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18th INTERNATIONAL SHIP AND
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VOLUME 3

COMMITTEE I.1

ENVIRONMENT

COMMITTEE MANDATE

Concern for descriptions of the ocean environment, especially with respect to wave, current and wind, in deep and shallow waters, and ice, as a basis for the determination of environmental loads for structural design. Attention shall be given to statistical description of these and other related phenomena relevant to the safe design and operation of ships and offshore structures. The committee is encouraged to cooperate with the corresponding ITTC committee.

CONTRIBUTORS

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Ryuji Miyake
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REPLY BY COMMITTEE MEMBERS

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1 DISCUSSION

1.1 Official Discussion by Sverre Haver

1.1.1 Introduction

As a member of this committee for the period 1994 – 2000, it is a great pleasure to have the opportunity to review and discuss the report of Committee I.1.

As stated in the mandate, the committee report deals with a broad range of environmental subjects being of concern for operation and design of ships and offshore structures. The mandate is very wide and involves topics well outside the area I am familiar with. In this discussion I will focus on the subjects I am most familiar with.

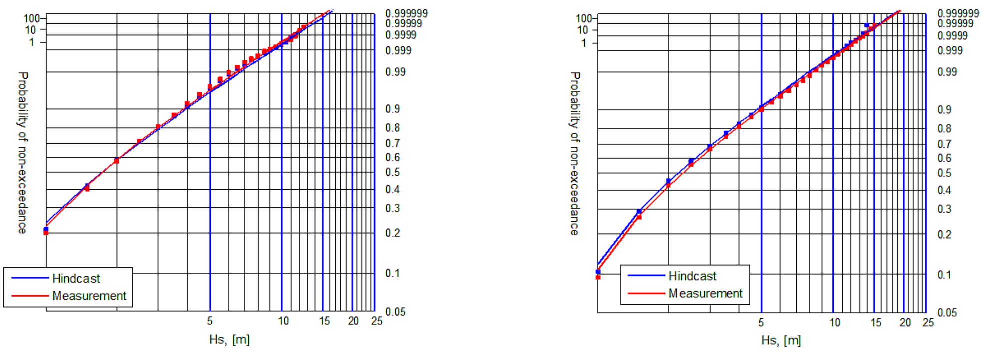
The present report fulfils the mandate with good margin. A large number of references are included for most of the subjects included in the mandate. The committee work is documented by a thoroughly written report.

I will congratulate the committee members with the report they have prepared. They have continued the ISSC tradition of solid review reports. In the end of this discussion, however, I will permit myself a brief discussion of the mandate for the committee and the interpretation of the mandate.

1.1.2 Sources of Environmental Data

This is an extensive review of availability of environmental data. I do not have much to add.

The committee correctly points out that the issue of data ownership remains a problem. In that connection I will draw the attention to the new Norwegian Hindcast Data Base, NORA10. This data base is also referred to by the committee. The reason for mentioning it here is that these data are to my knowledge open to all for a rather low cost. The data base provides wind and wave data at 3-hourly intervals from 1958 to present and covers North Sea, Norwegian Sea and Barents Sea. Comparisons with available measurements suggest that both wind and waves of the data base seem to be of good quality, see Figures 1 and 2. It is seen from Figure 2 left that when hindcast is compared to measurements from a weather mast, excellent agreement is observed. At the right of the figure results are shown when hindcast data are compared to measurements from a large platform. Hindcast data are apparently on the low side



Ekofisk, North Sea **Haltenbanken , Norwegian sea**

Figure 1: Comparison of distribution function for 3-hourly significant wave height for NORA10 and measurements (Ref. In-house Statoil report)

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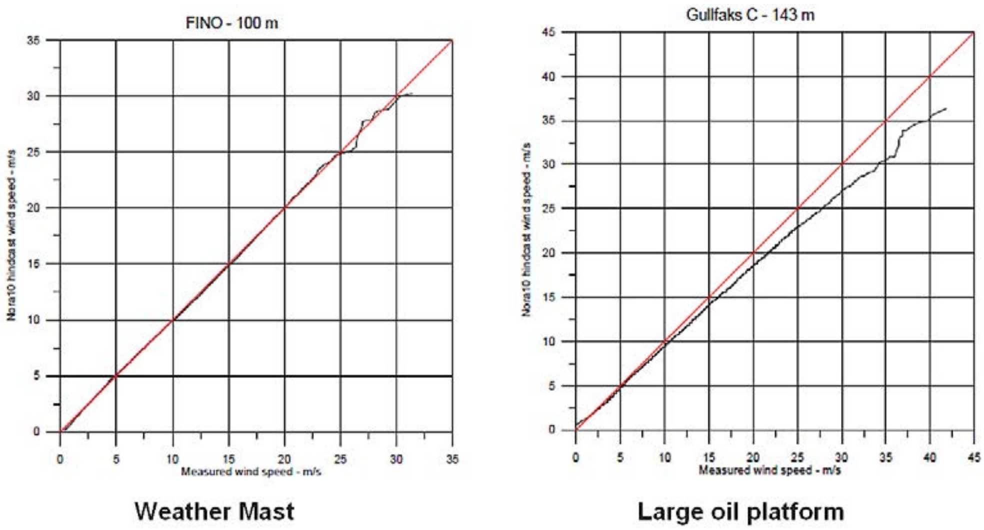

Figure 2: q-q plot for hindcast and measured wind speed at two locations. (Ref: In-house Statoil report)

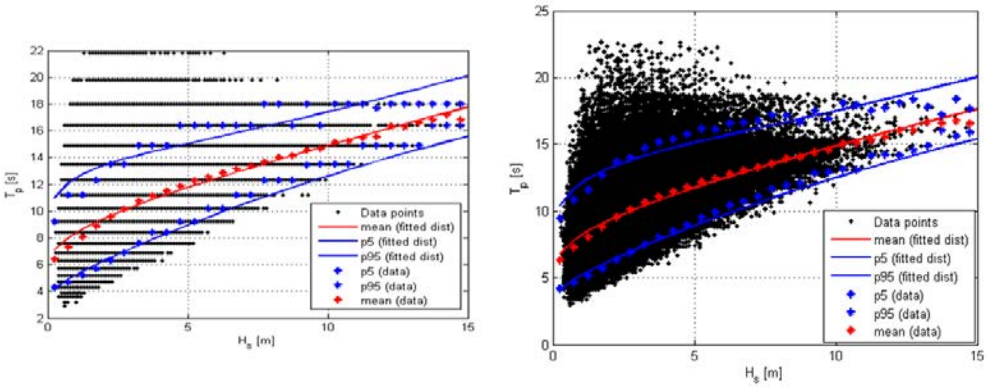

Figure 3: Scatter diagram for hs and tp before and after non-discretizing the spectral peak period.

regarding extremes. However, we think this is caused by the fact that the platform data are disturbed by the presence of this huge structure.

When using hindcast data for response analyses one should keep in mind that hindcast data bases usually will have a limited resolution regarding spectral peak frequencies. This is also the case for NORA10, see scatter diagram in Figure 3 left. If the problem under consideration has a critical frequency regarding amplification or cancellation, the spectral peak frequencies of hindcast data must be non-discretized prior to the response analysis, see Figure 3 right.

In Chapter 2.2.1 of the committee report, there is an interesting reference to an observation from Typhoon Krosa (Babanin *et al.* (2011a), see reference list of committee report). At 38 m water depth, a maximum wave height of 32 m and a significant wave height of 24 m were measured. These are rather extreme measurements in view of water depth. What type of sensor was used for measuring waves at this site? I guess that there must have been a considerable dissipation due to wave breaking in this area,

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ratio between maximum wave height and significant wave height is rather low if the time window associated with the observations at least is in the order of an hour or so suggesting that depth is clearly limiting maximum wave height. Are time histories for the surface process presented in the paper?

There is a comment at the end of Chapter 2.2.3 about bias in significant wave height when calculated from the zero-spectral moment as is done by the numerical wave models. Why is linearity referred to as the reason for this bias? The significant wave height calculated from spectral moment will be slightly biased (compared to the mean of the upper one third of wave heights) in a linear sea state also – I think? Is this bias (which must be rather small) of importance? As of today is it not most common to define significant wave height as 4 times the standard deviation (square root of zero-spectral moment)? At least I guess this is the case within the offshore industry? What would the committee prefer as the standard way of estimating the significant wave height?

I have a question regarding numerical modelling of current. Both wind - and wave hindcast have reached a level of accuracy making them applicable as data for design and operation of marine structures. Based on the review of papers and on-going research projects made by the committee is it is possible to give summary regarding the status of current modelling for the same purposes?

A final comment to this chapter is that data regarding marine growth is missing. Was this subject not prioritized (which I have full respect for in view of all subjects to be covered) or did not the committee find any data of interest? I think lack on marine growth data is a problem for design of slender structures in deep water? Will marine growth exist below $500\,m$ or $1000\,m$?

1.1.3 Modelling of Environmental Phenomena

This chapter gives a good overview of what has happened regarding this subject during the last few years. It is a rather extensive review. In view of my own primary interest, I found Chapter 3.2 very useful. I appreciate also that that several pages are spent on ice. I guess snow could also have been included. As the oil exploration moves into colder regions and ship traffic in the same regions also will increase, the future design and operation will definitely call for more and better information regarding ice, icebergs and maximum accumulated snow during heavy snow falls.

In this discussion, however, I will focus on the modelling of wind, waves and current. It is referred to in Chapter 3.1.3 that a joint modelling of significant wave height and wind speed have been presented based on the Nataf model. If one primarily is interested in rare combinations of these quantities, i.e. combinations corresponding to return periods well outside the time covered by the data, how adequate is the Nataf model for such an application? How close would contours based on CMA and Nataf be when applied to the same data set?

A considerable part of the wave chapter is devoted to freak or rogue waves. I appreciate that. The selection of references regarding this subject seems to give a very good overview regarding status. I think that the possible existence of wave events well outside our design scenarios is one of the few topics in Metocean for which the conclusion can have a significant impact on future design recipes and safety of existing structures.

Over the years, a number of reports have suggested the existence of wave events well beyond what is likely under the given weather conditions. The problem with observations of possible freak or rogue waves, however, can be illustrated by the following two

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questions: Is the observed event a very rare realisation from a typical, slightly non-Gaussian population of surface elevation processes (i.e. the observed value is merely a result of nature filling the tail)? Or: Is the observed event a typical realisation of a rare, strongly non-Gaussian population of surface processes? I think we will need an enormous amount of data before it is possible to conclude (with a reasonable confidence) to which question the answer is yes.

To me the solution to a conclusion regarding existence of freak waves lies in developing mathematical and physical wave models that have the potential of including freak waves among their population of solutions. Such models seem to be available, however, it is very difficult to anchor the occurrence of freak wave events to the physical quantities used in design. This should possibly not be surprising. A freak wave event is a rather local event (a spatial extension of some few wave lengths) and is also short lived (merely some few wave periods). So if the freak wave phenomenon exists, it is most probably a rare "instantaneous" combination of some sea state parameters within a limited area that is required to initiate a freak wave development. Sea state characteristics used in engineering are quantities averaged over 20-minutes – 3 hours, i.e. short living parameter combinations are smeared out over long time and – possibly – will not be recognised in the estimated engineering characteristics.

From adequate time domain simulations based on models including the potential of freak wave developments we could possible go back and identify some local governing characteristics immediately before the onset of the freak wave development. If we can identify a freak wave sub-domain of local characteristics, we could probably also empirically estimate the probability of being inside the freak wave sub-domain given the averaged sea state characteristics.

A forecast of freak wave occurrence directly based on averaged sea state characteristics (wave spectral shape or an associated kurtosis), I would not expect to be very robust.

In the end of the discussion of freak waves and breaking waves, the committee refer to a paper by Babanin *et al.* (2011b), see reference list of committee report), where it is suggested that the onset of wave breaking and freak waves are governed by the same physics. Onset of wave breaking is definitely governed by local characteristics suggesting that focus should also be on local quantities in order to explain onset of freak waves or rogue waves.

Breaking waves in deep water has got increasing focus in the offshore industry during the last few years. From model tests it is suggested that the impact pressure measured if a breaking wave hits a platform column is much larger than obtained using recommended practises and available standards, see Figure 4. The rate of breaking waves and type of breaking waves out in the open sea and the particle kinematics in connection with wave breaking is definitely of interest.

In Chapter 3.2.3 it is referred to the Crest JIP run by Marin. The last sentence of that paragraph could possibly be made more precise. I think Forristall 2. order crest height model was the best model also regarding field data? No better model was found, but the model was not the perfect model regarding upper tail. This can possibly be taken as an indication of the existence of crest heights beyond what is should be expected under a second order hypothesis regarding the surface elevation process, i.e. an indication of possible freak wave events.

Regarding current, I have mainly one comment. I would have appreciated some focus on joint occurrence of waves and current. Most joint data will be available for rather

		lower limit	upper limit	mean value
Sleipner	DNV	1076.5 kPa	1709.8 kPa	1393.2 kPa
	90% band	3273.1 kPa	3848.9 kPa	3561.0 kPa
	$\texttt{err}_X$	+204.0%	+125.1%	+155.6%
Gjøa	DNV	892.0 kPa	2048.9 kPa	1470.5 kPa
	90% band	1336.0 kPa	4094.0 kPa	2715.0 kPa
	$\texttt{err}_X$	+49.8%	+99.8%	+84.6%
Snorre	DNV	836.0 kPa	2079.9 kPa	1458.0 kPa
	90% band	1395.0 kPa	4782.0 kPa	3088.5 kPa
	$\texttt{err}_X$	+66.9%	+129.9%	+111.8%

Figure 4: Comparison of 10^{-4} annual probability impact pressure estimated using a DNV-recipe and model tests, respectively. From: Clauss *et al.* (2010).

low and moderate sea states. In such conditions current speed and significant wave height appear to be more or less independent or very weakly correlated. When designing a jacket structure where extreme loads are governed by the drag term of Morrison equation, both waves and current are important. In Norway the standard approach is to combine 100-year wave and 10-year current in order to estimate the 100-year load. In other areas one shall use an adequate associated current together with the 100-year wave. How to extrapolate the conditional current statistics given the significant wave height estimated based on sea states up to $9\,m$ significant wave height to a significant wave height level in the range $15 - 20\,m$? This will be a challenge if it is to be based purely on observed data! Have the committee during their review of publications for the last few years seen any efforts regarding joint modelling of waves and current for extreme response predictions?

1.1.4 Special Topics

Climate Change

In next to last paragraph of Chapter 4.1.3 it is referred to a paper Vanem and Bitner-Gregersen (2012) (see reference list of committee report) suggesting an increase of significant wave height of about $2\,m$ during 21st century. Assuming this increase to refer to a 100-year return period significant wave height, we are talking about an increase of more than 10%. This result seems to deviate significantly from all other projected increases I have seen? How does this figure compare to other predictions? Is this methodology a pure statistical approach? Or is it anchored in a physical climate model? I think this result should be discussed relative to the various scenarios of IPCC. Does it reflect an expected change or is it a very pessimistic scenario?

CFD

Regarding CFD a computer code that is frequently used within offshore industry is COMFLOW developed by University of Groningen through a JIP. This program is using the VOF method. We have used the program for calculating loads on platform deck in case the wave crest height reaches the deck level. Compared to model test results, the numerical predictions seem to be significantly on the low side. However, we think this to a large extent is due to inaccuracies in the start conditions given to COMFLOW. We did specify a Stokes 5th order profile as start condition, but from

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the model test experiment it seems that the waves giving the largest loads are waves in the process of breaking.

This demonstrates that a critical part of a CFD analysis is the input conditions given. And realistic input is difficult to determine by simple tools. We will probably have to start the CFD calculations earlier while the target wave is reasonably close to a Stokes 5th order profile. This will significantly increase computation time.

The cases reviewed by the committee are they more or less idealised computations based on known initial conditions or are various approaches for determining proper start-up conditions discussed.

1.1.5 Design and Operational Environment

In 3rd paragraph of Chapter 5.1.1 it is said that generally the offshore industry considers instrumental data superior to model derived data. I think that statement can be questioned. At the Norwegian Continental Shelf this has been the case up to now, but with the introduction of NORA10 (the new Norwegian Hindcast Data base) we (Statoil at least) have changed our preference and are now using NORA10 for design purposes.

In the end of the paragraph the committee states that hindcast data for wind and waves are available for several basins worldwide. However, these data are generally not available for those that have not joined the JIPs paying for the hindcast. But regarding the Norwegian Continental Shelf and adjacent waters, NORA10 hindcast data are generally available for a limited cost.

In the 3rd paragraph of Chapter 5.1.2 it is said that uncertainties/errors in the estimated long term distributions often leads to gross errors in the predictions (of what?). What does the committee define as a gross error in the predictions? And how should we distinct between uncertainties and gross errors in the estimated long term distributions?

A joint long term environmental model will be difficult to establish if more than 3-5 weather characteristics are to be included. If more parameters must be included, a peak-over-threshold (POT) long term response analysis should possibly be adopted for estimating annual extreme value distributions for target response quantities. Instead of looking at 3-hour events (as is typical adopted when a joint environmental model approach is adopted) "short term events" are now represented by storm histories of the selected characteristics for the storms exceeding a selected threshold.

Regarding contours, I guess they can be prepared under various philosophies. It is. however, important to remember the main aim of the contours is to represent an approximate way for obtaining long term extremes without having to do a full long term analysis. The environmental contours by them self are not so interesting. It is therefore important that various contour formulations are accompanied by a procedure how to estimate load effects corresponding to a given annual probability of being exceeded.

In Chapter 5.1.3 designing for climate change and rogue waves is discussed. I think it would be useful to introduce planned structural life time as an important parameter when it comes to designing against effects of climate changes. If one shall design a structure to be in operation for 100 years or more, I guess one should account for best estimated effects of climate change. But for structures with a planned life time of 20-40 years, is there any reason to be concerned of climate changes regarding the design process? A discussion around this would be appreciated.

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Regarding rogue waves, it is hard to make a rational design approach accounting for such events before we know more about their existence, frequency of occurrence and magnitude of amplitude. At present, all one can do from my point of view, is to take some actions ensuring some robustness against unexpected wave events. At the Norwegian Continental Shelf, the regulations followed for design of offshore structures require that the structure shall checked against wave crest heights corresponding to an annual exceedance probability of 10-4 as pointed out by the committee. This gives some robustness against rogue waves, because it is not very likely that the wave group including the 10-4 – annual probability crest height (predicted without accounting for freak wave mechanisms) also shall experience onset of a freak wave development.

In addition, Statoil has an internal requirement that a structure shall not be put at risk even if it is hit by a wave crest height 10% larger than the crest height predicted above. This is to account for a number of uncertainties and not only possible freak wave developments.

In last paragraph Chapter 5.1.3, the report refers to some few papers indicating that rogue waves may have to be accounted for by ship and offshore standards. Is it possible to briefly say what these indications are? Does the committee have any idea how one should account for them in standards?

In Chapter 5.2.2 it is said that kurtosis is a parameter accepted to be related to higher probability of rogue wave occurrences. I don't question that if kurtosis is high there is a larger probability for high waves. What I would appreciate to see is the correlation between a forecasted kurtosis and actual observed rogue waves at the site during forecast period. Have such a study ever been presented based on forecast and actual observations? How is the forecast kurtosis determined? I guess it must start with a forecasted wave spectrum, i.e. some average sea state properties. When estimating kurtosis from data, it is a rather uncertain quantity – in particular if one accounts for the fact the measured time series do not consist of independent data? How is confidence bands on the forecast values established?

1.1.6 Conclusions and Recommendations

The conclusions present a good summary of the previous chapter and I have nothing to add.

The recommendations are as precise as we can expect in this sort of a broad review report. I agree with all of them – in particular the one dealing with rogue waves. Recommendations like this, however, will not be explicitly of use in design work. I therefore consider the recommendations to basically be recommendations to the next committee.

1.1.7 Some comments to mandate and interpretation of mandate

The mandate of the committee is a far as I know defined by the standing committee. The mandate for the environmental committee has more or less remained unchanged for decades. During the same period the number of published paper has increased enormously. This is clearly illustrated by the committee early in the report by listing the number of conferences that they find of interest. In addition there are several journals that the committee should consider if a complete literature search should be performed. This report includes close to 400 papers! The selection of papers will to a large extent be committee dependent. There is no doubt in my mind that some of these papers will find (or have already found) their role as papers of permanent interest. However, most of the papers although they may well be important and useful in the context for which they are prepared, will not end up as being papers of permanent value.

What should we require for a paper to be included in the ISSC congress report? This question I will encourage the Standing Committee to discuss. There must be an element of new information and as far as possible, new findings/suggestions should be independently verified. Papers must have been through a review process – if necessary by committee members with adequate background. Furthermore, for a paper to be included in the report, maybe we should require that several committee members agree on this. There is no reason to believe that there are more than 50 papers of permanent value that are prepared within the fields and the period covered by the Metocean Committee. Maybe the Standing Committee should give a maximum number of references that can be included, for example 100?

I will also encourage the standing committee to narrow the mandate for the Metocean committee. There is for example no reason to have a full review of environmental data sources every 3 years. That goes also for the modelling of environmental phenomena. Our progress on complex problems does not progress that fast.

1.1.8 References

Clauss, F.C., Haver, S. and Strach, M. (2010): Breaing Wave Impacts on Platform Columns – Stoochastic Analysis and DNV recommended Practise, *Proceedings of* 29^{th} *International Conference on Ocean, Offshore and Arctic Engineering (OMAE 2010)*, June 2010, Shanghai, China.

1.2 Floor Discussions

1.2.1 Shengming Zhang

The official discusser mentioned the breaking waves and associated impact pressure. My question is: What is the size of measured area which gives the impact pressure in the range of $3 - 4\,MPa$?

1.2.2 Ryuji Miyake

Thank you for your contribution and useful information. I belong to EEDI division of ClassNK.

In order to reduce CO_2 emissions from international shipping, EEDI (Energy Efficiency Design Index) regulation for new ships was adopted by IMO at MEPC and enters into force on 1st of January 2013.

EEDI is a factor of new ship's specification and shows 'maximum energy efficiency' which the ship can achieve. If the new ship does not satisfy EEDI requirement, the new ship cannot enter operation.

Especially, EEDI is governed by the ship speed. Therefore, it is important to verify the ship speed with accuracy. EEDI verification is carried out in accordance with 'Guideline for verification of EEDI'.

The measured ship speeds in sea trial are allowed to correct the effects of wind, tide, wave, shallow water and displacement. Especially, since waves significantly affect the measured ship speeds, it is important to measure the waves with accuracy. Therefore, the waves should be measured using devices such as wave buoys, wave radar and wave scanner. However, actually, it is difficult to use such wave measuring devices in sea trial. Therefore, EEDI verification guideline accepts visual wave observation by multiple observers including an experienced captain as the wave measurements.

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Because of this situation, I would like to ask you two questions about the wave measurements:

1. What is the best method for wave measurements with accuracy?
2. In the case where an experienced captain conducts visual wave observation, what is the percentage of errors?

Thank you for your response.

1.2.3 Bruce Hutchison

The Committee has presented results regarding a possible climate change increase to 99th percentile of H_s by $2\,m$ by 2080. The Committee's presentation observed that this might imply a 10% to 15% increase in the weight of the deck of a tanker.

Does this suggest that class societies should be offering an additional class notation for vessels with additional strength to meet the anticipated demands of climate change?

1.2.4 Carlos Guedes Soares

The Committee presented hindcast data. The problem with hindcast data is that they may not be validated against extreme measurements. I would appreciate to hearing comments on it?

2 REPLY BY THE COMMITTEE

2.1 Reply to Official Discussion

2.1.1 Introduction

The Committee Members thank Professor Sverre Haver for evaluation of the Committee I.1 Report. The Committee appreciates to hearing that the Report I.1 fulfils the mandate with good margin and that the Committee work is documented by a thoroughly written report.

The Professor Haver's Official Discussion contains several interesting comments to which we will reply below.

2.1.2 Sources of Environmental Data

The Committee I.1 appreciates that Prof. Haver is satisfied with the review of availability of environmental data provided by the Committee and that he has not much to add.

Further, we are glad to hearing that the Official Discusser agrees with the Committee I.1 that the issue of data ownership remains a problem. We thank Prof. Haver for pointing out that the Norwegian Hindcast Data Base, NORA10 is open to all for a rather low cost. Several new met-ocean data bases mentioned in the Report I.1 are open to all (e.g. Argoss, Fugro-Oceanor database) but the price for using them is varying.

The Committee I.1 appreciates that Prof. Haver has included in his discussion the figures from the in-house Statoil report regarding the accuracy of the Norwegian Hindcast Data Base, NORA10. We fully agree that the resolution of the wave model is of great importance. It is mentioned in the report but due to limited number of pages the report could contain, this is not exploited in details. We agree that for some applications the discretization of the spectral peak frequencies of hindcast data should be removed prior to the response analysis.We would like also to add that the Norwegian Hindcast Data Base, NORA10 is used by the EC EXTREME SEAS project coordinated by DNV being the Chairman of the Committee I.1.

The maximum wave height which occurred during Typhoon Krosa (Babanin *et al.*, 2011a) was recorded by a wave buoy. Three wave buoys were deployed in relatively close proximity and represent a standard long-term deployment by the Central Weather Bureau (CWB) of Taiwan. They are moored foam floats, 2.5 m in diameter, equipped with three-dimensional Watson accelerometers SHR-A1360-2A-30/105. The data buoy was patented by the Republic of China (Taiwan) (patent number No. 087358, their period of validity runs from 1997 to 2016, see also Kao *et al.*, 1999).These buoys have been operational since 1997, and data are recorded for 10 min every hour sampled at 2 Hz, thus providing 1200 values for each of the three accelerations and three angles. The vertical component is then double-integrated to obtain the surface elevation. As stated by the authors this is a standard data-processing routine, see also Kao *et al.* (1999). In the ocean area investigated by Babanin *et al.* (2011a) one can expect a considerable dissipation due to wave breaking. The time histories for the surface process are not presented in the paper of Babanin *et al.* (2011a). Prof. A. Babanin is a recognized expert on wave breaking but the paper is a bit controversial.It is difficult to prove that the measurement is accurate although the authors state that it is physically realistic. It was found by Babanin *et al.* (2011a) that neither SWAN nor WWMII model are able to reproduce the observed conditions, however, the wave models have also limitations due to the assumptions they are based on.

The original definition of the significant wave height is the mean of the upper one third of wave heights. Today $H_s = 4\sqrt{M_0}$ is commonly used, where M_0 denotes the zero wave spectral moment. The relation $H_{1/3} = 4\sqrt{M_0}$ is based on the assumption of the Gaussian sea surface (linear waves) and Rayleigh distributed wave heights, see Longuet-Higgins (1952), Cartwright and Longuet-Higgins (1956), also Thornton and Guza (1983). The Rayleigh distribution of wave heights is correctly applied only to linear waves and it is based on the assumption of the narrow-band wave spectrum. The latter will introduce additional uncertainty as real sea surface is for most of the conditions not narrow-banded.The bias between these two significant wave height estimators is usually rather small. However, when rogue waves are present it maybe of importance, see Bitner-Gregersen and Magnusson (2004). The Committee I.1 has pointed this it out so that a user is aware of this deviation between the two estimators. It will be up to the user to decide how important the bias is for an application considered. We believe that the significant wave height calculated from the wave spectrum will continue to be commonly used in the future, particularly as use of wave hindcast data in design and operations of marine structures is increasing.

Regarding the current modelling the Committee I.1 is referring to the GODAE project (see Section 2.3.3):

"An overview of the GODAE (Global Data Assimilation Experiment) project and the various products were introduced in the previous Committee I.1 report."

Further, in the "INTRODUCTION" section a link to GODAE Ocean View is given where additional information on current modelling can be found:

"Success of the global and basin-scale ocean models development with data assimilation under the GODAE (Global Ocean Data Assimilation Experiment) program opened a new era of operational oceanography. GODAE ended in 2008 and continues as GODAE Ocean View: `https://www.godae-oceanview.org/`".

Due to the requirement of the limited number of pages of the report the GODAE project was not exploited in details, however, reference is made to the ISSC 2009 I.1 Report where the GODAE products are discussed. The GODAE products sited in the

Committee I.1 report are still coarse for direct use in designing ocean structures; this is pointed out in Section 3.3 "Modelling of Environmental Phenomena". But these products can be used as a boundary condition for a fine-mesh model. The Committee expects that GODAE Ocean View will continue its effort in coordinating downscaled regional circulation model development. At the scale of ocean structures such as ocean energy converters, regional high-resolution models are developed individually. Interaction of ocean current (which is supplied by GODAE and GODAE-like products), tidal current and coastal topography gives us a challenge in modelling the regional circulation, see e.g. Wada *et al.* (2012).

The ocean current models are reviewed shortly in Section 3.3.

The marine growth data are not reviewed as the Committee I.1 has not seen anything in the literature on the topic that warranted specific mention in the 2012 ISSC I.1 Report. However, marine growth is mentioned in Section 5.1.3 in relation to climate change: "... The predicted increase in marine growth may increase loads on marine structures in some ocean regions, e.g. the Baltic Sea."

We agree that the lack of marine growth data for design remains a problem.The photosynthesis occurs only down to about 100 – 200 m, and sunlight disappears altogether at 1000 m or less, while the ocean descends to a maximum depth of about 11 000 m. Thus it is not expected to see marine growth below 500 m or 1000 m. However, until recently the deep sea was largely unexplored, therefore more investigations are needed to reach firm conclusions. In any case an increase of ocean temperature will be a challenge in the future.

2.1.3 Modelling of Environmental Parameters

We are pleased to hearing that Section 3 gives a good overview of what has happened regarding modelling of environmental parameters during the last few years, and that Section 3.2 is very useful for the Official Discusser.

Professor Haver appreciates that ice has got several pages in the report but would appreciate to see also snow in the report. Snow is mentioned in the Committee I.1 Report, e.g. see Section 2.5.2: "... The authors produce a five year time series of freeboard elevation in the arctic, which in spite of uncertainties in snow thickness, indicates that overall sea ice freeboard has decreased during the considered observation period."

Snow is also mentioned in Section 2.5.3 dedicated to environmental data. There are several references regarding snow included in the reference list. However, the Committee I.1 has not seen anything in the literature on snow modelling that warranted specific mention in the 2012 ISSC I.1 Report. We expect that investigations dedicated to snow modelling will get increasing attention in the coming years.

When establishing a joint fit of met-ocean variables, if the available information about the simultaneously occurring variables is limited to the marginal distributions and the mutual correlation, then, as shown by Der Kiuregihan and Liu (1986), the Nataf (1962) model can be used. Applicability of the Nataf model to describe environmental parameters has been discussed in the literature for some time and is also discussed by Bitner-Gregersen (2012); the reference included in the reference list of the Committee I.1 Report.

The correlation between environmental parameters considered in the Nataf model can be computed in the physical space or in the normal space. The definition of the Nataf probability density function (pdf) is consistent with the given information, but the density function is not necessarily the "true" joint pdf. If the x-space variables are Gaussian, the Nataf model corresponds to the multi-normal distribution. If the x-space variables are random lognormal, the Nataf model corresponds to the multi-lognormal distribution. It thus follows that the Nataf model gives a good approximation for the distribution of the physical variables if the vector of the transformed standard normal variables is close to being multi-normal.

Bitner-Gregersen and Hagen (1998) applied the Nataf model to approximate the joint significant wave height and zero-crossing/spectral peak period distribution. The data from two locations: the Norwegian Sea (NS) and the Northern North Sea (NNS) have been used. For both locations the fitted Nataf models have been biased with respect to the significant wave height as well as the zero-crossing/peak wave period. It was concluded that the CMA (Conditional Modelling Approach) model is the superior to the Nataf model, and that the Nataf correlation model should be used with care. Applicability of the Nataf transformation has also been investigated by Sagrilo *et al.* (2008, 2011) for wave (wind sea and swell), wind and current parameters and direction. The statistical dependence between these parameters has been modelled by using concepts of linear-linear, linear-circular and circular-circular variables correlation. The joint probability established is taking into account the dependence between the intensity and direction of all variables. By improving modelling of correlation between environmental parameters a satisfactory joint fit has been obtained (see Sagrilo *et al.* 2008, Sagrilo *et al.* 2011).

The ISSC 2012 I.1 Report refers also to the ISSC 2009 I.1 Report where the accuracy of the Nataf fit is discussed. Therefore it was not repeated again in the text of the present report; mainly due to the requirement of the limited number of pages the present report should contain.

The Committee Members thank Professor Haver for the kind words regarding the section on rogue waves. We agree that the existence of these wave events can have a significant impact on future design recipes and safety of existing structures.

In addition, it needs to be noted that the conditional extremes model of Heffernan and Tawn (2004) is a flexible framework for general multivariate extreme value modelling which is easily implemented and extended. The major advantage of the Heffernan and Tawn conditional approach compared with previous conditional models, is that the functional forms for marginal fitting and conditional modelling are motivated by asymptotic arguments. The method is based on a parametric equation for the form for one variable conditional on a large value of another, valid for extremes from a wide class of multivariate distributions with Gumbel marginals. The conditional approach has been used as the basis for studies, including joint extremes of wave spectral parameters (Jonathan *et al.*, 2010) and extreme current profiles with depth (Jonathan *et al.*, 2012).

The Committee has noticed that Professor Haver raised the same questions: "Is the observed event a very rare realisation from a typical, slightly non-Gaussian population of surface elevation processes (i.e. the observed value is merely a result of nature filling the tail)? Or: Is the observed event a typical realisation of a rare, strongly non-Gaussian population of surface processes? I think we will need an enormous amount of data before it is possible to conclude (with a reasonable confidence) to which question the answer is yes.", in the last 10 years.

Some Committee members would say that an answer is "Yes" to both questions, others will say that these questions cannot be answered because the ocean waves will make a

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gradual transition from a state of slightly non-Gaussian population to a strongly non-Gaussian population. This should not be a question like: *to be or not to be.* At least for the mechanism of freak (rogue) wave generation based on weak nonlinearity, numerous research has now been conducted demonstrating the gradual transition from Gaussian to non-Gaussian waves, experimentally, numerically, theoretically and observationally. The hypothesis is that the sea states are changing in time and therefore at each instance of time different wave statistics are expected. The paper of Waseda *et al.* (2011) suggests that a strongly non-Gaussian population might be related to certain weather condition. Further, Waseda *et al.* (2012) have analysed recently numerous marine accidents in Japan and found that a lot of cases can be related to narrowing of the directional spectrum. Since direct causes of marine accidents are not known, it is rather speculative to use this result as an evidence of the relationship between narrowing of the directional spectrum and strongly non-Gaussian population, but, maybe good enough to illustrate how rapidly the wave spectrum can change in time in the ocean.

The occurrence of rogue waves, called also abnormal or freak is related to mechanics generating them. Today several physical mechanisms to explain the extreme and rogue wave phenomena have been suggested. These include: linear Fourier superposition (frequency or angular linear focusing), crossing wave systems, wave-current interactions, quasi-resonant interaction (modulational instability) and shallow water effects. In particular, it has been demonstrated that the contribution of high order nonlinear mechanisms such as the modulational instability of uniform wave packets, may give rise to substantially higher waves than predicted by common second order wave models.

Due to randomness of wave surface we can observe rogue waves satisfying the commonly used rogue wave definitions in the linear wave model as well as in the second-order wave models, if we simulate water surface elevation long enough. Rogue waves can also be observed in higher order solutions, beyond the second order, where the modulational instability is present. It should be mentioned, however, that the physics described by the linear and second order wave models is different from the one provided by higher order solutions of the water wave problem.

The Committee I.1 agrees that the solution to a conclusion regarding existence of freak waves lies in developing mathematical and physical wave models that have the potential of including freak waves among their population of solutions. Such models are available today and reported in the ISSC 2012 Committee I.1 report. A question remains how to link freak (rogue) waves to the physical quantities used in design. The EC EXTREME SEAS project is working on this topic and hopefully some answer will be able to be provided when the project is completed in 2013. Further, we do agree that a freak wave event is a local event while sea state characteristics used in engineering are quantities averaged over 20 minutes – 3 hours; this makes it more difficult to select the right wave parameters characterizing a freak event for design. We would like to mention also that intense time domain simulations of higher order solutions of water surface have been carried out in the last years in order to identify local characteristics of freak waves; they are published in several papers reported in the ISSC 2012 Committee I.1 Report. So far it has been difficult finding correlation between local freak wave characteristics and sea state parameters. However, it has been shown by Waseda *et al.* (2011) that characteristics of a weather pattern seem to correlate better with the observed freak wave occurrence (from the North Sea data).

A forecast of freak wave occurrence based directly on averaged sea state characteristics

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(wave spectral shape or an associated kurtosis) is a first step of freak waves' forecasting, and we can agree with Professor Haver, that such forecasting may not be always very robust. Further investigations are still called for.

Professor Haver states in his comment as following: "the committee refers to a paper by Babanin *et al.* (2011b), where it is suggested that the onset of wave breaking and freak waves are governed by the same physics." The Committee believes that Professor Haver has misunderstood the study. The paper of Babanin *et al.* (2011b) illustrates that with narrowing of the directional distribution the quasi-resonance (or the modulational instability) becomes more relevant in increasing the local wave steepness. For broad directional spectrum, the steepness of the wave was primarily due to random superposition of Fourier modes. These processes do not explain the breaking itself. When the initial wave steepness is high enough, the wave may steepen as a result of nonlinear or linear focusing, but locally the kinematics of breaking may be the same, as suggested by Professor Haver. The Committee I.1 thinks that freak waves may not necessarily be breaking. It is not clear to the Committee what Professor Haver means by "onset of freak wave." It may be clearer if the word "onset" is replaced with "generation mechanism".

We thank Professor Haver for valuable comments regarding wave breaking and offshore industry standards for the impact pressure on the platform, and for the reference to the paper of Clauss *et al.* (2010). However, the Committee I.1 regards that the paper being outside the Committee's I.1 mandate.

We can, however, mention that a paper regarding wave kinematics when rogue waves are present (without breaking) was presented on the last OMAE 2012 Conference in Rio, Toffoli *et al.* (2012). The direct numerical simulations of the Euler equations (the HOS method has been used) were applied in the analysis. A number of sea states with different wave steepness, spectral bandwidth and directional spreading were considered.

Sergeeva and Slunyaev (2012) has included in a simplified form a wave breaking in a 2D non-linear code; the results were presented at the EXTREME SEAS meeting but are not published yet.

Professor Haver is missing a statement regarding the Forristall (2000) distribution when the CresT project results are mentioned: "I think Forristall 2-order crest height model was the best model also regarding field data? No better model was found, but the model was not the perfect model regarding upper tail. This can possibly be taken as an indication of the existence of crest heights beyond what should be expected under a second order hypothesis regarding the surface elevation process, i.e. an indication of possible freak wave events. "

There is a theoretical 2nd order model of wave crest due to Tayfun (1980) which gives very similar results as the Forristall (2000) distribution based on curve fitting to the second order wave simulations. This model was not investigated by the CresT project. The Tayfun and Forristall models were compared, however, in several publications, e.g. Bitner-Gregersen *et al.* (2008), see Figure 5.

Fedele and Tayfun (2007) (see also Fedele, 2008 and the ISSC 2009 I.1 Report, 2009) suggested a crest distribution in a form that generalises the Tayfun model (1980). The authors show that for large waves the Tayfun model is an exact second order model for describing the crests and troughs of wind waves under general conditions at deep or finite water depths, irrespective of any directional and bandwidth constraints. Large

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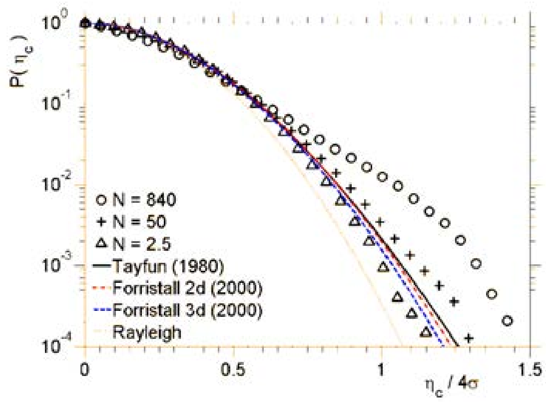

Figure 5: Wave crest distributions, Bitner-Gregersen *et al.* (2008).

waves are defined by the authors as waves characterised by $a >> M_0^{1/2}$ ($a = H_s/2$, where H_s denotes the significant wave height and M_0 is the zero-spectral wave moment). The model has been validated by wave measurements from the North Sea. The relative validity and accuracy of the generalised model in representing statistics of large wave crests is dependent on how a dimensionless wave steepness parameter is selected.

The Committee agrees that the Forrisall crest model is not a perfect one regarding the upper tail. It has been suggested at the last OMAE 2012 Conference in Rio by Bitner-Gregersen and Toffoli (2012b) an alternative crest model which gives better fit to the tail than the Forristall model does.

We agree with Professor Haver that for load calculations on a jacket structure joint occurrence of waves and current is of importance. There is an interesting paper of Winterstein *et al.* (2011) addressing North Sea current data set measured at Ormen Lange and probabilistic description of current. The paper should indeed get attention. However, the probabilistic description of current presented in the paper is closely related to the load procedure, therefore the Committee regarded the paper as being outside the mandate of the present Committee.

The current modelling work developed in the Safe Offload project coordinated by Shell did not produce anything conclusive, certainly nothing with respect to a joint relationship with either wind or wave. Even if we are able to model a joint relationship between current and wind or waves for extreme events, how can be confident that the relationship would also hold for very extreme conditions; it is an issue. From a purely statistical point of view of modelling joint extremes, the Heffernan and Tawn (2004) approach could be mentioned, well founded and quite easy to apply.

2.1.4 Special Topics

Climate Change

The results presented in the paper of Vanem and Bitner-Gregersen (2012) seem to be consistent with other investigations regarding climate change in the North Atlantic. Vanem and Bitner-Gregersen (2012) show the 100-year trend (by 2100) for the mean monthly H_s maxima of $1.6\,m$, and for the associated standard deviation of $0.39\,m$ in the North Atlantic location. It is stated in the Committee I.1 Report in the same section: "... Studies carried out before 2009 have reported increases of $0.35 - 1.15\,m$ in the seasonal maxima of H_s by 2080 and of $0.2 - 0.8\,m$ in the 20-year H_s in the 50-year period (2001- 2050) in the northeast North Atlantic. These positive trends have

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also been confirmed by later studies, e.g., Dragani *et al.* (2009), Wang *et al.* (2009), and Dodet*et al.* (2010). ..."

It is also stated in the report that uncertainties in climate change projection can be of the same size as the estimated extremes.

The model applied by Vanem and Bitner-Gregersen (2012) is a probabilistic model, the physics is included in historical data used in by the model. Further, the model includes terms reflecting the physics and uncertainties. This is a limitation of the model. The model is, however, more flexible to use by industry than physical climate models. The results presented do not reflect the most conservative IPCC scenario, scenarios are not included in this version of the model, only through the term reflecting uncertainty. A later version of the model (Vanem, 2012) includes terms reflecting explicitly different IPPC scenarios.

Due to the requirement about the limited number of pages of the report it was not possible to have extended discussion of the model. The intention was to mention the model as an alternative model to the climate/WAM models.

CFD

We thank you Professor Haver for the valuable comments. A discussion of loads is outside the Committee I.1 mandate. The intension of this section was to inform the shipping and offshore industry that CFD (Computational Fluid Mechanics) methodology is getting increasing attention in modelling of water waves and it is expected that this will continue.

The COMFLOW code is known to the Committee I.1. Both DNV and Shell have participated in the JIP projects where the code was developed. The code is used by DNV, but also other CFD codes are used by DNV.

We agree that the input conditions represent a critical part of a CFD analysis.

2.1.5 Design and Operational Environment

In 3rd paragraph of Section 5.1.1 it is said that *generally* the offshore industry considers instrumental data superior to model derived data. It does not exclude that some parts of the offshore industry may prefer the hindcast data for design purposes. Today hindcasts include assimilated satellite data (calibrated usually against buoy data) and hindcast data are commonly validated against measurements.

Some hindcast data for wind and waves are available for several basins worldwide (e.g. ERA40 data, Argoss database, Fugro-Oceanor), and not all where generated in the JIPs projects; some were developed by internal funding or within EC projects (e.g. Fugro-Oceanor database, HIPOCASS database). Use of some databases is free for research purposes (e.g. ERA40), however, not for the industrial applications.

It is stated in the Committee I.1 report: "It is recognised that uncertainties/errors in the estimated long-term distributions often leads to gross errors in the predictions." We agree with Professor Haver that this sentence alone is not complete as one can ask what predictions one is talking about. However, the sentence is preceded by the sentence explaining which predictions the Committee is talking about:

"Long-term distributions of sea states are often employed in the prediction of met-ocean characteristics or various responses that a marine structure will experience."

There are different definitions of gross errors in the literature. In the report "gross errors" referred to "biases". An uncertainty will include both a systematic error (bias) and a random error (precision).

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The Committee I.1 agrees that a joint long term environmental model maybe difficult to establish if more than 3-5 weather characteristics are to be included. It will depend on a location and data available. A peak-over-threshold (POT) long term response analysis could be a good alternative.

We thank Professor Haver for the comments regarding environmental contours. Indeed, the contours represent an approximate way for obtaining long term extremes without performing a full long term analysis. This has been discussed in the literature for some time and it is also mentioned in the paper of Bitner-Gregersen (2012) referred in the present report. Therefore it was not repeated explicitly in the report. We agree that it is important that various contour formulations are accompanied by a procedure how to estimate load effects corresponding to a given annual probability of being exceeded.

The Committee fully agrees with the Professor Haver comment that it would be useful to introduce planned structural life time as an important parameter when it comes to designing against effects of climate change. There are still significant uncertainties related to climate change projections. The studies showing impact of climate change on marine structures, referred in the report, represent initial investigations only and not procedures for design. Therefore a discussion regarding a structure life time has not been mentioned. However, the studies clearly state for how long time periods the climate change projections are given. Due to the non-stationary nature of design criteria associated with climate change, the design criteria for a structure with a planned life time of 20-40 years will change as a function of when that 20-40 year life time occurs – e.g. the 100-year return-period significant wave height will increase with time, if we believe the predictions.

We thank Professor Haver for interesting comments regarding rogue waves. We agree that the Norwegian offshore standards (NORSOK Standard, 2007) taking into account extreme severe wave conditions, by requiring that a 10000-year wave does not endanger the structure integrity (ALS), give some robustness against rogue waves. The robustness is even increased by the Statoil internal requirement that a structure shall not be put at risk even if it is hit by a wave crest height 10% larger than the crest height predicted according to NORSOK.

Regarding the last paragraph of Section 5.1.3, there are indications e.g., in the paper of Bitner-Gregersen and Toffoli (2012a) that rogue-wave-prone sea states can actually occur more often than once in the 20-25-year period, which is currently used as a return period for ship design. Also the highest observed sea state within the 10-year time period analysed ($H_s > 15\,m$) is characterised by $k_p H_s/2 > 0.13$, the conditions which may trigger the modulational instability. The investigations carried out by Bascheck and Imai (2011) based, between on wave buoy data, support this conclusion. It is important to note, however, that hindcast data used in the study of Bitner-Gregersen and Toffoli (2012a) are generally subjected to a number of uncertainties, which may have influenced the authors' results.

How to account for rogue waves in standards is a subject of the EC EXTREME SEAS project coordinated by DNV and planned to be completed in 2013. Therefore it is too earlier to comment on it at present.

According to the knowledge of the Committee I.1 (see Section 5.2.2 of the report) a study showing the forecast kurtosis and the one calculated from the actual observations has not been published yet.These investigations are ongoing, e.g. in the EXTREME SEAS project. There are, however, several studies showing comparison of kurtosis evaluated from numerical simulations and model basin data. They are reported in Section 3.2.1 and 3.2.2 of the Committee I.1 Report. When analysing observed data, the correlation with freak wave occurrence and parameters such as kurtosis, Benjamin-Feir index, is rather low. It is probably because the sea states are rapidly changing in time and are affected by uncertainty due to sampling variability as pointed out by Michel Olagnon during the EXTREME SEAS Workshop in Geneva in 2011. Waseda *et al.* (2011) showed by analysing the North Sea data that if we make a conditional averaging for the "freakish sea state" only, the correlation becomes much clearer. It should be noted that the authors had to make use of the wave model to estimate directional spreading, which is a limitation of the study.

Forecast kurtosis can be evaluated from the directional wave spectrum, see Morri*et al.* (2011). We agree that kurtosis is an unstable parameter and it is more affected by sampling variability (uncertainty due to limited number of observations) than the integrated parameters like significant wave height and spectral/zero-crossing wave period. Estimation of confidence bands on the forecast values will be of importance. Data allowing estimation of confidence bands are available today, as ECMW and meteorological offices are providing ensemble forecasts.

2.1.6 Conclusions and Recommendations

The Committee members appreciate that Professor Haver finds that the conclusions present a good summary of the report sections and that the Official Discusser has nothing to add.

The presented recommendations are directed to both, academia as well as the shipping and offshore industry. They should not, however, be regarded as receipts for design procedures. They indicate the directions of future research needs being of importance for design and operations of marine structures.

2.1.7 Some Comments to Mandate and Interpretation of Mandate

The Committee disagrees with the suggestions of Professor Haver regarding changes of the Committee's mandate.It maybe sometimes very difficult to appreciate the full value of a paper right away: sometimes it reveals its usefulness not until many years later; an example can be the earlier work of Newton. Therefore it might not be a good idea to leave some papers out because of a limitation in the number of publications. Further, when focusing only on a small number of publications we might miss the "big picture". The Committee thinks that it is more useful if we give the reader a general overview (a short version of highlights) and leave it to the reader to get more detailed information from the originals. Further, sometimes only some investigations presented in a paper maybe of permanent value, and not the whole paper.

When papers are presented in the ISSC report the Committees' members have to agree on inclusion them; papers in press, or submitted, are already reviewed.

The Committee I.1 thinks that the present Committee's mandate covers well the needs of the shipping and offshore industry and by adopting it, directions for future research needs, being of importance for the marine industry, are also covered. Therefore the Committee I.1 does not see any reason for revision of the mandate. However, different Committee I.1 terms could have different topics in focus. This is to some extend taken care of already by the section "Special Topics".

We would like, however, to suggest increasing the number of pages required for the report.

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2.1.8 References

We thank for the reference of Clauss *et al.* (2011). The Committee I.1 finds the paper being outside of the Committee I.1 mandate.

2.2 Reply to Floor and Written Discussions

The Committee would like to thank the discussers for their valuable contribution to the floor discussion.

2.2.1 Shengming Zhang

The question was addressed to the presentation given by the Official Discusser Professor Sverre Haver and was answered by Professor Sverre Haver. In general, consideration of impact pressure in Clauss *et al.* (2010) is outside the scope of the mandate of Committee I.1.

The test for the Gjøa semi-submersible, presented by Clauss *et al.* (2010), was carried out at Marintek in Trondheim, Norway, while the one for the Snorre A TLP was done at Marin in the Netherlands. The model scales were 1:55 for Gjøa and 1:62.5 for Snorre. The impact loads from breaking waves were measured using force transducers. In case of Gjøa height transducers with a full-scale area of $10 : 89\,m^2$ were applied, in case of Snorre two with a full-scale area of $6.2\,m^2$ were used. In the case of the third platform, the Sleipner A GBS, the test model was built at a scale of 1:100. For measuring the wave-in-column loads 16 piezoresistive force pads with an area of $15\,mm$ x $15\,mm$ at model scale ($1 : 5\,m$ x $1 : 5\,m$ or $2.25\,m^2$ at full scale) were used.

2.2.2 Ryuji Miyake

The instruments measuring waves are under continuous improvement and their accuracy is also continuously improving. A recommended choice of an instrument will dependent on a wave parameter (or parameters) needed to be provided. For example,wave buoys are recognised as very good instruments for measuring integrated wave parameters like significant wave height and zero-crossing wave period but should not be used for measuring a wave profile. For getting accurate measurements of wave profiles lasers will be recommended to apply.

Visual observations of waves collected from ships in normal service are currently used in design and operations of ship structures.Wind speeds (Beaufort Scale) and directions, and wave heights in a coarse code have been reported since 1854. Observations of wave height, period, and direction have been collected from ships in normal service all over the world since 1949. Collection of visual observations is made in accordance with guidance notes from the World Meteorological Organisation (WMO). The utility of visual wave observations depends on appropriate calibration versus accurate measurements of the wave characteristics. Particularly, accuracy of visually collected wave periods has been questioned in the literature.Accuracy of visual wave height should be within 10%, when the guidelines of WMO are followed. It should be noted that visual observations include bad weather avoidance.

2.2.3 Bruce Hutchison

Classifications Societies are following closely investigations on climate change and their impact on met-ocean conditions. So far it has not been introduced an additional class notation for vessels with additional strength to meet the anticipated demands of climate change. There are still significant uncertainties related to climate change projections. However, the maritime industry has initiated studies aiming at quantifying potential impact of projected climate change and associated uncertainties on ship structures; see e.g. Bitner-Gregersen *et al.* (2011).

2.2.4 Carlos Guedes Soarres

Recently various hindcast and satellite databases have emerged and the work of comparing and of assessing differences and uncertainty level involved in their use is not yet properly explored, although significant progress has been achieved for some databases. The previous I.1 Committee Report noted a lack of validation of numerical wave models with instrumented data beyond 12 metres, but some studies have included such extreme data since that time. Cardone and Cox (2011) demonstrated that the current 3G models are capable of accurately hindcasting significant wave heights above 14 metres in very extreme storms. Similar studies are carried out at different met-offices world-wide.

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