Response to Referee #1

Jan-Victor Björkqvist et al., 24.9.2021

The referee comments are marked in blue italics, while our responses are in normal font.

General comments

R#1: *I have twofold feelings about this manuscript.*

On the one hand, it is a nice insight into the structure of wind waves in the Baltic Sea that highlights several interesting features, such as short duration and thus great intermittency of formal swell events, extensive spatial variation in the probability of predominance of formal swell events, low swell heights, overall short periods of swell waves that almost overlap with the typical periods of windseas (most frequently 2–4 s), frequent co-presence of windsea and swell.

On the other hand, some established features strongly signal that attempts of partitioning of the Baltic Sea wave fields into windsea and swell do not necessarily lead to sensible results and may even be deceptive. Even though many such aspects are discussed professionally, the ramifications of possible misunderstandings are not really made clear for the reader.

Our response: Thank you for agreeing to review our manuscript. It is greatly appreciated. We agree that swell in the Baltic Sea might not fulfill the expectations one might have from looking at swell in oceanic conditions. We still have to disagree with the statement that the results would not be sensible. That said, the distinction you raised in the comments should be better clarified to avoid confusion. We have reworked the manuscript to this end. Please also see our response to the first specific comment.

Specific comments

R#1: *One of the main results is that the areas where swell wave energy predominate form only a narrow strip along some 50% of the length of coastlines of the Baltic Sea (Fig. 3). This feature is counter-intuitive because energy of (longer) swell waves should decay faster in the shallow area than energy of (shorter) windseas. Also, the (longer) swell waves experience stronger refraction in areas with variable depth than (shorter) windseas. This effect also leads to systematically faster decrease in the energy of swell waves in the nearshore. Differently from the open ocean conditions, the Baltic Sea waves often approach the shore under relatively large angles. It is thus likely that this effect is stronger in the Baltic Sea compared to its impact on the open ocean shores.*

As I am sure that there is nothing wrong with the simulations and the evaluation of the share of swell, the results essentially prove that the definition of swell (that has been derived for the open ocean conditions) is simply not applicable for the Baltic Sea. This *aspect should definitely be clarified for the benefit of readers outside the Baltic Sea basin.*

An attempt to explain this feature in the last paragraph of Section 3.4 (lines 164–173) is somewhat unfortunate and basically expresses the same point. In particular, I strongly disagree with the statement on line 174 that "The coastal locations are more heavily and constantly tainted by swell" as practically the same wave system that exists a dozen km from the shore approaches the coast – and is just renamed swell because a decrease in the wind speed in the nearshore. This aspect is mildly expressed in Discussion on lines 182–184 but not really made clear.

A possible misunderstanding is reinforced in Conclusions by saying on line 283: "swell was mostly created simply by a decaying wind, turning the existing wind-sea waves to swell in the partitioning". It would be correct to say: the wind-sea was just interpreted as swell in the nearshore in the used framework even if its properties did not change. Also the conjecture that the probability of having swell-dominated wave fields (expressed in terms of the correlation between swell and windsea height) substantially changes from offshore to nearshore belongs to the same pool of ideas and apparently reflects the "impact" of the particular swell definition.

In this context, part of Discussion is also deceptive. The text on lines 213–218 relies on the usually distinctly different properties of swell and windseas in the open ocean. The presentation has made clear that "swell waves" is just a name for the same wave system, with the same period; just wind has ceased. Also, Wang et al. (2014) look at the global ocean where swell waves are usually longer and well organized, closed to monochromatic ones – that is typically not the case of Baltic Sea swell (as proved by the authors). The real point to address is formulated on lines 236–237: "We found that in the Baltic Sea even ECMWF's simple partitioning flagged a minimal amount of energy as swell during the growth phase of the wave field, resulting in spurious non-zero swell weights".

In general, it seems that the authors are partially victims of the major success of high-resolution wave simulations in the Baltic Sea. The spatial resolution is now so high that effects that are not visible in global wave hindcasts start to play a large role in the interpretation of the local results. The overarching conjecture from the manuscript could perhaps formulated as follows: the existing separation methods of windsea and swell make only sense for offshore conditions, and should not be used in the nearshore and in archipelago areas.

Based on the above considerations, I recommend reshaping the discussion and conclusions so that the situation would be unambiguously clear for non-experts in the field. It may make sense to add a few sentences that clarify the difference between citizens' perception of swell as organised, highly directional, and often almost monochromatic wave field and the wave modellers' understanding of swell as some collection of wave components.

Our response: We agree that the difference between the conceptual perception of swell waves doesn't necessarily coincide with the quantitative definition used in e.g. wave models. However, the quantitative definition still has real physical meaning, and the conceptual definition is hard to quantify. In addition, the results derived from the World Ocean will also include these "old wind sea" events, even though a large part of the swell energy might come from waves that also fulfill the more subjective definition.

We do not agree that the definition of swell is "not applicable" in the Baltic Sea, since the definition is based on a real physical criteria that affects e.g. wind-wave growth. Our results simply show that the types of waves that fulfil the generally accepted definition of swell are not similar to the typical conditions in the oceans. We don't agree that the minimal energy that the ECMWF criteria flags as swell is a serious issue, since it has no significant impact on the results. Also, these artefact might well exist also elsewhere, but because of its minimal amount it is revealed only in sheltered areas that can be completely free from remote swell. It more shows that the quantification of swell is a complicated issue (e.g. Portilla et al., 2009), and that using even more elaborate schemes to separate swell is probably not warranted in the Baltic Sea.

The question of if waves are simply "renamed swell" after the wind ceased, or if they "become swell" after the wind ceased is almost a semantic argument. As mentioned, we do still agree with your point that this distinction should be made, and especially the last sentence of your comment nailed it. We have revised the manuscript, starting from the abstract and introduction, to 1) make clear that this distinction between "conceptual swell" and "quantitative swell" exists, and 2) make clear that we are using the latter definition in this paper. We have also modified the Discussion and Conclusions to reflect this. The main additions to the manuscript are:

Start of the **abstract**:

"The classic characterisation of swell as regular, almost monochromatic, wave trains doesn't necessarily accurately describe swell in water bodies shielded from the oceanic wave climate. In such enclosed areas the locally generated swell waves still contribute to processes at the air and seabed interfaces, and their presence can be quantified by partitioning wave components based on their speed relative to the wind."

Added paragraph to the **introduction**:

"Conceptually swell is often thought of as regular and long-crested waves that are almost monochromatic and highly directional (e.g. Holthuijsen, 2007, page 47). In the World Ocean this characterization is apt (Barber and Ursell, 1948), and might also coincide with a layperson's view on swell. Nonetheless, in wave measurements and wave models swell needs to be quantified, and the definition of swell is typically (loosely speaking) taken as waves outrunning the wind (e.g. Bidlot, 2001). As a result, swell is taken simply as a selection of wave components that fulfil this criteria. While the conceptual regular swell waves also fulfil the quantitative criteria, the opposite is not necessarily true – although this is subjective. Be that as it may, the quantitative criteria is

still well motivated from a standpoint of air–sea interaction, including wind-wave growth (e.g. Komen et al., 1984; Kahma et al., 2016).”

Addition to first paragraph in **discussion**:

“While these waves fulfill the swell criteria, they might differ from a classical concept of swell as ordered, almost monochromatic and directional, waves generated by a distant storm. Our results show that even without remote swell the amount of swell – as defined using Eq. 5 – can still be significant (Fig. 3). Although differentiating between different types of swell is complicated, this kind of old wind-sea is bound to be present also in swell statistics compiled for the World Ocean.”

Addition to the first paragraph of the **conclusions**:

“The partitioning scheme used a typical criteria based on the wave phase speed relative to the wind speed (see Eq. 2). While this definition has a physical motivation, it also identifies waves that do not fit our conceptual, somewhat idealised, view of long crested, monochromatic and unidirectional wave trains. “

R#1: *Technical issues: A short (not full) list of minor aspects that might need attention:*

Line 38: The reference to Semedo et al. (2014) is to a certain extent deceptive as this source is visible only as an abstract. Also, the poster available in internet has a different team of authors: Alvaro Semedo, Roberto Vettor, Oyvind Breivik, Andreas Sterl, Magnar Reistad, Daniela C.A. Lima, Carlos Guedes Soares.

Our response: Thank you for pointing this out. Since all of the authors listed in the abstract are also listed on the poster (but not vice versa), we decided to remove the details concerning the abstract. The citation is therefore now unambiguous and all authors are still credited for the work. We acknowledge that posters are typically not cited, but in this instance there exists no proper literature that could be cited instead.

R#1: *Lines 45–46: The information on these lines is highly cryptic. Please explain the acronyms.*

Our response: We have now explained the acronyms.

R#1: *Line 51: The acronym ERA5 also requires an explanation and a reference.*

Our response: We have added the explanation and the reference to the article.

R#1: *Line 84, Equation (5) and also below: it is recommended to unify the use of “S”/”s” as subscript for quantities that characterize swell.*

Our response: The only variable that uses a subscript to characterize swell is the swell weight, WS. The use of a capital S here is in line with Semedo et al. (2011). In other cases we explicitly spell out “swell” or “sea” as a superscript, with a lowercase “s” simply signifying that it is the significant wave height. The use of superscripts is also in line with

Semedo et al. (2011), although they use "s" and "w" instead of "swell" and "sea". Nonetheless, we see no possibility for confusion here, and have therefore decided to keep the notation as is.

R#1: *Line 85: probably "dominated".*

Our response: Yes, this was indeed what we meant. It has now been corrected to the manuscript.

R#1: *Line 99: it is recommended to use the Lithuanian "dotted e" in KlaipÄ−da.*

Our response: Thank you for bringing this to our attention. This has been corrected throughout the manuscript, including the figures.

R#1: *Line 107, also caption to Fig. 1, line 111, 114: the superscript "th" should use normal font, not italics.*

Our response: We found five instances of this in the text. They have all been corrected.

R#1: *Figure 1, explanations to both scales: "H_s" should be in italics; also the upper scale should have a space between H_s and (m).*

Our response: This has been corrected in the figure.

R#1: *Caption to Fig. 2: should be "storm"*

Our response: Thank you for pointing this out. We have now correct *"took plave during the sotrm"* to *"took place during the storm"*.

R#1: *It is recommended to use "Baltic proper" as it is not really a proper name; however, there exists also a tradition of capitalizing "Proper".*

Our response: Our group has had the convention of using the capitalized form, and we therefore prefer to keep it as it is. Although we do appreciate you bringing this to our attention.

R#1: *Line 118–119: "the averaged swell direction were" is inconsistent*

Our response: This has now been changed to "averaged swell directions were"

R#1: *Lines 128–129: the conjecture "In the Baltic Proper the highest swell weights were along the eastern coastlines, which is expected because of the prevailing southwesterly winds (Karagali et al., 2014)" is only partially true. In fact, the NNW winds also add substantially to the wave fields so that the overall maximum wave height (Björkqvist et al., 2018) and particularly the maximum of wave energy flux (Nilsson, E., Rutgersson, A., Dingwell, A., Björkqvist, J.V., Pettersson, H., Axell, L., Nyberg, J., Stromstedt, E. 2019. Characterization of wave energy potential for the Baltic Sea with focus on the Swedish Exclusive Economic Zone. Energies, 12(5), 793, doi: 10.3390/en12050793) are located between Gotland and the Gulf of Gdansk. It might be also mentioned that refraction* *plays usually a larger role in the propagation of (longer) swell than for (shorter) windseas in the relatively shallow Baltic Sea.*

Our response: It is true that a prevailing wind direction in itself does not necessarily mean that the highest winds (and waves) are from the prevailing direction. There is, however, a well established east-west asymmetry in the Baltic Sea wave climate, with larger waves being found more to the eastern part of the main basin (Tuomi et al. 2011, Fig. 11; Björkqvist et al. 2018, Fig. 5; and our Fig. 1c in the manuscript). This is a result of the strong winds from the sector between west to south. For the wave energy potential Nilsson et al. 2019 (Fig. 8) showed that the highest wave energy fluxes were also modelled for southerly and westerly wave directions.

It is true that the highest waves in the Baltic Sea are probably between Gotland and the Gulf of Gdansk. Nonetheless, they are typically from the west-to-south sector, thus ending up on the eastern coasts of the Baltic Sea main basin.

We have edited the sentence slightly to make it clearer, and it now reads:

"In the Baltic Proper the highest swell weights were along the eastern coastlines, which is expected since the prevailing southwesterly winds (Karagali et al., 2014) lead to an east–west asymmetry in the wave climate of the Baltic Proper (e.g. Tuomi et al., 2011; Björkqvist et al., 2018, and our Fig. 1c)."

We have added a mention of the stronger refraction of the swell compared to wind waves to the section dealing with wave directions.

R#1: *Line 139: please explain the abbreviations NBP and GoF.*

Our response: This has been corrected in the manuscript.

R#1: *Line 237: remove one "the" in the middle of the line.*

Our response: This has been corrected in the manuscript.

Response to Referee #2

Jan-Victor Björkqvist et al., 24.9.2021

The referee comments are marked in blue italics, while our responses are in normal font.

***R#2:** Swell waves play a critical role in air-sea interactions. In this study, some interesting results of the swell in the Baltic Sea are drawn based on 20 years of high-resolution wave simulation data. These results are interesting to the Baltic Sea research community.*

Our response: Thank you for taking the time to review our manuscript. It is much appreciated.

I have the following comments/suggestions about the study.

General comments:

***R#2:** The wind condition varies significantly with the season which may result in the variation of swell probability and energy weight. Authors give the swell height distribution in winter and summer. I am wondering if there are any seasonal variations of the swell energy weight, swell probability, swell period. If so, I would recommend including those analyses?*

Our response: Thank you for this comment. We have updated the swell prevalence figure to show the seasonal variation of the swell energy weight and probability of swell dominance. Including the seasonal variation also for these variables makes our results more comparable to those of Semedo et al. (2011, 2015) for the World Ocean and Nordic seas. The updated figure is shown as Fig. 1 in this document, and will also be updated to the revised manuscript. The results, discussion and conclusions have been rewritten to incorporate the new seasonal results.

The relation between the periods and the significant wave height did not show any interesting seasonal variation. Longer waves are generated (and thus turned to swell) during the winter compared to the summer, but this fact is hardly surprising. The seasonal figures are shown in Fig. 2 & 3 of this document, but the figure in the manuscript was kept as is, since we feel that the seasonal variations did not warrant splitting the scatter plots up to several figures.

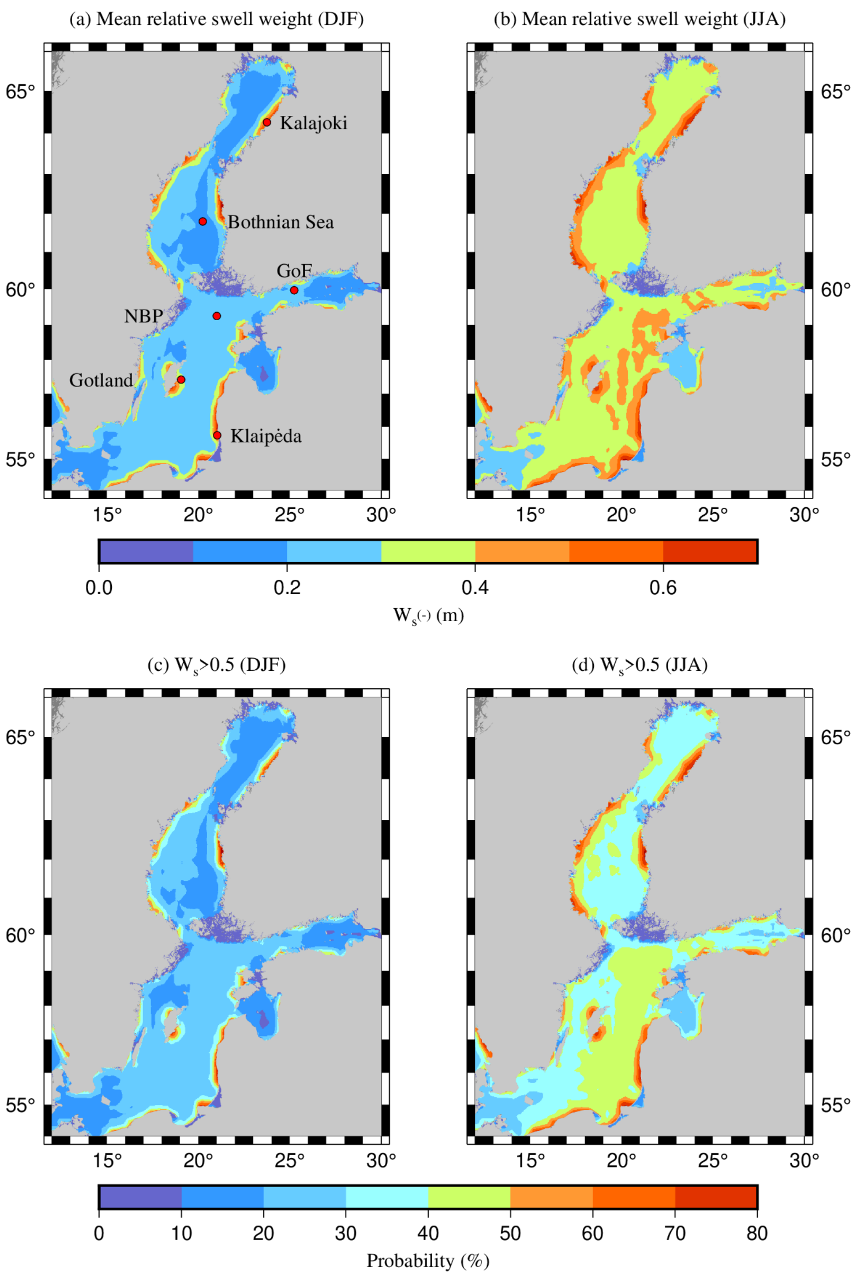

Fig. 1. The updated Fig. 3 of the manuscript where the swell weight and probabilities are separated by season.

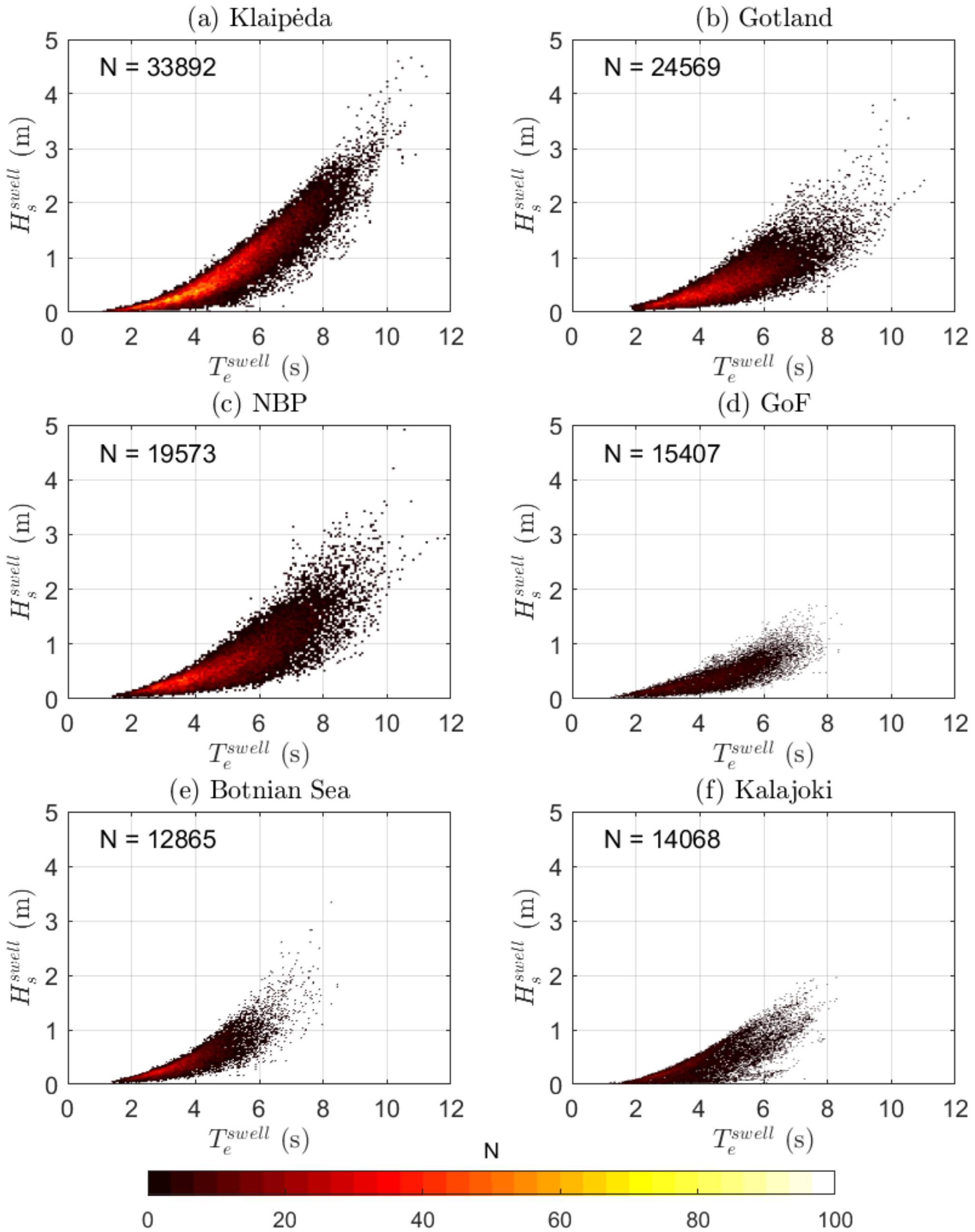

Fig. 2. Same as Fig 4. in the manuscript but only for the winter season (DJF).

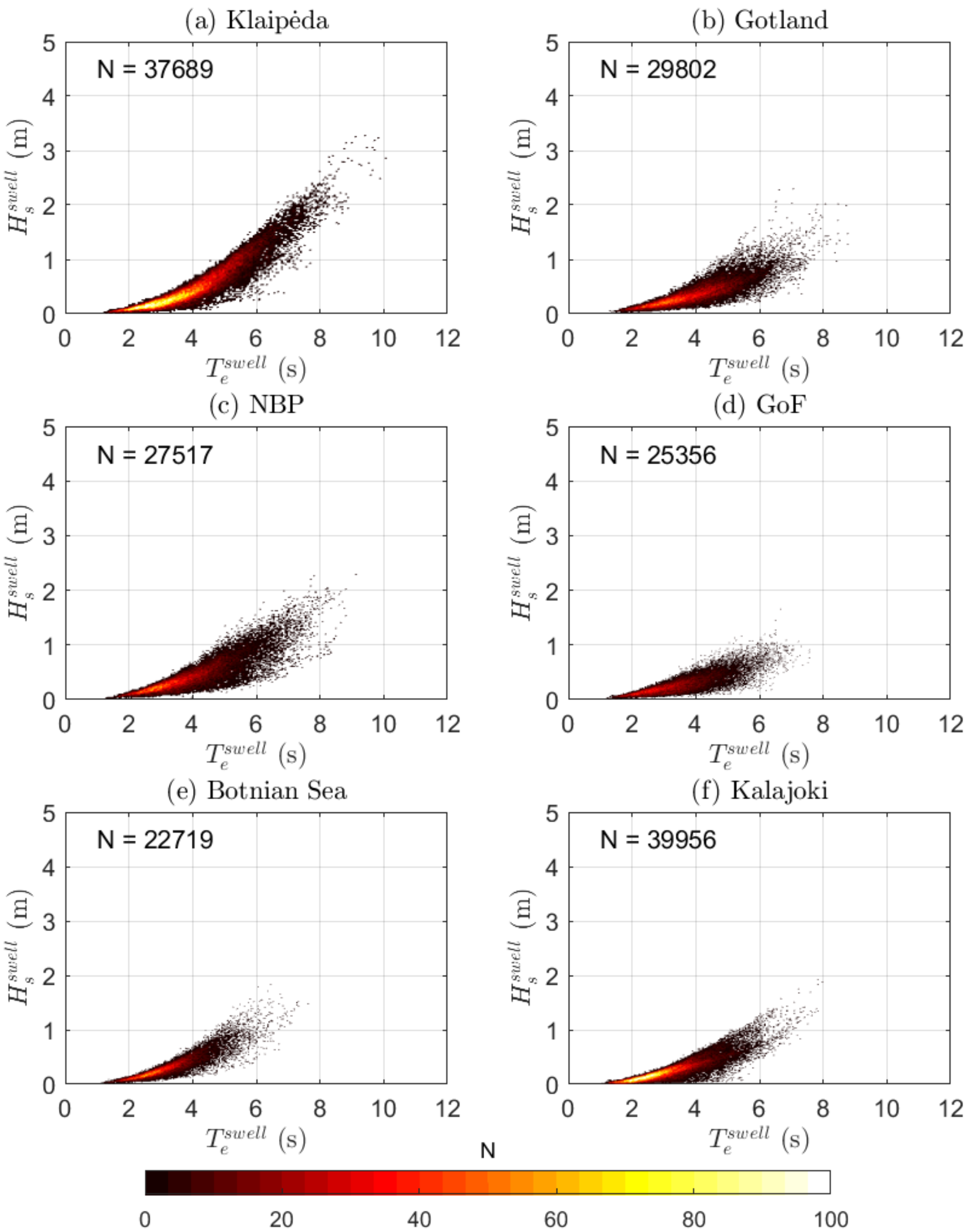

Fig. 3. Same as Fig. 4 in the manuscript, but only for the summer season (JJA).

***R#2:** In the introduction, the authors point out the importance of the misalignment between wind and swell direction (L20). With the data, I think the authors can analyze the distribution of the swell-wind angle. If so, I would suggest the author add one section about it.*

Our response: We calculated the distributions of swell-wind directional misalignments for the six locations. For all data (including also very low swell heights) the general distribution of misalignment is the same for almost all locations (Fig. 4 in this document). For the 99th percentile of the swell height we can see that the open sea points in the main basin behave differently from the coastal points. Namely, the most probable amount of misalignment in the open sea is 60-100 degrees, while at coastal locations the most probable misalignment is below 20 degrees.

These results reinforce the correlation analysis that the open sea and coastal zones have different generation mechanisms for swell. In the open sea high swell waves are generated when a weather system passes and the wind turns (and the wind speed simultaneously drops), as seen in e.g. Fig. 2 of the manuscript. In the nearshore areas the wind can simply attenuate near the coast, causing an increase in the wave energy classified as swell even though the weather system has not passed, and the direction might be well aligned with the wind direction.

We have added a separate section for the wind direction, and included the below figure and the accompanying analysis there.

R#2: *In section 3.4, the authors give some interesting results about the correlation of wind-sea and swell. The negative correction is contributed to the decaying wind. Based on Eq. 2, the wind direction is also an important factor determining if a wave mode is swell or wind wave. Did the authors have some analysis about the contribution of the variation of wind direction? If you look at Fig 2, the wind direction change is also significant for the variation of swell and wind wave height.*

Our response: Because of the cos-term in Eq. 2, the relative wind-wave direction is definitely an important factor. Typically drastic and sudden changes in the wind direction is expected to be related to the passing of a weather system. This is expected to be seen simultaneously also in the wind speed (see e.g. Fig. 2 in the manuscript). The analysis in the swell-wind misalignment (see Fig. 4 of this document) suggests that for the highest swell heights the directional difference between the swell waves and wind are large in the open sea. At the coast, again, the directional difference is typically small.

However, just because the directional difference is large, that doesn't mean that it would be the dominating factor. Even for a 90 degree difference the wind speed might simultaneously be low enough that a significant amount of energy would be classified as swell even if the wind and swell waves were perfectly aligned. In other words: what is the main reason for swell is the directional difference is close to 90 degrees and the wind speed is close to 0? Differentiating between the contributions of these two correlated sources is tricky. We therefore decided not to pursue this type of analysis beyond that presented in Fig. 4 and the newly added section on swell direction. The main results show the combined effect of these two connected phenomena (wind speed and direction) through the wind speed being projected to the wave direction in Eq. 2.

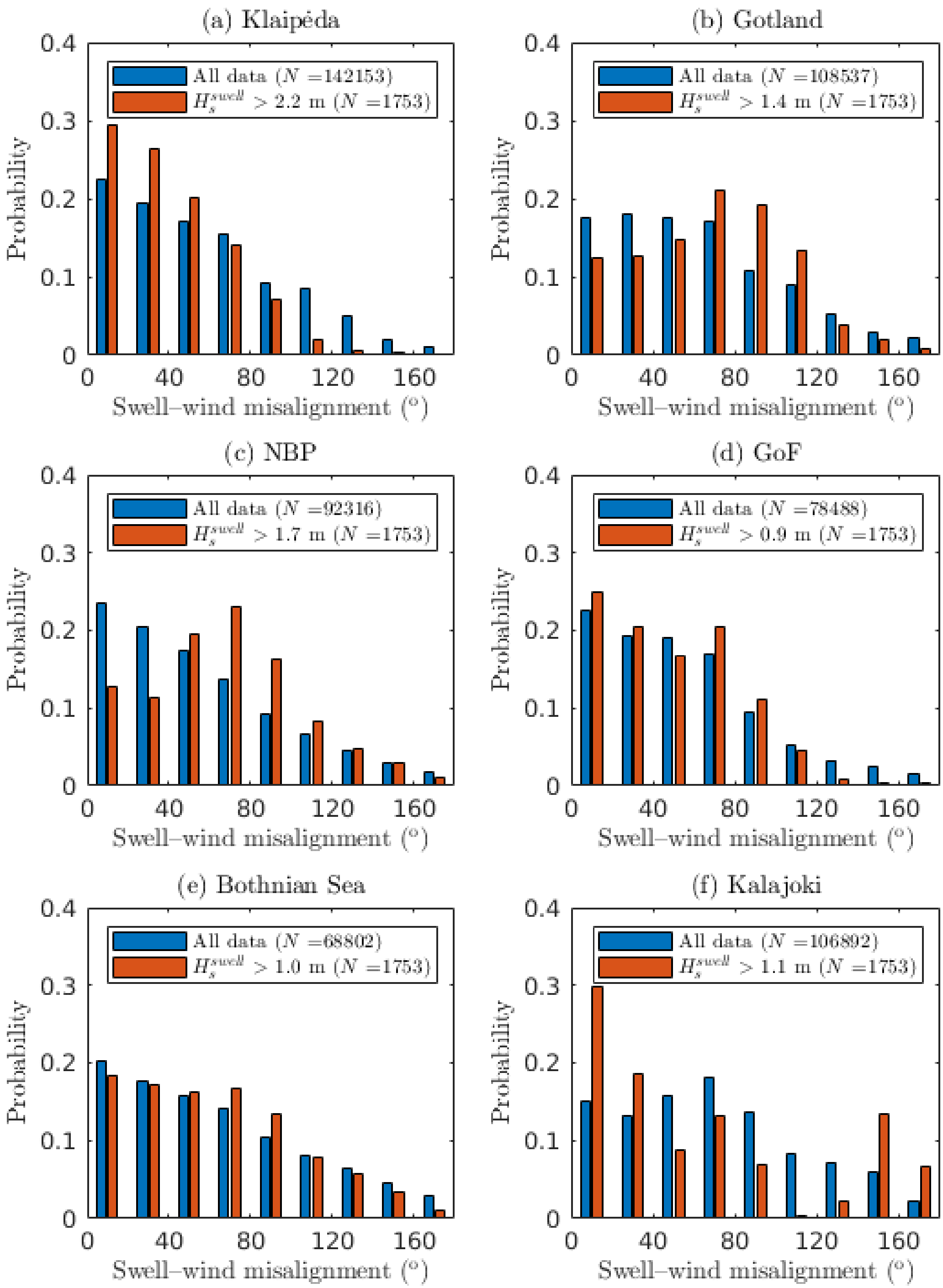

Fig. 4. The probability of differences in swell-wind angles. Swell weights over 0.05.The red bars signify cases where the swell height is above the 99th percentile.

***R#2:** In the analysis, the correlation is used to show the relation between swell and wind. For the correlation section, why use wind-sea wave height, but not wind speed? They may do not show a significant difference since wind wave height has a highly linear relationship with the wind.*

Our response: The correlation analysis was not done to show a relation between the wind speed and the swell, but rather the wind-sea and the swell. The idea is that in oceanic conditions (for distant swell), the swell height would be uncorrelated with the local wind sea. However, since distant oceanic swell is absent in the Baltic Sea, the question arose to which extent the swell is “distant enough” to be detached from the locally generated waves. This analysis gave us some insight into the structure of the wave field in the Baltic Sea, as presented in the results and discussion.

As you suspected, the strong connection between the wind speed and the wind-sea height means that the correlation structure calculated between the wind speed and the swell height is very similar to the results presented between the wind sea and swell (see Fig. 5 of this document). Since we were, in the end, interested in the structure of the wave field, we feel that including the wind speed at this stage obscures the point, although it would ultimately lead us to the same conclusions.

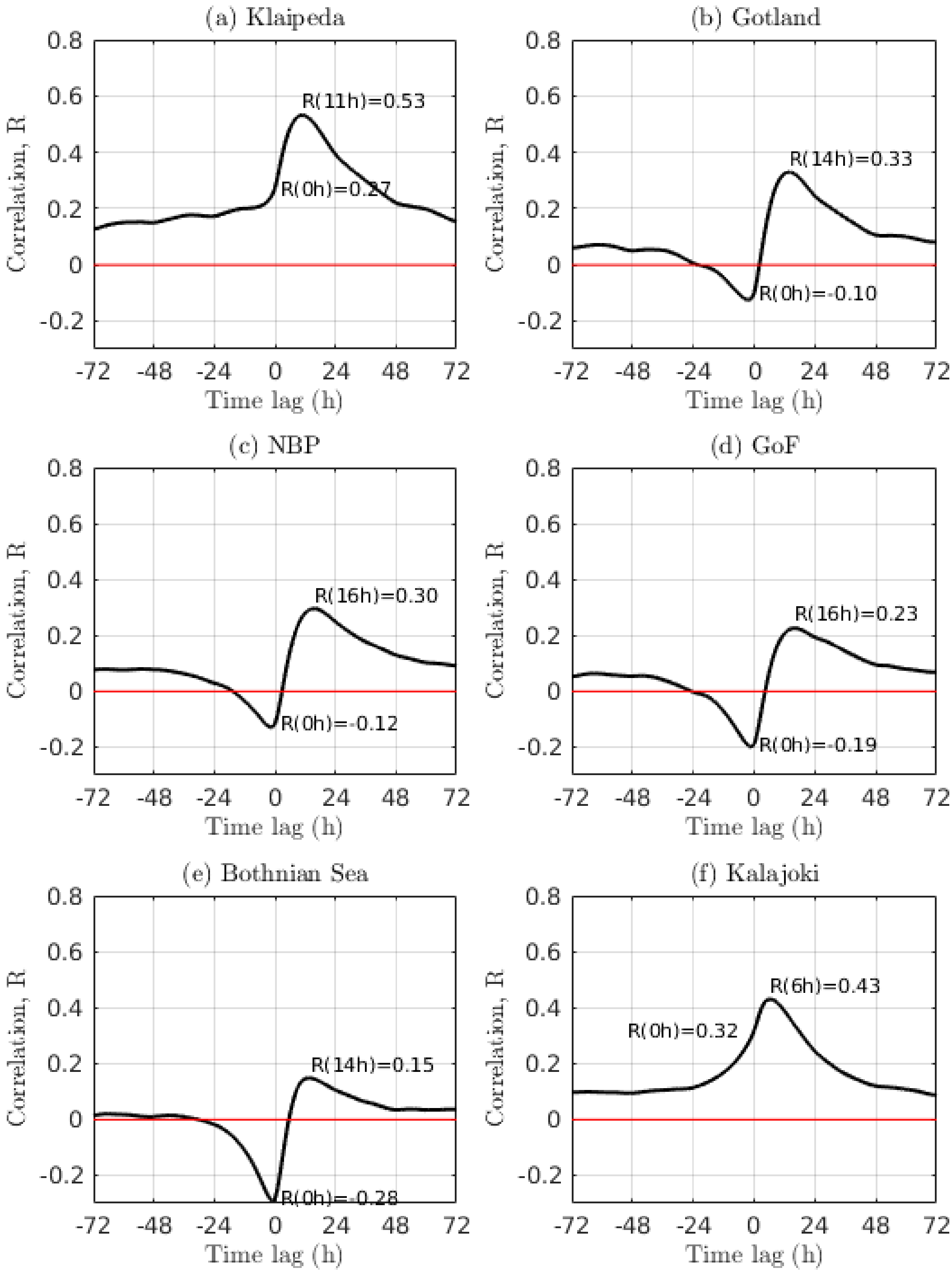

Fig. 5 The correlation between the swell height and the wind speed. The results are very similar to the correlations between the wind-sea height and the swell height.

R#2: *Minor comments:*

- *L10: suggests→suggest*

Our response: This has been corrected.

- *L19: upwards→upward*

Our response: This has been corrected.

- *L55: What is the temporal resolution of the ice data?*

Our response: The temporal resolution of the ice data varies. Between 1992 and 2005 the resolution is 3-4 days (updated twice a week) and from March 2005 onward the resolution is mostly 1 day. This information has been added to the manuscript.

- *Figure 2: Give the full words of the abbreviations NBP*

Our response: This has been corrected in the figure caption.

- *Figure 4: Authors can consider using the normalized distribution since the total data points vary with stations which may be easier to show the distribution (in particular for GoF). The total number of data points should be given.*

Our response: In the end we chose not to use a normalized distribution, since the current figure also contains visual information about the differences in the swell climate between the locations, which would be lost in the normalization. The total number of points have been added to the plot.