. 11b. Can the hysteresis loop be observed if the ice drift speed is normalized by wind stress?

Page 12, line 14 - page 13, line 15: As pointed in major point, I think the basic strategy for the sensitivity experiment and analyses are not suitable for quantifying 'feedback'. If the authors intend to quantify 'feedback', the entire framework should be reconsidered..

Page 14, line 16-18: Why such hysteresis loops are observed in the modeled sea ice? Since observation (IABP) does not show such a feature, I guess this is an artifact coming from insufficient modeled physics..

Page 14, line 21-28: The summary provided here should be reconsidered, by taking the second paragraph of the major point into account. Although the authors generally provided descriptive result of their analyses on ice drift - thickness relations, they did not show any results on thermodynamic analyses. Therefore I cannot follow nor rely on the arguments associated with thermodynamic effects..

5. Reference

Lindsay, R. and A. Schweiger, 2015: Arctic sea ice thickness loss determined using subsurface, aircraft, and satellite observations, The Cryosphere, 9, 269-283, doi:10.5194/tc-9-269-2015.

Szanyi, S., J. V. Lukovich, D. G. Barber, G. Haller, 2016: Persistent artifacts in the NSIDC ice motion data set and their implications for analysis, Geophys. Res. Lett., pp. 10800-10807, doi:10.1002/2016GL069799.

Tschudi, M., C. Fowler, J. Maslanik, J. S. Stewart, and W. Meier, 2016: Polar Pathfinder Daily 25 km EASE-Grid Sea Ice Motion Vectors. Version 3. Boulder, Colorado, USA. (https://nsidc.org/data/docs/daac/nsidc0116_icemotion.gd.html)