# Reply to RC1

**We thank the reviewer for their thorough reading and constructive comments, which will help to improve the manuscript. In the following, we cite the reviewers' text and add our own answers and planned text modifications in** green.

My primary concern with this study is that the derived drag coefficents are only as accurate as the Garbrecht et al. (2002) parameterization scheme used to convert from topographic measurements to drag. While Garbrecht et al. (2002) show a good comparison between parameterized and measured values of drag, the observations they use are still limited to just a few distinct, short measurement campaigns. Furthermore, the parameterization still relies on some unconstrained factors; namely the coefficients of resistance, the surface roughness length, and the effects of sheltering (different values and formulations for each of these have been suggested throughout the literature).

> **We agree, the data base is limited, but the scheme is based on more than just the Garbrecht et al. (2002) (GA02) paper. E.g., it represents a combination with the scheme of Lüpkes et al. (2012) (LU12) (as proposed by Petty et al. (2017) and as it was foreseen already by LU12). Both together reflect the present state of the art. In the revised version, we will better explain that the parametrizations of LU12 and GA02 are based on the same principal equations to determine the form drag caused by sea ice features like floe edges and ridges. The difference is just the geometry of the obstacles. In the present manuscript, we mentioned only GA02 as a validation but it was also validated based on measurement of turbulent fluxes over a single ridge (Garbrecht et al. 1999), by Vihma et al. (2003) (based on a model study) and by a PhD thesis (Ropers, 2013) based on further aircraft data of direct flux measurements. Ropers (2013) tested also different assumptions concerning the geometry of ridges and found that for closed sea ice cover a 2D geometry as used by LU12 for the marginal sea ice zone is not improving the results because of too many unknown parameters. However, since the basic idea via the calculation of the dynamical pressure is based on the same equations, one can also mention validations of the LU12 scheme as a justification of the GA02 scheme. Such a validation has been obtained by Elvidge et al. (2016) and Renfrew et al. (2019) using own aircraft flux measurements and another one based on ship measurement of fluxes by Srivastava et at al. (2022).**

Additional testing of the parameterization scheme is obviously beyond the scope of the current study and the data available. The lack of additional experimental verification of the scheme does not invalidate the results of the present study. However, the uncertainties associated with the scheme should be acknowledged by the authors, and quantified where possible. For example, how sensitive are patterns of spatial or temporal variability to the chosen formulation for the coefficient of resistance, or to the floe edge drag coefficient?

> **Sensitivity studies with different values for the coefficient of resistance (e.g., Garbrecht et al., 2002, Ropers, 2013) will be conducted. Same for the sheltering function, which will also be evaluated on the ICESat-2 data for comparison's sake.**

Would the results be similar if other schemes (e.g., one of those from Lupkes et al., 2012) were to be adopted instead? It's likely that such changes would slightly modify the drag coefficients without majorly impacting the spatial or temporal patterns of interest—but this should be tested. I recognize that previous studies that similarly convert ice topography to drag coefficients (Castellani et al., 2014; Petty et al., 2017) don't include such discussion but I nonetheless feel that it is important to acknowledge some of these unknowns.

> **We stress that the Lüpkes et al. (2012) scheme should not really be seen as an alternative because it was the idea from the very beginning of Lüpkes (here coauthor) to combine both schemes. Just for simplicity the drag coefficient for a region with 100 % sea ice cover had been prescribed to a constant value in this derivation but it was clear that in the inner Arctic especially during winter when no melt ponds are present, the topography is governed by ridges. It is described by LU12 that the assumption of constant drag coefficients over a region completely covered with sea ice is just the simplest possible approach but can be improved. An application of the LU12 scheme alone departs from the assumption of constant drag coefficients only in case of a large fraction of open water (e.g., $F_w > 10$ %). Such conditions exist only in the marginal sea ice zones or during the summer months also in the inner Arctic (mainly June – August). But already during August leads and ponds appear to be overfrozen which makes it difficult to identify such structures by remote sensing. This is the reason why in the current results, the drag coefficients caused by floe edges alone is probably underestimated. This will be better explained in the Discussion part of the revised version.**

Despite this critique, I find the subject matter and results of this study to be important, the methods and analysis to be robust, and the quality and readability of the manuscript to be good. I recommend this study for publication after revisions to address my concerns with the parameterization scheme and some further minor comments, below. I don't believe that these revisions should take a substantive effort, but am marking my recommendation as a major revision on the basis of what I feel is the importance of discussing the uncertainties associated with application of a parameterization scheme.

General comments:

 - I appreciate that the study accounted for the spatial resolution of the ICESat-2 sampling by making comparisons with OIB ATM data. The authors chose to do so by developing a scaling

(eq 6) based on linear regression between the computed form drag coefficients from each data source. I am interested in the choice of regressing the computed coefficients (which depend non-linearly on each of the two topographic variables) versus regressing the topographic variables He and xe directly. Based on some of the details gleaned from Fig 1 and related text, one might expect that the mean obstacle spacing xe is most impacted by the smoothing rather than obstacle height He, so separate regressions might yield better results. Did the authors explore these different options? It is not necessary to provide a detailed analysis in the study exploring all of the different options for regressions, but the authors should acknowledge (either in §3.1 or in an appendix) if they performed these tests and either (a) overall scaling was the same; or (b) they chose to use the option with the best regression.

> **The alternate options for regression, now tested at the time of writing up this reply, will be addressed in the text and discussed.**

- The results of this study highlight the spatiotemporal variability of drag coefficients and suggest an importance of being better able to characterize obstacle statistics in numerical models. I think that there are some opportunities in the study to share some other information about the obstacle statistics that might be useful for other researchers thinking about empirically-based or simplified parameterizations. In particular, in some figures in the study the mean He and xe appear to fairly strongly negatively correlated with one another (consistent with under-ice measurements from Brenner et al, 2021; https://doi.org/10.1029/2020JC016977). I would be curious how robust such a correlation may be, or if it differs in space/time or for FYI/MYI? Furthermore, Martin (2007 Thesis; and others) suggest relationships between sail height and level ice thickness (which can be found from the modal surface level in these measurements) that might be worth sharing for these data.

> **First of all, thanks for the hint concerning the work of Brenner et al ( 2021). We will include this in the revised version. In this paper we do not aim to develop a parametrization and leave this for our future work. For this reason, we do not look into many further details. However, in the revised version we will include a discussion of the results concerning the correlation between ridge distance and ridge height. We expect that this correlation holds only for the large scale averaging, but probably not on a local basis. We will investigate this and present some graphs in the revised version considering the connection between ridge height and ridge distance on a local scale.**

> **In this connection it is important also to reflect on the limitations of ICESat-2 with regards to detecting ridges (most recently highlighted in Ricker et al. (2023)) when considering these types of correlations. The procedures which we applied, lead to a certain overestimation of ridge spacing and occasional underestimation of ridge height, referred to as "smoothing" in the paper. Corresponding scatter plots and**

**associated correlations with the pan-Arctic, MYI and FYI ice averages have been added in the appendix.**

Specific comments:

§1

- L201-22: I wholly agree with the statement that "the surface roughness of sea ice... needs to be better understood". However, the authors don't properly justify that statement here. Why is roughness important? Some of this motivation is found later, e.g., in L33-37, but I would introduce the qualitative importance of roughness before explaining it's origin.

> **We will modify the text as suggested by the reviewer, mentioning e.g. recent modeling studies by Schneider et al. (2021) as well as Yu et al. (2020) highlighting the importance of surface roughness for polar climate modelling. We will further refer to other publications who explained that surface roughness is an essential parameter in the Monin-Obukhov theory for the determination of surface fluxes and thus of the surface energy and momentum budget. We will also comment on the importance of surface roughness of operational needs of Arctic stakeholders (i.e. shipping) and local populations (i.e. safe travel on sea ice) (Dammann et al. 2018).**

- L41-45: Consider restructuring these sentences to improve the flow of the text: by starting with the definition of 'z' and its additional explanation, you disrupt the list of variables which makes it awkward to come back to the rest of them on L43. Instead shift the other items on the list (rho, U, k, theta) earlier and end with z. As a personal preference, I don't like sentences starting with a variable if it can be avoided. Rearranging the list would avoid starting a sentence with rho. Similarly, the sentence introducing the stability function, fm, can be combined with the previous one: "The drag coefficient Cd is usually written as a product of Cdn and a surface roughness dependent stability function fm"

> **The formula variable description has been modified as suggested by the reviewer.**

- L60: The statement that the parameterization has been used "successfully" can probably interpreted in multiple ways. To me, it implies that the application of the parameterization has matched observational estimates of drag; however, the cited papers don't test that. Are there any observational studies that test the parameterization aside from Garbrecht et al. (2002)?

Yes, there are such papers (see also above). As concerns comparison with independent data sets, these are the papers by Garbrecht et al. (1999) and the thesis of Ropers (2013). Furthermore, Castellani et al. (2014) show that at least the average drag coefficient, obtained from the GA02 parametrization with parameters as in our manuscript, agrees well with values given by Andreas et al. (2010) using SHEBA data. Furthermore, the approach was used by Vihma et al (2003) in a mesoscale modelling study. They showed that the application of the scheme led to a very good agreement of modelled and observed meteorological variables. Furthermore, including form drag in the marginal sea ice zones by the schemes of LU12 and Elvidge et al. (2016) resulted in an improvement of atmospheric model results described by Renfrew et al. (2019). Also Birnbaum and Lüpkes (2002) investigated the effect of floe-edge generated form drag in the marginal sea ice zone on meteorological parameters by applying a mesoscale nonhydrostatic model. The modelling can be called successful in the sense that they obtained realistic fluxes when the form drag parametrization was included and that the fraction of skin drag over form drag depends on the meteorological forcing when stability effects are accounted for. Finally, Martin et al (2016) show that the inclusion of atmospheric form drag leads to improvements in the modelling of sea ice drift. The latter work addresses only floe edge form drag but one can expect that further improvement might be possible when ridge form drag is included as well.

The term "successfully" has been omitted to avoid confusion.

§2

- L105-113: What is the effective horizontal resolution of the ATM data after processing? Does the 1m footprint result in a 1m horizontal spacing?

Yes, that is correct; that is now mentioned in the text.

- L130-134: Some type of schematic/visualization somewhere around here might be useful for understanding the data spacing referenced here and visualizing how satellite tracks fit in the 25 km gridboxes, the associated time-space variability of the data within a gridbox (i.e, colouring tracks in the schematic by time offsets or similar), and maybe the overlapping 10km segmentation.

A schematic has been produced for clarity. We thank the reviewer for pointing this out as we were not decided on whether this was necessary at first, but now see that the need for it is clear.

- L148: "...Rayleigh Criterion (explained below)" Where is the explanation for the Rayleigh criterion? I was expecting this to be defined somewhere following the numbered list, but don't see an explanation or relevant citation in the manuscript.

> We have now added the following explanation:

> "The Rayleigh Criterion states that two maxima (obstacles) must be separated by a minimum that is less than half the value of the higher maxima for them to be classified as two separate features (e.g., hibler 1975, wadhams 1986). After omitting all elevation maxima that do not fulfil the Rayleigh Criterion, the obstacle heights and the spacing between them (both in meters) are averaged over each 10 km segment, before calculating the neutral drag coefficents at this same scale."

> We thank the reviewer for pointing it out; it must have been omitted accidentally at some point during the correction process.


- L204-205: Mention where the cw formulation comes from (this is the one recommended by Garbrecht et al. 2002, but they also test a few other formulations). You may also be interested in seeing Zu et al., 2021 (doi.org/10.1029/2020JC016976) who use laboratory and numerical modelling in an attempt to constrain the formulation for this coefficient as applied to under-ice ridges.

> As discussed previously, the sensitivity studies with different coefficients of resistance will now be included in the paper. We thank the reviewer for recommending reading additional literature on the topic.


- L226-228: It might be worthwhile to provide some context for the version of the edge-drag scheme introduced here, especially given the later suggestion (L442-443) to further estimate this.

> We have added context with the following sentence:

> "The parameterization is not fit to observations but rather derived from physical concepts and assumptions based upon drag partitioning scheme by Arya (1973, 1975)."


- L233: In my opinion there is no need to repeat the value of Cd,s from equation 4; it's already listed on L213.

> The repetition has been removed as suggested by the reviewer.

§3

- L273 and Fig 2: In my opinion, it doesn't seem necessary to include the 15m and 45m filters when it is already established that the ICESat-2 data have an effective 30m spacing and the 30m filtering produces the best result. If they are kept as a part of the figure it might be helpful to include some justification about why they may be of interest to readers.

**We have added the following justification:**

**"Box sizes 15 m and 45 m are shown for comparison's sake and are meant to demonstrate how both too little and too much of this smoothing can fail to produce values comparable to that of ICESat-2."**

- Fig 2: For the no-smoothing-fit, it is hard to see the different colour contours in the heatmap (on initial look I thought it was all a single colour). For the 15m and 30m-fits it is hard to see the different colour contours in the heatmap because the 45m-fit heatmap is overlapping. In fact, I only realized that there were different colour contours because of the visibility of the core of the 45m-fit heatmap.

**The figure has been optimized keeping the concerns of the reviewer in mind.**

- L278-280: To be clear, the drag coefficients here just the form drag coefficients, not the total drag coefficients? (Consider using the Cd,o notation from eq. 5)

**Yes, that is correct; the notation has been changed accordingly throughout the text.**

- Fig 3 caption: mention the equations for calculating drag coefficients in panels C, D (e.g., eq. 3 & 5 for C, and eq. 3, 5 & 6 for D).

**The figure caption has been modified as suggested. However, importantly, equation 5 is not yet used for this figure and instead we report the drag coefficient as the sum of skin drag and form drag from obstacles (identical to the approach used for column 3 of figures 5, A3 and A4).**

- L300-305 and Fig. 4: Despite the fact that the two data sets are not perfectly co-temporal, I would initially have expected a better regressor slope than 0.5 given that the basis for much of the analysis (binning in time/space) is implicitly based on slowly-varying statistics. My initial reaction to seeing this is some suspicion that eq. 6 is not valid across all different obstacle statistics, and wondering if separate scalings of He and xe (as suggested in my general comments, above) would produce a better fit. That initial thought may prove not to

be correct, but in any case I think that the authors are fairly quick to dismiss the value of the slope. In fact, I became more hung up on the slope value than necessary since the histograms in Fig 4B and discussion in L305-310 show that the difference between the datasets is not that drastic and generally a good fit. Perhaps some reorganization here could help prevent similar hang ups?

> **We will address the mentioned issues; we will modify and rearrange the text and omit Fig. 4A as reviewer 2 has suggested.**

- Fig 4B: are the histogram bar heights probabilities? Probability densities? Counts? Label the y-axis.

> **Label has been added.**

- L333-334: Roughly what is the percent coverage of ICESat-2 data relative to total sea ice area?

> **More information on this discrepancy has been included in the text. Unfortunately, there is no study, to our knowledge, which quantifies this discrepancy. However, it is fairly clear that the total area, as a rule, will be less due to clouds and returns with ice concentrations < 15% not being processed. The more clouds over the Arctic, the higher this discrepancy will be.**

- Fig. 6A: It would be interesting to see this broken up by the contribution of all the different terms in eq. 5 (perhaps in an appendix figure), or at least similarly to columns III and IV in fig. 5 (panel A in fig. 6 corresponds to column IV in fig. 5; an additional panel could be included in fig. 6 corresponding to column III).

> **The figure mentioned here was originally discussed and considered, however, since the skin drag component is a constant and likely for the gridded data product, both the open water skin drag and the floe edge form drag components are going to exhibit the same general pattern, it seemed to be rather redundant information given figure 5 (and A3 and A4). In the data files on Pangea (cited in the reference section of this document – will be updated along with the finalized paper and re-uploaded as version 1.0), these contributions are separated – this will be mentioned in the text. If we do decide to include this type of figure, it will likely be only in the supplement.**

- Fig. 6D: Can this panel also include the total sea ice area for reference? If not reasonable to do so, I'd recommend changing the title to "Total sampled area" or something similar.

> **We have changed the title as suggested.**

- Fig. 6: Date label format is hard to read. Also, if including axis ticks for only 2 months of each year, consider using March and September to correspond to the annual sea ice maximum/minimum.

**We have shifted the x axis to use March and September as suggested.**

- L359-368: A number of areas are mentioned here (and elsewhere in the paper) by name when describing spatial characteristics of the ice, but not everyone is familiar with these different geographic features. Including the names on a map or figure somewhere would be helpful.

**We will add these different geographic features to figure 3 if they are clearly visible while at the same time they do not overcrowd the figure. However, we expect most researchers working on sciences associated with the Arctic to be familiar with the features mentioned. Perhaps describing their location relative to surrounding continents might also be a solution.**

- L376-377: The use of ATL03 data for similar statistical measurements should be mentioned much earlier (back in §2.1). The use of ATL07 seems well justified by the discussion in this section and it's appropriate to keep most of that discussion here, but the ability to use ATLO3 to derive roughness should be acknowledged before this section.

**We have added the following text to figure 2.1:**

**"Here we would like to mention that ATL03/L2 has been successfully used to provide more accurate information on individual ridges using an alternate processing to ATL07/L3A (see Duncan and Farrell 2022). However, for this study, we opted to use the more readily available ATL07/L3A since we are dealing with spatial averages that do not sufficiently benefit from alternate sampling routines and are in need of adjustment (discussed in section 3) regardless of the data product used."**

- §3.6: I appreciate the inclusion of this section and explicit statements of the study's significance.

- L392-393: Accounting for stability effects is (rightfully) outside the scope of the present study; however, seasonality in the surface stress in eq. 1 will depend on both seasonality in neutral drag and seasonality in the stability. It would be beneficial if the authors were to very roughly describe the impacts of stability here: specifically, would it be expected to it enhance the neutral drag coefficient seasonality or counter it?

> **We will add text addressing the stability function. But in general, over closed sea ice the near-surface stratification is more stable in winter than in summer. This means that drag coefficients will be reduced in winter relative to summer. It is more complex for fractional sea ice cover thus especially over the MIZ. There, the effect depends strongly on the sea ice fraction, temperature conditions and on wind direction (on-ice or off-ice). The reason is that form drag depends on the stratification over both sea ice and open water as shown by Lüpkes and Gryanik (2015). Thus, in this region both is possible especially during winter, namely reduction and enhancement of the drag coefficient when unstable conditions over open water have a stronger impact than the stable conditions over sea ice (see Lüpkes and Gryanik, 2015).**

- L401-402: "FYI ice peaks sometime in July-August (blue line in Fig. 6A)" This is hard to discern from figure 6A, especially as (I think) the blue line doesn't seem to even include July-August data (unless I'm misreading the date labels). The secondary peaks in ~August for each year are only in the "all ice" black lines, and are below the primary peaks. Elevated values of drag seem to exist for FYI each September.

> **Yes, that is correct and a result of the MYI masks generated from the MYI data (which are not available for some summer months). The following clarification has been included in the text as follows:**

> **Added clarification: "We observe that MYI ice exhibits highest drag in May (red line in Fig 6), due to an increase in the form drag due to obstacles, and FYI ice peaks sometime in July-August (according to the secondary peaks in the black line [all data] in Fig 6A and the associated presumed trajectory of the blue line [FYI data]) from a longer ice-water edge and the associated floe edge drag in summer months."**

- L410: "All other Arctic Seas (mostly FYI)" Consider including a figure showing FYI/MYI extents when available, perhaps as another column in fig. 5? (This isn't totally necessary, but would be helpful for the discussion here and in §3.3).

> **We appreciate the suggestion but do not see the need to overcrowd figure 5 with an additional column of a parameter that is not central to the study. The MYI concentration dataset as well as the associated literature is cited and the readers are invited to look at the data on the browser for spatial distributions of MYI for the time period covered.**

- L418-419: This whole study is based on the use of a form drag parameterization and frequently cites Tsamados et al., (2014) who describe the implementation of a similar parameterization in a modelling framework; however, this statement argues that further

drag parameterization is necessary. This seems contradictory and should probably be rephrased. Nonetheless, I agree that more work needs to be done in this regard; specifically, the results indicate the need for further ability to model obstacle heights/spacings and for the implementation and use of form drag parameterizations. Also see my general comment regarding additional data presentation for simplified parameterizations.

> **The Tsamados et al. (2014) implementation is not directly derived from observations but instead uses a theoretical approach. This difference will be made clearer in the text.**

§4

- L425: This is nit-picky, but I dislike the phrasing here. Saying that the study *relates* the topography to drag coefficients could be construed to mean that the two are measured independently. The study *uses* measured sea-ice topography to calculate temporal and spatial variations of atmospheric drag coefficients.

> **We agree with the concerns of the reviewer, and have modified the phrasing.**

- L443-444: Not a subject of this study, but I am curious how you plan to account for the floe sizes when considering the edge drag component. Do you intend to also determine those using ICESat-2 data (e.g, Horvat et al., 2019; https://doi.org/10.5194/tc-13-2869-2019)?

> **ATL07 includes surface classification information (lead or ice returns), and these have been shown to agree well with coincident satellite imagery from Sentinel-2 (e.g. Petty et al., 2021), opening up the possibility of deriving floe size and floe edge information directly from ICESat-2. However, the classification scheme was derived with a focus on sea surface height and freeboard derivation, and issues around misclassified dark leads (Kwok et al., 2021) and the lack of a distinct melt pond classification remain.**

> **As a first step, as discussed previously, we might include gridded freeboard from ICESat-2 to further modify the Lüpkes et al. (2012) parameterization to observer it's spatial variability.**

§A

- L457-460: I am finding this sentence hard to parse (the one beginning "As a result...").

> **Sentence has been revised as follows:**

"As a result, the regional drag coefficient estimates at higher latitudes are more representative of the time periods shown in Figs 3, 5, A3 and A5, whereas those at lower latitudes are computed with fewer height measurements (often just a few select days). In other words, rather than a temporal mean of surface topography, it is a data set that is sewn together with the best representation of the temporal mean near the pole hole."

Other grammar and typos:

- L5: Grammar error in: "though it cannot resolve as well airborne surveys"

Corrected

- L200: Use an in-text citation instead of a parenthetical citation for Castellani et al., 2014

Corrected

- L271: Suspected missing decimal in "OIB ATM occupy a wider range (~03–1.3·10^−3 )"; should probably be 0.3 instead of 03

Yes, it was missing - now corrected

- L300-303: Awkward sentence structure for sentence that begins: "Correlation (0.51) and slope...". Consider revising or maybe breaking up the sentence.

Sentence restructured as follows:

"The correlation (0.51) and slope (0.5) of the bivariate distribution, given the two data-sets are retrieved on different days with ice drifting in between, are reasonable and positive suggesting some base similarity in the local spatial variability of surface drag."

- Figure 6: mismatched capitalization in plot titles for panels B and C

Corrected

- L408: Missing figure reference

**Corrected**

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