Review on:
"An analytical model for wind-driven Arctic summer sea ice drift"

1 Summary

The authors derive a reduced expression for the sea ice momentum balance in the mixed layer for weak ice coverage, which also accounts for the Ekman layer. Given data for the stress velocities and under the assumption of strong ice coverage, a set of equations is derived, that enables an efficient way to determine the sea ice velocity field. This model is evaluated for the case of near 100% ice cover, against data from an ice-tethered profiler. Compared to observations, the model results represent the considered dependencies in the right manner.

In a study to identify the impact of southerly intensification events on the ice coverage, the model reveals, that the Ekman layer is an important contributor in the reduction of the sea ice concentration due to strong southerly wind events.

2 Notes on the article

The presented work provides an efficient approach to determine the impact of wind stresses on the ice drift, in terms of considering tendencies. Particularly, when it comes to evaluate observational data, it provides an important tool.

The section on the evaluation presents the abilities and limitations of the analytical model. This is a necessary part of the article, as presumptions in the derivation of the model equations and in the evaluated data differ. This is the crucial drawbacks of the present study and causes confusions (though the general structure of the article is clear) when reading the article. Limitations and applicability could thus be more clearly pronounced, see also below.

It is also of interest and importance to show the differences to and similarities with former approaches. This has been done in Section 2.3.

The abstract contains too much information. It should address the main results and aspects of the article. I suggest to shorten it.
For instance, you start to explain, that a winter period is not suitable for comparison, but that your model works fine (to my mind, this itself should not be part of an abstract). In the article itself however, you tell, that the considered time period was somewhat exceptional (p.3 l.19ff), which is a different message than what you write in the abstract. I would skip it here. For instance, I would reduce the second paragraph to something like:

Compared to recent observational data from the first ice-tethered profiler (ITP-V), the model is able to capture the dependencies of the ice speed and wind/ice/ ocean turning angles on the wind speed. The model is used to derive responses

to intensified southerlies on sea ice concentration; the results compare closely with satellite observations.

In the upcoming, I would like to announce parts of the article, which should be modified:

- I was confused about the fact, that on the one hand $\varphi < 1$ has been used in the derivation of the simplified set of equations, but on the other hand $\varphi \sim 1$ has been used throughout the article, whenever the model was used.
  I would reorder the results (maybe even as first part in the evaluation section), in order to more clearly pronounce the impact of changes in the ice cover on the ice speed and velocity angles.

- I wonder about the differences in the solutions in Section 2.2.1 ($\varphi \sim 1$) and Section 2.2.2 ($\varphi \ll 1$). This also relates to the effect of neglecting internal stresses in the momentum balance on the solution. I would add some notes corresponding to that in the summary and outlook section.

- Further, I do not get a clear statement, when considering the graphics and reading the appropriate texts. On the one hand, $K^* = 0.1$ leads to improved solution compared to the observations, which is misleading as rheology has been neglected (and we consider winter data). Then you state that the IOBL enhances the model results, as $K^* = 0.028$ and $K^* = \infty$ lead to different results. How do you deduce that?
  Maybe I missed it, but when you start the evaluation section, you could clearly state, that you are interested in representing the trends in the dependencies right (which you can), as you are currently not able to mathematically represent the case $\varphi \ll 1$ in a better way.

- p.11 eq (20,21): I had difficulties in deriving (20) and the first equality in (21).

- p.11 l.17ff: You argue for the case $\phi < 1$, but in the caption of Fig. 2 you write, that the plot is derived from eq. (20), which is based on $\phi \sim 1$. Thus, you can not use Fig. 2 as argument for $\phi < 1$.

- p.18 l.11: I am not convinced about the best available estimates for the parameters. You use $K^* = 0.028$ as this is the parameter which stems from observations, but your evaluation shows that $K^* = 0.1$ fits better to the observations in your case. Similar applies for C_{io}.

3 Typos and minor issues

There are multiple typos and some incomplete sentences within the text. Please, look through the text carefully and correct. Some typos are listed below. Furthermore, I added some suggestions for an easier understanding of the content.

- Throughout the article, lots of articles are missing, such as: always before "Rossby similarity theory", often before "internal stress", "sea surface tilt", etc.
- Look for "fee drift" and change to "free drift" (I found it two times).
- Change "internal (ice) stress" to "internal (ice) stresses" (singular → plural).
- Use adverbs to characterize a verb, e.g. p.3 l.10 (fully coupled), caption in Fig. 2: "nominally enhanced", p.16 l.20: "individually observed"
- p.3. l.11: You do not exploit the *efficiency* (I do not see any comparison to a fully coupled model there). Instead, you exploit the performance/quality of the analytical model. Please, rephrase.
- p.3, l.21: No full stop in "... IOBL was small compared ..."
- p.3, l.22: "Consequently, our model largely captures..."
- p.5 l.3: "time scale of one week"
- p.6 l.2: "$\vec{u}_0^*$ is the stress velocity"
- p.7 l.8f: In order to start with the original intention and finish the sentence with the resulting task, I would rephrase the sentence like
  In order to derive a solution for the ice velocity $\vec{u}_i$ we now solve the previously derived equations (8) and (9) for the stress velocities $\vec{u}_{io}$ and $\vec{u}_o$.
  But it is up to you. (For me it is easier to get the message in that way.)
- p.7 l.13-15: In Section 4 you do not show, that your approach provides a close approximation to the *general* solution for ice concentrations greater than 50%.
  It might be better to formulate, that *there are* regimes where the model provides a close approximation to the solution for ice concentrations greater than 50%.
- p.7 l.17: What does it mean: "... that the solution be given in full"?
- p.8 l.13: For an easier reading you may insert "due to (16)" in "Note that the right hand side of (11).."
- p.9 l.1: $|\vec{u}_{ai}^*|$ is the wind *speed*
- p.9 l.5: "for which it would be...". You could also skip the part of the sentence after "100%". There is no new information.
- p.9 l.6f: I would rephrase to emphasize that you aim to solve this case numerically. For instance by "As there is no closed-form of the analytical solution to the model equations in general, we determine a solution numerically."

- p.9 l.26: upper case: "section". Check for consistency with other Section references, e.g. p.9 l.8, l.12, where you use the abbreviation "Sec."
- p.12 l.1: one times "a larger" is sufficient.
- In the beginning of this section I would mention the time interval you are intending to consider, otherwise the reader could get lost.
- p.15 l.1: "therefore"? I think, setting $h_i = 1.5m$ is motivated by the observations. You could just skip the word and add something like "in agreement with the observations".
- p.15 l.27ff: I see a linear relation only in the range between 4 - 11 m/s.
- p.15 l.20: delete "a" before "constant"
- p.16 l.17: swap $K_o^* = 0.1$ and $K_o^* = 0.028$
- p.16 l.21: "factor of 10"
- p.17 l17: "ITP-V data are not suitable"
- p.18 l.18: change 60° to 50°
- p.18 l.4: This is confusing to me: You wrote on p.17 l.14, that the ITP-V data are not suitable for a comparison, but then you mention, that they compare well with the analytical solution. In Fig. 5 you use the data of ITP-V.
  Maybe you could pronounce it in p.17 l.12 that way: "As the shallowest ITP-V data are at 7m depth, we use the solution of the Ekman spiral and compare our theoretical results in that depth against those data."
- p.18f l.29ff: When it is surprising, why do you immediately come up with an answer? Better reformulate.
- p.19 l.24: "velocity typically is large compared to"
- p.20 l.22: "These" change to "The" or "The identified". (In the sentence before you treat a different topic).
- p.20ff l.23ff: Why do you introduce a new variable (I_c) for sea ice concentration and do not apply the variable (φ) you used throughout the article?
- p.20 l.24f: dt is not part of the equation, which you refer to in the first part of the sentence. I would make two sentences out of the one.
- p.20 l.26: "climatology mean dI_c from the daily dI_c". Something is wrong here.
- Section 5.2: you explain: SIC decreases by 7-8%, but in the Figure you use a colorbar range of ±6.5%.

- p.21 l.17: You use the word "possibility". If you want to indicate, that the content of the sentence before, in l.16f, is something you think that might be the reason (for sth.), then I would say it in the sentence in l.16 (e.g. "We suggest, that ...")
- p.22 l.13: Please, finish the formerly started sentence in right grammar.
- p.23 l.17: "a quadratic drag law. "
- p.23 l.22: You wrote: "This makes the model straightforward to interpret". What should be interpreted? Something else than the model or the model itself? If you mean the latter, use passive; else add the object!
- Fig. 2. You could add "for $\varphi \sim 1$".
- Fig. 5: inconsistency between x axis label and caption, see also p.17 l.26
- Fig. 6: inconsistency: H is used in the plots, while h_i is used in the text.
- You do not comment on Fig. 6c, if you do not need this graph, delete it.
- Fig. 9: l.13f: This is no sentence (verb is missing).