

Anonymous Referee #2 comment (1):

General comments:

The authors address the effects of global climate change on POPs on the North Sea. They use two highly resolved models for ocean circulation and chemical fate, respectively, following an approach that has previously been used and validated in the literature. The topic is of high relevance and compatible with current discussions in the literature. Unfortunately, this literature is not cited here at all, neither there is a discussion of how the results fit into context with recent studies on climate change and POPs.

Answer (1):

We are very grateful to the reviewer for all comments and suggestions concerning our paper. We will incorporate into the resubmitted version of the paper, helping to improve the manuscript.

We will improve the quality of the discussion of the results by relating to other relevant publications in the field, while discussing the significance of the impact of climate change on POPs.

Anonymous Referee #2 comment (2):

My biggest concern about the methodology is that the authors model the effects of a global process (climate change) on a regional scale (the North Sea), without considering the forcings that act from outside on the regional system, such as the long-range transport of air pollutants, change in primary and secondary emissions on a global level, and other global processes such as sea-ice retreat that will have a global effect mediated by long range transport. Rather, the authors limit their analysis to forcings that result from regional changes in response to global change. In this way, the analysis is reduced to a process study, which also is interesting, but which is not informative in terms of the impact of global climate change on changing POP concentrations in the regional ecosystem of interest. Thus, also the manuscript title becomes misleading.

Answer (2):

The reviewer is correct, we did not consider external conditions, such as change in primary and secondary emissions on a global level. We did, however, consider global forcing: the regional model is forced by a global climate scenario model and the ocean open boundaries have also been acquired from a global ocean circulation scenario.

We will also add the following:

Concerning external pollutant conditions, since, to date, we have only vague ideas about future input changes of POPs through the atmosphere or rivers (net North Atlantic input into the North Sea is negligible), we cannot perform a projection for the POPs as we do for the climate itself. When considering changes in both climate and POP inputs, we would not be able to distinguish which of these is responsible for changes in POPs.

We will edit the title based on the revision and the suggestions of both anonymous referees.

Anonymous Referee #2 comment (3):

A further major drawback of the paper is that the authors do not discuss their results in the context of uncertainty. For example, they mention effects of a few percent of change, without acknowledging that this effect is quite small in comparison to input-parameter uncertainty.

Answer (3):

We will address the issues of uncertainty in the results and discussion section of the reworked version, in particular with respect to uncertainty due to climate change versus that of the input parameters (POPs).

Anonymous Referee #2 comment (4):

My suggestion would be to not publish the article in the way it is now. Either it should be rewritten to make clear that regional and local processes are studied, not so much a global effect, or the global forcings stated above should be included in the modeling, which would be a major revision.

Answer (4):

We are prepared to undertake the work suggested by the reviewer and include it in a revised manuscript. The revised manuscript will make clear that regional and local processes are studied. We realise that this work constitutes a major revision.

Anonymous Referee #2 comment (5):

Specific comments:

In their literature review, the authors fail to recognize the recent studies on the climate change effect on POPs. This would be also very beneficial for putting their own research into context. Some of the works that I would have liked to see referenced here and linked to the presented results are:

- Lamon, L.; von Waldow, H.; MacLeod, M.; Scheringer, M.; Marcomini, A.; Hungerbuhler, K. Modeling the global levels and distribution of Polychlorinated Biphenyls in air under a climate change scenario. *Environ. Sci. Technol.* 2009, 43, (15), 5818-5824.

- Gouin, T.; Armitage, J.; Cousins, I.; Muir, D.; Ng, C. A.; Reid, L.; Tao, S. Influence of global climate change on chemical fate and bioaccumulation: The role of multimedia models. *Environ. Toxicol. Chem.* 2013, 32, (1), 20-31

- Armitage, J. M.; Quinn, C. L.; Wania, F. Global climate change and contaminants-an overview of opportunities and priorities for modelling the potential implications for long-term human exposure to organic compounds in the Arctic. *J. Environ. Monit.* 2011, 13, (6), 1532-1546.

- Wöhrnschimmel, H., MacLeod, M., Hungerbuhler, K. Emissions, fate and transport of persistent organic pollutants to the Arctic in a changing global climate. *Environ. Sci. Technol.* 2013, 47, 2323–2330.

Answer (5):

We thank the reviewers referring us to these recent publications. We will refer to these publications to put our own work into context.

Anonymous Referee #2 comment (6):

Page 1533, line 8-15: here, the authors focus on interannual variability in the present and the future model runs, as they also do in other parts of the manuscript (page 1534, line 2-5; page 1536, line 1-9). It seems a bit pointless to me to discuss high and low values in specific years, if this is a result of re-using the same input parameterization for atmospheric concentrations and river input for future model runs. This way, the present day fluctuation is only propagated to the future, and is no real effect of interest in the context of climate change.

Answer (6):

The reviewer is correct. We will remove discussion of inter-annual variability in the revised manuscript.

Anonymous Referee #2 comment (7):

Page 1534, line 16-17: here the authors address increased volatilization as a result of storms. This is an interesting feature, given that climate models forecast an increased frequency of storms. I haven't seen this aspect discussed in the literature before, so maybe this is a good topic to focus on. Caution should be applied, however, due to the high uncertainties of climate model forecasts with regard to storm frequencies.

Answer (7):

We will take the reviewer's comments into consideration in the reworked manuscript. We will refer to the following paper: Weisse R, et al (2009) Regional meteorological-marine reanalyses and climate change projections: Results for Northern Europe and potential for coastal and off shore applications. Bulletin of the American Meteorological Society, 90, 849-860.

Anonymous Referee #2 comment (8):

Page 1536, line 11-20: here the authors describe qualitatively the effects of climate change on POP concentrations. In their wording, they use several times very vague descriptors ("diminishes somewhat", line 11; "the small increase", line 18; etc.). It would be desirable to be more quantitative here, and state in comparison to what the changes are small. Are they small in comparison to interannual variation? Or compared to input-parameter uncertainty? (See also my general comment on uncertainty).

Answer (8):

We agree. In the revised manuscript, we will be more quantitative, and clearer, with respect to these effects.

Anonymous Referee #2 comment (9):

Page 1538, line 3-14: here the authors explain the drop of gamma-HCH concentrations in front of the British coast. They argue with decreasing concentrations in the atmosphere and in the river influx previous to 2005. This is highly confusing to me, given that the authors state earlier in their paper (page 1530, line 21 and following) that they used the same atmospheric and riverine concentrations of the year 2005 as initial condition for every model run. It rather appears to me that we are observing a temperature effect (see also figure 3, panels a and b).

Answer (9):

Since the feature is visible in 2015, as well as 2055 and 2099, we think it is not a temperature effect (temperatures have not increased much in 2015 relative to 2005). Rather, since river and atmospheric concentrations were reduced up to 2005, the result is that concentrations in the water column decrease up to the end of 2005. Although 2005 values are used repeatedly in the three 10-year time slices (2006-2015, 2046-2055, 2090-2099), values are less in 2005 because the response is not instantaneous. The model has a response time, that is, there is a recovery time for concentrations to become steady. Since 2015, 2055 and 2099 are at the end of 10 years of steady input, the concentrations have become steady, to 1st order, at this time. We will clarify this point in the manuscript.

Anonymous Referee #2 comment (10):

Page 1540, summary and conclusions section: This section is very repetitive with the results section. I am missing some discussion on the results in the context with previous research, the significance of these results for the debate on climate change and

chemicals, and a discussion on the uncertainties inherent to these results.

Answer (10):

We will include and clarify the significance of these results for the debate on climate change and chemicals together with a discussion on the uncertainties inherent to these results in the discussion. The summary and conclusions section will be less repetitive with the discussion section.

Anonymous Referee #2 comment (11):

Technical corrections:

Page 1537, line 11: It is misleading to say that degradation decreases, since degradation increases with temperature. The authors might want to clarify this by referring to “degradation flux”, instead.

Answer (11):

We will clarify this.

Anonymous Referee #2 comment (12):

Page 1549, caption to figure 2: indicate explicitly what is shown in panel “c” and in panel “d”

Answer (12):

Done.

Anonymous Referee #2 comment (13):

Page 1550, panel 3 of figure 4: If you reduce the range of the y-axis to {0, 0.8}, the data would be better visible (less overlay).

Answer (13):

Done.

Anonymous Referee #2 comment (14):

Page 1554, caption to figure 8: This is not a histogram!!!! It's a barplot! You actually don't have to state what it is, rather what it shows!

Answer (14):

Done.

Anonymous Referee #2 comment (15):

Page 1555, figure 9: this figure might be more informative if plotted as relative change instead of as difference of concentrations. In this way the relative importance of the effect could be shown.

Answer (15):

We will do this. See answer to Anonymous Referee #1 comment (21).