

**Review of the manuscript #os-2018-61**

by Iwona Wrobel-Niedzwiecka et al.

“Air-sea momentum flux climatologies: A review of drag relation for parameterization choice on wind stress in the North Atlantic and the European Arctic”

Journal: Ocean Science

**Summary:** The authors use several parameterizations of the drag coefficient  $C_D$  necessary to calculate wind stress (momentum flux)  $\tau$  at the air-sea interface. The authors choose seven parameterizations of  $C_D$  as a function of wind speed  $U_{10}$ . Processing tool (software) FluxEngine is used together with wind speed data from 1992 to 2010 to calculate  $\tau$  in the North Atlantic (NA) and the European Arctic (EA). Area-averaged annual mean values of  $\tau$  are tabulated for different  $C_D(U_{10})$  parameterizations and compared to results obtained with the  $C_D(U_{10})$  parameterization of Andreas et al. (2012, JPO).

**Significance:** The parameterization of  $C_D$  has been a topic of enormous amount of research for many years. New studies try to add to the topic by reporting new data for  $\tau$  and  $C_D$ ; new parametrizations taking into account different processes and influences; comparisons between data and parametrizations; questioning the fundamental definition of  $C_D$ ; determining  $C_D$  under high wind speed conditions; explaining and/or modeling the leveling off under hurricanes etc. In this manuscript, the authors quantify the differences in  $\tau$  values obtained with different  $C_D(U_{10})$  parameterizations. In this sense, the topic of the manuscript is suitable for Ocean Science journal and useful for readers interested in this topic.

**Evaluation:** While each new research on  $C_D$  and  $\tau$  tries to add a bit more understating and propose improved  $C_D(U_{10})$  expression(s), it is hard to add something truly new after decades of investigations. Thus, it becomes important to clearly state new developments and/or new insights when publishing on this topic. In this sense, this study does not add new knowledge. It is routine. Still, I see some usefulness of this manuscript in the tabulated  $\tau$  differences for different  $C_D(U_{10})$  as this can serve as a reference to readers regarding which of many available  $C_D(U_{10})$  parameterizations to use. However, the manuscript needs much more work to be suitable for publication. In its present form, it lacks clear objective; could be better organized; and has weak conclusions. I recommend major revisions. Comments and suggestions follow.

**Major comments:**

- 1) The purpose/objective of the manuscript is not clearly formulated. The title suggests two things: a climatology and a review. Two more objectives are hinted in the text (details follow). Focusing on each of these possible objectives would require quite different analyses. With the purpose not well defined, none of the possible objectives is fully developed in the manuscript. Here are specifics.

- a. Is the objective a review of  $C_D$  parameterizations, as the title suggests? If yes, then it is incomplete and without deep physical discussion of the progress and problems regarding parameterizing  $C_D$ . The authors state in Line 43 that it is a must to “take into account other physical processes,” yet they then focus on “wind speed parameterizations, because wind speed is a parameter that is available in every atmospheric circulation model” (Lines 68-69). The authors initiate a review of formulas by dividing them in two groups (Lines 50-55), yet, again, stop short of further discussion on different formulation of roughness length  $z_0$ . I believe that review and physical discussion on  $C_D$  are not the motivation of this work. After all, Andreas et al. (2012) and Edson et al. (2013, JPO, DOI: 10.1175/JPO-D-12-0173.1) provide comprehensive recent reviews of the status of parameterizing  $C_D$ .
- b. Is the objective a climatology of NA and EA, as the title suggests? If yes, this motivation is not well justified and there is no analysis of the results in climatological terms. If climatology is the objective, then the authors need to tell us why they focus on the NA and EA regions? What atmospheric and oceanic conditions does the  $C_D$  parameterization need to represent well in these regions? If climatology is the goal, what is the temporal or spatial reference? It seems the chosen spatial references are the global ocean and the Tropics, to which the authors compare their results for NA and EA. But if  $\tau$  is obtained globally and over many regions (Table 1), then why emphasize NA and EA in the title? Why look into differences due to  $C_D$  formulation between northern regions and the Tropics, when it is certainly expected to have differences due to geography? As for a temporal reference, the authors should choose a period within or outside the 1992-2010 period which gives average atmospheric and oceanic conditions, not affected by long-term variations such as the North Atlantic Oscillation, which changes the position of jet stream and thus the wind and SST fields at the surface; these, in turn, change the wind stress. If climatology is the goal, then the authors should analyze their  $\tau$  results for trends and variations over the 1992-2010 period. Should give annual as well as inter-annual variations. Finally, in my opinion, to provide a comprehensive regional climatology, the authors should analyze long-term  $\tau$  values obtained with one, chosen  $C_D$  formulation in order to clearly isolate climatologically-relevant variations.
- c. Is the objective to evaluate  $C_D$  parameterizations and recommend a new one for use in circulation coupled models (Line 136)? A hint for such an objective comes from the authors’ conclusion “the parameterizations used in the models possibly need upgrading” (Lines 332-333). If this is the objective, then the authors should give us a list of parameterizations used in different circulation models; discuss the advantages and limitations of these currently-used  $C_D$  parameterizations; then demonstrate how other  $C_D$  parameterizations would do better. For climate and circulation models, the  $C_D$  parametrization is important for the mixing layer depth. So the authors should show how new  $C_D$  parameterization would improve the modeling of the mixing layer. The manuscript offers limited information on what the current models use (Lines 136-140). There is no analysis on how  $C_D$  would affect the performance of model variables related to  $C_D$ . So the conclusion in Lines 332-333 is not convincing for modelers.

- d. Is the objective to demonstrate/justify the need of new measurements in NA and EA for improved  $C_D$  parameterization in high latitudes? A hint for such an objective comes from Lines 188-189 regarding frequent ship deployment in EA, including “R/V Oceania, the ship of the institution the authors are affiliated with.” If this is the objective, the manuscript would take completely different direction with discussion and analysis related to measuring methods and quality of data necessary for  $C_D$ . Of course, this is not the objective because the authors say in Lines 65-68 that their intention “is not to re-invent or formulate a new drag parameterization ... but to revisit definition of the existing drag parameterization.”
- e. I am listing all these possible objectives only to make the point that the authors need a well stated objective in order to focus their analysis and discussion.

I believe the authors wish to assess  $C_D$  parameterizations only to decide which one to use in some larger project. For such an assessment, the authors only need to clearly tell us why they consider the formulations (8)-(14) (i.e., no need of comprehensive review). They do not need climatology to make this assessment. One year data for  $\tau$  is enough. However, to make the decision, the authors need not only to quantify  $\tau$  differences (this is what the current results offer). They need also to make a thorough analysis what causes the differences. They need to evaluate how much of the differences come from: (i) different functional  $C_D$  formulation; (ii) different quality of the data on which the parametrizations are based; and (iii) seasonal variations in NA and EA. The authors also need some reference to show them which  $C_D$  formulation is suitable for NA and EA. Perhaps comparison of their results to regional data? Perhaps an investigation of how well a feature specific to NA and EA is represented when using different  $C_D$  parameterizations? With such direction of the manuscript, the title may need revision to exclude claims on climatology and review.

- 2) There are several typos in the formulae that need fixing. Most importantly, it is necessary to check the coding for the calculations. These typos are as follows. In (2),  $U_{10}^2$  is in the denominator. In (7), why  $U_{10}$  is squared? The relationship between  $u^*$  and  $U_{10}$  is linear (Andresa et al., 2012, their eq. 1.10; Edson, et al., 2013, their eq. 22). In (14), need square on the wind ratio  $(u^*/U_{10N})^2$ ; in the parenthesis,  $U_{10N}^2$  is in the denominator; needs square on the parenthesis (compare to Andreas et al., 2012, their eq. 1.10).
- 3) Give better justification on choosing  $C_D$  parameterizations (8)–(14). For example, it seems you have chosen  $C_D$  parameterizations formulated as power law, linear, polynomial, constant. Why do you need (9) and (10)? They are so similar? Describe the merits of (11), (13) and (14), as well as their differences (e.g., data on which they are based). Do these formulations account implicitly for different processes in addition to  $U_{10}$ ?
- 4) Suggest re-organizing the Introduction to include Lines 37-72 plus one paragraph on why you focus on NA and EA, then another paragraph clearly stating the objective of the study. Suggest combining Lines 73-154 with Lines 195-215 in one section dedicated on  $C_D$  parameterizations. Only parts of the historical (incomplete) review in lines 73-154 are necessary. Start with the definitions in Lines 73-93. Then introduce

- (8)–(14) one by one. Add information on MOST (Lines 115-122) and circulation models (Lines 136-140) only when they are needed, e.g., when you introduce (11) and (12), respectively. Finish the section with Lines 155-160. With this organization you will avoid the current inconsistency of presenting Fig. 1 with all parameterizations before they are described. Remove lines 122-127 and Lines 130-135 because you do not use Trenberth et al. (1989) and COARE algorithm. Unless you decide to use COARE 3.5 as a reference.
- 5) Section 3 “Result” is straightforward. It describes Table 1, maps, and seasonal graphs. To make these results useful, you need to extend the analysis of these data, discuss what causes the differences; and suggest which  $C_D$  parameterizations is useful for NA and EA.

Additional comments:

Title: If possible (perhaps talk with the OS editor), revise the title to better reflect the purpose of your manuscript.

Abstract: Too long, dilutes what you did and what you have found. Suggest substantial shortening. Avoid giving references in the abstract. Refer to different  $C_D$  parameterizations by their specific characteristics (e.g., power law, linear, etc), not by author.

Lines 18-19 and Lines 227-230: Oldest vs newest  $C_D$  parameterization. This is not the most important difference. Frame your discussion around the functional form, the data they are based on, how well they represent low and high wind conditions.

Line 22-23: Suggest removing this sentence. This is common sense, no need to be in the abstract.

Line 30: “the sequence of values” is the least important thing to discuss about the differences. Discuss the physical behavior.

Line 76: Definition of  $\tau$  is already given in Lines 42-43. Here, and many other places, remove repeated definitions.

Lines 89-90: Suggest removing this sentence, repeats definition given in Line 83.

Line 140: I guess you mean here equation (5), which assumes proportionality; (6) modifies (5) to linear relationship.

Line 144: I think you mean here equation (7), not (8). Eq. (7) needs correction (see Major comment 2).

Line 147: Fix symbol  $U_{N10}$  to  $U_{10N}$ . Check all your math symbols for correctness and consistency.

Line 155: Fig 1 shows parameterizations whose equations are not yet introduced. Need to introduce (8)–(14) before referring to Fig. 1. See Major comment 4.

Line 168: Use symbol  $U_{10}$  instead of re-defining it again.

Line 168-169: How these data on sea roughness are used? None of your equations (8)–(14) uses sea roughness. Why then introduce these data here?

Line 175: Use symbol  $U_{10N}$  instead of re-defining it again.

Lines 177-179: You do not use wave data in (8)–(14), why do you introduce these data here?

Lines 180-183: Are all these details part of the FluxEngine software? Or are these done by you?

Lines 186, 226, 235: Suggest re-numbering Fig. 6 to Fig. 2, then all other figures. You refer to all other figures much later in the text.

Lines 195-215: Need to be introduced before Line 155 (see Major comment 4).

Line 217: “gridded global air-sea momentum” Why global when your emphasis is on NA and EA? Is global a good reference? You need representation of average conditions (either spatially or temporally averaged) for a reference. Need to work this out.

Line 229: Revise “sinusoidal”. The decrease at low winds is not due to sinusoidal behavior.

Lines 246-248: Why looking into global values for seasonal variations when it is clear that opposite seasons cancel the variations? For seasonal variations, it is better to compare to Northern (or Southern) hemisphere.

Line 276: “could be at statistical effect” What do you mean? Suggest revision for clarity.

Line 286: What proportionality do you mean? Not clear.

Lines 329-331: Not clear what is your conclusion here. Please revise.

Line 333: “need upgrading” From what expression? To what expression? You make all these calculations but in the end you do not recommend what is good to use. See Major comment 1c.

Line 499: Average annual mean: Area averaged? Or over the time period 1992-2010? Not clear. Please revise here and in the text.

Figure 3a: Should show data for the Northern hemisphere if you want to use this as a reference for seasonal variations.

Fig. 4: Why do you need this figure? What more does it shown than Fig. 1?

#### Writing style and corrections:

Line 32: I guess you mean “Because A12 parameterization..”

Line 37: Suggest revision to read: “Wind stress at the air-sea interface influences the wind-wave interaction, including...”

Line 43: Suggest revising “must” to other word. “Must” is a firm request, which you do not follow in your subsequent considerations.

Lines 45-46: Suggest revising to read: "...fifty years, as the collection of flux data has increased, many empirical formulas..."

Line 61: Suggest revising "we chose to check" to "we investigate" or "we quantify"

Line 67: Suggest revising "accommodate" to "represent"

Lines 82-83: You have  $u_*$  in italic and non-italic. Here, and everywhere, give mathematical symbols consistently.

Line 144: Abbreviation A12 should be introduced on first encounter, in line 50.