

# ***Interactive comment on “Sea ice diffusion in the Arctic ice pack: a comparison between observed buoy trajectories and the neXtSIM and TOPAZ-CICE sea ice models” by P. Rampal et al.***

**Anonymous Referee #2**

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The manuscript

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

by Rampal, Bouillon, Bergh, and Olason consist of two parts: (1) a model inter-comparison between TOPAZ-CICE and neXtSIM including validation with observed ice displacement, and (2) an analysis of the diffusive processes going along with sea ice motion. The paper is well written; figures are clear and support the arguments.

While the second part is very interesting and holds novel results useful beyond the scientific community, e.g. also for oil exploration, the model inter-comparison part suffers

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from the unfortunate experimental design (see details below). I thus suggest focusing on part 2 as the main message of the paper and reduce the extent of the pure model inter-comparison. I think that studying sea ice diffusion with two types of sea ice models – a “traditional” one represented by a version of CICE, and the “novel” neXtSIM model – is a great addition to earlier work on this topic by the first author (referenced as Rampal et al., 2009b in the manuscript), in particular as both are designed for sea ice forecasting systems. This should be emphasized more, possibly by changing the sequence of the presentation of results with the model inter-comparison becoming more of a model validation against the same buoy data in both cases.

The manuscript may be acceptable for publication after major revisions. In particular, I strongly recommend shifting the focus of the manuscript from the model comparison to the diffusion analysis and provide additional information for commercial applications.

I apologize for providing my review late.

#### Major comments:

From the beginning, incl. the title, the authors misleadingly address TOPAZ-CICE as “sea ice model” where it is in fact a coupled sea-ice/ocean model. This needs clarification. In this respect: I think the title does not need to include the model names but could be shortened to “Sea ice diffusion in the Arctic ice pack from observations and models”. The generalization is in order as TOPAZ-CICE represents a whole suite of “classic” sea ice models (as also stated in line 394f).

The design of the experimental set up has certain deficiencies that I think inhibit a clean comparison of the two sea ice dynamics models:

1. TOPAZ-CICE is a coupled sea-ice/ocean model whereas neXtSIM is a sea ice model run in stand-alone mode (swamp ocean with nudging to TOPAZ reanalysis,



which differs for the TOPAZ run used here). On short time scales of a few days the ocean forcing should not matter as the ice motion is mostly wind-driven. But this is not as clear for periods of 30 days and longer. This needs to be explained and the bias quantified if possible.

2. The atmospheric forcing of the two models differs. Wind forcing is a key driver of sea ice motion on short (daily) timescales however. This must have a major impact on the results, which briefly noted but not discussed.
3. Sea ice drift tracks used to compare the two models with each other and with observations are computed during post-processing using hourly model output from the TOPAZ system and during run-time in neXtSIM. It should be demonstrated that this has no major impact on the results.

While I understand that both models are tuned to produce “best guesses”, the authors need to present more convincing arguments that the uncertainties associated with above differences are smaller than the errors originating from the sea ice dynamics that are compared. Differences in the wind forcing, drag coefficients (0.0016 vs. 0.0076), ice strength and resulting ice thickness distribution can already explain some of the reported biases. It needs to be shown that this is not the case or that these biases have different characteristics and can thus be separated from ice dynamics issues.

Further, I strongly recommend restructuring Section 2. I would expect this section to feature three short sub-sections on “IABP buoy data”, “The TOPAZ-CICE model”, and “The neXtSIM model” just stating retrieval of sea ice velocities from the buoy data and the model set ups and experiments being used. For the buoy subsection the current texts at the beginning of section 2 and in section 2.1 should be merged. Finally, these three sections could be followed by a section “Modeled trajectories” describing the derivation of “float” trajectories from both models. Although I think such a subsection would rather belong to Section 3 Methods.



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comment

Some minor remarks (by line number):

line 1: "... activity in the Arctic ..."

4: "... simulated by the ocean/sea-ice model TOPAZ-CICE and the stand-alone sea ice model neXtSIM. We compare ..."

16–20: This aspect of the paper including related results of time and length scales should be emphasized more.

64: "... as follows: data sets and models are presented ..."

69: either "We use all 12-hourly buoy positions ..." or "we use the full data set of 12-hourly buoy positions ..."

73: "... we also generate virtual buoy trajectories by simulating "floats" in the TOPAZ and neXtSIM models. The float simulations are initialized ... buoys and stopped when the IABP buoy ..."

76: please rephrase sentence for clarity: "After removing ..."

86: from this statement it is not obvious that the 35-day trajectory chunks are independent since, for instance, the starting point of such a segment relates to the last 10 days of the previous segment. However, in line 406 you mention that only every 10th segment is used. These are independent estimates, I agree, but this must be stated near line 86 as well.

97 & 99: change "were" to "are" if this is still the standard procedure for the buoy positions and does not refer to some method used in the past

104: rewrite sentence for clarity, e.g. all buoy data north of 70°N are used if at least 100 km from coasts, only between longitudes XXX (Greenland) and XXX (Severnaya Zemlya) the southern limit is 80°N.

110: I don't think "embark" is the right expression here, maybe "installed"?

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114: at this point it is not clear why Figure 1 shows all buoy data from 1979-2010 since it seems that only years 2007-2010 are considered for the analysis. This should be clarified, e.g. by adding the statement from line 294. (also see comment on restructuring Section 2 above).

118: it would be nice to support this statement with a figure showing a single buoy track. In fact, Figure 4 shows just this. Please consider moving this figure to become Figure 2 to be referenced here. (You must not yet address the separation of mean and fluctuation also shown in the plot.)

128: I guess it is not the sea ice strength set to a constant value but rather the ice strength parameter ( $P^*$ )? Or is 27500 N a model mean?

151: replace “kills”

153: explain how the interpolation is done

172: rephrase: “... and finishing on May 15th for three consecutive winters from 2007 to 2010.

177: provide reference for low-biased TOPAZ ice thickness or show ice thickness distributions. In fact the latter would be a helpful additional plot to get a better idea of the behavior both models.

212: “... for the decomposition of the motion into a mean and a fluctuating part,  $u = \bar{u} + u'$ , we follow ...”

220: “evolve” (remove “s”)

229 “... is referred to as ...”

304 “... in the IABP data set, This is most pronounced in the winter 2007/2008, in which short IABP trajectories ... Archipelago are the result ...” Again, it would be helpful to see the modeled ice thickness distribution.

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317: remove “and so we did”

328: remove “and shall be”

332: remove “Unsurprisingly,”

334: I believe the correct term is Transpolar Drift Stream

339: replace “size” by “extent”

340f: remove “in the “lower” part of the Beaufort Gyre,” It is not clear what “lower part” means and “along the Canadian Arctic Archipelago” is sufficient anyways.

335–345: the whole paragraph relies on a rather visual, qualitative comparison. I would prefer to see a scatter plot of daily observed vs. modeled drift velocities and the associated regression for a more objective and quantitative comparison.

352: mean value of the mean drift: the PDF in Figure 6 somewhat emphasizes the extreme values at 20–25 cm/s. While this needs to be mentioned I think that the median should be used instead of the mean to limit the influence of these extremes.

383–396: This paragraph belongs in a discussion section, which is missing at all, by the way. Really, the model differences should be discussed in more detail with respect to the differences in the experimental set up. (also see major comment above).

397: I strongly recommend starting a new section here, possibly entitled “Sea ice diffusivity”. In my opinion the results presented in lines 397–469 are the more interesting ones. Consider to show these first and present all other results as model validation thereafter.

417–426: This part should be expanded. These numbers are really what a community interested in applications would be interested in. Consider providing a look up table listing all numbers for 1, 2, and 3 standard deviations for 5, 10, 20, and 30 days (decision makers may view a 70% or 95% chance to find the polluted floe as high already). Possibly also check numbers from before 2000 from buoy data to demonstrate a temporal

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evolution.

Figures:

Consider to shift Figure 4 to be shown earlier (see comment above). Also add a marker at the starting point of the track.

Figure 8: in upper panel dashed red line needs to be thicker. Examples of the “search area” would gain a lot from being shown bigger and with less trajectories, also indicating 1 and 2 standard deviations. Demonstrating the search radius is a key figure of the paper. Please make an effort to improve the graphical presentation.

Figure 9: labels of slopes “1” and “2” not clear; explanation missing in caption. I suggest using more intuitive labels, such as “Brownian” and “ballistic” or “Eq. 7” and Eq. 6”.

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Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2015-233, 2016.

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