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Ocean Science Discussions

Interactive comment on "Measurement of turbulence in the oceanic mixed layer using Synthetic Aperture Radar (SAR)" *by* S. G. George and A. R. L. Tatnall

Anonymous Referee #1

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The authors present and discuss results of numerical simulations of microwave radar signatures of ship wakes, based on Direct Numerical Simulation (DNS) of the turbulent flow field behind the ship and application of the numerical radar imaging model by Romeiser et al. of the University of Hamburg ("M4S") to the resulting surface current field. They discuss the dependence of the simulated radar signatures on radar frequency, radar look direction, and wind speed. They come to the conclusion that it should be possible to detect (and quantify to some extent) turbulent features such as the ones in ship wakes in radar images acquired by satellites.

Of course it is well known that turbulent ship wakes are visible in many radar im-

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ages, and the identified dependencies of the radar signatures on radar frequency, wind speed, etc. are consistent with many other studies on radar signatures of current features. The most interesting result of this work, in my opinion, is the fact that the relatively small spatial surface current variations in the turbulent ship wake can cause quite strong radar signatures (i.e. positive and negative deviations of the NRCS from its mean value), according to the models used here. This is the main reason why the paper deserves to be published. However, I have doubts about the validity and appropriateness of some elements of the authors' modeling approach, which need further investigation / discussion before I can agree that the model results are correct.

First, as pointed out on page 2858, lines 13..26, the weak hydrodynamic wave-current interaction theory, on which the numerical radar imaging model is based, assumes slow surface current variations on spatial and temporal scales that are long compared to the scales of the waves considered (where relevant spatial and temporal scales of the waves are determined by wavelength, time to propagate across current features, and relaxation time). The spatial and temporal limits of the theory may be violated here - as the authors say in lines 19..24, there may be rapid temporal changes in the current field, and the figures show that there are strong current variations on short spatial scales (on the order of a meter) as well. The wave-current interaction part of M4S computes modulations of waves that are much longer than these current variations. The authors should analyze how these violations of the assumptions / limitations of weak hydrodynamic interaction theory affect their results; maybe they need to apply a different theory to account for the fast spatial and temporal current variations properly.

Second, even if the computed wave modulations are sufficiently accurate, it is not clear if the waveheight spectral densities and mean square surface slopes are appropriate for the radar computations in M4S. A grid cell of the DNS model is of similar size as a Bragg scattering facet at X or C band and smaller than a Bragg scattering facet at L band. Will the computed mean square surface slopes make sense on these spatial scales? What do we know about possible mean linear surface slopes that might be

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present over the turbulent wake? The default mean surface slope in M4S is 0. If the water surface of the turbulent wake has slopes, these should be entered into M4S via the "surface elevation" input file.

Some smaller issues:

- Page 2857, lines 24..27: This description of long-wave contributions to the NRCS in a composite surface scattering model is not very clear. I suggest to explain that nonlinear variations of the Bragg NRCS with the incidence angle cause non-zero-mean contributions of longer waves to the NRCS. In M4S, these contributions are represented by terms proportional to the mean square surface slope in radar look direction.

- Page 2860, lines 20..21: No existing or proposed spaceborne radar can "resolve" the NRCS variations over the turbulence patterns considered here. This is discussed for the first time in the Conclusions. I would say much earlier that the simulated NRCS variations represent NRCS variations of the water surface only, but there will be no actual radar images at this spatial resolution in the near future. In fact, it would be interesting to reduce the simulation results to the spatial resolution of, say, TerraSAR-X and to do a comparison with ship wake signatures in actual TerraSAR-X imagery.

- Page 2863, lines 17..23: I don't agree with the authors' statement that the directional distribution of wave energy in a real-world scenario would be broader than the one obtained from M4S. The available parameterizations of the equilibrium wave spectrum in M4S use spreading functions that are consistent with results of real-world field measurements.

- Fig. 1: I think the term "Kelvin wave" is misleading in this context, because a Kelvin wave is something completely different. These are the "Kelvin arms" of the ship wake, or the "Kelvin wake".

- Fig. 2: This flow chart looks like an early draft version. For example, the three lower green boxes represent data products, while the upper green box says "Generate

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Surface Velocity Profile". Models and procedures seem to be represented by gray boxes, but why is there no gray box between "Resolution & Noise Data" / "Speckle Characteristics" and "Simulated SAR Image"? This needs more work.

- Figures showing NRCS maps: I suggest to normalize all such figures such that the mean level is shown in green color and positive and negative deviations go towards red and blue, respectively. It is well known that the absolute NRCS levels obtained from M4S are not very accurate, and they are not relevant for this study. It is more important to compare deviations from the mean value under different conditions.

- I think Fig. 8 a and b should better be Fig. 7 c and d.

- In Fig. 11 I would use the same vertical axis (with positive and negative deviations from the mean value, in dB) in all diagrams. This would make it easier to compare them.

Minor corrections:

- Instead of "composite spectrum model" (very uncommon) and "two-scale model" (more common in the context of analyses of long wave signatures) the authors should use the term "composite surface model".

- Page 2853, lines 1..2: Delete one "therefore".

- Page 2856, line 10, and caption of Fig. 4: I think the symbol that looks like a capital Pi on page 10 should be the same as the "II" in the figure caption.

- Page 2858, sentence beginning in line 8: I think a "characterised" is missing near the beginning.

- Page 2858, line 15: Insert "be".

- Page 2861, line 23: Insert "on".

- Page 2862, line 27: Insert "the".

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- Page 2862, line 28, insert "of".
- Page 2864, lines 23..24: Sentence doesn't look right.
- Caption of Fig. 4: Change "views" to "view".

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