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Comment

## ***Interactive comment on “The effects of climate change on persistent organic pollutants (POPs) in the North Sea” by K. O’Driscoll et al.***

### **Anonymous Referee #1**

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#### General comments:

The paper contributes some new understanding on possible impact of future climate change on the distribution and fate of POPs in the North Sea. The modeling methodology is adequate and the research questions addressed are relevant and interesting. The overall quality of the discussion of the results is poor since it does not relate to other publications in this field and does not discuss the significance of the climate change impact on contaminant fate. The explanations of the (however fairly small) observed differences between different climate conditions are generally clear and easy to follow. What is mainly lacking are scientifically interesting conclusions drawn from the results. The results are not put in a wider context. Results are mainly reported and explained, not really discussed.

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## Specific comments:

### Title

The title is a bit simplistic, should maybe extend to something like “The effects of climate change on levels/concentrations/distribution/fate of POPs in the North Sea”

### Abstract

The abstract is not complete. Only the ocean circulation model (HAMSOM) is mentioned by name, but not the POP fate and transport model (FANTOM). The language is a bit unclear, for example the use of “in situ” concentrations instead of “initial concentrations” (Rows 6-7 p 1526). Row 9 p 1526 “... our approach was to reutilize 2005 values”, state where these concentrations come from also in the abstract (i.e. measured or modelled?). Try to avoid diffuse statements like “Dry gas deposition and volatilisation of  $\gamma$ -HCH increase in the future relative to the present” (Rows 16-17 p 1526), give a number on how much, for example average % of present or similar, and also mention what aspect of climate change that caused this effect. “PCB 153 in sediment decreases exponentially in all three runs, but even faster in the future, both of which are the result of climate change” (Rows 19-20 p 1526) unclear what is meant by “even faster in the future”. Was the conclusion really that this decrease was mainly due to climate change? Degradation in sediment is mentioned in the results section as an important process, and this is not clearly dependent on climate change and would occur even if the climate was not changing. It is not clear from Fig 4-6 that the decrease in mass in sediments of PCB153 is faster in the 2090-2099 scenario compared to the other scenarios as stated in the abstract.

### Introduction

The introduction lacks several crucial components. First, it is not adequately motivated why this study is necessary. The first paragraph is not logical. The fact that (many, but not all!) POPs bioaccumulate in many organisms is not a good motivation to why the

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impact of climate change on POP levels is necessary to assess. Instead, the introduction should discuss what processes that determine the fate of organic chemicals that may be influenced by future climate change and to what extent.

Second, a number of previous modeling studies of POPs in the North Sea are listed, which is good, but is not stated what were the conclusions or key-results of these studies that can be related to the current study. For example, what knowledge is still lacking and how will this study fill these knowledge gaps? Do the previous studies address climate change for example? Some key publications are also not referred to in the introduction. Although not focusing specifically on coastal and shelf areas and not using high resolution models, there are publications addressing POPs and climate change which should be mentioned here. For example, the review by Gouin et al 2013 (Env TOx Chem 32:20) (this publication state that the direct impact of climate change on POP levels in the environment are in general fairly small, merely a factor 2 in most cases) and the modelling study by Lamon et al 2009 (ES&T 43:5818) addressing PCBs in future climates could be mentioned.

The methodology is adequate, although using the same forcing and initial concentrations in all scenarios is not realistic. It is understandable and makes it possible to look at climate change impact only, but the actual years chosen become meaningless.

#### Model description

The authors refer to their previous publication with a more detailed model description. However, they repeat the description of air-sea gas exchange. Units are not given, and it is not stated what “D” is. It is also not logical to repeat the description only for this process, since other processes turn out to be important for POP fate later in the manuscript, for example sinking of POC, resuspension, degradation in sediment etc.

It is not completely clear, but I assume that constant concentrations were used in air and rivers throughout all simulations?

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The statement that only one climate scenario is used (and not several) because the authors are interested in climate change impact only is not logical. This can be done, and would perhaps be done better, by using several climate scenarios and run simulations for a longer time period. “Again, we stress that we run only one climate scenario, because we are interested in the impact of the climate on the fate of the POPs in the North Sea. For investigating the future development of the POPs, different projections and even ensemble runs would be necessary” (Rows 12-15 p 1531).

## Results

The results are clearly discussed and the mechanisms behind observed changes are explained in a good way. However, in general the significance of these differences is not discussed, and the results are not related to findings by other researchers. This should be improved.

Some specific comments:

Results for hydrodynamics are in general relevant, but the salinity reduction is unnecessary to mention unless the salinity is related to e.g. POP solubility in water in the model?

Avoid using words like “quite” as in “which are quite important for our POP dynamics” (Row 12 p 1532).

The scale in Fig 3 c and d is difficult to read.

Row 25-28 p 1532: It is not explained why there are seasonal differences in dry gas deposition. Is this the result of emission seasonality or other mechanisms, e.g. temperature dependence of atmospheric concentrations? Authors should mention why dry gas deposition is increased (i.e. changes in temperature and wind speed, and hence the mass transfer coefficients?) and that it is the increased mass in the water column that leads to increased volatilization for HCH.

How can the decrease in total mass in water in the first year of all runs be consistent

with the 1996-2005 trend, which is due to decreasing atmospheric concentrations and river input, if these concentrations are held constant in the future simulations? Or are these concentrations not constant? Maybe clarify this.

Row 29 p 1534 “These events are also found in the sediment and  $\gamma$ -HCH records.” Please clarify, are the authors referring to measured data? Please give a reference in that case. Row 11 p 1535: “During storm events, erosion causes mobilisation and resuspension of PCB 153 into the water column, increasing its concentration there, thus resulting in a consequent increase in volatilisation.” At what depths can storms resuspend bottom sediments? How big fraction of the North Sea bottoms will be resuspended in this way? A brief discussion of this issue would be helpful here.

Row 16 p 1535: “This is in contrast to  $\gamma$ -HCH, which is generally net depositional from atmosphere to ocean, has total mass in water more than two orders of magnitude greater than that in sediment”. Looking at Fig 4-6, this is only true for the 2006-2015 scenario, and only the last 10 years or so.

Section 3.4: In general, very small changes in annual mass fluxes are observed due to the climate change. But this is not commented!

Section 3.5: Row 3 p 1538: “First, concentration in gas in the atmosphere is greatest along the British coast”, re-word this sentence. Again, it is not clear what scenario was used for the atmospheric and riverine concentrations. Are these concentrations decreasing during the simulations?

Figure 9-11: Using the absolute difference makes it difficult to judge the significance of the differences. An increase of at maximum 0.05 ng/L seems negligible. It would be better to use the ratio between 2005 and the other years.

Last sentence in this paragraph (Row 17 p 1532): “It is the effect of these changes on the studied POPs that are analysed.” It is not clear which changes that the authors refer to.

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## Summary and conclusion

This section is actually only a summary, and no conclusions are really drawn about the results. It should here, if not before, be discussed how important the simulated differences are, what this means for management of organic pollutants or for organisms exposed to these chemicals. All conclusions are statements like “more than 2005” or “less than 2005”, but the magnitude of these differences is not specified. This is a problem since the differences are actually very small, and therefore may have insignificant implications for management, and very little value for helping policy makers making well informed decisions.

Another issue with this paper is the lack of references to other publications in this field. Findings in this model experiment should be compared to results of other studies and it should be discussed how reasonable the results are. The entire results and discussion section is devoid of references to other papers but O’Driscoll’s previous paper and the paper where the atmospheric fate is simulated (Gusev et al).

### Specific comments:

Row 10 p 1541: “. . .result directly from changing hydrodynamic, climate, conditions and forcing.” This sentence does not make sense. It does not state what exactly impacts the dry gas deposition and volatilization, it just mentions basically everything that is changing in the model. In particular “conditions” is very general. . .

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