Response to Reviewers’ Comments:  
*>>There may be some discrepancies in line numbering between the manuscript we submitted and the version that the reviewers’ commented upon. This may be due to the PDF creation process. We downloaded the submitted version from the website and used the line numbers described accordingly. We had to guess in some cases, so our apologies if any of the comments did not fit the intended sentences.*

Reviewer: 1  
Comments to the Author  
Summary of ms:  
The ms presents an adaptation and application of a so-called forward/backward-Lagrangian model to PSAT data from Atlantic salmon in the northern polar region. The main focus of the ms is the development of the method, and to a lesser extent on the interpretation of the results. The motivation of the study is that information of the open-ocean migratory behaviour of salmon is currently lacking and PSAT data could be the way forward, however light-based geolocation in polar regions is not always possible so SST and depth recording must be used instead.  
  
General comments:  
I found the data used in this study interesting and worthwhile of publication, however the model developed in the ms was of disappointing quality and does not live up to today's standards for geolocation models. The model heavily relies on subjectively chosen procedures and parameter values, which are scarcely explained. The sensitivity of the results on the start and end location and dates is analysed, which is useful, but the sensitivity of the results on the subjectively chosen model parameters would be even more interesting. My recommendation would be to discard the model presented here and switch to a state-space model (SSM) for analysing the data, which is a well-documented and well-understood approach to animal tracking (Patterson et al. 2008, Schick et al. 2008). There exist SSMs which are designed for data without a light component (see specific comments below), and it seems that this tracking problem is so difficult that sensible (i.e. statistically sound) incorporation of the uncertainties in data is of high importance and should not be handled with heuristics. By switching to a SSM approach the authors are on solid ground modelling-wise and can spend less time on discussing the model and more time on what in my opinion is more interesting namely the novel salmon data.  
*>> The model that was used in this study may be seen as a type of state-space model. Patterson et al. 2008 describe the SSM as “a time-series model that predicts the future state of a system from its previous states probabilistically, via a process model.” In this case, the state is the position of all tracks at a given time, living in a 2\*N dimensional space (where N is the number of tracks). And the model treats the development of this state in time. The objective of this manuscript was to adapt the cod model to salmon to create a simple method that would estimate migratory pathways in polar regions using temperature, depth and some geolocations. The novel salmon data will be published separately with additional data using multiple modelling techniques. The introduction and discussion have been re-written to elaborate further and reflect the concerns of this reviewer.*

The field does not need another geolocation approach unless it is a significant improvement over the existing methods, and that has not been convincingly conveyed here. In fact some of the results (Figure 5) showed the lacking accuracy of the model. If the authors insist on their model they should, in order to convince their readers of its usefulness, at least provide a simulation study which validates the estimation capabilities of the model, and provides an understanding of e.g. what the “standard deviation ellipses” in Figure 5 mean and how they should be interpreted. In addition, a more thorough comparison of the presented model with existing state-of-the-art methods should be provided ideally in the simulation study mentioned above.   
*>>A simulation study would be very interesting, but would need to be conducted at lower latitudes to obtain geolocations throughout the entire migratory period. In polar regions, this is not possible, so the model had to be tested using only the geolocations that were available just before and just after the equinox. The manuscript is not describing another geolocation method—rather, we used filtered geolocations to test and increase the accuracy of the model. Figure 5 showed that the geolocations were within 100 km of the trajectories’ center of gravity, which is not a great amount of error, considering that geolocations themselves generally have about the same amount of error (Musyl et al. 2001). The standard deviation ellipses in Figure 5 were further explained in the Results section. Further information about the model and more background on modelling methods have been added to the manuscript.*

Specific comments:  
20-21: I don't really see the relevance of classical particle tracking here. Tracking fish is a wholly different task.  
*>>This sentence was removed.*   
  
22-25: Split this sentence in two (drop and).  
*>>This was done.*  
  
25-27: Based on the results presented later I don't agree with these conclusions. See comments below.  
84-85: This is an unconventional use of the term “state-space”. The state-space typically refers to the state-space of a state-space model (SSM), which in most tracking applications is the set of accessible locations. In this sense the use here is meaningless since a state-space is given and not something to predict. The location, on the other hand, of migrating organisms can be predicted.  
*>>The term “state-space” was removed from this sentence.*  
  
91-93: It can be misleading to say that “Bayesian models … provide likelihood-based estimates of location”. Location estimates from Bayesian models are based on the posterior distribution and not the likelihood alone.  
*>>”Likelihood-based” was removed from this sentence.*  
  
97: The reference to Patterson et al. is incorrect, should be 2008. This is also wrong in line 471. In addition, in the present state of the introduction I don't see why this reference is relevant here since it does not mention forward/backward-Lagrangian models, but is concerned with SSMs.  
*>>The Patterson reference was removed here and corrected in other instances, later in the manuscript.*   
  
103-106: It is concerning that the authors in their introduction do not mention SSMs, which are a another (possibly more) generic class of models that are popularly applied to ecological tracking problems (Patterson et al. 2008). A review of existing SSM literature should be included and could possibly replace the, in my opinion, lesser relevant comparison with classical particle tracking models.  
*>>A review of SSMs was added to the introduction and the classical particle tracking model discussion was reduced.*   
  
111-115: I like the use of bullet points to summarise the objectives of the study.  
120: It is odd to refer to “the model” before it is formally introduced.  
*>>These were changed as suggested.*  
[\*\*BAA: The first new bullet point still refers to “the model”. Better to keep the old formulation,

An attempt is in the text\*\*]

120-136: This procedure sounds rather invasive. Do studies exists that investigate the effect of the procedure and the drag of the tag on the behaviour and swimming ability of salmons? a large PSAT tag plus an additional acoustic tag must have some effect?  
*>>A tag effects paper for PSAT tags on salmon has not been published yet but we are working on it.*

*[\*\*BAA: This could be mentioned in the introduction\*\*]*   
  
139: Perhaps mention the size of the fish already in the beginning of this data section?  
*>>The size of the fish was moved to the start of the section.*  
  
143: What was their programmed pop-up date?  
*>>The programmed pop-up date was December 1, 2008.*  
[\*\*BAA: OK as an answer to the reviewer, I agree not necessary to put in the text\*\*]

151: The size of these corrections should be reported. Perhaps plot both reported pop-up and corrected pop-up.  
*>>The corrections were added to the Materials and Methods section.*  
  
156: How many missing days were there?  
*>>Four days, as described in the Materials and Methods.*   
  
159-160: Are there any references to support this claim other than Figure 1?  
*>>Not really, there aren’t any articles tracking fish with PSATs in polar regions yet.*   
  
162-179: I am not convinced that the development of another method is warranted. Furthermore, I don't agree with the subjectiveness of the selected approach and the way uncertainties in data are handled. This paragraph is problematic. Specific comments below:  
163: It is well-known that light-based geolocation is uncertain, i.e. that geolocation reported from PSATs cannot be regarded as the true location, but rather the true location plus an observation error Musyl et al. (2001). It is therefore not sensible to exclude geolocations on land since they do contain information about the true location, instead this uncertainty should be part of the model. And does this mean that the geolocations in water areas are without error?  
*>>The filtering method used SST as a validity check, therefore tags that were on land could not be deemed valid. Error was included in the geolocations that passed the filter screenings; these were used in checking how closely the model fit the geolocations, and they were averaged and smoothed for use in refining the model. Thanks for the Musyl et al. reference--we referred to it in the methods and discussion.*  
  
163: What are the reasons for selecting 10 days?  
*>>The error becomes too great at 10 days on either side of the equinox, and could be observed in the very strange geolocations during those periods (e.g. in the southern hemisphere, in the Pacific Ocean etc.).*   
  
165: Only the latitudinal component of the geolocation become inaccurate.  
*>>This was corrected in the text.*  
  
166: What are the reasons for selecting 0.25 degrees as the limit? this seems like a subjective choice which is highly concerning. And why employ a strict criterion? why not use a statistically sound approach as in Nielsen et al. (2006), or Pedersen et al. (2011)? I don't see why effort is invested in a new and rather heuristic approach when well-established solutions to this problem exist.  
167-172: The reasoning behind the formulation of these four “parameters” should be provided. And why is three out of four the passing criterion? is four out of four not alright? why 0.5 deg lat and lon? this seems very dubious. What about the uncertainty in the SST archive (database)? that should be included in the model as well or at least discussed.  
*>>Nielsen et al. and Pedersen et al. use specific models that integrate their tag data with the SST data. We opted to use a more simple approach to check the tag locations directly against the SST data using a method that had been developed previously at CEFAS by David Righton. Additional explanations and references regarding the OSTIA dataset were added to the Methods.*  
  
173-174: What was the time interval? is this swimming speed unrealistic for all time intervals?  
*>>The time interval varied between geolocations. As data was very patchy and only a few geolocations were possible to estimate at the high latitudes where the study occurred, swimming speed was used as a guide to filter out those locations that exceeded all possibility of the salmon being able to feasibly travel to in the given amount of time.*  
  
176: This averaging procedure is unclear. How many geolocations are part of the averaging and how many averaged locations are returned? only two?  
*>>The numbers of geolocations were added to the Materials and Methods section for clarification.*  
  
162-179: This overall procedure relies on a number of subjectively chosen parameter values. What is the sensitivity of the end result to the choice of parameter values?  
*>>See below comments on sensitivity testing of parameter values.*  
  
184: Is this the corrected pop-up location?  
*>>Yes, only the corrected pop-up locations were used in the study.This is now stated explicitly in the text.*  
  
186-188: This is unclear and should be explained with an equation.  
*>>We are not sure exactly which sentence requires an equation here. The description of the model was revised for clarity. Equations are given in Ådlandsvik et al. (2007) which is sited in the paragraph.*  
  
192: Has the previous position not already been checked in the previous step? otherwise it should have been eliminated already? this bit is unclear, perhaps provide an illustration.  
*>>The kelt may for instance have taken a deep dive since the previous step, making the previous position too shallow. . This is what the model is checking for. The sentence was edited for clarity.*   
  
196: How is this threshold chosen and how sensitive are the results to this choice?  
204-210: More seemingly subjectively chosen parameters. What are the justification for their exact values and what is the sensitivity of the end results of the model to these choices. What is the definition of a “good selection pressure”? what is characterised by a “reasonable subset of active trajectories”? these seem like vague criteria for selecting parameter values. Why not use a statistically based approach where model parameter values can be estimated from data instead requiring subjective input?  
214: This is a hard constraint which seems overly restrictive given that the difference between tag SST and the SST archive contains potentially large errors from the tag recording and in particular the spatially and temporally interpolated SST archive. A soft constraint would be more appropriate.  
222: What is the reason for this formulation? how are the parameter values chosen? how sensitive is the result to this choice?  
243: This seems like few “surviving” tracks. This number is likely quite sensitive to the selected parameter values?  
*>>The parameters for maximum deterministic swimming speed and random swimming speed were selected based on estimated maximum values for Atlantic salmon? The depth factor and additive extra depth allowed for bathymetric error and the temperature tolerance allowed for error in SST estimates? The surviving tracks were sensitive to parameter values…*

*>>The parameter values are determined by trial-and-error. If the constraints are too wide, the model looses selective power. If the constraints are too narrow, there may be none or too few surviving tracks. Soft constraints like in Andersen et al., 2007 could be used, but choosing a probability distribution leads to similar subjective considerations. The text has been clarified on this point. The results are not sensitive to the precise value of the criteria, as long as they are in a “sensible” range.*  
  
224-227: I like that this sensitivity analysis is conducted, but it should be extended to include all model parameters. What would happen to the analysis if the start and end locations were entirely removed? would it be possible to reestimate these based on the SST and depth data?  
  
*>>The model requires start and end locations to function? Adding sensitivity analyses for every parameter… would take up too much space in this manuscript, as the maximum number of figures had already been met? Bjorn how difficult would it be to run a couple analyses using different parameters? I think you have already done this many times (when selecting the parameters)… some killed off all the trajectories, right? How can we report this to satisfy this reviewer?*

>> The method needs start and stop positions (or more generally waypoints) to start the forward and backward tracks. A superensemble using several start and stop positions is theoretically possible, but would be computer demanding and throw away the most reliable position information. More sensitivity studies has been done for selecting the parameters, but all can not be presented here. The start and stop positions are in our experience the most important. See the point above.

254-255: The plot doesn't say to which tag the two panes belong. How many were validated for each tag?  
*>>The plot was labeled and the number of validated geolocations was added to the Materials and Methods.*   
  
255-259: This testing procedure should be explained in detail here. What is the theoretical reasoning behind this procedure? In statistics for a result to be deemed significant 95% confidence is required. Even though this is not a statistical procedure it is still concerning to me that only 75% and 9% (one out of 11) passed this test. These are the results that support the statement on line 25-26 in the abstract – in my opinion this conclusion is not valid. I might have misinterpreted something, but then the point should be made much clearer.   
*>>The objective was not for the geolocation to overlap the center of gravity of the modelled particles, but to see what sort of error there was between the two. In this case, all geolocations were within 100 km of the centers of gravity, which meant that the model had a high degree of accuracy, since the geolocations themselves likely had a similar error. This was discussed further in the methods and discussion.*

262-266: This analysis should include all subjectively chosen model parameters.  
*>>See above comments.*

269-275: In this section it seems that the uncertainty inherent in the geolocations is disregarded and that they are taken as “true” locations – this is highly inappropriate cf. the authors' own discussion in line 59-64 and 344-348.  
*>>See above comments.*

*>> The geolocations are used as way-points, steering the tracks. However, the overall picture from the tracks are not too sensitive to the errors in these way-points.*  
  
274: I wonder if the correction of the pop-up locations plays a role in this deviation?  
*>>The pop-up locations remained the same for both model runs. The trajectories were forced to pass through the validated and averaged geolocation points only.*   
  
286-288: I am not convinced that this increases the model accuracy since the “validated” i.e. the geolocations that survived a rather dubious elimination procedure, still are only geolocations, that is, uncertain observations of the true location (as is also indicated in line 344-348). Using these as constraints without accounting for this uncertainty increases the chance of relocating the track to a wrong location while at the same time conveying a false sense of accuracy, rather than increases the accuracy. The bit in the abstract (l 26-27) should be removed.  
*>>The error associated with light-based geolocations was estimated to be within less than 100km by Musyl et al. 2001. This was discussed further in the manuscript methods and discussion. With the rigorous validation method we used, incorporating SST and swimming speed, the “valid” geolocations are as close to accurate as was possible. We were checking whether the model fit the validated geolocations. We acknowledged that both the geolocations and the model have error, but the manuscript is just reporting how closely the two estimates were located (in this case, less than 100 km in all cases, which we found to be quite proximate and a good fit).*  
  
289: I am not convinced of the potential of the method. There is no real evidence in the results to support this claim.  
293-304: This paragraph can be omitted since it seems irrelevant to the discussion. Could possibly be moved to introduction.  
309: Please provide references for optimisation and genetic programming.  
309-310: Why not use a more intelligent approach to the track generation as is the case in many Markov chain Monte Carlo and sequential Monte Carlo approaches? It seems most of the computing time for this method is invested in generating track that are discarded in the end.  
311: Please provide a reference to control theory.  
312: Please provide a reference to assimilation techniques.  
318-319: It seems that the authors have overlooked state-space models (SSMs) in their evaluation of modelling alternatives?  
321-333: This paragraph contains little discussion of the results and can be cut down.  
*>>The discussion was rewritten to address SSMs and remove extraneous information about particle tracking models.*  
  
334-336: I disagree with this claim for the above stated reasons.  
343: I disagree with this claim for the above stated reasons.  
344-348: SSMs are by construction able to handle this type of uncertain data, so it is surprising that an SSM has not been consider for this data analysis.  
375-382: The conclusion should be rewritten with the above comments in mind.  
*>>The conclusion and discussion were rewritten to reflect the reviewer’s comments.*

Figure 4: This plot came out very dark on my printer. Perhaps get rid of the SST background and make land green like in the other plots. Also, the “Tag temperature within tolerance” and “Tag temperature too cold” colours are very hard to discern. Which plot belongs to which tag?  
>>Thanks for these suggestions. Figure 4 was adapted to reflect the tag numbers. The colours are very visible on a computer screen.  
  
References:  
Musyl, M., R. Brill, D. Curran, J. Gunn, J. Hartog, R. Hill, D. Welch,  
J. Eveson, C. Boggs, and R. Brainard, 2001. Ability of archival tags to  
provide estimates of geographical position based on light intensity. Elec-  
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Nielsen, A., K. Bigelow, M. Musyl, and J. Sibert, 2006. Improving light-based  
geolocation by including sea surface temperature. Fisheries Oceanography  
15:314-325.  
  
Patterson, T., L. Thomas, C. Wilcox, O. Ovaskainen, and J. Matthiopou-  
los, 2008. State{space models of individual animal movement. Trends in  
ecology & evolution 23:87-94.  
  
Pedersen, M., T. Patterson, U. Thygesen, and H. Madsen, 2011. Estimating  
animal behavior and residency from movement data. Oikos 120:1281-1290.  
  
Schick, R., S. Loarie, F. Colchero, B. Best, A. Boustany, D. Conde, P. Halpin,  
L. Joppa, C. McClellan, and J. Clark, 2008. Understanding movement data  
and movement processes: current and emerging directions. Ecology Letters  
11:1338-1350.

*>>All of these suggested references were added to the manuscript. Thank you very much for the suggestions, they were very helpful. Thanks also for the time you spent on this review. We feel it has greatly improved the manuscript.*  
  
Reviewer: 2  
Comments to the Author  
General comments:  
In this paper, a forward/backward-Lagragian model developed by Ådlandsvik et al. (2007) for cod was used as a starting point to plot the migratory pathways of two PSAT-tagged Atlantic salmon in the Arctic. While cod are bottom-dwellers, Atlantic salmon are surface-oriented, and one main task was to adapt the model to Atlantic salmon behavior data. The methods used appear sound, and the results are convincing and rather unique. Using this method on a higher number of PSAT-tagged individuals will significantly increase our knowledge of migration routes of Atlantic salmon.  
I consider this paper highly relevant to Fisheries Oceanography, and I recommend it for publication after some minor revision.  
  
Minor comments:  
Consider to include pelagic or surface-dwelling in the heading of the paper.  
*>>We decided not to include these words as the model could be used for bottom-dwelling fishes too.*  
Be consistent in using the correct name (Atlantic salmon) throughout the text, when you mean Salmo salar.  
*>>This was carried out throughout the manuscript.*

Line 100: a dot is missing.  
*>>The sentence was re-written.*

Line 123: the Alta River is not shown in Fig. 2.  
*>>This sentence was re-written to describe where the Alta River was in relation to the Altafjord, shown in Fig. 2.*  
  
Line 138: Where are the main fishing areas? (the release site is not shown in Fig. 2)  
*>>This sentence was re-written for clarity.*

Lines 135-140: The choice of starting date of the model is unclear. The fish were released on the 22 May and it took approximately three days to leave the fjord. Why choosing 27 May as the starting date for the model? Specify.  
*>> The reason for changing the start date of the model was for running sensitivity analyses to see if the trajectories were affected. The 25 May was used as a baseline for start point “B”, at the entrance of the Altafjord. The 27 May was used for start points that were approximately two days’ travel from “B”.* This is clearified in the Material and method section.

Line 286: Be more specific than too much.  
*>>The wording has been changed.*  
Line 304: Miller et al 1998 is missing in the reference list  
*>>The discussion of Miller et al. 1998 was removed from the manuscript.*   
  
Lines 336, 340, 342, 352. Does the Journal accept references to figures in the Discussion section?  
*>> All figure references were removed from the discussion.*  
  
Line 388. Correct to Laksefiskeri.  
*>>This was done.*  
  
Lines 404-406. Batchelder 2006 is not referred to in the manuscript  
Lines 420-421: Gifford et al 2007  is not referred to in the manuscript  
Lines 473-478: Pedersen et al 2000 and Pedersen et al 2003 are not referred to in the manuscript  
*>>These references were removed*.  
  
Table 1. Is the random swimming speed of 2.0 m/s correct?  
*>>Yes, 2.0 m/s was used as a random swimming speed in the model and a maximum speed for filtering geolocations.*   
  
Table 2. Specify that these parameters are for Tag 1, only.  
*>>This was completed.*  
  
Fig. 2. The locations of Alta River and the release site are missing.  
*>>Descriptions were added in both the text and figure legend.*  
  
Fig. 4. Are the two figures interchanged?  
*>>Yes, they were. Thanks for noticing! This has been fixed now.*