Final author response for the manuscript tc-2015-233 submitted on 24 Dec 2015 with the title:

“Sea ice diffusion in the Arctic ice pack: a comparison between observed buoy trajectories and the neXtSIM and TOPAZ-CICE sea ice models”

by Pierre Rampal, Sylvain Bouillon, Jon Bergh, and Einar Ólason

**Anonymous Referee #1**

Received and published: 25 February 2016

Dear referee,

First of all, we would like to thank you for your in-depth review, insightful comments and suggestions, which greatly helped us improving our manuscript. Like all the other reviewers you agreed on the interest of the sea ice diffusion analysis and suggested to emphasis that part of our work in the revised version, while reducing the weight given to the inter-comparison of the two sea ice models. We agree with this point and, therefore, largely restructured the manuscript following your recommendations and those of the other reviewers. Our manuscript is now entitled: “Arctic sea ice diffusion from observed and simulated Lagrangian trajectories”. In addition to the restructuring, we tried to answer your questions and comments as carefully as possible.

Below, you will find your original **comments in bold**, our answers in red and the **added text** to our manuscript in **bold red**.

**Please note that our new manuscript containing all the changes we made is attached as a supplementary material to the present document.**

**The manuscript describes a sea-ice drift field analysis of two different sea ice model in the context of buoy drift data. The paper is clearly written (with a few smaller problems that I marked in the annotated PDF, e.g., sometimes the language is a little sloppy and has a colloquial tone not appropriate for a scientific text), but the scientific focus of the paper is not very clear. On the one hand, the “main goal” is to evaluate the models (and there are many figures comparing these models), but on the other hand most of the conclusions section focusses on the “secondary objective” to illustrate how statistics of sea ice drift can be useful. Frankly I find the “secondary objective” more interesting and scientifically better handled (although not much of that is really new) than the “main goal”. The model results of the neXtSIM model are remarkable, but the model comparison is biased and the conclusions that are drawn are likely to be either very specific or misleading. Therefore I recommend a major revision to redefine the focus of the paper properly (or change the weight given to the topics in the conclusions section) and change the nature of the model comparisons. I include an annotated PDF with notes and comments that I made while reading the text. They are meant as well-meaning suggestions (or can be found again below).**

We agree that the objectives of our study were not correctly presented in the first version of the paper and that the structure was not appropriate.

**The paper has been restructured with a stronger introduction presenting the context and the objectives of the study. In the first part of the paper we present the method used and its application to a reference dataset, whereas the second part of the paper presents the application of the diffusion analysis to simulated and observed trajectories. The highlight is not anymore on the comparison of the model results but on their comparisons to observations in the context of long-term trajectories modelling. The conclusion and the abstract have also been completely rewritten.**

**Details of my critique:**

**The model comparison is not very meaningful because the models start from different initial conditions, are driven by different atmospheric forcing and ocean conditions. In fact, all of these aspects tend to favor the neXtSIM simulation (initialization from observations rather than spinup, removed thickness bias, higher resolution forcing data, assimilated surface ocean) with respect to realism, so that I find the conclusion that the neXtSIM model performs better very much confounded by the totally different initial (important for a short integration period) and boundary conditions.**

We agree that we cannot conclude that a model is better than the other one.

**All the statements related to model comparisons have been removed.**

**At the same time the text seems to try to “sell” neXtSIM, which appears to inappropriate for a scientific paper.**

Yes, we agree, it may seem like that.

**The new version of the manuscript contains a longer analysis and discussion of the results, and also highlights the differences between the results of neXtSIM and the observations.**

**E.g. I can clearly see from Fig6+7 that neXtSIM statistics are better than those for TOPAZ, but at least in Fig7, the performance is not as great as the authors are trying to make me think (l382/Fig7: “follow an exponential distribution” may be true below 30cm/s, although it’s hard to know if this significant, but clearly it is as bad as TOPAZ above 30cm/s which goes almost unmentioned in the text, or:**

Yes, we agree, and this is a very good point as it indicates that the two model setups have similar deficiencies for the highest values of the fluctuating speed.

**The range in which the exponential distribution is followed is now indicated and the missing high values of fluctuating speeds are discussed.**

**On page 13, ll388 the authors discuss the initial and boundary value issues and acknowledge them as the weak point in the comparison, but still state that they expect TOPAZ to be a reasonable reference for other models, which is not based on any evidence. From Bouillon et al (2013), we know that standard EVP is not getting it right.**

As the quality of the simulated trajectories seems to greatly depend on the details of the simulation setup (initial conditions, parameters, forcings), it would be hazardous to generalise the conclusions coming from one specific setup to other simulations obtained with different setups and models.

**We removed the statement of using TOPAZ as a reference for other models and now insist on the use of the diffusion analysis as a prerequisite before further analysing simulated trajectories.**

**neXtSIM is said to be tuned to fit observations but TOPAZ uses a drag coefficient that leads to too high drift speed? This adds to the non-comparability of the models. The first thing I would have tried is to reduce the TOPAZ drag to 1e-3 to reduce the drift speed bias.**

The values of the parameters of the neXtSIM setup are those found to be optimal in Rampal et al. (2015, still under review) for the same forcing and a similar setup. The values of the parameters of the TOPAZ setup are those found to be optimal and currently used for the operational forecast platform (only documented in an internal report). As the overestimation of the drift in the TOPAZ setup is not homogeneous but mainly localised along the CAA where the ice is thick and almost immobile, playing on the air drag parameter would probably not help much.

**The reference to the full description of the two setups are now added in the setup descriptions.**

**Or use the re-analysis/assimilated solution of TOPAZ to begin with.**

We are not sure to understand this suggestion. If the reviewer means that we should analyse the reanalysis instead of a free-run of TOPAZ, we do not agree because it would require to rerun the reanalysis as the archived outputs do not have a high enough temporal resolution, and that would be too expensive and difficult to handle (100 members to analyse). If the reviewer means that we should start the free-run from the reanalysis, we agree that it would maybe be better but still not enough to have a proper comparison of the two models (we should use the same atmospheric and oceanic forcings, the same thermodynamical model, tune all the parameters of the models with the same approach, etc...). However, as the goal is not the comparison of the model setups, but their respective evaluation compared to observations, the two model setups do not need to be the same.

**No changes**

**c\_a = 0.0076 is a drag coefficient much higher than “standard” (although I acknowledge that c\_a has a tuning range). And c\_a has been measured or at least been inferred from observations, e.g., SHEBA observations have a mean of 1.7e-3, a general range is maybe between 1e-3 and 2e-3 (e.g. Nguyen et al 2011), maybe locally values of 5e-3 are OK, but such a high global value smells like a problem in the model (that is compensated by the high drag coefficient). I have had many discussions with meteorologists who work on atmosphere-ice-drag (both observations and atmospheric models) about the functional form and value of the drag coefficients and believe me, they would not accept drag coefficients outside the range of observations. It is surprising that neXtSIM still produces so slow drift. Should be discussed in a few sentences, what is compensated by this value (forcing?) and why nothing is done to adjust the TOPAZ value.**

The value of the air drag parameter used for the neXtSIM setup is the one found to be optimal in Rampal et al. (2015, still under review) for the same forcing (ASR-i 30 km) and a similar setup. In that paper, we did the same exercise for the ERA-interim forcing and found an optimal value equal to ca = 0.0023, which is in the range of classical values. Those values have been optimised for free drift events only and are then independent to the mechanical parameters (ice strength,...). The high value found for ASR-i then does not compensate any “smelly” problem in the sea ice model but is directly linked to the low bias documented for ASR-i surface wind. This is discussed in details in Rampal et al. (2015, still under review). The value of the air drag coefficient for TOPAZ is the one optimised for the operational forecast system (see also the answer here above).

**We now clearly indicate that the parameters used here for the two models are those found optimal in previous study/report for similar setups and forcings. The information asked by the reviewer can be found in Rampal et al. (2015, under review), whereas the information on the optimisation of the air drag coefficient for TOPAZ is not publicly available.**

**What’s wrong in summer in the model(s) that everything is focussed on winter? Summer is notoriously harder to simulate with “classic” models. Does neXtSim offer new opportunities or new problems? Is this discussed anywhere (in the Rampal et al TCD paper, doi:10.5194/tcd-9-5885-2015 ?)?, otherwise it would be good to say a little about this somewhere in the text.**

We do focus on winter because it has been identified as critical for pollutant tracking. We do not completely agree with the sentence: “Summer is notoriously harder to simulate with “classic” models.”. It may be true for the thermodynamics (due to many processes, melt ponds, albedo feedback,...) but not for the dynamics. We suspect that the ice being thinner and less concentrated, the rheology will play a less important role in summer, and sea ice dynamics may then be more easily reproduced even with simple free-drift models. For that reason and also due to the availability of the data, we have so far only analysed the simulated sea ice drift and deformation for winter periods (Bouillon and Rampal, 2015; Rampal et al., 2015, still under review).

**We now justify and document why we focus on the winter season in the context of pollutant transport by sea ice.**

**I suggest to either drop the comparison to TOPAZ, because it is unfairly couched, or repeat the comparison on “equal footings”.**

We agree that the proposed analysis of model outputs cannot be used for model comparison and we then drop any statement on model comparison. As the definition “equal footings” would always be debatable and could be expanded to all the aspects influencing the simulations (initial conditions,external forcings, resolution, computational time, number of tuning parameters, number of degrees of freedom, tuning method,...), we prefer not to follow the second suggestion.

**We now evaluate separately the results of the TOPAZ setup and those of the neXtSIM setup by comparing them to observations. All the sentences about comparing models have been removed.**

**The data analysis and suggestions for using ice drift statistics are interesting and appear useful, but it should be made more explicit, what is new compared to Rampal et al. (2009), and what it reproduced from Rampal et al. (2009).**

We agree.

**The similitude and differences compared to Rampal et al. (2009) are now clearly stated.**

**Sometimes, the buoy data analysis start unexpectedly, e.g. l397, and while most aspects are compared to model simulations, the result of Fig8 are not (why?).**

We agree.

**We now clearly split the analysis of the reference buoys data set and the analysis of model simulations. The presentation of the method is also better structured, starting with the presentation of the reference data set, the decomposition of the sea ice motion and then the application of the diffusion theory. This section ends with the presentation of the results of the analysis on the reference dataset and the discussion.**

**Fig8, compared to Fig14 of Rampal et al (2009) has 10 times larger values of r’. Why is that so? As far as I can see, if really r’ =sqrt(<r’ˆ2>) is plotted then, I would expect no larger differences of the geometrical sum than a factor of sqrt(2).**

The magnitude of the displacement in Fig14 and variance in Fig15 of Rampal et al (2009) are wrong by a factor 10 and 100, respectively. This can be verified by looking at the inconsistency with the value given for the absolute diffusivity.

**We now discuss this difference along with the explanation on how we checked the consistency between the computed and estimated values of the fluctuating displacements.**

**Please also note the supplement to this comment: http://www.the-cryosphere-discuss.net/tc-2015-233/tc-2015-233-RC1-supplement.pdf**

Our responses to these additional remarks are made in the pdf attached as supplementary material