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On Validation of Directional Wave Predictions: Review and Discussion

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14. ABSTRACT

This report consists of supplementary materials for an article, accepted for publication in the *Journal of Atmospheric and Oceanic Technology*, dealing with directional wave model validation by the same authors. These materials provide important background information for this companion paper, but were not included due to journal page limitations. Part I of this report provides a review of literature related to directional validation of wave models. Part II of this report discusses a number of issues that were raised by reviewers to the paper with particular focus on extensive non-directional validation of wave models. Also presented are validation of higher order moments, a summary of alternate methods of validation of directional spreading, a discussion of data-adaptive methods, and discussion of sensitivity of results to the handling of the high frequency range of the wave spectrum.

15. SUBJECT TERMS

Directional spreading — SWAN model
Wave model validation — Spectral density

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CONTENTS

Introduction:

This report consists of materials prepared for a submitted journal article dealing with directional wave model validation by the same authors. The materials provide important background information for the paper. Much of the material herein was part of the original submission, but was removed later due to journal page limitations. We, however, believe that the material will be useful to many readers dealing with the subject of directional wave model validation. Because the supplementary nature of this materials, we strongly encourage readers to read and understand the journal paper first before reading this report. This journal article will soon be available in the Journal of
Atmospheric and Oceanic Technology.

Part I of this report provides a review of literature related to directional validation of wave models. Part II of this report discusses a number of issues that were raised by reviewers to the paper with particular focus on extensive non-directional validation of wave models.

Part I: A review of literature relevant to directional validation of wave predictions

A number of methods for directional validation of wave predictions have been applied over the years. To provide appreciation for the evolution of these methods and the trends in the adoption of methods by various investigators, we present here a chronological listing of the development of methods and example directional validation studies in the literature. A few landmark papers on closely related subjects are also included. Following this chronological listing, we provide a thematic indexing of the references, by number.

1. Longuet-Higgins et al. (1963) developed the methodology which we still use today for deriving directional properties from heave-pitch-roll buoys. This paper, with companion paper Cartwright et al. (1963), proposed the "$\cos^{2s}$ model" for directional distribution, $D(\theta) = \cos^{2s}\left[(\theta - \theta_0)/2\right]$. The "*s*" parameter is (inversely) a measure of directional spreading. Since the $\cos^{2s}$ model is a model, we chose not to use it in Rogers and Wang (2006/7). However, it has been widely used in the literature.
2. Mitsuyasu et al. (1975) were the first of many to propose an empirical, parametric form for the directional distribution $D(f,\theta)$ based on field measurements. Their "*s*" parameter was a function of the frequency relative to the peak, f / f_p, and the wave age U_{10} / C_p. Other parametric forms followed (not all of them based on the $\cos^{2s}$ form). In particular, the dependence on wave age has been questioned. A good review of these forms and related discussions can be found in Young et al. (1996), Ewans (1998), and Young (1999).
3. Forristall et al. (1978) used the $\cos^{2s}$ model to quantify directional spreading. The study was motivated by the need to calculate forcing on a structure for the oil industry without assuming unidirectional waves, which would give incorrect calculations. They compared directional spreading and mean direction from a wave model hindcast for a hurricane event to that of directional spectra estimated from electromagnetic current meter measurements. They did not integrate these two parameters across frequencies. Thus, they did not show time series, but rather comparisons as a function of wave frequency for three specific time periods during the hurricane wave event. They concluded that their early generation wave model compared well with the measurements.

Manuscript approved June 22, 2006.

4. Komen et al. (1984) is the landmark paper in the early development of third generation wave models. Directional characteristics are of minor concern in this study. Model directional spreading in a fetch-limited scenario is validated, with Hasselmann et al. (1980) as ground truth.
5. Hasselmann et al. (1985) introduced the Discrete Interaction Approximation (DIA) for four-wave nonlinear interactions, S_{nl4}. The DIA is the approximation used by all operational third generation wave models today. Using idealized test cases, Hasselmann et al. (1985) and Young et al. (1987) compared two-dimensional spectra obtained from a model using the DIA to those from a model using more rigorous calculations of S_{nl4} (EXACT-NL). The former appear to be considerably more directionally broad than the latter, at least in the higher frequencies. Comparisons such as this have led to belief by some that third generation wave models tend to overpredict directional spreading, due to the use of the DIA.
6. Kuik et al. (1988) provide an excellent discussion and analysis of methods used to interpret buoy directional measurements and suggest four directional metrics that can be calculated from buoy measurements without the use of any model, such as the $\cos^{2s}$ form. These four "model-free" metrics—mean direction, directional width, skewness, and kurtosis—are each calculated as a function of wave frequency. Their calculation of directional width is used in a frequency-integrated fashion by third generation models such as SWAN (Booij et al. 1999, Booij et al. 2005) and WAVEWATCH-III (Tolman 1991, Tolman 2002, denoted "WW3" herein). Kuik et al. (1988) is usually given as the reference for the directional spreading calculation, though the metric is used in earlier articles (e.g. Hasselmann et al. 1980, Long 1980, Vlugt et al. 1981). Mardia (1972) is given as the original source in some instances (an updated reference is Mardia and Jopp (2000)). We refer to the metric as "circular RMS spreading".
7. WAMDIG (1988) is the introduction of the WAM model. There is a directional validation in this paper: modeled mean wave direction (as a function of frequency) is compared to measured values at several instants in time for a hurricane case. Modeled two dimensional spectra are compared to two dimensional spectra derived from Synthetic Aperture Radar (SAR) at a few instants in time in the North Atlantic. The disadvantage of both methods of presentations is that only sample results are possible, rather than long time series. The approach of side-by-side comparisons of sample two dimensional spectra is necessarily rather qualitative.
8. Guillaume (1990) compares mean wave direction from second and third generation wave models to buoy data at one location (the "BEATRICE" buoy location) for a time period slightly less than one month. They use one mean wave direction metric that is integrated across all frequencies, a second metric for 0.17 Hz frequency band, and a third metric for the 0.12 Hz frequency band. Mean direction is compared at all frequencies for a shorter (two day) time period. This paper is a good example of a struggle to condense directional comparisons into a readable presentation without rendering the presentation meaningless.
9. Tolman (1991) includes a validation of directional spreading using the circular RMS spreading metric. Since it is a validation of refraction in the model using an idealized test case, there is a known, analytical solution that is used as ground truth. The waves are monochromatic, so there is no concern about the presentation of frequency variation of directional spreading.

10. Holthuijsen and Tolman (1991) present directional spreading predicted by models with differing forcing. That metric is a frequency-integrated version of the circular RMS spreading metric.
11. Beal (1991) is a collection of papers, with nine papers dealing with wave models in some fashion. Two dimensional spectra at specific instances in time are compared to radar-derived spectra. Three of the papers make very interesting qualitative comparison of times series, with wave height (model vs. observations), direction, and frequency indicated for various waves systems. Some of these systems occur simultaneously; see pages 145, 161, and 185 in that book.
12. Van Vledder and Holthuijsen (1993) make model-data comparisons of the "dimensionless response time scale of the mean wave direction" to shifts in the wind direction. In simulations of idealized cases, a frequency-integrated version of the circular RMS spreading metric is presented. Their model uses exact computations of nonlinear interactions. They observe that simulated time scales of directional response to changes in wind direction are "considerably larger than the observed time scales".
13. Komen et al. (1994, Chapter V.4) compare the directional spreading inferred from SAR data to directional spectra of a global WAM simulation (for a one month duration). The metric for directional spreading is that of Yamartino (1984). It is integrated across frequencies, and the comparison is a scatter/density plot.
14. Banner and Young (1994) include relatively extensive comparisons of directional distributions and directional spreading (at the peak wavenumber and four times the peak wavenumber). They are able to present in this level of detail because they remove the temporal dimension by considering only idealized time-independent fetch-limited simulations (and observations). Empirically derived parametric directional distribution functions are used as ground truth. They use a different calculation for directional spreading, also used in Young (1999). Young and Van Vledder (1993), Banner and Young (1994), and Young et al. (1995) argue that in the spectral region higher than f/f_p=2 (i.e. frequency greater than twice the peak frequency), spreading is controlled by the four-wave nonlinear interactions S_{nl4}.
15. Khandekar et al. (1994) use the method of side-by-side comparison of two-dimensional spectra, similar to that of WAMDIG (1988), to evaluate the performance of a first generation and third generation wave model. Buoy-derived two-dimensional spectra are used as ground truth (inferred from buoy motion via a data-adaptive method). In the examples shown (four instants in time), directional spreading of the third generation wave model appears to be too broad.
16. Jensen et al. (1995) compared directional spreading from a WAM hindcast to that of measurements at three locations near Duck, N.C. during a large synoptic-scale northeaster (dubbed IOP-2). For the directional spreading metric, they use that of Yamartino (1984). At the one nearshore location (8 m depth), agreement is fairly good, while the modeled directional spreading is too narrow at two further from shore (27 m and 47 m depth). The results are noteworthy since they are contrary to conventional wisdom (that third generation wave models overpredict directional spreading).
17. Heimbach et al. (1998) took a new approach to validating mean wave directions: they compared climatologies of WAM mean wave direction to SAR mean wave direction over the globe (on a 5°×5° grid). The comparisons are made for four separate seasons and sea and swell are presented separately.

18. Forristall and Ewans (1998) focus specifically on directional spreading. Similar to Forristall et al. (1978), the concern is the reduction (associated with directional spreading) of wave forces on structures. They use a metric associated with this force reduction, as well as the circular RMS spreading metric (both with and without frequency integration). Similar to earlier works, Forristall and Ewans (1998) avoid the problem of presenting time series by using the idealized time-independent fetch-limited scenario as a basis for comparing models to observations[2]. They observe that both the EXACT-NL-based third generation wave model and the DIA-based third generation wave model (a WAM variant) "are broadly consistent with the empirical distribution, but the Exact-NL spreading is lower than the [WAM] spreading at higher frequencies." The frequency-integrated circular RMS spreading metric indicates greater spreading with the DIA-based model (approximately 36°) than that from the EXACT-NL-based model (approximately 29°).
19. Forristall and Greenwood (1998) also focus specifically on directional spreading. Based on idealized test cases, they argue that WAM has a tendency to overpredict directional spreading and that this overprediction is primarily due to the DIA approximation for S_{nl4}. A long-term (multi-year) hindcast with a third generation wave model (with an extended version of the DIA) is compared to measurements at two sites in the North Sea. Frequency-integrated directional spreading is used as the metric. They conclude that there is reasonably good agreement between the hindcast and measured directional spreading. A figure shows the model spreading is underpredicted by the long-term model hindcasts[3] (too narrow) for cases with significant wave heights below 6.5 m, but in discussion of the figures, the authors state that the hindcast spectra are too broad, which if true, would be consistent with the trend observed in their comparisons for idealized cases. The paper also compares hindcasts from two models (a first generation model and the third generation model mentioned above) for Hurricane Opal (Gulf of Mexico, 1995) to directional measurements at an oil platform. Both models greatly overpredict spreading (too broad) during most of the storm duration.
20. Babanin and Soloviev (1998) does not pertain to model validation, but does introduce a new metric for inverse directional spreading. The method is quite intuitive; it is frequency-dependent, calculated as simply the area under the directional distribution first normalized such that the maximum value is unity. They reference an earlier work written in Russian, Belberov et al. (1983). The method is applied in Young and Babanin (2005). It requires knowledge of $D(\theta)$, so it is less useful in cases where only a few Fourier coefficients are known.
21. Krogstad et al. (1999) acknowledge the difficulty of comprehensive directional validation and make qualitative comparisons of two-dimensional spectra.
22. Alves and Banner (2003), like Banner and Young (1994), consider the reduced fetch-limited case. Directional spreading is one of several metrics used to evaluate the performance of variations of a third generation wave model. The directional spreading at the peak wave number k_p and $4k_p$ is plotted against wave age, with observation-based relations also shown. With the models used, there is a tendency to overpredict directional spreading at the spectral peak, particularly more mature stages of wave development.

[2] Other cases are studied in the paper, but the fetch-limited case is the only case where the models are applied (our focus is model validation).
[3] Their spreading parameter, inversely related to spreading, is overpredicted by the model.

Model performance by this metric is poor relative to performance by other metrics (such as total wave energy or peak frequency).

23. Moon et al. (2003) compare SRA data to high resolution WAVEWATCH-III hindcast results for a hurricane case. Mean wave direction at the spectral peak is one of the primary metrics for evaluation; the authors report that it is simulated very accurately. They also make side-by-side comparison of collocated measured and modeled directional spectra (18 collocated points), similar to the comparisons made by WAMDIG (1988). Again, excellent agreement is reported, though the authors observe that "the model produces smoother spectra with narrower directional spreading than do the observations when the real spectrum has multiple peaks". One can reasonably expect that this behavior is associated with the complexity of the wind regime, rather than a systematic tendency on the part of the model to generate wave spectra that are too narrow.
24. Ardhuin et al. (2003) are specifically concerned about the directional spreading. They validate a wave model developed to simulate shelf-scale processes (refraction, shoaling, bottom friction, Bragg scattering). Their metric is the circular RMS spreading over a band of frequencies near the spectral peak. Directional buoys and a wave gage array are used as ground truth. They argue that the change in directional spreading across the shelf (in their case, at least) is a balance of the effects of refraction (which tends to narrow spectra) and Bragg scattering (which tends to broaden spectra), and possibly some other unknown physical process(s) which tends to broaden spectra.
25. Wyatt et al. (2003) made extensive directional comparisons of sub-regional WAM simulations to wave observations by buoy and radar instruments for a duration greater than one month. The methods they use are 1) time series of one-dimensional directional wave spectra (i.e. two-dimensional spectra integrated across frequencies; a normalized comparison), 2) seven-day time series of circular RMS directional spreading integrated across all frequencies , 3) seven-day time series of a peak direction metric 4) statistics for mean wave direction at the peak frequency, 5) statistics for mean wave direction integrated over four different frequency ranges (which are constant in time), and 6) sample side-by-side comparisons of directional spectra (four instants in time, qualitative). Inspection of comparison (2) suggests a persistent tendency by WAM to overpredict the frequency-integrated spreading during the seven days shown. The authors report that there is evidence that WAM "responds slowly to changing conditions perhaps due to the coarser resolution in wind forcing".

A thematic indexing of the above references follows:

- Ground truth for directional spreading validation:
    - Gauge array in situ data : 3,24
    - Buoy in situ data: 11,15,16,19,24,25
    - Radar : 7,11,13,23,25
    - Empirical/parametric model : 14,22
    - Analytical solution : 9
- Directional spreading metric:
    - "*s*" spreading parameter in "$\cos^{2s}$" form: 1,2,3
    - "circular RMS spreading" (defined in Section 3): 6,9,10,12,18,24,25
    - "mean spectral width" and "mean directional width" (defined in Section 3): 14,22

    - Yamartino (1984) definition: 13,16
    - force-based spreading factor: 19
- Directional spreading validation:
    - Reduced comparison (infinite fetch or infinite duration): 14,18,19,22
    - Reduced comparison (one or more time/location pairs) : 3,5,7,15,23,25
    - Idealized case (not a hindcast) : 5,9,14,18,22
    - Short duration hindcast : 3,7,11,16,19,23,24,25
    - Medium duration (15 days) or longer hindcast : 13,15,19
- Wave model development:
    - Numerical : 4,7,13,22
    - Parametric : 2
- Validation of wave model directional spreading:
    - Quantitative: 3,9,13,14,16,18,19,22,24,25
    - Qualitative: 5,7,11,15,23
- Medium duration (15 days) or longer hindcast for mean wave direction validation: 8,17
- Hindcast directional spreading validation:
    - Quantitative non-data-adaptive comparisons with in situ data ground truth:16,19.24,25.
    - Qualitative comparisons with frequency variatioins:3
- Any frequency variation of a directional spreading metric, with quantitative model validation (not all are hindcasts): 3,4,9,18,19,22
- Data-adaptive models and data-processing methods: 1,6
- Evaluation/discussion of directional comparison methods: 8,11,21
- Forces on structures as a focus/motivation: 3,18,19
- Numerical wave model validation: focus on mean wave direction: 8,17
- Climatology (model and observations) to evaluate modeled mean wave direction:17
- Model with “exact” four-wave nonlinear computations as ground truth; effect of DIA (approximation of four-wave nonlinear interactions) on wave model directional characteristics: 5,18,19
- Study of model behavior (not validation): 10, 12

Time	**Authors (years)**
1960s	**Longuet-Higgins et al. (1963)**
1970s	**Mitsuyasu et al. (1975); Forristall et al. (1978)**
1980s	**Komen et al. (1984); Hasselmann et al. (1985); Kuik et al. (1988) WAMDIG (1988)**
1990s	**Guillaume (1990); Tolman (1991); Holthuijsen and Tolman (1991); Beal (1991); Van Vledder and Holthuijsen (1993); Komen et al. (1994); Banner and Young (1994); Khandekar et al. (1994); Jensen et al. (1995); Heimbach et al. (1998); Forristall and Ewans (1998); Forristall and Greenwood (1998); Babanin and Soloviev (1998);Krogstad et al. (1999)**
2000s	**Alves and Banner (2003); Moon et al. (2003) Ardhuin et al. (2003); Wyatt et al. (2003)**

<u>Part II: Details and discussions not included in the article, RW</u>

Here "RW" refers to the submitted manuscript, Rogers and Wang (2006/7).

2.1 Validation of non-directional spectra *F(f)*

In the RW manuscript, we state:

"These wave heights are also compared to data in scatter-plot form, along with mean period, the mean-mean wave direction, and true peak period.... By the standards of a wave model which uses only wind forcing, the agreement is very good for all four metrics. The good prediction of wave height and mean period suggests that the non-directional wave spectra *F(f)* are fairly well-predicted. This provides confidence that the hindcast is suitable for detailed study of accuracy of prediction of directional spreading."

The phrase "fairly well predicted" is obviously rather vague. The original submission of RW only included bulk parameters as evidence that *F(f)* is fairly well-predicted. A reviewer requested that we add actual comparisons relating to *F(f)*. We agreed that this is helpful to interpreting the directional comparisons, so two new plots dealing with non-directional comparisons were added in the final version of the paper.

The purpose of this section of the supplemental report is to scrutinize in much greater detail the accuracy of *F(f)* in this hindcast. The results of these plots are consistent with the two plots added to RW, but provide some minor additional information.

2.1.1 Bulk parameters

Bulk parameters are shown in Figure 1. Results look very good, but taken as a whole, these three metrics (wave height, mean period, peak period) do not tell us whether spectral width (in frequency space) is accurate. This is stated in RW. To explain further: excellent agreement in these parameters may hide two possible "bad" scenarios:

1) overprediction at low and high frequencies, underprediction at the peak frequency (i.e. spectral width is too broad).
2) underprediction at low and high frequencies, overprediction at the peak frequency (i.e. spectral width is too narrow).

Another inherent problem is that inaccuracies in less energetic regions of the spectrum will be downplayed in the bulk parameter comparison, since they are moments. This may not be a desired consequence in many cases.

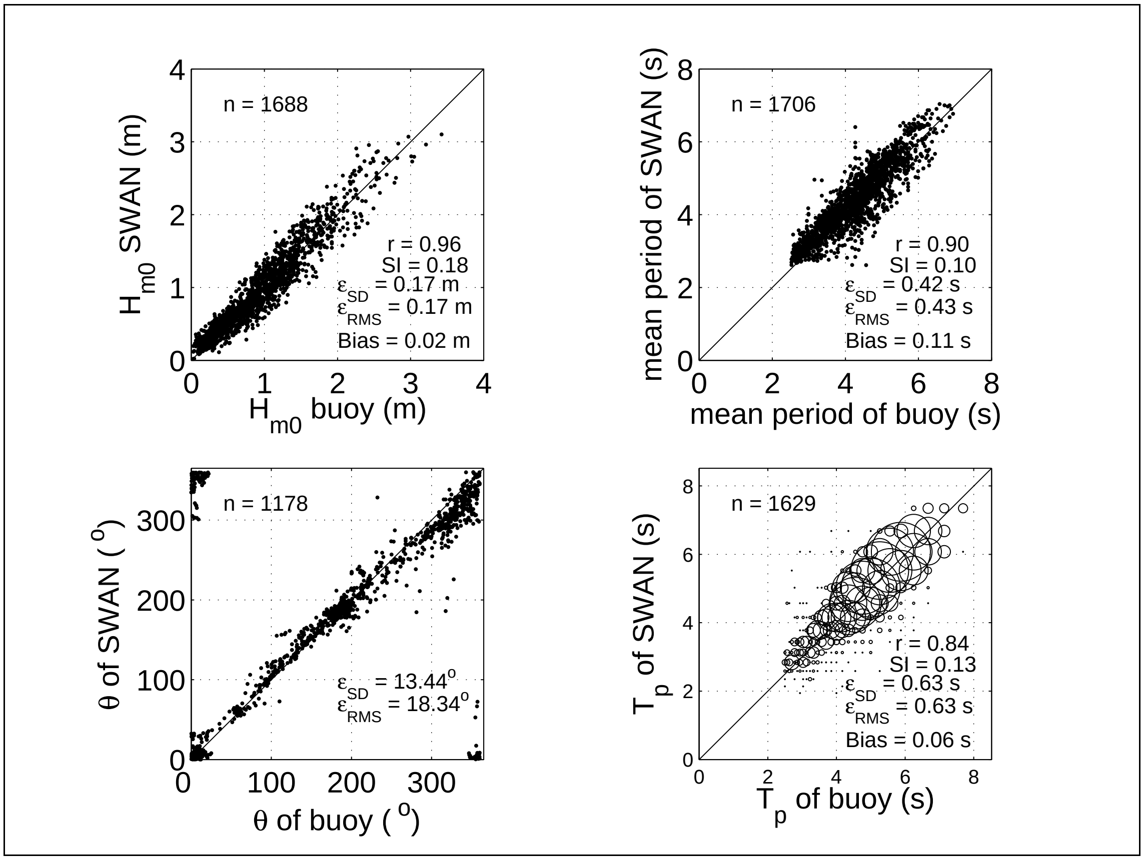

Figure 1. Comparison of bulk parameters. This figure also appears in the submitted article. Note that the populations for wave height and period are relatively large here, greater than 1500, because we are including all cases, not just those for which Hs>0.5 m.

2.1.2 Mixed seas and swells

In Lake Michigan, mixed sea/swell conditions are not common, but do occur occasionally after sudden changes in the wind speed/direction. Thus, it is important to know whether the model is sufficiently reproducing this effect. Based on the comparison of wave height, mean period, and peak period here, there is no concern that model spectra are often uni-modal while buoy spectra are bimodal or vice versa; persistent coincidental agreement over a long time series is not possible. In fact, one could argue that only two of these bulk parameters would be sufficient to alert a user of this problem, if it is persistent.

2.1.3 Averaged $F(f/f_p)$

The mean spectra, as a function of f/f_p is shown in Figure 2. Each individual spectrum was binned according to f/f_p and each bin integrated to calculate variance. At each bin, a time-average was calculated to get the mean value for the hindcast, shown in the top panel. The lower panel of Figure 2 shows the population used in the time average of each bin.

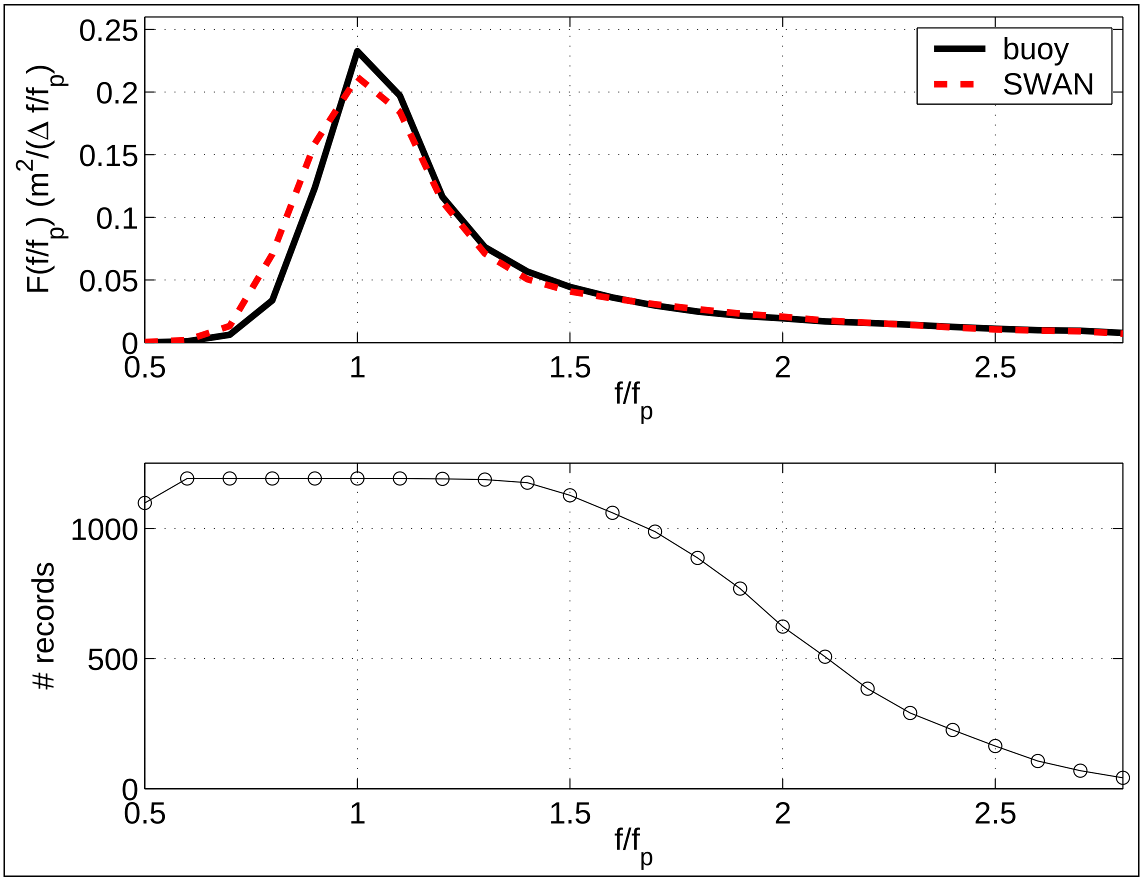

Figure 2. Comparison of mean spectral energy density as a function of f/f_p. The population used at each frequency bin is shown in the lower panel. In this plot, f_p corresponds to the individual synthetic peak period for model and buoy. This figure is included in the new submission.

2.1.4 Choice of method of calculating f_p for validation results

Unfortunately, choice of peak frequency does affect the results. For a given frequency, there are six options for calculating f/f_p:

1) use synthetic peak frequency of SWAN for the SWAN *F(f)*, and synthetic peak frequency of buoy for the buoy *F(f)*
2) use true peak frequency of SWAN for the SWAN *F(f)*, and true peak frequency of buoy for the buoy *F(f)*
3) use the synthetic peak frequency of buoy for both buoy and SWAN *F(f)*
4) use the true peak frequency of buoy for both buoy and SWAN *F(f)*
5) use the synthetic peak frequency of SWAN for both buoy and SWAN *F(f)*
6) use the true peak frequency of SWAN for both buoy and SWAN *F(f)*

Figure 3 shows the sensitivity of the results to this choice. The vertical axis of Figure 3 is the $F(f/f_p)$ of SWAN minus the $F(f/f_p)$ of the buoy (i.e. the blue line is the difference between the two lines in the top panel of Figure 2. Note that at f/f_p =1, there is no consensus regarding the sign of the bias. This plot tells us that the general trend in bias is robust (i.e. the same for all six methods) at all frequency bins *except* the "f/f_p =1" bin. Thus, we cannot conclude from Figure 2 whether the energy level at f/f_p =1 is overpredicted or underpredicted (the plot shows underprediction). At the other frequency bins, Figure 2 should provide a good indication of bias.

So from Figure 2, we can say that

1) The model has a tendency to overpredict low frequency energy.
2) At the bin closest to the peak, Figure 2 is ambiguous.
3) Above the peak, the model has little bias: a slight tendency to underpredict energy.

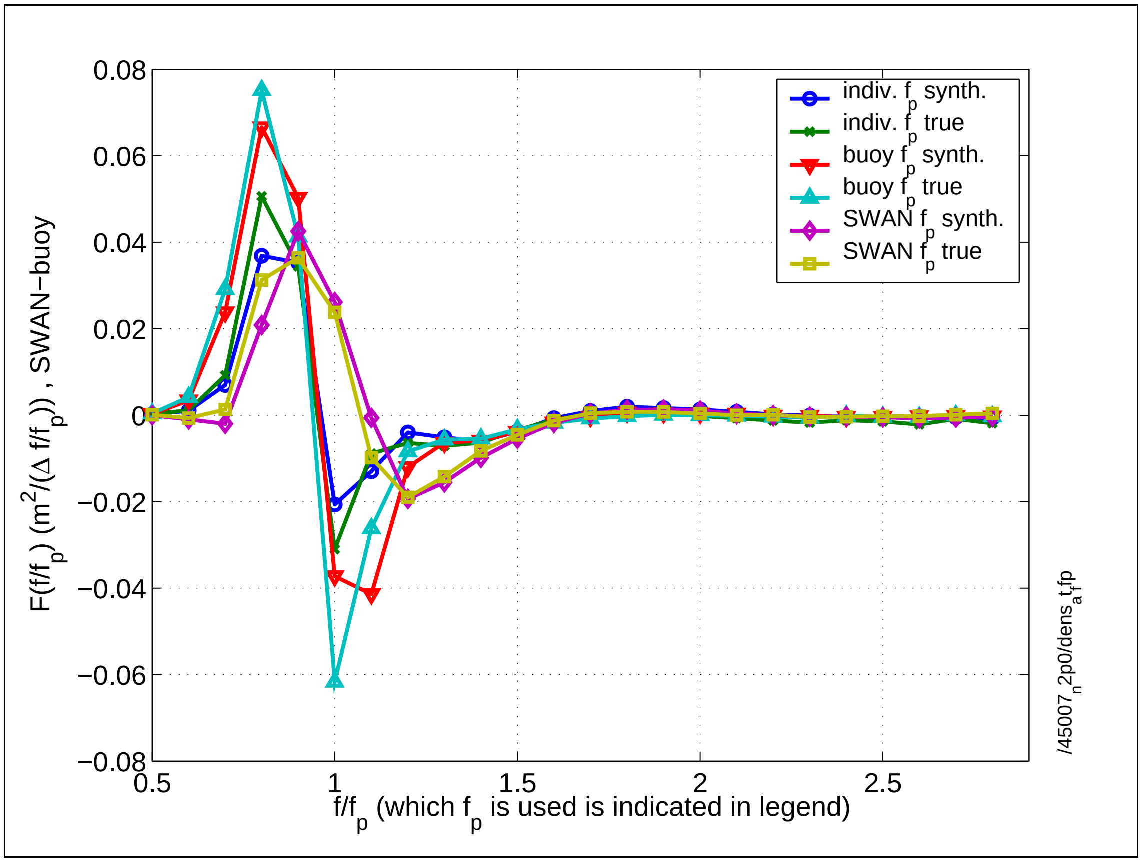

Figure 3. Model bias (in spectral density) as a function of f/f_p. This plot quantifies the sensitivity to the choice of method of calculating f_p.

2.1.5 Comparison without temporal summation (scatter plots)

Figures 2 and 3 show averaged results. Since we have time-collocated model and buoy variance values at each of 24 frequency bins, it is possible to show the data used to create Figure 2. This is shown in Figure 4. The "partial wave height", $H_{m0,partial} = 4\sqrt{\upsilon_{partial}}$ is shown. The plot shows excellent agreement near and above the peak, but the tendency to overpredict energy below the peak is clear.

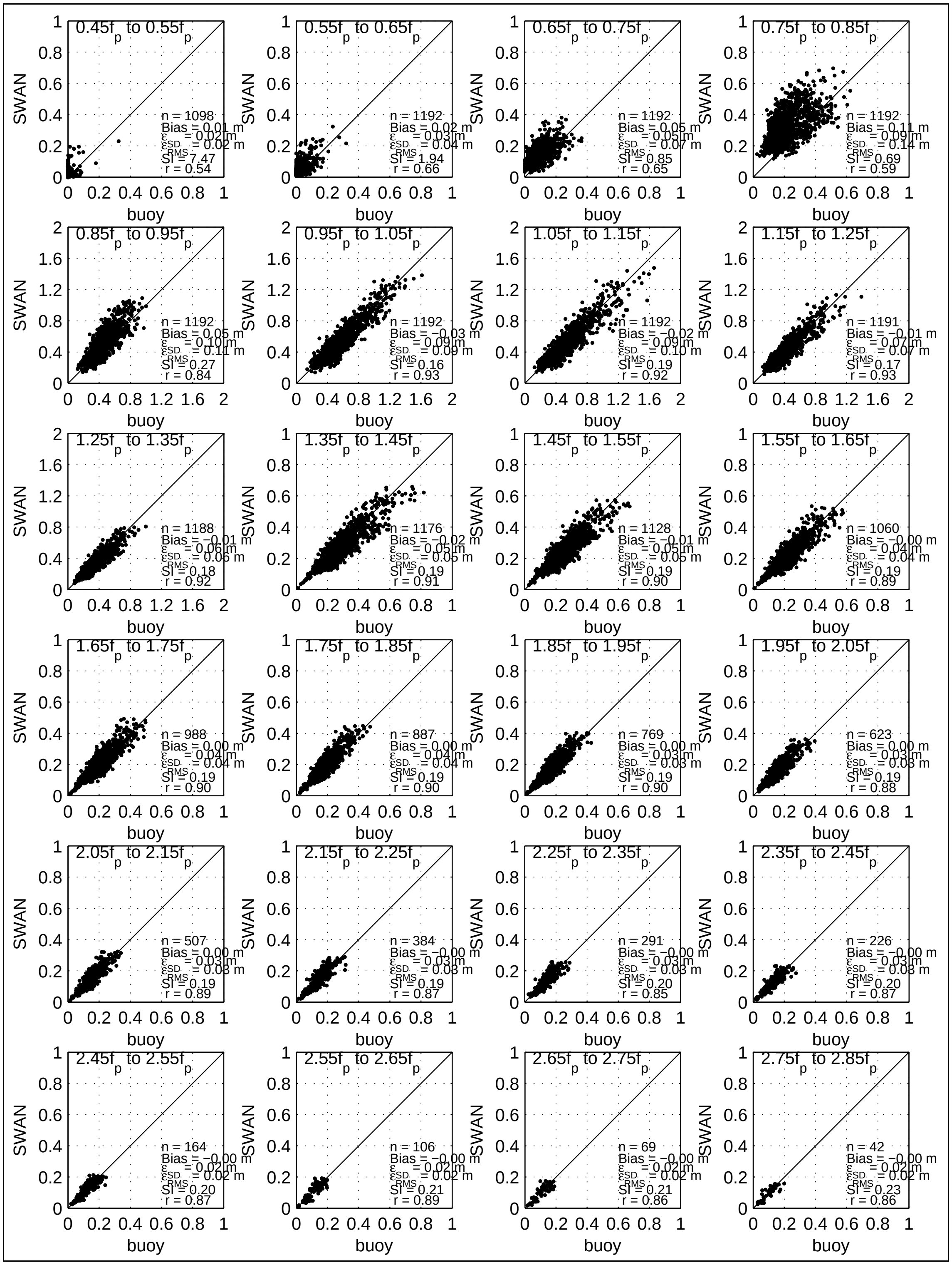

Figure 4. Comparison of "partial wave height" for 24 frequency bins.

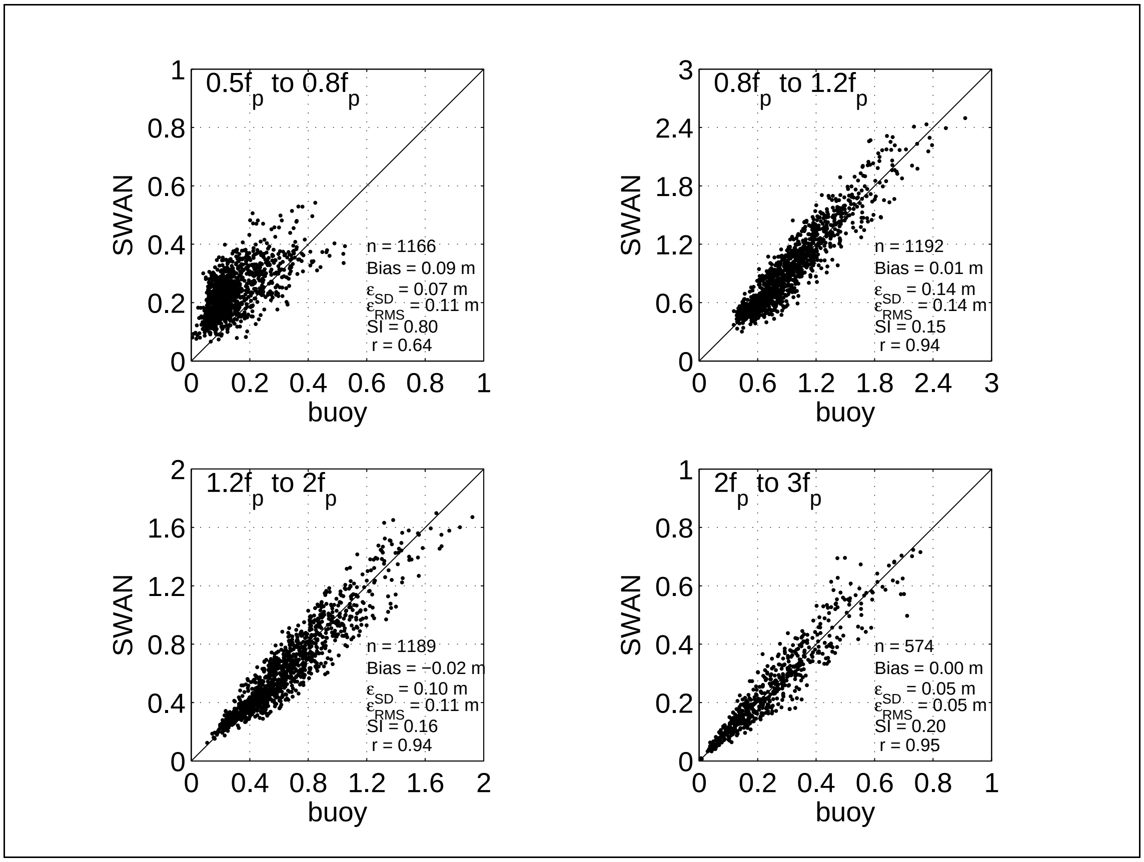

Figure 5. Comparison of "partial wave height" for 4 frequency bins, consistent with the bands used to compare directional spreading in the original submission of the paper. This figure is included in the submitted manuscript.

2.1.6 Summed F(f)

Since we have time-collocated model and buoy *F(f)*, we can simply perform a summation in time. Unlike the scatter plots of Figures 4 and 5, this does not quantify random error; instead it is more similar to the type of comparison shown in Figure 2. However, it is another useful way of looking at bias, and is not subject to sensitivity to the method of calculation of peak frequency. This is shown in Figure 6a,b.

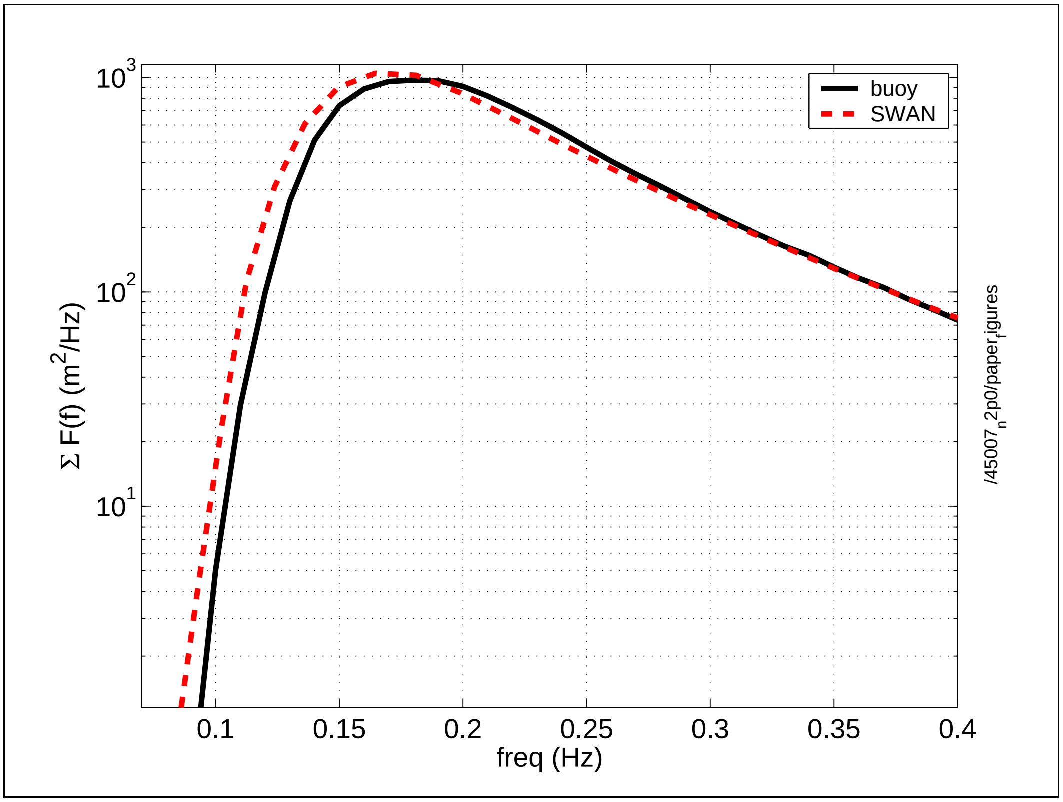

Figure 6a. Summation of buoy and model *F(f)*, on log scale.

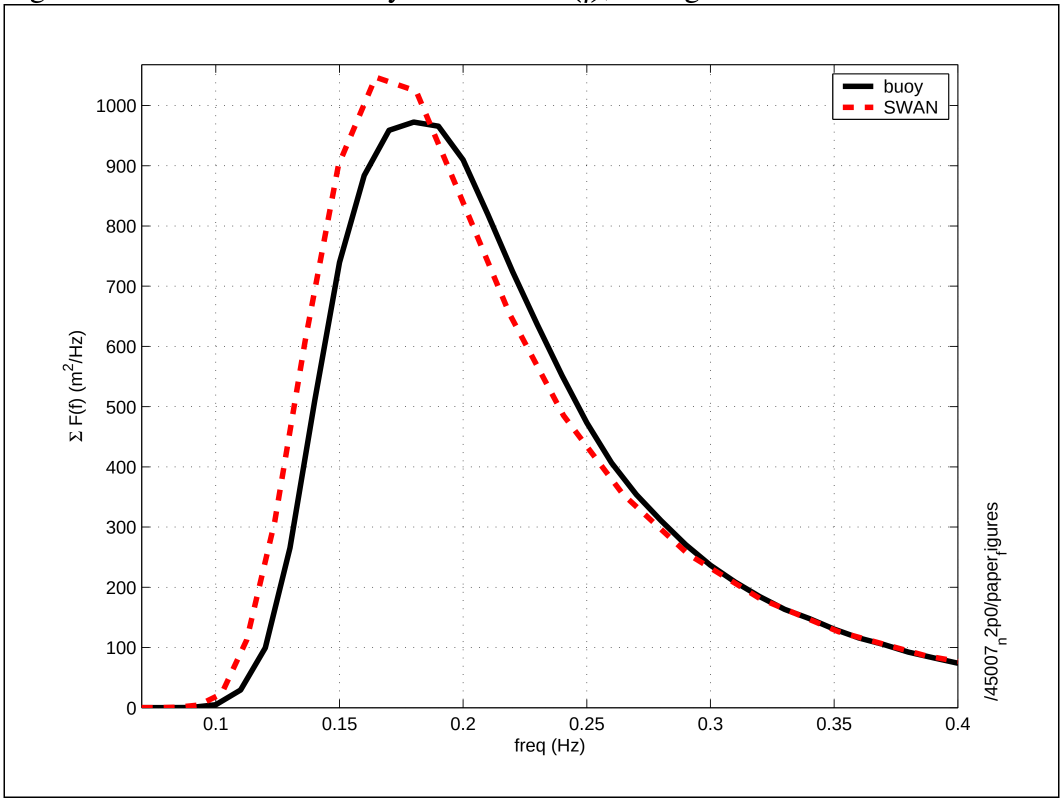

Figure 6b. Identical to Figure 6a, but on linear scale.

2.1.7 Pass/Fail Analysis

Next, we present an ad hoc pass/fail analysis. During the reviews, a reviewer recommended that we use an objective comparison of model non-directional spectra using an approach such as Alves et al. (2002). We do so here, albeit in a modified fashion. The Alves et al. [2002] method of overlaying one-dimensional spectra is not practical in our case, because we have 1759 spectra, as opposed to 132 in Alves et al. However, we did experiment with the approach of Alves et al., essentially producing an equivalent of their Table 7, but in graphical form, and with a simpler (albeit more arbitrary) pass/fail criteria. We experimented with a few different criteria; they all suggested the same thing: that the most persistent problem with the model was an overestimation of energy in the 0.7 and 0.85 f/f_p bins used in our analysis. The 0.85 f/f_p bin was the worst.

The idea here is to look at each model/data $F(f)$ comparison and for each frequency bin, to identify whether the model has "passed" or "failed (underestimation)" or "failed (overestimation)". Since we don't want to include low-signal cases in our comparison, we have a fourth condition/result: "both model and data have low energy levels", which we define as having spectral energy density $F(f)$ below 0.3 m^2/Hz.

The pass/fail criterion of Alves et al. (2002) is based on statistical uncertainty associated with degrees-of-freedom issues. It does not consider instrument error. This is justifiable, since there are no reliable estimates of instrument error in measurements of $F(f)$. Instead of basing our pass/fail criterion on statistical uncertainty, we use a simpler and rather arbitrary criterion based on our best guess of the error bounds of the measurements. Specifically, the model is consider to pass if $F(f)$ of the model is between 50% and 200% of $F(f)$ of the buoy. Because this method is very arbitrary, we do not feel that this comparison is appropriate for inclusion in a peer-reviewed journal. However, we present it here in this less formal document.

Figure 7 shows an example evaluation of a model spectrum. The example show "failure (overestimation)" at a frequency band below the peak. Figure 8 shows the actual results.

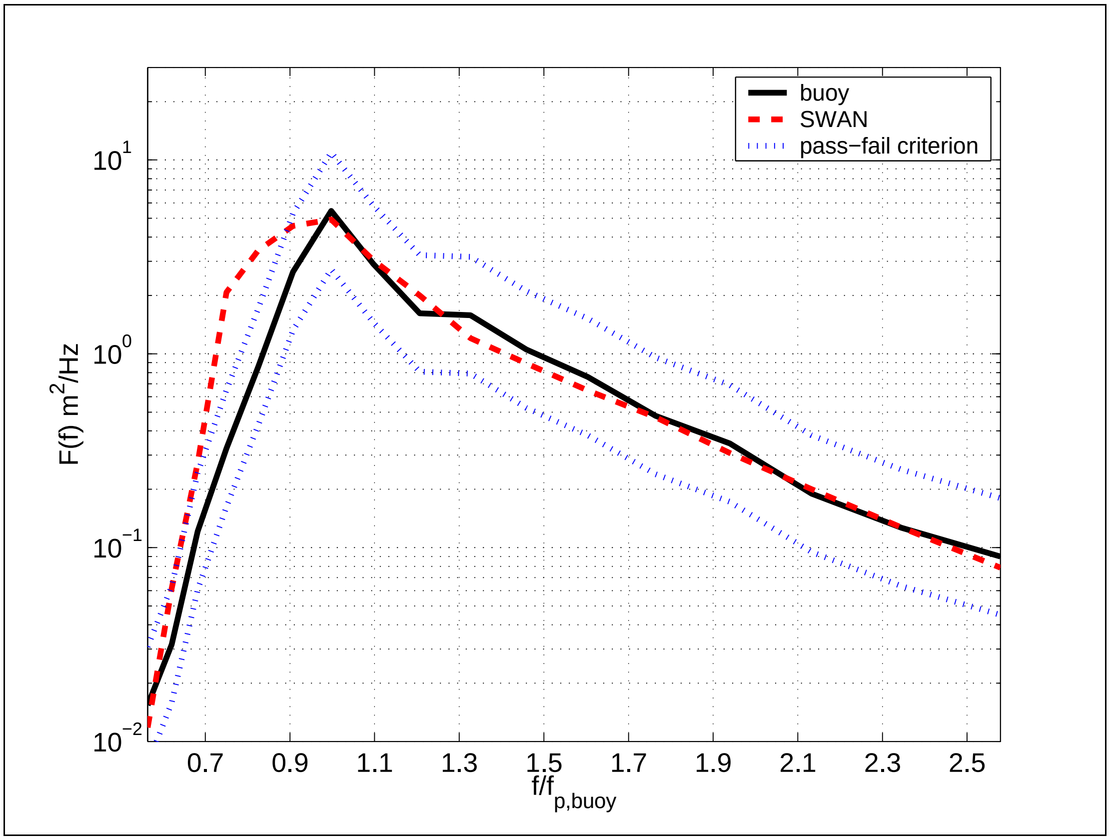

Figure 7. This plot shows a manually chosen example spectrum. This spectrum is chosen because it demonstrates a typical "failure" characteristic (overprediction of energy below the peak).

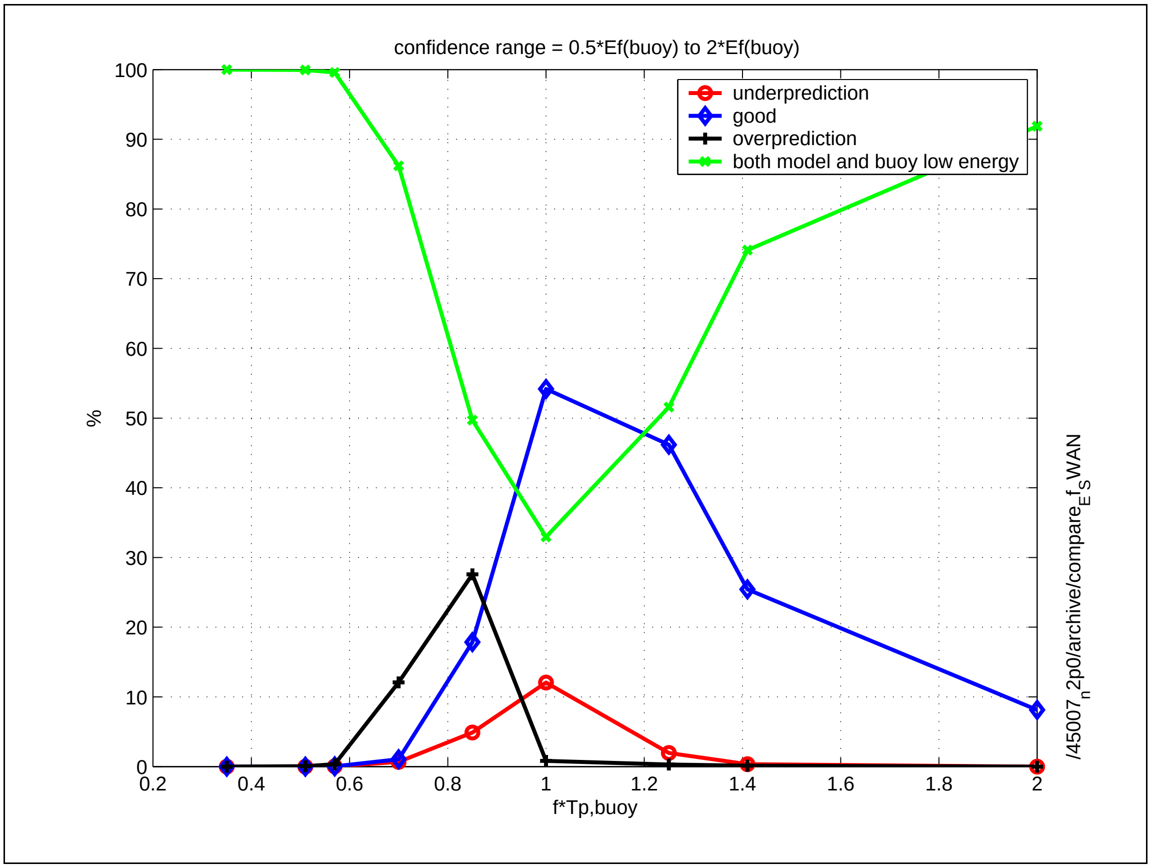

Figure 8. Results of the pass/fail analysis. This is on a coarsened frequency grid (8 bins). Note that a given coarse-grid bin may contain multiple components from the same spectrum; the percentage is based on the fine-grid population at each coarse grid bin.

2.1.8 Sufficiency of bulk parameters comparison

Of course, all of this analysis is quite time-consuming. It is reasonable to ask "Is it enough to simply include the comparison of bulk parameters (Fig. 1)?" We have already argued above that these bulk parameters will not always alert us to problems with spectral width, so the answer would have to be "No. Comparison of these bulk parameters will not always be sufficient". Another reasonable question: "Would it have been sufficient in this particular case?". Figure 9 below shows a subset of the total population of the hindcast time series. The subset consists of the cases in which the SWAN spectral energy density is more than twice the buoy spectral energy density in this frequency bin. Mean period is overpredicted at almost every point. Thus, it is apparent that a scatter-plot comparison of mean period is an effective method of revealing this most serious defect in this simulation, with regard to prediction of non-directional wave spectra. Figure 9, is very convincing, but we must concede that the problem is visually more alarming and less ambiguous in the case of the *F(f)* comparisons.

At any rate, the reviewer's insistence that we include comparisons of non-directional spectra in the manuscript was justified, significantly improving the paper (our opinion).

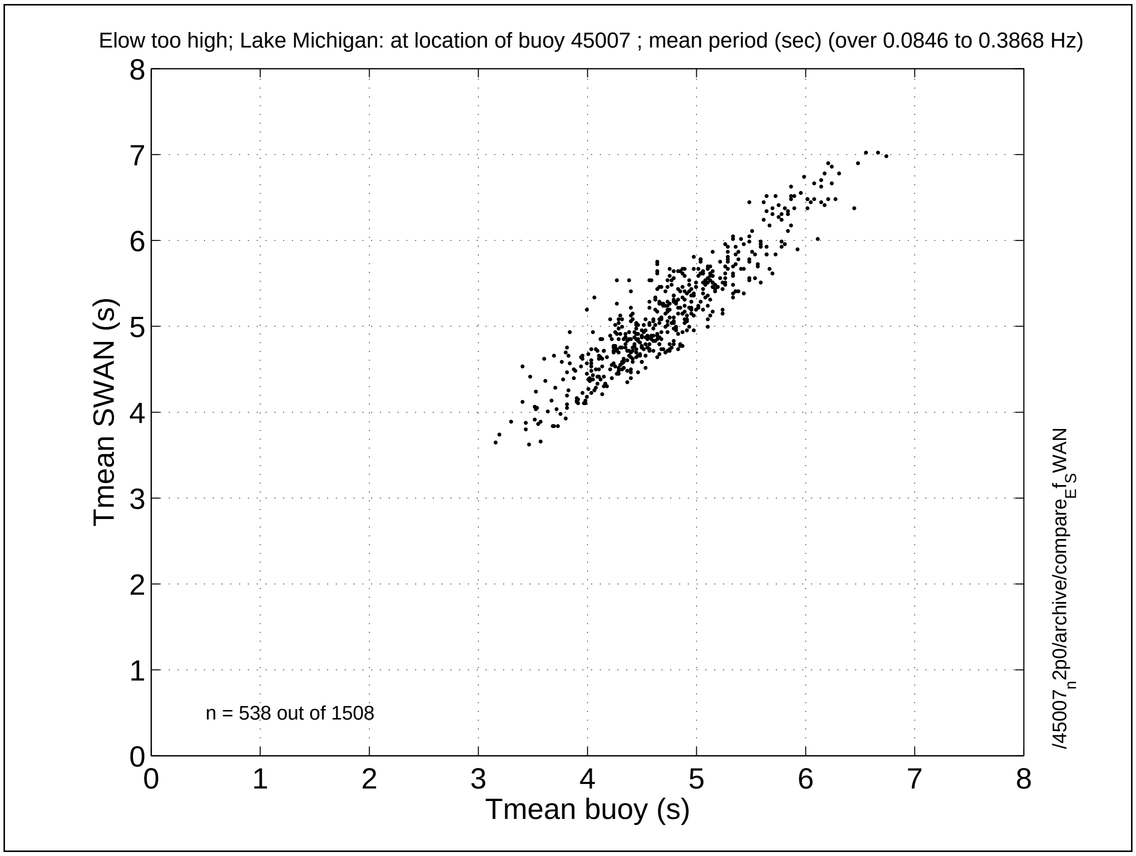

Figure 9. Scatter plot comparison of T_{mean} for a subset of the population for which at least one spectral energy density component near f/f_p =0.85 is overpredicted by SWAN.

2.1.9 Accuracy of F(f) relative to the "state of the art"?

Despite the apparent modest positive bias in mean period in Fig.1, the overall accuracy of mean period is still very good, and in fact the accuracy of the frequency distribution compares favorably with the accuracy of the most accurate of six model permutations tested by Alves et al. This is not accidental. Nor do we mean to imply that SWAN is somehow superior to these variants of WAM. Rather, we deliberately chose this type of simulation (Lake Michigan modeled with SWAN) because of skill with regard to non-directional wave spectra demonstrated in previous exercises. This accuracy makes it possible to validate the directional characteristics. This is discussed in RW.

There are not many previous comparisons of *F(f)* in the literature for long duration hindcasts; Alves et al. (2002) is one of them. We believe that the level of agreement as demonstrated in Figure 4 is as good, or better than, all previous evaluations of model performance. As such, we believe that it is a good representation of the best that can be achieved with the current state of the art. However, the results are still not entirely satisfactory; they are only "fairly accurate". Significant problems are evident below the peak.

Of course, this judgment of "fairly accurate" remains subjective and we unfortunately do not have the resources to make adequate comparisons to prior simulations by other researchers.

2.2 Directional spreading validations.

2.2.1 Directional Spreading Results: Are they "Universal"?

One of the reviewers of the manuscript repeatedly stated that the manuscript claims that results obtained with the SWAN model are representative of other 3G models.

In RW, we do not intend to give the impression that our observations about model behavior in this hindcast are universal, i.e. that readers should expect to see the same behavior in all other simulations. Of course, we would like to believe that the results are not an anomaly, but we implicitly acknowledge that more evidence would be required to make sweeping statements.

It is, in fact, common sense that results are not universal. If ECMWF reports a positive bias in wave height with the WAM model in the North Sea, would they be able to claim that the NWS running WW3 in Lake Superior will also see a positive bias? Of course not.

In the manuscript, we do not address the issue of sensitivity of results to the model used. However, we did perform a new SWAN simulation, this time with the weighting of the relative wavenumber *n* in the steepness-limited dissipation term as *n*=1.5 instead of *n*=2.0. In fact this does change the results. See Figure 10. There is not much change in bias at and below the peak, but in the third frequency band (1.2 f_p to 2.0 f_p), the bias is 5° (instead of 1°). In the fourth band (2.0 f_p to 3.0 f_p), the bias is 5° instead of -1°. Thus we demonstrate what should be already obvious: that results presented in the manuscript are not "universal".

Despite the debate, there is agreement on the main point, which could be summarized as: "It would be inappropriate to make claims in the manuscript that the hindcast results presented in the manuscript are universal to all models."

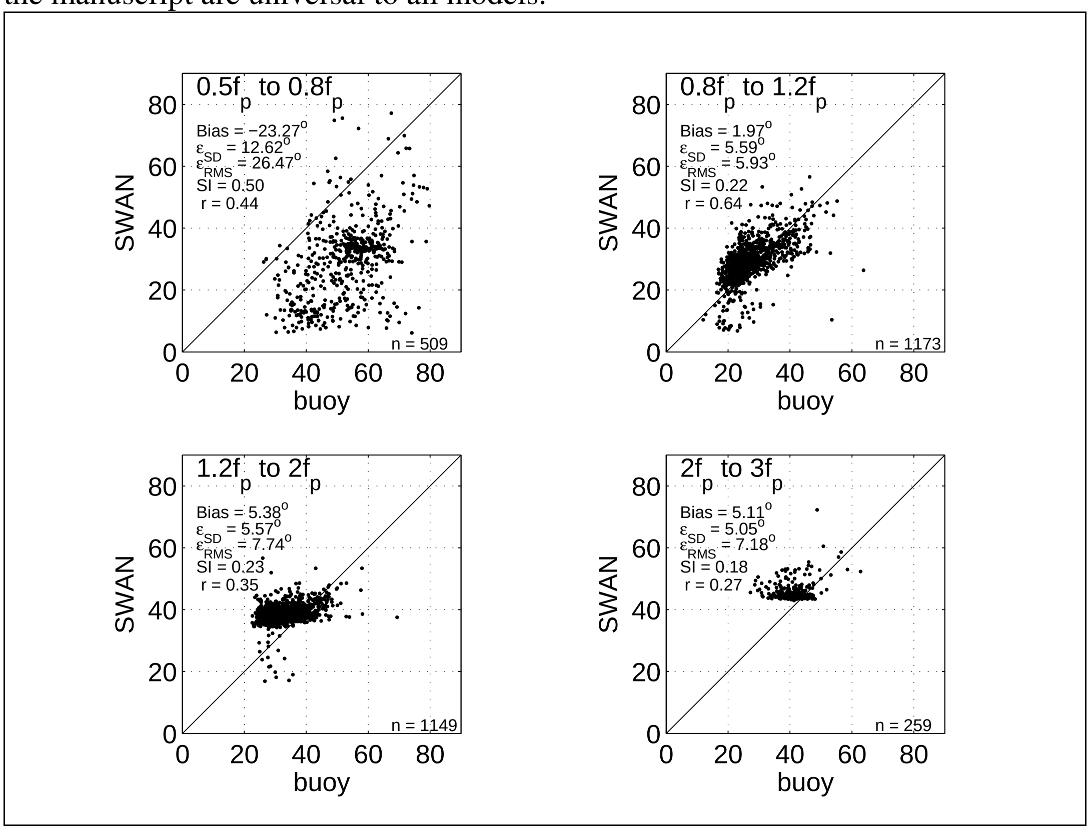

Figure 10. Directional spreading comparison using *n*=1.5 instead of *n*=2.0 in S_{ds}. This demonstrates that the RW results are not universal.

2.2.2 Validation of other models

A possible extension of RW would be to repeat our hindcast with another model. We have, in fact, simulated the same case using WAVEWATCH III. They were not included in RW primarily due to page limits of the journal. However, we further decide not to reproduce them here for two reasons:
1) The reproduction of the non-directional spectra was unsatisfactory, so presentation of directional statistics was not justified. As an aside, the unsatisfactory results do not necessarily reflect poorly on that model; it may be due to omission of stability effects, for example.
2) We do not want the readers to be distracted by concern over which model is “better”. Determining this is not an objective of this study. When one presents statistics for two models, determination of the winner and loser inevitably focuses some readers’ attention. Further, we believe that such “bake-offs” are a negative trend in the literature; these types of conclusions can be meaningless unless closely scrutinized, since models can be right for the wrong reason and vice versa.

2.2.3 Other methods of calculating directional spreading

Besides the circular RMS directional spreading metric, some other methods are: 1) the Yamarinto (1984) [also Komen et al. 1994] method, 2) the Babanin and Soloviev (1998) method, 3) $\cos^{2s}$ method (via fitting) used in Forristall et al. (1978), 4) the force-reduction method used by Forristall and Ewans (1998), and 5) the method used by Young (1999, pg. 128), which is referred to as the “mean directional width” and the “mean spectral width”, which is:

$$\theta_\sigma(f) = \frac{\int_0^{2\pi} E(f,\theta)\left|\theta-\theta_0\right| d\theta}{E(f)} ,$$

…except insofar as the absolute value operation is not included in Young (1999).
To provide a sense for how the calculations differ, example calculations by the two methods are shown in Fig. 11.

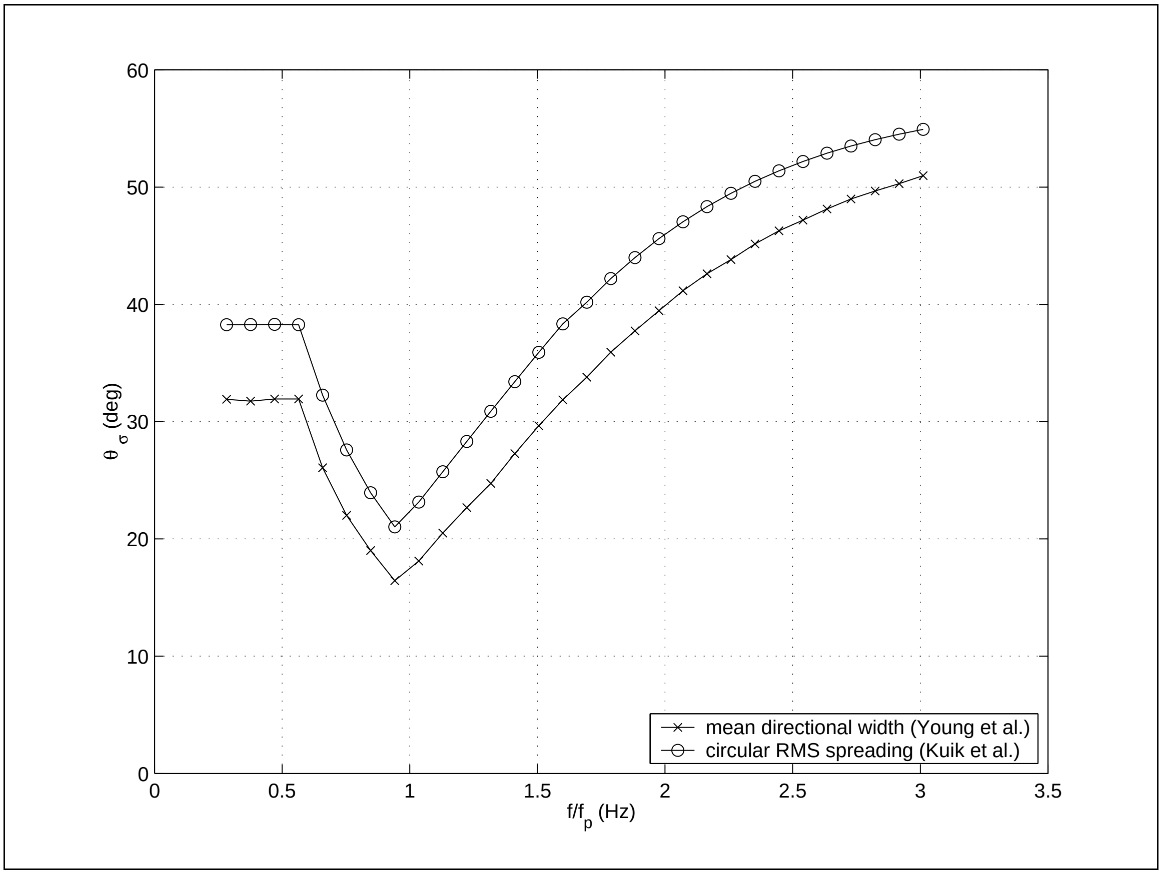

Figure 11. Directional spreading $\theta_\sigma(f)$ of an arbitrary spectrum, according to the metrics used by Young (1999) and Kuik et al. (1988). The circular RMS spreading used by Kuik et al. is also used in this study.

2.2.4 Skewness and Kurtosis

One possible criticism of the submitted paper, RW, is that it only deals with directional spreading and (to a lesser extent) mean direction. Skewness and kurtosis of the directional distribution function can also be inferred from buoy motion. Thus it can be argued that RW is not a complete "directional validation using buoy observations". As is mentioned in RW, measurement uncertainty is greater with these higher order moments (Kuik et al. 1988 give quantitative estimates of uncertainty, summarized in Anctil et al. 1993 and RW). Measurement uncertainty is already problematic enough with the lower order moments. Thus, though comparisons of skewness and kurtosis were performed, the comparisons were not included in RW. Two figures here—from the idealized simulations—are examples of omitted plots.

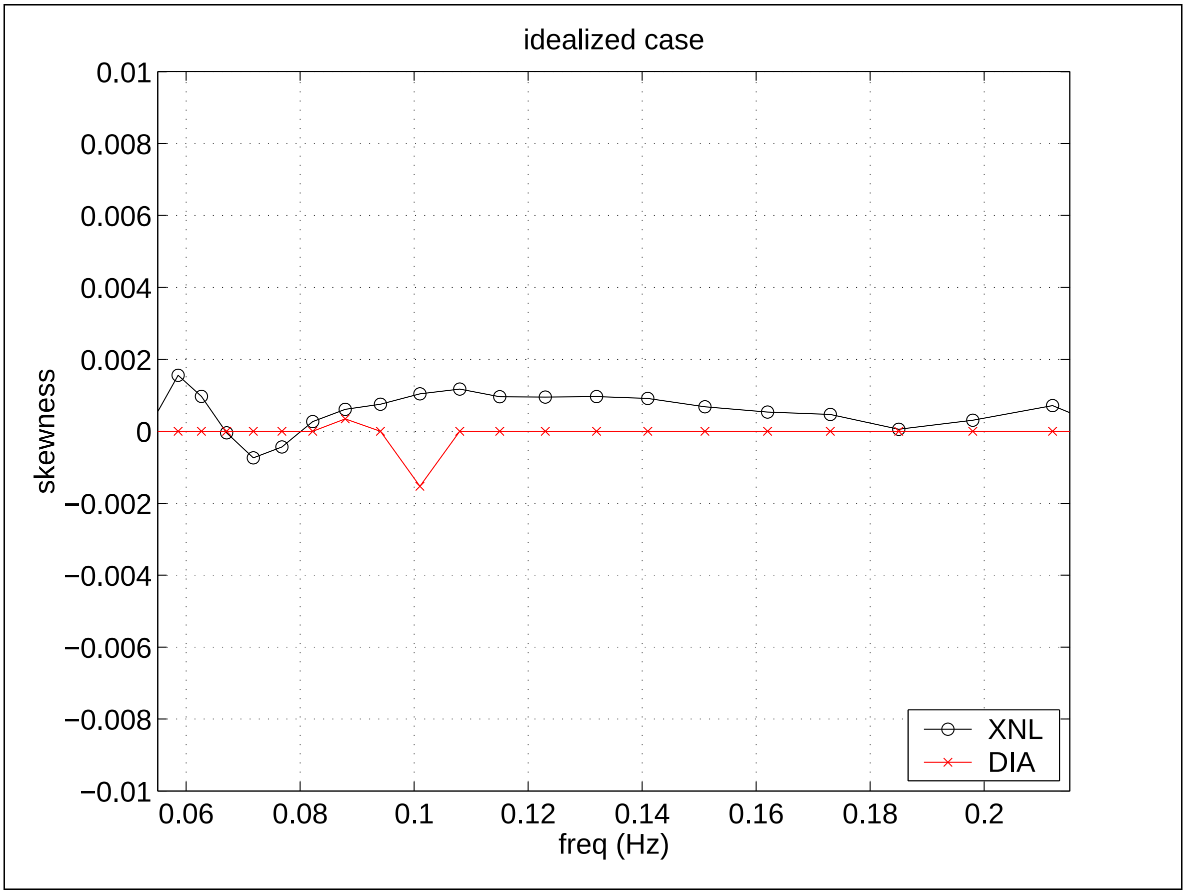

Figure 12. Skewness comparison for the idealized case of the submitted manuscript (XNL here is produced using WW3).

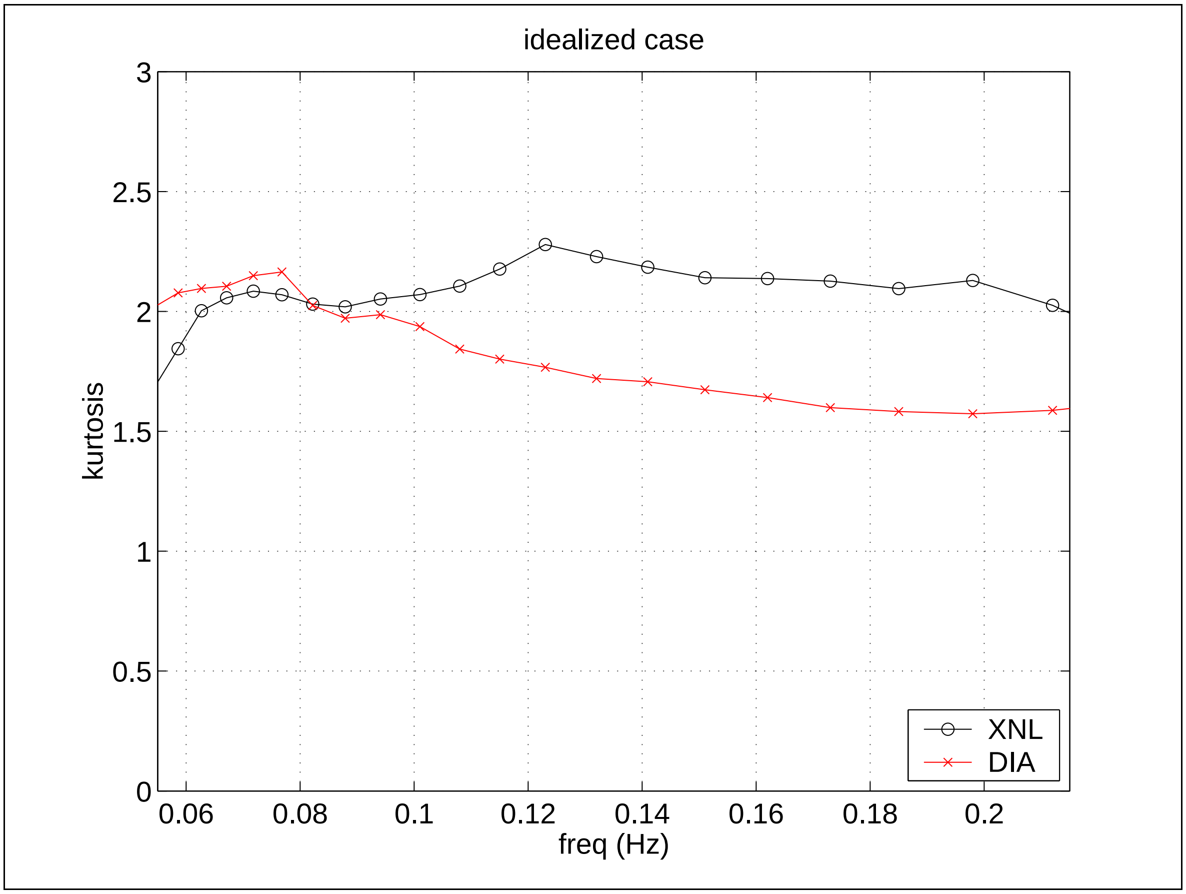

Figure 13. Kurtosis comparison for the idealized case (XNL here is produced using WW3).

2.2.5 Data-adaptive methods/models

Data-adaptive methods and models are practical to use and some (e.g. Maximum Entropy Method, Lygre and Krogstad 1986) do not change basic parameters like mean direction and directional spread. Probably, anyone who applies such methods will be aware of the limitations, but our experience is that someone who is simply provided with the resulting directional spectrum is often unaware of the limitations.

As stated in RW, we choose not to use these methods because they introduce an element of subjectivity that can be avoided by utilizing the Fourier coefficients directly. One of the reviewers of RW was sensitive to criticism of the use of data-adaptive methods and models in validation exercises, so it was necessary to soften the tone of this discussion. For example, the original submission stated with regard to data-adaptive models such as the $\cos^{2s}$ form, "but these models [and methods] can be misleading, since they give details of $D(f,\theta)$ that are not actually determinable from buoy motion." Sensitivities aside, it remains an interesting point of debate. Kuik et al. (1988) state it well: "All these methods of the first class suffer from various shortcomings for routine applications: they may suggest a misleading directional resolution, the shape assumption may not be justified or the results require skilled interpretation. Reconstruction of $D(\theta)$ from pitch-and-roll buoy data should therefore be undertaken only in a non-routine manner, if the distribution of wave energy over directions is a strictly required quantity in any further processing." [As an aside, we should be cognizant of the distinction between data-adaptive methods (such as Maximum Entropy Method) and data-adaptive models, such as the $\cos^{2s}$ model; we strongly recommend reading this discussion in Kuik et al. (1988) within context.]

One interesting defect of the $\cos^{2s}$ data-adaptive model (and presumably most other types) is that it introduces limitations on the ranges of the four directional moments (mean direction, directional spreading, skewness, kurtosis) and dependencies between these moments which are not defensible (Kuik et al. 1988), for example skewness has a functional dependence on spreading with the $\cos^{2s}$ model.

When the Maximum Likelihood Method (or "Maximum Likelihood Estimator", described in Oltman-Shay and Guza 1984) is applied to buoy Fourier coefficients (a_1,b_1,a_2,b_2) to create a directional distribution $D(\theta)$, and then subsequently integrated back to Fourier coefficients (a_1,b_1,a_2,b_2), the original Fourier coefficients will not be recovered: the resulting coefficients suggest a more broad directional distribution than the moments actually measured by the buoy. In our experience, the Iterative Maximum Likelihood Method (Oltman-Shay and Guza 1984) also suffers from this problem. However, this experience is apparently contradicted by a statement in Benoit et al. (1997), so we hesitate to generalize. The Maximum Entropy Method does not suffer from this problem. However, Benoit et al. (1997) criticize that method for overpredicting the height of directional peaks and sometimes creating spurious bimodal distributions.

2.2.6 Directional resolution

In RW, a directional resolution of 10° is used. Thus, it is not much higher than the resolution used in most operational models today (15°). Are our results sensitive to directional resolution? Perhaps; for example we do not know how sensitive the nonlinear computations are to directional resolution for our case. However, one should keep in mind that the calculations of the directional moments are integrations. Therefore, fact that the RMS directional spreading and the resolution are the same order of magnitude should not be cause for alarm.

2.2.7 Calculation of synthetic peak period

In the hindcast analysis of RW, we use a "synthetic peak period" which is a simple function of the mean period, a more stable quantity than true peak period. The relation is determined using a simple linear regression of the two metrics for the time period of the hindcast. The mean period is calculated over the frequency range of 0.07 to 0.4 Hz.
For the modeled values, the result of the regression is:

$$T_p = 1.2142 T_{mean} - 0.7126$$

For the buoy, the regression is:

$$T_p = 1.2325 T_{mean} - 0.70509 .$$

The small discrepancy between the two suggests a modest problem in modeled spectral shape, already discussed above.

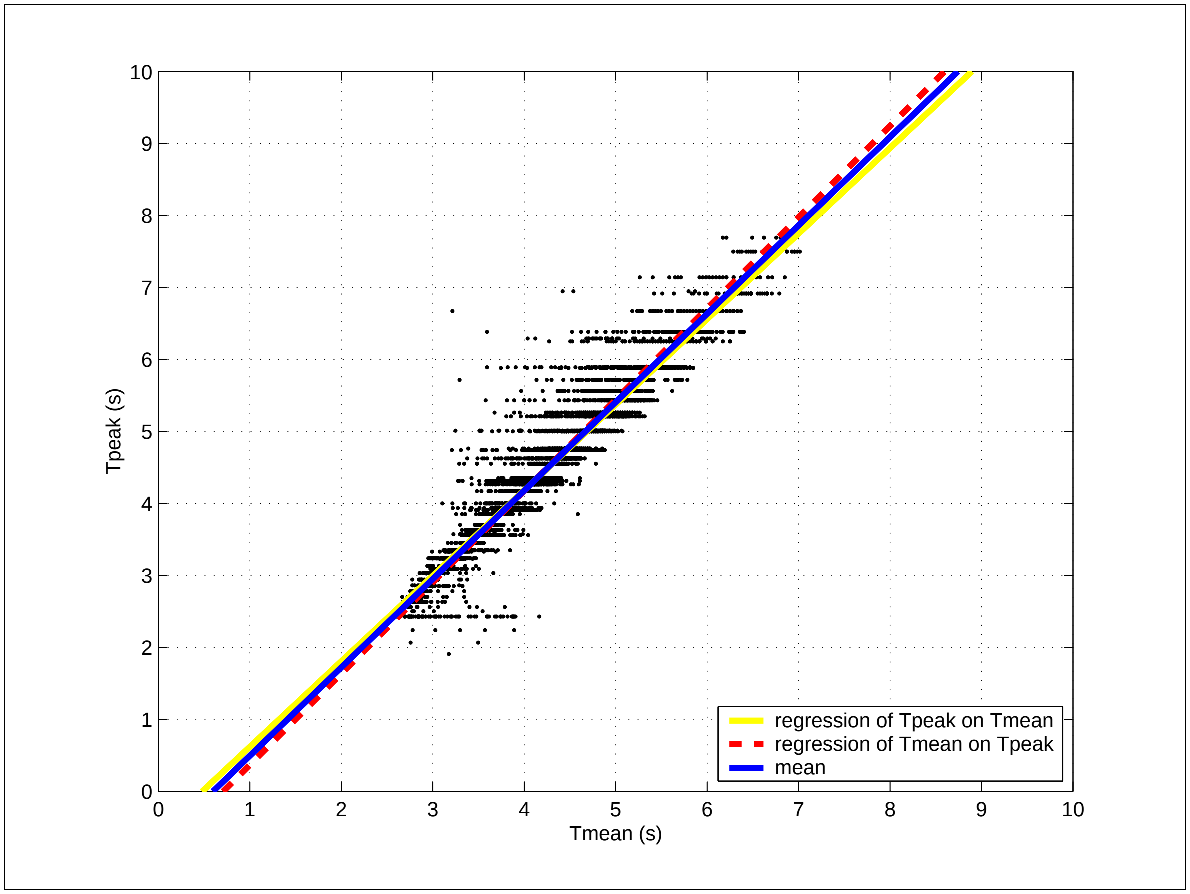

Figure 14. This shows how we create a "synthetic peak period" using mean period. It is more stable than the actual peak period. Here, both buoy and SWAN values are used in the regression.

2.2.8 Narrow fetches

One reviewer made the comment that geometry can have an effect on directional spreading. This is almost certainly correct. However, the reviewer went further by stating that the geometry of the lake might invalidate our results. This was based on two assertions. There are major errors in both cases:

1) One assertion was that two cited papers show that current wave model physics cannot deal properly with wave growth in limited fetch geometries. Only one cited paper makes this assertion and it does so without any substantiation. The other cited reference *directly contradicts* this assertion and is substantiated by a citation. Further, we expect that the assertion is wrong, based on our own experiments (see below).
2) The other assertion was that our case was a "narrow fetch". The reviewer cited "open ocean" (his/her wording) data in Ataktürk and Katsaros (1999). However, Ataktürk and Katsaros (1999) actually call it "larger bodies of water" data, and it turns out that this larger body of water is Lake Ontario, similar in size to Lake Michigan (see below).

Our detailed response follows:

"The reviewer makes an excellent point, and these are some interesting papers, which we had not previously read. We agree with the reviewer and the cited references—Kahma and

Pettersson (1994), and Ataktürk and Katsaros (1999)—that directional properties can be significantly affected by fetch geometry. Of course, this isn't necessarily a problem for the paper. It is only a problem if the 1) geometry is narrow and 2) the model physics cannot handle narrow geometries. We can discuss two questions separately: 1) "From the position of NDBC 45007, is Lake Michigan narrow?" And 2) "Do the models have problems with narrow fetches?"

To answer the first question, we would say "sometimes, but mostly, no". If one looks at the position of NDBC 45007, one will notice that the fetch might only be considered "narrow" in cases where the wind is blowing from the north. We will also point out that in the Ataktürk and Katsaros, the "open ocean data" are actually labeled as data from a "larger body of water" and are in fact from Lake Ontario! The latter may be expected to have fetch characteristics similar to Lake Michigan. Anyway, we do admit the potential for modestly narrow geometry in our hindcast, and we move on to the next question…

"Do the models have problems with narrow fetches?" We do not see any evidence that 3G wave models are incapable of reproducing this effect. This effect is actually produced by model kinematics—as opposed to adjustment of the source/sink terms—and if one can accept linear wave theory, we know that the models are quite good at kinematics. In Kahma and Pettersson (1994), the authors do state, "…WAM …shows little difference in the energy of wave growing from a straight shoreline or in a narrow bay, provided the grid is fine enough that the bay contains more than one gridline." However, this statement occurs in the introduction and is not supported (an anomaly in an overall very nice paper). We performed our own experiments with SWAN and one does see a very significant difference in wave growth, as one would expect (Figure [15] included here). So perhaps the statement in Kahma and Pettersson (1994) is based on some problem with early versions of WAM or is due to some sort of misunderstanding. In fact, Ataktürk and Katsaros (1999) cite WAM4 results which, similar to our SWAN tests, *do* exhibit the expected effect of geometry on wave growth. Of course, the exhibited effect is qualitatively correct but not necessarily quantitatively correct. At any rate, the geometric effect is worth mentioning, so we have added a discussion to the text."

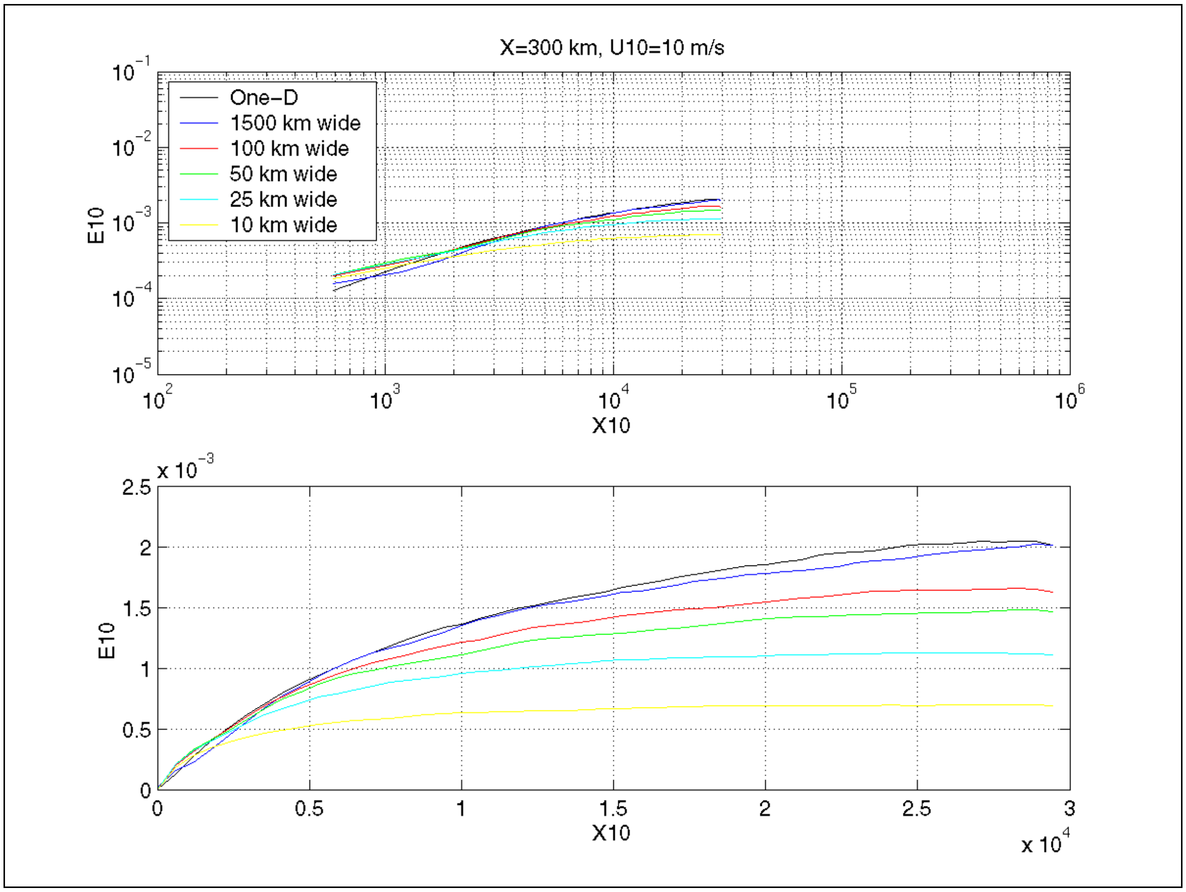

Figure 15: Wave growth tests with SWAN, with variable width. "One-D" is essentially infinite width. Upper and lower panels show the same information, on different scaling. Results are non-dimensionalized with U10. The total length of the computational grid is 300 km. The model shows the expected reduction in wave growth with more narrow geometries. SWAN does exhibit some strange behavior at small fetches, visible in log-log plot, but we expect that this would disappear with increased resolution.

2.2.9 Sensitivity to the high frequency tail

Banner and Young (1994) compare results with a dynamic cutoff, which is a shortcut used in WAM and WW3 to reduce computation time, versus results using a more rigorous cut-off, fixed at a high frequency. They note drastic differences in the results, which is of course alarming, since the shortcut is standard in WAM. A reviewer argues that our results may be invalid, claiming that SWAN uses a dynamic cut-off between the prognostic and diagnostic portions of the spectrum, and citing these drastic differences, presented in Banner and Young (1994) and Alves and Banner (2003). The reviewer is wrong. SWAN uses a fixed cut-off, not a dynamic cut-off (Booij et al. 2005), and the fixed cut-off that we use in RW is even slightly *more conservative* that the fixed cut-off presented as a *rigorous* method in the comparison of Banner and Young (1994). Regarding the *idealized simulations*: in the original submission, we *did* use WW3 (which employs the shortcut). We replaced these results with SWAN (which does not employ the shortcut) simulations, but as it turns out, the results were qualitatively similar. Thus our experience is inconsistent with Banner and Young (1994). Our detailed response follows:

“On the subject of the high frequency tail with the SWAN simulations, we used a high frequency limit of 1.0 Hz. With the hindcast, we are comparing with buoy data; the highest frequency in the directional buoy data is 0.35 Hz. Thus the frequency range of 0.35-1.0 Hz is part of the computational grid but is not included in comparisons. In other words, there is a large separation in frequency-space between the diagnostic tail and the presented results. In fact, our cut-off frequency of 1.0 Hz is higher than the cutoff used by Banner and Young (1994) in their “unconstrained tail” calculations, 0.92 Hz. Quoting Banner and Young (1994) “Extensive numerical sensitivity tests showed that, provided the explicit computational grid was extended to quite high wavenumbers, such as used there, the computational tail extension only biased the highest two or three spectral wavenumber bins.” Further, SWAN uses a fixed tail rather than a self-adjusting cutoff. The SWAN manual states “Above the fixed high-frequency cut-off (typically 1 Hz for field conditions) a diagnostic *f* tail is added. This tail is used to compute nonlinear wave-wave interactions at the high frequencies and to compute integral wave parameters. The reason for using a fixed high-frequency cut-off rather than a dynamic cut-off frequency that depends on the wind speed or on the mean frequency, as in WAM and WAVEWATCH-III,…”.

On the subject of the high frequency tail with the WW3 simulations (for the idealized cases in the original submission), we again used a high frequency limit of 1.0 Hz, but in the context of WW3, this has a different meaning. Computations are abbreviated well below 1.0 Hz, at 0.25-0.31 Hz (see normalized spectrum from XNL simulation in figure below). Fortunately, the figures shown in the manuscript do not include frequencies outside this range; they stop at 0.22 Hz. The bimodal structure is clearly visible in the region of the spectrum (up to 0.22 Hz). Unfortunately, we still have the problem that the diagnostic tail may be polluting the prognostic part of the spectrum. This is the problem that the reviewer appears to be concerned with. To address the reviewer’s concern, we could disable this adjusting-cutoff feature in the WAVEWATCH-III. However, since we wrote the original manuscript, XNL has been implemented in SWAN. Therefore, we take the easier route of replacing WW3 simulations with SWAN simulations. The differences turn out to be relatively minor. It would be interesting to contrast this observation with the very different observation of Banner and Young, but that would be a separate study.”

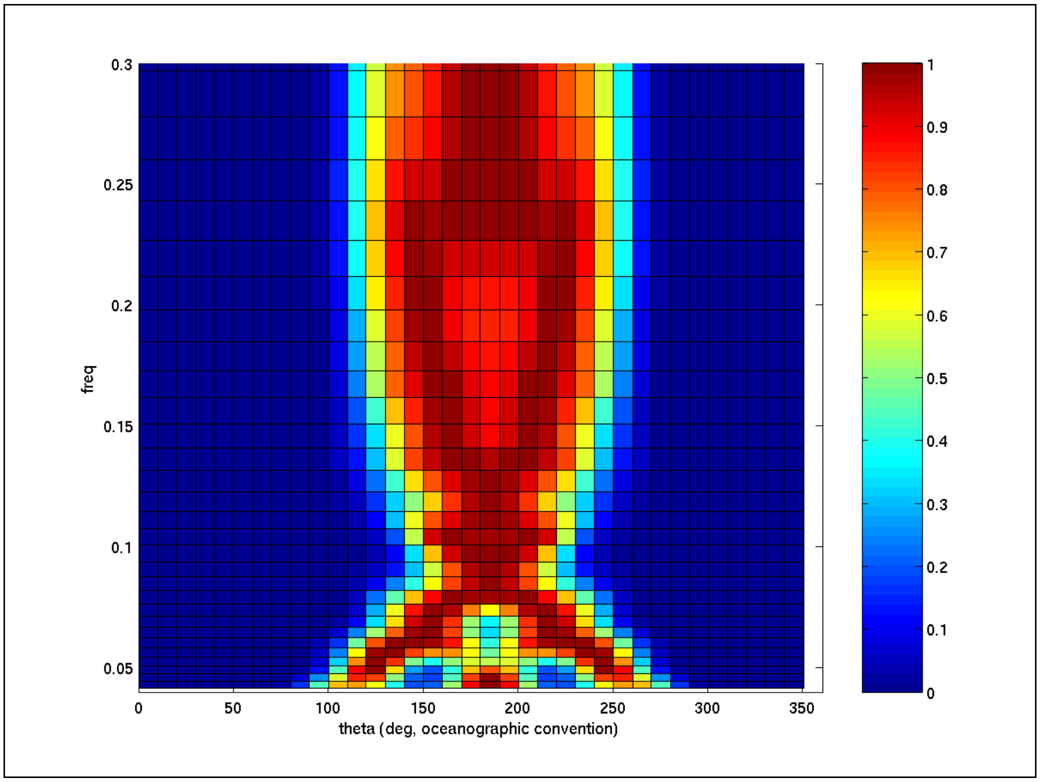

Figure 16: Normalized spectrum from XNL simulation. The figure may not reproduce well in black and white, but still the bimodality at 0.05-0.075 Hz and 0.15-0.20 Hz should be evident.

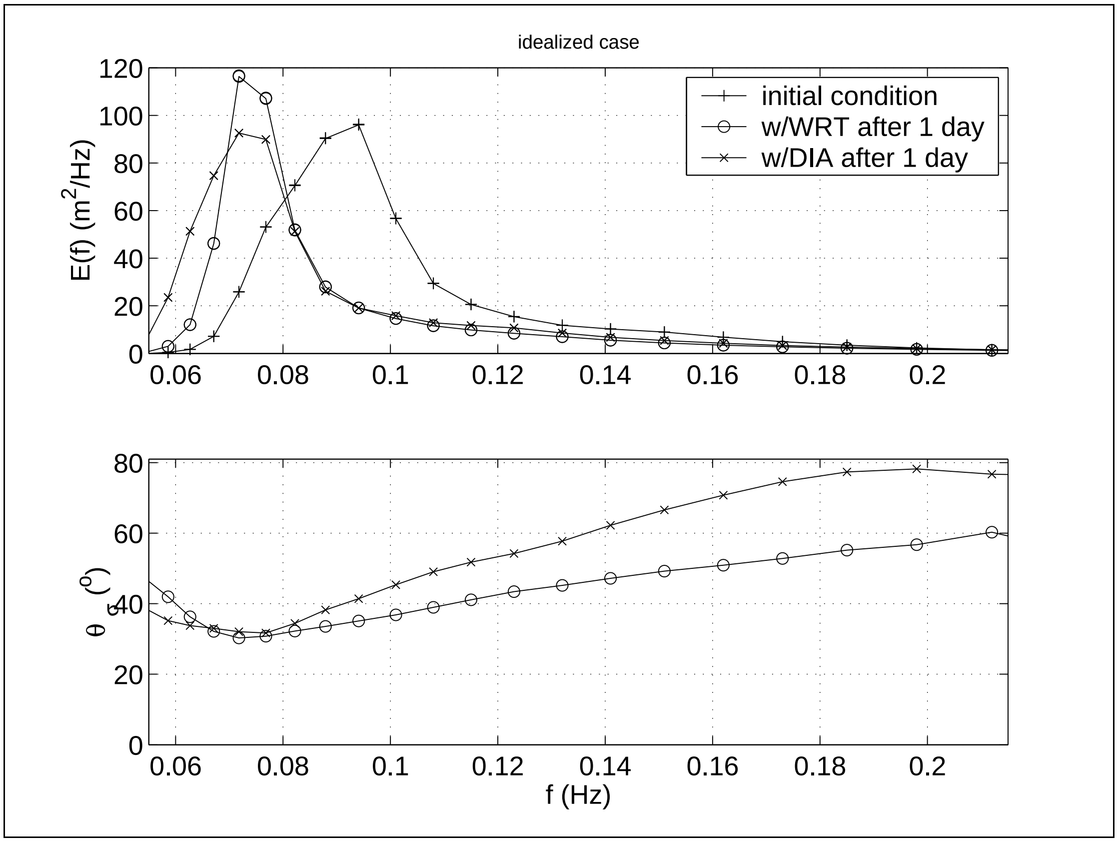

Figure 17a. This is from the original submission. In this figure, Wavewatch-III was the model used. The figure is included here, to contrast with the figure in the new submission, which shows results from the SWAN model.

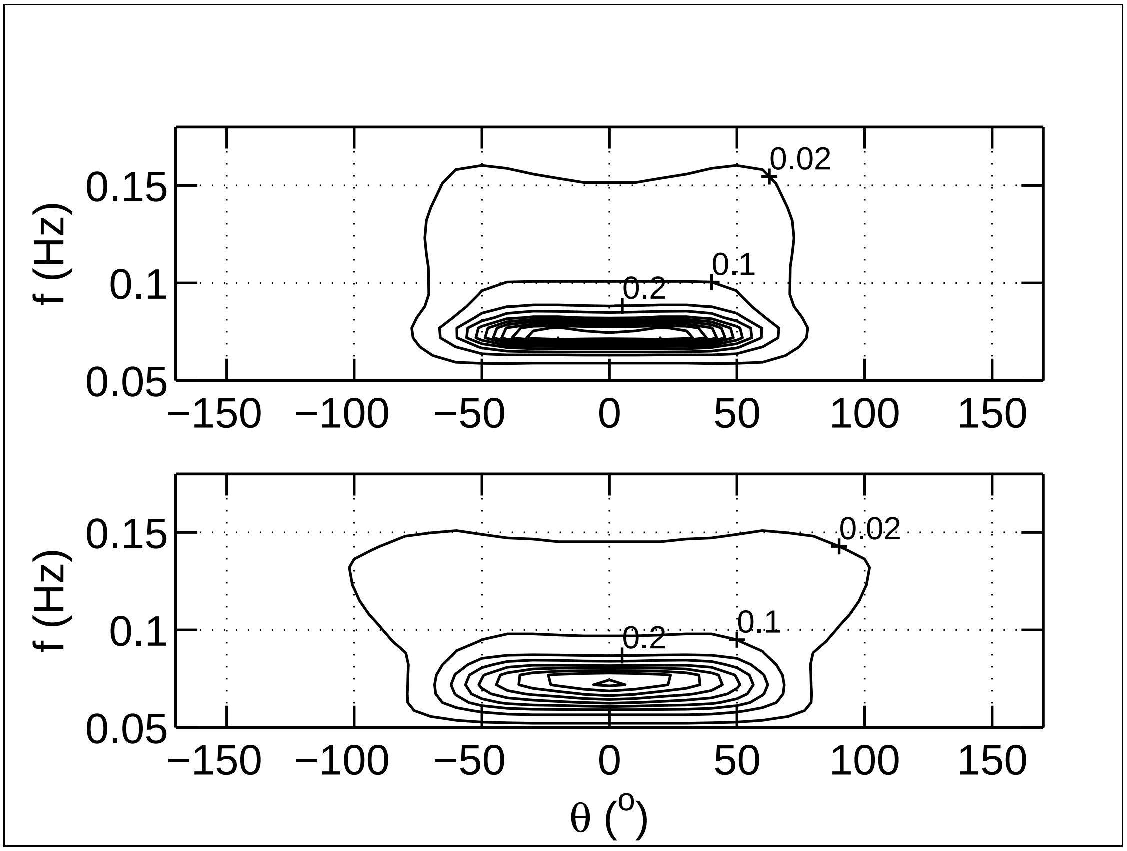

Figure 17b: As with previous figure, this is a figure from original submission. In this figure, Wavewatch-III was the model used. The figure is included here, to contrast with the figure in the new submission, which shows results from the SWAN model.

2.2.10 Additional figures

Additional figures, not included in the submitted manuscript, are given here

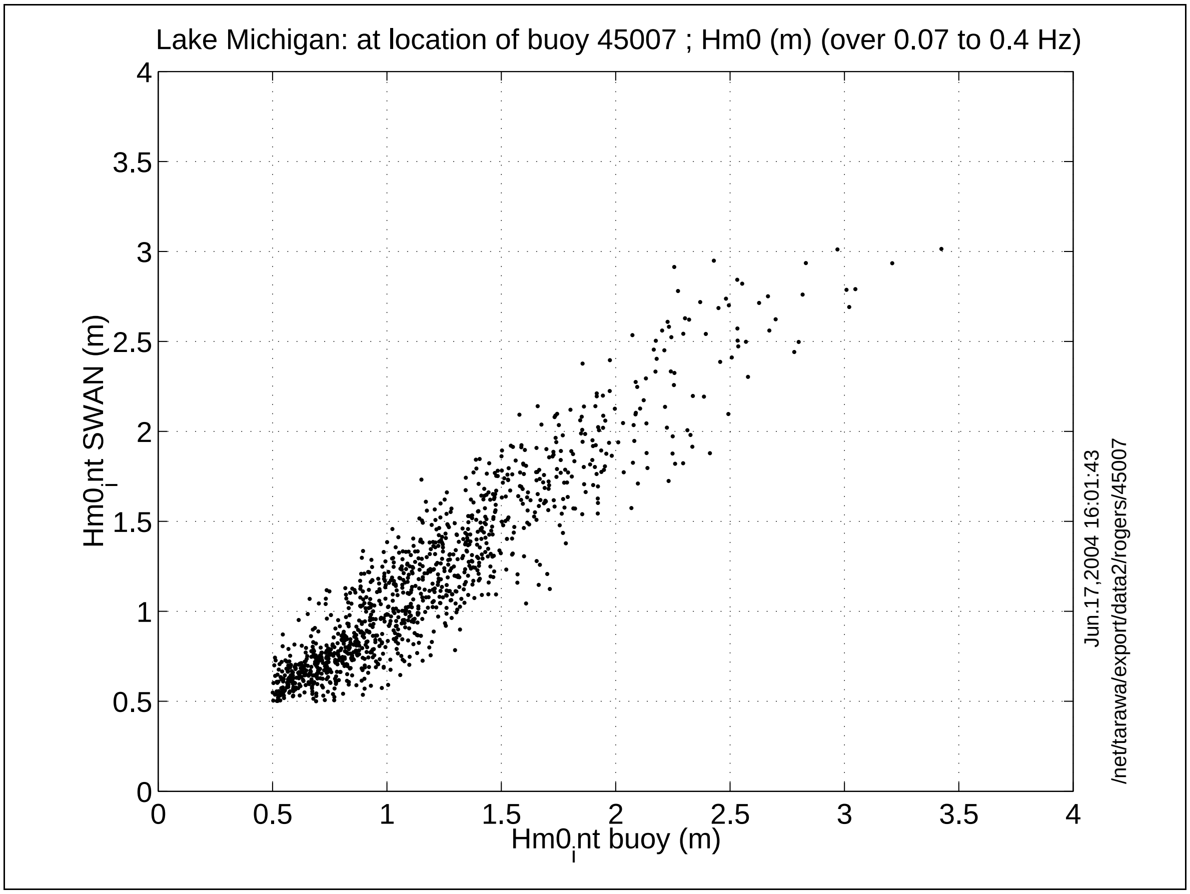

Figure 18. These are the points actually included in the directional spreading comparison (H>0.5m).

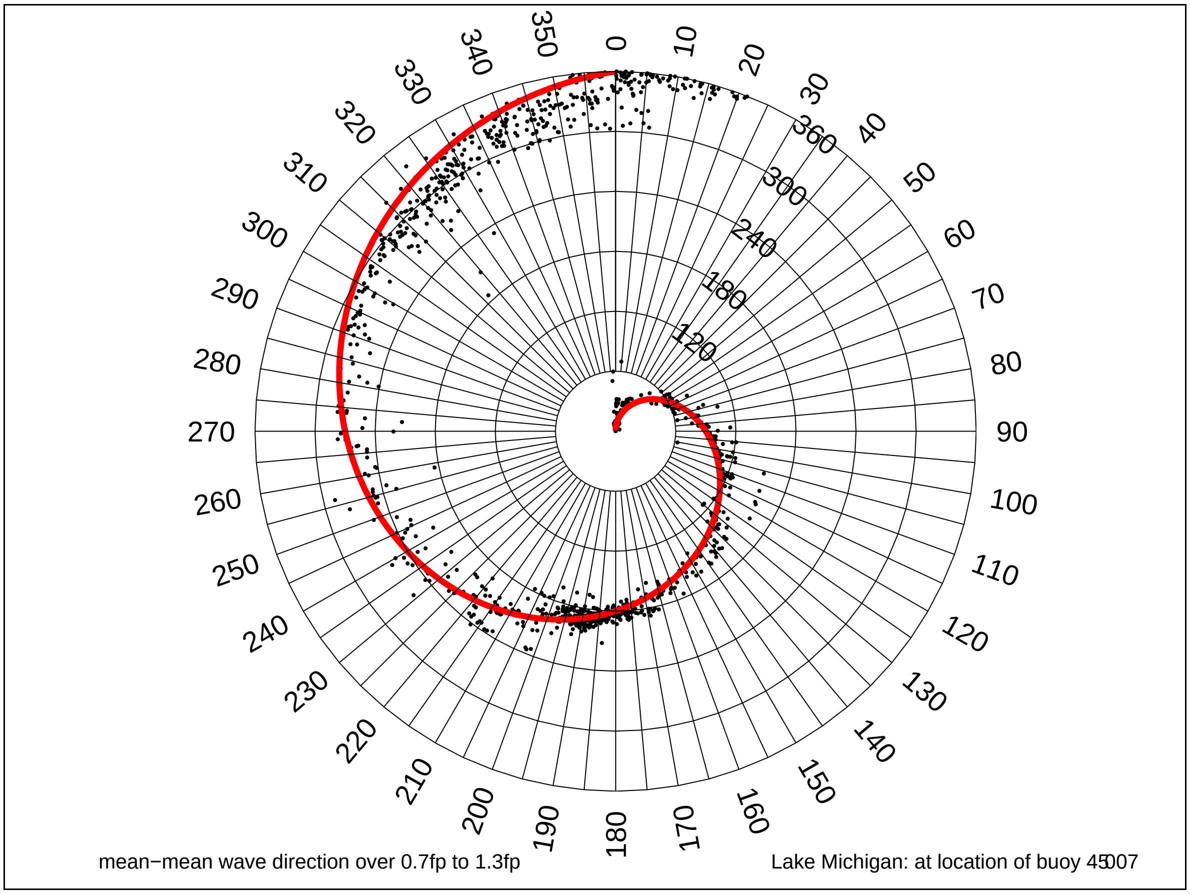

Figure 19. Mean-Mean direction validation of SWAN results (0.7 f_p to 1.3 f_p).

Acknowledgements

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