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Interactive comment on "Numerical modelling of physical processes governing larval transport in the Southern North Sea" *by* M. C. H. Tiessen et al.

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We would like to thank Mrs. Lacroix for her careful examination of the presented paper. We have carefully considered the points raised, and we address them point by point here. Please note that the original comments are presented in **bold** whereas our replies are presented as normal text. Please find enclosed a revised version of the document, attached as a supplement. Alterations to the original document are shown highlighted in grey.

The comparison of larval trajectories with drift buoy paths is interesting and useful because it is a first step towards the particle tracking model validation.

This part of the study should be further developed (see specific comments).
We thank the referee for the positive appraisal and for the suggested extensions. Please find our reply with regards to this subject in the "specific comments" section.

The authors consider properly related larval transport modelling work for plaice in the North Sea. The discussion would be further improved by including comparison with outcomes from other studies for the same region even if for another species (ex. Savina et al., 2010, Lacroix et al. 2013, Rochette et al., 2012).
We have included a section into the discussion comparing the findings presented in this manuscript with other studies, also including other species. Additionally, the suggested papers are now also incorporated.

Specific comments:
The strong impact of hydrodynamics on interannual variability of fish larval dispersal in the North Sea and the English Channel has already been demonstrated for instance for plaice (e.g Bolle et al. 2009 for 1996-2003) and sole (e.g Lacroix et al., 2013 for 1995-2006, Rochette et al., 2012 for 1991-2004). What is missing is a better quantification and understanding of the relative impact of wind/current/temperature/river discharges. What are the particular conditions that enhance/reduce larval settlement?
This is an interesting point that we have investigated further. We have quantified the various aspects concerning plaice settlement success. These are now incorporated into the various parts of the paper. Additionally, we have included two tables into Section 3.3 quantifying the variability in settlement and the link with hydrodynamic conditions. The impact of river run-off has not been quantified, as its contribution to the overall variability in drift direction and settlement success is assumed to be minor.

Also, in this study it is not possible to disentangle the role of 'pure' transport (wind/current) and the temperature effect on the drift duration.

With regard to examining the impact of wind alone, we have carried out additional simulations where the effect of temperature on the development was excluded. The findings have been added to the section "sensitivity analysis".

In Discussion "The aim of this research was to investigate whether changes in hydrodynamic conditions alone can potentially cause significant interannual settlement variability of North Sea plaice juvenile. . .'". What do you mean by significant? It is not quantified. The authors should make a distinction between 'purely' hydrodynamic conditions (current) and the effect of temperature. In conclusion "The present study serves therefore to give an indication of the extent of variability that can be attributed to physical factors". The extension of variability is not quantified.

We have included an analysis of the inter-annual variability in forcing conditions and settlement success, linking the changes in settlement success in the different areas to hydrodynamic conditions. This has now been incorporated throughout the results section.

The discussion would be further improved by including comparison with outcomes from other studies for the same region even if for other species. For instance, in this study, the authors have chosen the years 1996 and 1998 as contrasted years. They compared distances drifted in both years (p. 1775, l2-4). The distances drifted by sole larval for different years are presented in Lacroix et al., 2013. The results could be compared.

We have now included a separate paragraph in the "Discussion" comparing the findings in this paper to other published results.

Generally, larval transport models lack of validation. In such, the comparison between larval trajectories and drift buoy paths presented in the study is really useful. But the statement "the model reproduces the general trend of the drifters reasonably well" or "the accuracy of the model predictions is fairly reasonable" or "the model reproduces the hydrodynamic conditions reasonably well" is a little bit too vague. What are the criteria used to assess the accuracy? I would expect more detailed comparison and probably further sensitivity analysis to better understand the origin of divergence. Only the effect of time-step increase (from 45min to 24h) has been tested. What would be the effect of a decrease of the time-step? In section 2.1 and section 4.2 ". . ., the hydrodynamic conditions were stored at 45 min intervals to provide a high enough temporal resolution to describe the M2 tidal cycle". What is the evidence that a shorter temporal resolution would not give more realistic results? What would be the effect of an increase/decrease of the number of vertical layers? Is there any wind driven effect on the drift buoy?

We agree that the figures presented in the original paper did not provide an actual quantifiable value with which to determine the accuracy of the model.

Fig. 1 of this response shows the relative error between the drifter-buoy and the single particle released at the same time and location as the drifter-buoy (colour indicates the different buoys). The absolute error is divided over the total drifted distance by the drifter-buoy. The figure shows that after an initial period when small errors amount to big relative errors, the error between each of the buoys and the modelled counterparts is below 10

The present set-up with 26 layers in the vertical to describe the North Sea domain has been used before (Lenhart et al., 2010; Van Leeuwen et al., 2012; Aldridge et al., 2012). The effect of increasing the number of vertical layers would only result in minor changes as the current set-up was designed to give an accurate vertical distribution of the current profiles, whereas a (significant) decrease in the number of vertical layers would result in a decrease in accuracy (for a detailed description of the vertical layer structure in GETM: http://www.getm.eu/data/getm/doc/getm-doc-devel.pdf).

Concerning the wind-driven effect of the surface buoy on the displacement of the entire drifter-buoy (including a drogue at approximately 10 m depth): The drogues of the drifter are tubes of approx 1.5 m diameter and 7m long - giving a surface area of 32m2. In comparison the surface drifters are mostly tubes of 1m height and 15cm diameter, with about half in the sea and half in the air. Giving a surface area of 0.45m2. Niiler et al., 1995 recommend than when the drag area ratio of the system is > 50 this restricts wind slip to less than 1 cms-1 for winds up to 10 ms-1. In the present set-up this value would be significantly higher than 50, and so we assume the effect of the surface buoy to be minor. Please note that the position of the particle is at the shallow end of the drogue depth, partially compensating for this effect.

The model lacks a bit of realism, especially regarding the spawning distribution and period. Larval supply to nurseries is strongly impacted by both. I am not a specialist of plaice but it is the flatfish that has been the most studied in the North Sea. I wonder if it is not possible to increase a bit the realism of spawning area/period on the basis of existing literature and data? In the study, the larval duration is temperature-dependent but the spawning period not. Why not? Some examples: - "For plaice, the location of the spawning grounds is well documented" (Bolle et al., 2009). - Loots, C., Vaz, S., Planque, B., Koubbi, P., 2010. Spawning distribution of North Sea plaice and whiting from 1980 to 2007. Journal of Oceanography, Research and Data. 3, 77-95. - C. Loots, S. Vaz, P. Koubbi, B. Planque, F. Coppin, Y. Verin. 2010. Inter-annual variability of North Sea plaice spawning habitat. JSR 64:427-435. - "Profusion of studies for spawning period of plaice in the North Sea (e.g.Simpson, 1959a; Harding et al., 1978; Coombs et al., 1990)". In Ellis, T., and Nash, R. D. M. 1997. Spawning of plaice Pleuronectes platessa L. around the Isle of Man, Irish Sea. – ICES Journal of Marine Science,

54: 84–92. - "Plaice spawns between late December and April in the southern North Sea" (review in Hufnagl et al. 2013).

We did not choose specific spawning grounds and periods, since we wished to study connectivity between the different parts of the North Sea and English Channel and the different nursery areas. An a-priori spawning ground selection would inhibit such relationships for certain parts of the studied domain. The motivation for the present lack of specific spawning grounds and periods is presented in more detail in section 2.3. The present set-up however leaves every future opportunity to select specific spawning grounds and periods, using the same simulations.

With the regards to the selection of spawning grounds and periods: it is true that these are generally well know, however the boundaries in both are less well defined. Taylor et al. (2007) shows that according to ichthyoplankton surveys from spring 2004, there is only limited scope to define hard boundaries for separate spawning grounds, as only a variability in the abundance of plaice eggs could be observed across large parts of the North Sea. The two articles by Loots et al (2010a and b) show that the population distribution of spawning plaice also shows inter-annual variability. Concerning both aspects, we decided to construct broad but realistic thresholds for spawning by selecting the spawning area and period to encompass all known spawning grounds and periods, which in principle is similar to the approach chosen by Hufnagl et al. (2013).

On the overall, the manuscript is clear and well presented but is it not always easy to follow the different model set-ups. For instance, some results are based on simulations where only 100000 particles are released. For some simulations the period of release is between 1 Dec and 31 March, for others it is from 25 Dec to 6 Jan. It is not clear in the text why and for which simulations? This should be better explained in the MM.

We have included additional information to emphasize when only a selection of the total number of released particles is shown in a figure.

I wonder how are the results sensitive to the choice of 120 days as a maximum life span? What is the reason to truncate the simulations to 120 days? This must lead to bias in conclusions. P. 1783 l. 26-28 "This limit was chosen based on . . . and a small change (on order of 10 days) would not result in massive increase in particle settlement". Did you test?
We re-examined our results carefully and found a minor error in our numerical code. The implementation of the 30-day settlement threshold was flawed, and resulted in a number of particles settling beyond this threshold (however, within the 120-day limit). This only occurred for higher water temperatures, and is therefore mainly confined to particles released in the English Channel.

Fig. 2 of this response presents the impact of the altered code. The top graphs show the original results, whereas the bottom graphs show the new results. The overall settlement characteristics per area show a slight decrease in the number of settling particles, which can be attributed mainly to the settlement in the English Channel (shown in black) settlement in the other areas remains nearly unchanged, and all show the same inter-annual variability as presented in the original document. Settlement characteristics in the Western Wadden Sea (shown on the right) show a slight reduction in the number of settling particles at the end of the 120-day drift duration, and at the end of the numerical simulations (shown along the y-axis for values higher than 150). Please note that, although all affected figures presented here and in the paper have been redrawn, the presented outcomes and conclusions have not been altered as a result of this.

Concerning this 120-day numerical simulation limit. The impact of this was investigated, by extending this to 180 days for the three years examined in the paper (Fig. 20). The findings are presented in the Sensitivity analysis section of the paper. Generally, the impact is small, although for 1996 a significantly higher number of

particles are able to settle in the Western Wadden Sea.

The results of the sensitivity analysis to the size of settlement areas on the settlement success (section 4.2) are expected. By increasing the size of a nursery, it seems obvious that more larvae can settle. It is possible to improve the realism of the definition of the nurseries from the literature? For instance on the basis of sediment type? It is not mentioned in the discussion (section 5.5).
We have now adjusted the text and included this aspect in the Discussion.

In section 2.2. The random walk model used is not clearly explained. It is well known that 'naïve' random walk formulation gives erroneous particle accumulation in low- diffusivity areas (Visser, 1977). Which formulation has been used in the study?
We have now included the correct reference to the random walk model used in our work [Hunter et al. 1993], in which the erroneous accumulation of particles in low-diffusivity areas is avoided.

In section 2.2. "The internal time-step of 10 s is chosen to ensure accuracy of the tracer model". What is the criterion to ensure accuracy?
The time-step chosen for the particle tracking routine is based around the flow velocity and grid distribution of the hydrodynamic model, as the drift of particles through the hydrodynamic grid is based on interpolation of the velocities at the different grid-points. If a particle propagates outside a certain cell during a certain time-step, the interpolation has to be carried out again considering the velocity points valid for the new cell (Wolk, 2003).

"After the final development stage, particles went into a settlement stage lasting

up to 30 days. During this stage, a particle would settle if it would encounter favourable conditions" (p.1770). The sensitivity of the results to such a delay must be strongly significant. It should be discussed.
We have now included a text to the "Discussion" highlighting this sensitivity. However, we would like to argue that since the majority of settling particles do so at the onset of the settling period, a reduction in the duration will have smaller impact than the size of the reduction itself would imply.

In section 5.4 "However, a change in the relation between sea water temperature and the development rate of plaice larvae might result in accelerating the particle development, and therefore result in more realistic settlement figures in 1996". If I correctly understood the parametrization of larval duration in function of temperature is the one of Bolle et al. (2009). It has been based on experimental studies. Why this parametrisation could be 'bad' just for the year 1996?
We now realize that our statement was incorrect. The suggestion that the relationship between development rate and settlement could be solely responsible for the discrepancy between the observed and by our model predicted settlement figures is flawed. As presented in the manuscript, the present model set-up combines various assumptions, among which is the lack of mortality (also post-settlement) and the simulation duration limit, which all contribute to the relative lack of settlement in the Western Wadden Sea. We have removed this statement from the present manuscript, and we would like to thank the referee for bringing this up.

Technical corrections:
P. 1768 l.23 Burchard and Bolding (2002) instead of (Burchard and Bolding, 2002).
We have corrected it in the present version of the manuscript.

P. 1768 l. 25 and p. 1769 l.3 van Leeuwen et al instead of Leeuwen, van
We have changed this throughout the document, and also updated the position of this article in the list of references.

P. 1769 l.5 nautical miles instead of nautical mile
It has been corrected.

P. 1773 l.11-13. The position of the Dover Straits or the Doggerbank is not known by everybody. It should be added on the map. The same remark for the Wadden Sea (p. 1774 l. 10) and for Marsdiep-Vlie (p.1777 l.16).
We have now included a figure showing the position of the different areas and locations (Fig. 1).

P. 1775 l.23 interannual instead of intern-annual
We have now corrected this.

P. 1776 l. 9 "The total number of settling particles showed only limited variability (Fig. 11)". Seems obvious because it represents the number of particles released (106).
We think that we might not have made it clear that the number of settled particles in this figure is the blue part of the graph. We clarified this in the text. The number of settling particles is, apart from the number of particles released, also the result of variability in forcing conditions. The purpose of this sentence was to highlight the fact that although forcing conditions change significantly over the years, the actual settlement figures remain fairly constant.

P. 1777 l.13 The characteristics

Corrected.

P. 1777 l.14 replace is presented by are presented
Corrected.

P. 1778 l. 25 “the right conditions”. Which conditions?
We have added a comment pointing out that these conditions would entail the right wind conditions.

P. 1779 l.1 “visible for instance in” instead of “visible for for instance”
Corrected.

P. 1779 l.4-6 Sentence “The arch . . .c and d)” not clear. Needs more explanation.
We agree with the referee, this sentence has now been re-written.

P. 1784 l.9 “Similarly to the addition of a velocity threshold discussed in section 3.5”. There is no section 3.5. I suppose that you mean a depth threshold and not a velocity threshold.
This is correct, we discovered a mistake in the numbering of the separate paragraphs. The section “Inter-annual variability” should be a subsection of “Results” along with “Settlement in the Western Wadden Sea” and “Sensitivity Analysis”. This has now been corrected. The sentence containing this statement has now been removed, as it was confusing.

P. 1784 l.14 good instead of goed
Corrected.

P. 1787 l.26 van Leeuwen instead of Leeuwen, van
Corrected.

P. 1791. Fig. 2. Add the label for latitude
We have re-created the figure (Fig. 3 in the present document), which now also show the y-axis label. (We noted the same mistake in Fig. 17 (now 18), where it has also been corrected.)

P. 1792. In the legend “during the drifter-buoy” instead of “during the dirfter-buoy”
Corrected.

P. 1792. In the legend “around the drifter” instead of “around the the drifter”
Corrected.

P. 1792. In the legend “circle around the central” instead of “circle around the the central”
Corrected.

P. 1794. In the legend “particles that eventually settle”. Do they settle or not?
Yes, the particles do settle. We agree that the “eventually” was not useful and has been dropped.

P. 1803. The figures a, b, c, d, e and f should be identified
Done. This has been changed.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/10/C949/2014/osd-10-C949-2014-supplement.pdf

Interactive comment on Ocean Sci. Discuss., 10, 1765, 2013.

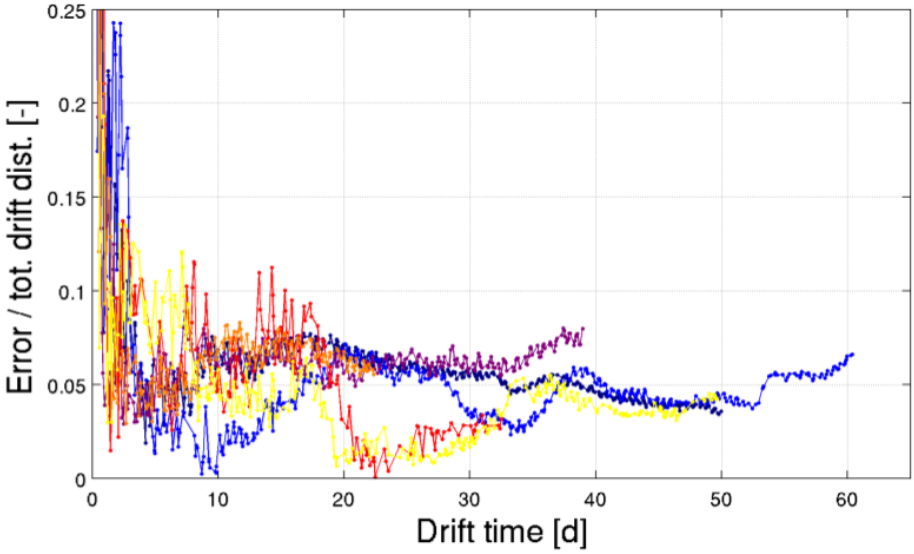

Fig. 1.

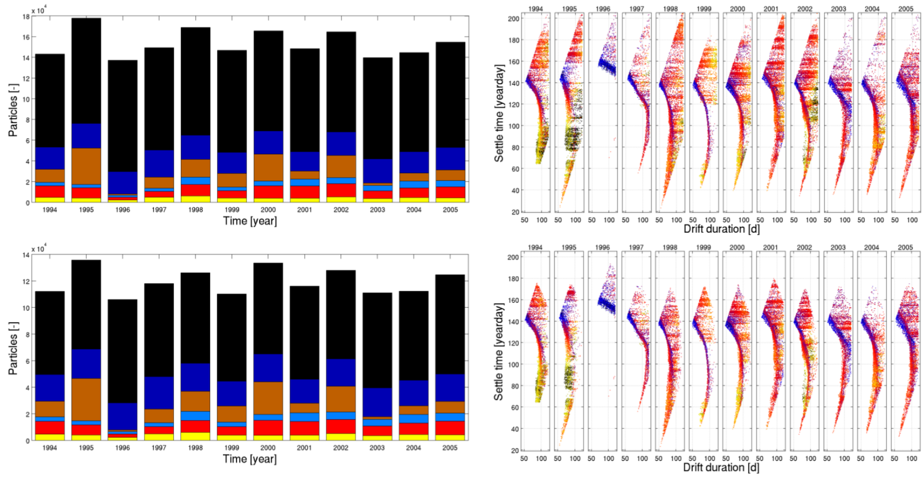

Fig. 2.