

## ***Interactive comment on “Interannual variability and future projection of summertime ocean wave heights in the western North Pacific” by W. Sasaki et al.***

### **Anonymous Referee #1**

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General comments.

This paper presents a predictor of summertime extreme significant wave heights in the western North Pacific based on an empirical relationship to surface wind anomalies in the region. The authors then make use of this relationship to make future projections of significant wave heights for an increased CO<sub>2</sub> scenario using output from an atmospheric global climate model.

I recommend the manuscript to be published after moderate revision. In its present state, the manuscript lacks sufficient detail in sections, and the discussion of interannual variability, and future projections is too brief and disjointed - all of which make the

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manuscript quite difficult to read.

More specific comments.

Abstract.

T106 AGCM should be more adequately described in the abstract.

The final sentence is misleading. It's a very general statement saying that SWH in the WNP will increase by up to ~0.4m over a large region under a 2XC02 scenario. The empirical model developed in the model applies to summertime extreme SWH only. This sentence should be refined.

Section 2.1

ERA-40 is a 45-yr re-analysis of the atmospheric and oceanic fields.

There is no comment on the accuracy of the ERA-40 wave heights in the WNP region. Previous studies (Caires and Sterl, 2005) of the ERA-40 SWH show that ERA-40 typically underestimates large waves. Some assessment of the reliability of the estimates of the summertime extreme wave heights in the WNP should be included.

Section 2.2

The PAS convection scheme results in much greater cyclone activity in the east of the region than is observed. The manuscript states later in the text the influence position of TC activity has on wave heights in the region. Given this difference, can it be assumed that the AGCM can produce surface winds which drive a realistic wave climate in this region? Perhaps a sensitivity of projections could be produced using the Kuo scheme as well. Although I think the method applied has merit, I see that the unreliability of the AGCM to produce realistic surface winds in this region as a weakness of the manuscript. Can this be addressed?

Section 3.1

Paragraph 3. The source of data for U10N is not stated when first introduced. Given that the source of data to compute U10N varies throughout the manuscript, the source should be stated clearly on each use (including in Figure 1b). Is U10N a summertime only anomaly, or an annual mean anomaly? This is not stated at all.

Figure 3. The data plotted on this plot needs to be more clearly described. There are only 30 small squares on this plot, which suggests to me that the authors have plotted summertime mean H90 and U10N from 1970 to 2002. Is this what has been done - it needs to be stated if so. If so, the authors must also state explicitly why they chose to ignore the 1960's data (which I assume is due to no satellite monitoring of weather events). I also assume that the TOPEX/NCEP-NCAR data is summertime means for the period 1994-2003. This should also be outlined in the figure caption.

Section 3.2 It is not clear to me from Figure 3 how “A pair of time-slice experiments shows that calculated U10N at present-day is somewhat larger than actually observed”.

Figure 4 is not clear. There are no contour labels detailing change in H90, and the interpretation of the difference in SW vectors is unclear. Without having a map of present day winds to refer to, the vectors could be interpreted a number of ways. The larger vectors south of 20dN could correspond to a weakening of easterly winds, a strengthening of westerly winds, or a shift from easterly to westerly winds. I suggest overlaying vectors for the present day and 2XCO<sub>2</sub>, giving different colors or arrow heads to more clearly display how surface winds are changing.

Section 4. Conclusions.

Like the abstract, the conclusion comes across as a generalisation of the results of manuscript, without stating so. Reference to H90 throughout the conclusion should be changed to summertime extreme SWH to ensure that no confusion is made with regards to the applicability of the derived empirical model.

References. I cannot find reference to Tolman (1999) in the manuscript. Remove this

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reference.

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