

Interactive comment on “Safe-economical route model of a ship to avoid tropical cyclones using dynamic forecast environment” by L. Wu et al.

Anonymous Referee #2

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General description

The manuscript presents an attempt to compute a ship route in presence of tropical cyclones, taking into account related economical cost and risk of capsizing. Resulting route is claimed to be optimal. The manuscript is to a great extent a rewriting of already published papers:

- L.C.Wu, Y.Q. Wen, D.Y. Wu "Safe-economical route and its assessment model of a ship to avoid tropical cyclones using dynamic forecast environment" Nat. Hazards Earth Syst. Sci. Discuss., 1, 1857–1893, 2013
- L.Wu, Y. Wen, S.Peng, J.Zhang, C. Xiao "Optimizing the routing of ship's tropical cyclone avoidance based on the numerical forecasts." Natural hazards 69.1 (2013): 781-792.

General comment

The manuscript shows quite poor scientific and presentation quality. The main criticism that I feel to move is:

- a) Inaccurate scientific notation and improper use of English language make very hard understanding the actual contents (see #3,4,5,9,15,18 below);
- b) The algorithm at the core of the proposed method (Sect 3.2) fails in my opinion in its ambition of optimality (see #2,13,16,19 below);
- c) The physical model of the interaction between the ship and the marine environment and specifically the Tropical Cyclone, despite mentioning of very complex effects, is oversimplified in the actual implementation done for producing the results of the manuscript (see #14,21,22 below).

In my opinion, the manuscript does not match the criteria for publication in a peer-reviewed journal and should be rejected.

Detailed analysis

Specifically, the main weakness of the manuscript are analyzed in the following:

1. The title is not appropriate. In consideration of the unclear definition of the time C3351

variable in the method proposed by the authors, (see comment item #16 below), use of expression “dynamic forecast environment” in the title is exaggerated;

2. The abstract claims that the presented case study demonstrates optimality of the method proposed by the authors and superiority with respect to traditional methods. However, no proof of such optimality is presented, but an individual case study. In general, no method can be demonstrated by a single case study. In the case of present manuscript, such inherent limitation is further constrained by the scientific weakness of the method itself (see items #13,16,19 below). Also the superiority with respect to other methods such as the 1-2-3 rule is not proven (see #26);
3. The analysis of literature reported in the introduction contains repetitions: (Chen and Zhang, 2004), (Holweg, 2000), (Wisniewski et al., 2009) are all quoted both at page 4909 and 4910 with nearly the same words, without adding any new insight;
4. Integral definition Eq.2 misses the measure of integration dt , which is indeed present in its discrete counterpart Eq.5 (d/v factor);
5. Symbols n and k are used inconsistently at line 8 of page 4914 and in Eq.4-6. Is the number of waypoints of the RATC equal to k or to n ?
6. Eq.6 is simply replacement of Eq.4 and Eq.5 into Eq.3. Thus, in my opinion, Eq.6 could be skipped, as it is not adding any value to the understanding.
7. Wave slope α_0 used in Eq.9 is never defined, despite its crucial role in the method (see #22);
8. Eq.13 is a reprint of Eq.9;

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9. The quantity D used in Eq.15-16 is never defined: how does it depend on the other model parameters? In particular, how does sea forcing expressed by Eq.9 enter D ?
10. The relation between marginal probability density $P(x, t)$ and conditional probability $P(x_1, x_2|\Phi)$ is not provided, despite many other formulas are provided in the manuscript;
11. Since the integration argument in Eq.18 dimensionally is a ship rolling angle, it seems quite odd to extend integration up to infinite;
12. The lapidary statement written in the first sentence of Sect. 3.1.3 should be substantiated by at least one reference;
13. The discussion about “artificial waypoints” done in Sect. 3.1.4 is obscure and fails to address the real issue, which is: what is the computational performance of the method proposed by the authors? Use of words such as “intractable”, “artificial”, “number of waypoints cannot be very small”, “boundary condition” seems to me to be improper or not informative at all. Instead the authors should provide figures such as: computing time vs. number of alternative waypoints.
14. At the end of Sect.3.1 it is stated that it is “much safer to bypass a TC from the left [...] in the northern hemisphere”, arguing that this is due to anticlockwise rotation of the cyclone. I cannot follow this argument. Are authors thinking at a vessel approaching the TC from the North or from the South?
15. In Sect 3.2 is written that the straight-line route between departure and arrival waypoint is divided into n parts. Thus, intermediate waypoints should be at $i = (1, n - 1)$, not at $i = (1, n)$ as stated by the authors.
16. Transit times $t_{i,j,k}$ are computed using ship velocity at an undefined time t , as seen from integrand in Eq.21. I think that, for a truly dynamical environment, it

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is crucial to specify time t at which such speed is evaluated. However, in the following of the manuscript, ship speed is even set to a constant (row 21 at page 4923): thus Eq.21 is far too general and misleading in consideration of the basic implementation done in the manuscript;

17. Eq. 22 is a bare repetition of Eq.2 on a different interval of integration, including missing integration measure. I think it would be more elegant to write it just once;
18. Use of adjectives “turbulent” (waves) and “real-time” (data) in rows 1-2 of page 4922 is completely inappropriate;
19. Step 5 in the method proposed (p. 4922) indicates that at each stage i the locally best choice is done for advancing up to stage $i + 1$. However, such a choice does not imply a global optimization of the whole path from departure to arrival waypoint. The authors indeed do not provide any theorem of optimality, nor a comparison of their approach with exact methods such as dynamic programming, that are known to guarantee path optimality.
20. The route smoothing algorithm (improperly called “optimization algorithm” by the authors) proposed starting from line 17 at page 4922 is at least contradictory. Indeed, such algorithm should apply to the route constructed by means of steps 1-5 at the beginning of Sect.3.2. Such a route already avoids unnavigable areas, according to step 3 (line 9). Why ever should the smoothing algorithm once more looking for unnavigable segments?
21. In Sect 4., line 21, authors assume - without motivating it - that ship speed is constant. Then, Eq. 19 and subsequent unnumbered formula in Sect 3.1.2 are never used and should be removed from the manuscript. Also Eq.1 seems to be too general in consideration of the modest modeling effort of ship dynamics actually implemented in the system developed by the authors. Indeed, Eq.1 is never referenced in the following of the manuscript.

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22. In Sect. 4.2, line 9, wave slope is set to the constant value of 0.729. The authors should then answer following questions:
 - How is wave slope defined?
 - If wave slope is -as usually is- the ratio of wave amplitude to wavelength, how is such extremely high value assessed? How is even possible to provide 3 decimal digits for such a slope, especially in consideration of the discussion the authors make on the stochastic nature of weather and sea-state forecasts (Sect 3.1.3)?
 - Since wave slope depends on the maturity stage of a storm (see e.g. Fig.22 in Strom-Tejsen, Jorgen, Hugh YH Yeh, and David D. Moran. Added resistance in waves. Society of Naval Architects and Marine Engineers, 1973), I would expect that wave slope may assume very different values, depending on the distance from a TC. Then, I cannot understand from the manuscript how is the TC influencing the route at all, since -for the authors of the manuscript under evaluation- wave slope is constant in space and time and it is not even clear how other environmental parameters enter the method (see #9).
23. In Sect 4.3 line 26 it is written that the grid for the route computation is equally spaced (0.35 degree) in the direction perpendicular to the straight line joining departure and arrival point. Since these two points are not located at the same latitude, I don't understand how is it possible to define a constant angular spacing for such a grid.
24. In Sect.5, line 7, the expression “weathering routing” is incorrect. In marine sciences, the word “weathering” is used for the chemical transformation processes driven my weather, such as those occurring on oil-leaks. The authors probably mean “weather routing”.

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25. Fig.8: the values of ship heading should print using a larger fontsize;
26. Fig.9: according to the text on page 4925, lines 3-5, the trajectory shown is not a mere sketch, but refers to an actual computation. Thus, geographical axes should provided. Furthermore, since in Sect.1 authors mention the 1-2-3 rule and its limitations, this figure would be a good place for drawing also the 100-200-300 miles radius circles for the +24h +48h +72h time steps and compute related cost and risk, in order to compare their results.
27. Fig.10: This picture is the most important result of the authors and should be much larger, for appreciating differences among the various routes. Furthermore, ship and TC directions of advance should be indicated. However, as stated in #2, a single case-study is not enough for convincing about the method proposed by the authors. In particular, in view of the large number of parameters and numerical manipulations described in the text, more applications on various environmental conditions would be necessary. Finally, the caption should contain a legend to the various routes displayed.