General comments

In this paper, the Gaussian Quadrature (GQ) method is used to calculate the spray-mediated heat flux, instead of the current full-size spectrum integral (A92) and the fast algorithm (A15). A global atmosphere-ocean-wave coupled model CFSv2.0-WWW3 is employed, and two time periods of 56 days in boreal summer and winter are conducted to test the sensitivities of SST, 10 m wind speed and significant wave height to the new scheme that the authors proposed. Although the improvement on spray-mediated heat flux is not physical, the computational time is about 36 times less than that of A92. In addition, the introduction of this new method improves the simulation of SST, 10 m wind speed (WSPD10) and significant wave height (SWH). Based on above reasons, I think this manuscript can be considered for publication if the authors address all my queries and comments below.

Specific comments

1. My first comment is about the title of this manuscript. I think the ‘improved’ is not appropriate. As I mentioned above, the new method that the author proposed is not physical improvement on spray-mediated heat flux, and it has not been validated against the directly or indirectly observed sea spray heat flux. Although SST, WPSD10 and SWH are has been improved, it can’t be said that the sea spay-mediated flux is ‘improved’. Perhaps ’**Accelerated** Estimation of

Sea Spray-Mediated Heat Flux Using Gaussian Quadrature method and……' is more in line with the content of this manuscript.

2. Line 57, I do understand the positive effect of sea spray on tropical cyclone, but what is the 'negative effect of enhanced surface drag'? The author needs to be more specific in the text.
3. What are the prerequisites for the use of Gaussian-Legendre quadrature for f(x)? just smooth?
4. Line 118, the authors said 'the sorting leads to high complexity of GQ comparable to A92', thereby the authors try to avoid sorting. However, the authors still sort Q_S and Q_L from largest to smallest (lines 124-125). Is it contradictory?
5. Equations (3) to (5) really make me puzzled. According to the author's description in Appendix C, $r_i = \frac{b-a}{2} x_i + \frac{a+b}{2}$, if $x_1 = -0.775, x_2 = 0, x_3 = 0.775$, and the lower and upper limits of sea spray radius are $a = 2, b = 500$, I calculate the values of GQ 3-nodes and get the following results $r_1 = 443, r_2 = 251, r_3 = 58$, which are almost consistent with the 3-nodes values of Q_L given by the authors. How did the authors get the GQ 3-nodes for Q_S? why Q_S and Q_L use different GQ 3-nodes? Given that the potential users may be interested in this new method, the authors need to clarify how the 3-nodes $r_{s1}(r_{l1}), r_{s2}(r_{l2}), r_{s3}(r_{l3})$ are obtained by sorting Q_S and Q_L as much detailed as possible.
6. Lines 127-128, How can we see that r_{l3} is related to WPSD10 from

Figure 2c?

7. Figure 2 is very difficult for me to understand. As far as I can understand from the text, if 3 GQ nodes are determined, then 3 percentage values are determined. But in Figure 2, the authors use $r_{s1}(r_{l1}), r_{s2}(r_{l2}), r_{s3}(r_{l3})$ as the name of the x-axis, and there are so many bars in each panel of Figure 2, what do the bars represent? I strongly recommend that the authors devote more space to the introduce the new methods in their manuscript to respond to my comments 5-7.
8. The authors need to briefly describe how the atmospheric and oceanic components of CFSv2.0-WW3 are initialized. I also note that WW3 is not global, then what dataset is used to provide open boundary conditions for WW3?
9. The difference between the spray-mediated heat fluxes calculated by A15 and A92 schemes is so significant. Can the authors comment on what cause this large difference? Extrapolation of V_S and V_L at high wind speeds？ or due to the use of single-radius droplets to represent the full-spectrum integral? As Andreas et al (2015) said, V_S &V_L in A15 are extrapolated at high wind speeds, while SSGF (sea spray generation function) in A92 are deduced. The lack of discussion on the causes of this discrepancy will greatly diminish the importance of this article. I am seriously concerned about this.
10. It would be better to superimpose the Mean Error onto Figure

6c,7c,8c,9c,10c, and 11c.

11. According to Andreas et al. (2015), the effect of sea spray become significant at wind speed of 10-13 ms^{-1}. How strong can the simulated WSPD10 be? Therefore, I would like to see the global distribution of WSPD10 simulated by CFSv2.0-WW3.
12. Line 178, what does equivalent neutral wind mean? Do the authors mean that the equivalent neutral winds in OAFLux are larger than those in ERA5?
13. Lines 230-231, The expression is not accurate, the reduced wind and weaker mixing can lead to warmer SST?
14. As we know, satellite scatterometer and altimeter data are usually used to validate WSPD10 and SWH for short term weather forecast. I don’t know why the authors use ERA5 reanalysis as validation data for seasonal prediction.
15. Does the introduction of sea spray can improve the simulation of other elements? For example, air temperature and humidity.
16. Line 246, there is a grammatical mistake in this sentence. And Fig10b&11b do not support ‘The SWHs in SPRAY-GQ improve compared with those in SPRAY-A15’. Do the authors mean that there is a significant difference between SPRAY-GQ and SPRAY-A15?
17. As far as I know, the sea spray algorithm codes of Andreas are open source, please upload the author's modified A92 codes to a repository

so that others can repeat the author's results.

18. Lines 255-259, the authors try to discuss the physical mechanism that responsible for the accelerated surface wind. However, the citation does not seem to support the author's conclusions. I can understand that the increase of air-sea heat flux could promote air convection in the vertical, but how does it promote the downward transmission of momentum from the upper layer of atmosphere? By affecting the large-scale atmospheric circulation? Please provide appropriate citations or give your own analysis to support your points.
19. Lines 263-264, again, without verification by direct/indirect observation, we can’t say that SPRAY-GQ is more accurate than A15. All we can say is that the difference between SPRAY-GQ and A92 is smaller than that between A15 and A92.
20. Finally, I think the current experimental design is insufficient, a reference experiment without sea spray effect is missing in manuscript. Although it may be expensive to conduct a new set of experiment, it makes sense for the scientific community to understand the importance of spray-mediated heat flux for seasonal and intra-seasonal prediction.