An analytical model for wind-driven Arctic summer sea ice drift

H.-S. Park and A. L. Stewart

Response to Anonymous Reviewer #2

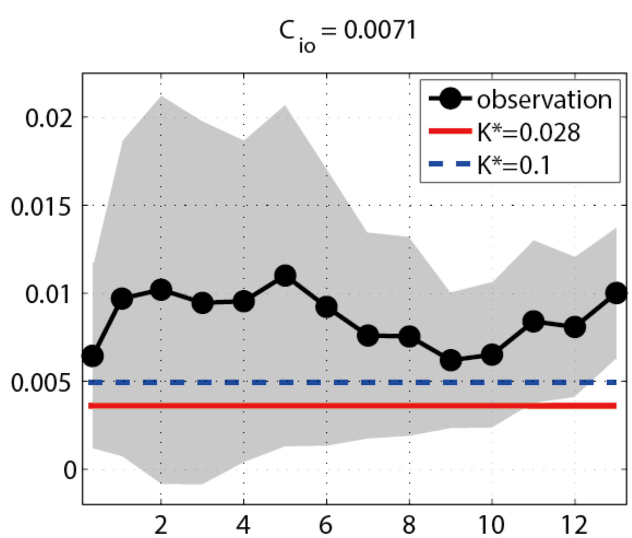
We thank the reviewer for carefully reading and commenting on our paper for another round. We have revised the manuscript in accordance with these comments. Below we address the reviewer's comments individually.

(1-1) Figure 3: The authors have added a figure in order to show the variation of the ice-drift velocity with wind speed (used as a substitute for interface stress). I would have preferred an explicit plot of quadratic drag coefficient (interface stress squared divided by total shear across the IOBL squared) against the wind speed to assess this variation in parallel with the variation in turning angle.

➔ The IOBL drag coefficient is more difficult to estimate accurately from the data because it requires the geostrophic "interior" velocity $\vec{u}_g$: ,

$$\sqrt{\overline{u'w'}^2 + \overline{v'w'}^2} = C_{io}\left[(u_i - u_g)^2 + (v_i - v_g)^2\right].$$

To obtain an estimate, we neglect the geostrophic velocity and retain only the ice velocity; typically the velocities at depth are indeed much smaller than the ice velocities in the ITP-V data.



The above figure shows the ice-ocean quadratic drag coefficient calculated from the data of Cole et al. (2014) (black dots with shadings) and estimated from our analytical model (equation (10a), red and blue dotted lines) as a function of 10m wind speed. As the reviewer indicates, this variation as a function of surface wind speed is not very large. Additionally, the vertical eddy momentum flux at 6m depth is very likely to underestimate the stress velocity, so the drag coefficient is likely to be overestimated.

Though the quadratic drag coefficient and turning angle across the full IOBL may be more familiar to readers, we have tried to avoid using these quantities for validation purposes because of the difficulty associated with estimating the “interior” geostrophic velocity. Instead we validate our model predictions against quantities directly measured by the ITP-V, for example the wind-ice and ice-ocean turning angles. We have therefore opted to exclude the above estimate of the IOBL drag coefficient, though we have now briefly discussed its insensitivity to surface stress in the Appendix.

(1-2) However, from Fig. 3 as it is now, I infer that this variation is not very large, if there is any variation at all (the only case where I would expect such a variation is the phi<<1 case, though, which does not feature very prominent in Fig. 3 as it is now). My comment from the first review, that the drift speed is more important than the turning angle, is still valid. The authors might wish to emphasize the phi<<1 case for the drift speed more, but I can accept if they choose not to.

➔ See comment above. We have chosen to leave Fig. 3 unmodified because reducing φ from 1 to 0.5 only produces a small change in the ice speed predicted by our analytical model, and the observational data are sourced from a region with relatively high ice concentration (85-90%).

(2) p. 25, Appendix:

What you calculate as C_{io} in the appendix is the drag coefficient between ice and surface ocean (6m, so only slightly into the Ekman layer), not across the entire IOBL. I am not overly surprised you don't find any dependence on interface stress there, since this dependency is a result of the varying height of the constant-stress layer, *together with the full Ekman layer*.

➔ We agree with the reviewer’s comment: particularly for large wind speed/ice-ocean stress, the combined effect of deeper constant-stress and Ekman layers may be expected to reduce the error associated with using a measurement depth several meters below the ice base. However, we have no quantitative means to evaluate the depth of the constant-stress layer from the data, and limited means to evaluate the depth of the Ekman layer. We have therefore simply expanded our discussion at the end of section 4.1, concerning the issues associated with using measurements at 6m depth rather than at the ice base itself.

(3) p. 3, l. 22 remove "is"

➔ This is corrected. Thank you.

(4) p. 16. l. 18 increases ice speed *by* up to

➔ This is changed as the reviewer suggests.

(5) p. 22, l. 13 changes *in* shape

➔ This is changed as the reviewer suggests.

Response to Anonymous Reviewer #3

We thank the reviewer for carefully reading our paper, and for providing detailed comments, particularly on the writing and presentation. We have revised the manuscript in accordance with these comments. Below we address the reviewer's comments individually.

2. Notes on the article

The presented work provides an efficient approach to determine the impact of wind stresses on the ice drift, in terms of considering tendencies. Particularly, when it comes to evaluate observational data, it provides an important tool. The section on the evaluation presents the abilities and limitations of the analytical model. This is a necessary part of the article, as presumptions in the derivation of the model equations and in the evaluated data differ. This is the crucial drawbacks of the present study and causes confusions (though the general structure of the article is clear) when reading the article. Limitations and applicability could thus be more clearly pronounced, see also below.

➔ We agree with the reviewer that the model evaluation using ITP-V data, which mostly covers winter season, risks confusing readers because our analytical model is more suitable for summer. We have attempted to improve the clarity of the article by addressing the reviewer's specific points below.

The abstract contains too much information. It should address the main results and aspects of the article. I suggest to shorten it. For instance, you start to explain, that a winter period is not suitable for comparison, but that your model works fine (to my mind, this itself should not be part of an abstract). In the article itself however, you tell, that the considered time period was somewhat exceptional (p.3 l.19ff), which is a different message than what you write in the abstract. I would skip it here. For instance, I would reduce the second paragraph to something like: *Compared to recent observational data from the first ice-tethered profiler (ITP-V), the model is able to capture the dependencies of the ice speed and wind/ice/ocean turning angles on the wind speed. The model is used to derive responses to intensified southerlies on sea ice concentration; the results compare closely with satellite observations.*

➔ Following the reviewer's suggestion, the second paragraph of the abstract is re-written.

In the upcoming, I would like to announce parts of the article, which should be modified:

(1) I was confused about the fact, that on the one hand $\varphi < 1$ has been used in the derivation of the simplified set of equations, but on the other hand $\varphi \sim 1$ has been used throughout the article, whenever the model was used.

I would reorder the results (maybe even as first part in the evaluation section), in order to more clearly pronounce the impact of changes in the ice cover on the ice speed and velocity angles.

➔ The derivation of the model equations allows for any value of φ, but error associated with neglecting internal stress may be expected to increase as φ approaches 1.

➔ The reviewer's statement that "$\varphi \sim 1$" has been used throughout the article, whenever the model was used" is incorrect. We restrict our attention to the $\varphi \sim 1$ case only in section 2.3, to aid physical interpretation of the model; this is clearly stated at the start of section 2.3. In section 4 we use $\varphi = 1$ as a reference case because the sea ice concentration in the winter Beaufort sea is close to 100%, as we identify clearly at the start of section 4. In section 5, φ is allowed to take any value between 0 and 1.

➔ To clarify the use of $\varphi = 1$ as a reference parameter in section 4, we have added the following sentence in the first paragraph of section 4.1: "We use $\varphi = 1$ as a reference case because, as we will show below, the ice drift speed and angle predicted by our model are insensitive to φ for sea ice concentrations greater than ~50%."

(2) I wonder about the differences in the solutions in Section 2.2.1 ($\varphi \sim 1$) and Section 2.2.2 ($\varphi \ll 1$). This also relates to the effect of neglecting internal stresses in the momentum balance on the solution. I would add some notes corresponding to that in the summary and outlook section.

➔ We have now expanded our discussion in section 6 of the caveats associated with the sea ice concentration and the neglect of internal stress.

(3) Further, I do not get a clear statement, when considering the graphics and reading the appropriate texts. On the one hand, $K_o^* = 0.1$ leads to improved solution compared to the observations, which is misleading as rheology has been neglected (and we consider winter data). Then you state that the IOBL enhances the model results, as $K_o^* = 0.028$ and $K_o^* = \infty$ lead to

different results. How do you deduce that?

Maybe I missed it, but when you start the evaluation section, you could clearly state, that you are interested in representing the trends in the dependencies right (which you can), as you are currently not able to mathematically represent the case $\varphi \ll 1$ in a better way.

➔ Following suggestions from the previous round of reviews, we have been quite explicit about our motivation for presenting the $K_o^* = 0.1$ case. From the second paragraph of section 4.1: *"Extensive measurements of the ice-ocean boundary layer suggest that the annual mean value of the dimensionless vertical eddy diffusivity K_o^* is about 0.028 (McPhee, 1994; 2008). Below we also present model predictions using a nominal enhanced value of $K_o^* = 0.1$, which yields improved agreement between the model and the observations. A possible explanation for this is that the ITP-V observations mostly cover winter season (from October to March), when surface buoyancy loss due to sea ice formation can enhance the vertical eddy diffusivity by a factor of up to 10 (McPhee and Morison, 2001). However, it is more likely that internal stresses in the ice impede its motion, so the canonical value of $K_o^* = 0.028$ overestimates the ice drift. Thus the reader should not infer from our results that using a larger value of K_o^* is more physically realistic."*

➔ Perhaps the word "enhances" was an unfortunate choice, as this reviewer has interpreted it to mean that the IOBL "improves" the model results. Where we say that the IOBL "substantially enhances the wind-induced ice speed", we mean that the wind-induced ice speed increases when the IOBL is included (i.e. for finite K_o^*). We have replaced "enhances" with "increases" to be clearer about this.

(4) p.11 eq (20,21): I had difficulties in deriving (20) and the first equality in (21).

➔ We have now more clearly set out the steps required to derive equation (20). The steps required to derive equation (21) are already explicitly stated in the text, but for the reviewer's benefit we include more detailed steps in this reviewer response. We have refrained from including these steps in the manuscript because they would detract from the clarity of section 2.3.2. However, we have included the definition of the air-ice turning angle in equation (21) as an intermediate step in the derivation.

We start from the definition of the air-ice velocity angle:

$$\cos(\theta_{ai}) = \frac{\vec{u}_a \cdot (\vec{u}_i - \vec{u}_g)}{|\vec{u}_a||\vec{u}_i - \vec{u}_g|} \qquad (21)$$

As stated in the text preceding equation (21) in the article, we make use of several previously-derived relations:

$$\vec{u}_i - \vec{u}_g = \left(\frac{1}{\sqrt{C_{io}}} + \frac{1}{\sqrt{2K_0^*}}\right)\vec{u}_{io}^* - \frac{1}{\sqrt{2K_0^*}}\hat{Z} \times \vec{u}_{io}^*, \qquad (10a)$$

$$\vec{\tau}_{ai} = \rho_a C_{ai}|\vec{u}_a|\vec{u}_a = \rho_a|\vec{u}_{ai}^*|\vec{u}_{ai}^*, \qquad (6a)$$

$$u_{io}^{*\parallel} = \frac{1}{k_a}\frac{|\vec{u}_{io}^*|^2}{|\vec{u}_{ai}^*|^2}(1 + k_o|\vec{u}_{io}^*|), \qquad (14)$$

$$u_{io}^{*\perp} = -\frac{1}{k_a}\frac{|\vec{u}_{io}^*|^2}{|\vec{u}_{ai}^*|^2}(1 + \alpha). \qquad (15)$$

It immediately follows from (6a) that

$$\vec{u}_a = \frac{1}{\sqrt{C_{ai}}}\vec{u}_{ai}^*. \qquad (i)$$

We can obtain an expression for the numerator of (21) by taking the dot product of (*) with (10a),

$$\vec{u}_a \cdot (\vec{u}_i - \vec{u}_g) = \frac{1}{\sqrt{C_{ai}}}\left(\frac{1}{\sqrt{C_{io}}} + \frac{1}{\sqrt{2K_0^*}}\right)|\vec{u}_{ai}^*|u_{io}^{*\parallel} + \frac{1}{\sqrt{C_{ai}}}\frac{1}{\sqrt{2K_0^*}}|\vec{u}_{ai}^*|u_{io}^{*\perp}. \qquad (ii)$$

Substituting (14) and (16) into (**), we obtain

$$\vec{u}_a \cdot (\vec{u}_i - \vec{u}_g) = \frac{1}{\sqrt{C_{ai}}}\frac{1}{k_a}\frac{|\vec{u}_{io}^*|^2}{|\vec{u}_{ai}^*|}\left[\left(\frac{1}{\sqrt{C_{io}}} + \frac{1}{\sqrt{2K_0^*}}\right)(1 + k_o|\vec{u}_{io}^*|) - \frac{1}{\sqrt{2K_0^*}}(1 + \alpha)\right]. \qquad (iii)$$

Recalling that $\alpha = \sqrt{2K_0^*/C_{io}}$, we can further simplify (***) as

$$\vec{u}_a \cdot (\vec{u}_i - \vec{u}_g) = \frac{1}{\sqrt{C_{ai}}}\frac{1}{\sqrt{2K_0^*}}(1 + \alpha)|\vec{u}_{io}^*||\vec{u}_{ai}^*|\frac{k_o}{k_a}\frac{|\vec{u}_{io}^*|^2}{|\vec{u}_{ai}^*|^2}. \qquad (iv)$$

We obtain an expression for the numerator of (21) by taking the magnitudes of (i) and (10a),

$$|\vec{u}_a||\vec{u}_i - \vec{u}_g| = \frac{1}{\sqrt{C_{ai}}}|\vec{u}_{ai}^*| \cdot |\vec{u}_{io}^*|\sqrt{\left(\frac{1}{\sqrt{C_{io}}} + \frac{1}{\sqrt{2K_0^*}}\right)^2 + \left(\frac{1}{\sqrt{2K_0^*}}\right)^2}. \qquad (v)$$

Equation (v) can be simplified as

$$|\vec{u}_a||\vec{u}_i - \vec{u}_g| = \frac{1}{\sqrt{C_{ai}}}\frac{1}{\sqrt{2K_0^*}}|\vec{u}_{ai}^*| \cdot |\vec{u}_{io}^*|\sqrt{(1 + \alpha)^2 + 1}. \qquad (vi)$$

Finally, dividing the right-hand side of equation (iv) by that of equation (vi) yields

$$\cos(\theta_{ai}) = \frac{\vec{u}_a \cdot (\vec{u}_i - \vec{u}_g)}{|\vec{u}_a||\vec{u}_i - \vec{u}_g|} = \frac{k_o|\vec{u}_{io}^*|^2}{k_a|\vec{u}_{ai}^*|^2}\frac{1+\alpha}{\sqrt{1+(1+\alpha)^2}} = \frac{|\vec{\tau}_{io}|}{|\vec{\tau}_{ai}|}\cos(\theta_{IOBL}). \qquad (21)$$

(5) p.11 l.17ff: You argue for the case $\varphi < 1$, but in the caption of Fig. 2 you write, that the plot is derived from eq. (20), which is based on $\varphi \sim 1$. Thus, you can not use Fig. 2 as argument for $\varphi < 1$.

➔ This sentence should have been at the end of the preceding paragraph, and has now been moved there. Fig. 2 is not intended to demonstrate the point made by this sentence.

(6) p.18 l.11: I am not convinced about the best available estimates for the parameters. You use $K_o^* = 0.028$ as this is the parameter which stems from observations, but your evaluation shows that $K_o^* = 0.1$ fits better to the observations in your case. Similar applies for Cio.

We do not argue that a larger value of vertical diffusivity is physically meaningful.

➔ On page 15, lines 13-15, we state that "Thus the reader should not infer from our results that using a larger value of K_o^* is more physically realistic".

➔ On page 16, lines 4-8, we also state that "Fig. 3a shows that the analytical model with the canonical value of K_o^* ($K_o^* = 0.028$) overestimates the observed ice speed by 20–40%, whereas a larger vertical diffusivity (blue-dotted line; $K_o^* = 0.1$) fits better with the observations. As stated above, this is probably because the internal stresses in the relatively concentrated sea ice (85–100% in winter) impede the ice drift".

3. Typos and minor issues

There are multiple typos and some incomplete sentences within the text. Please, look through the text carefully and correct. Some typos are listed below. Furthermore, I added some suggestions for an easier understanding of the content.

(1) Throughout the article, lots of articles are missing, such as: always before "Rossby similarity theory", often before "internal stress", "sea surface tilt", etc.

➔ We appreciate the reviewer's suggestion, and we have checked the highlighted instances. Articles are omitted preceding "Rossby similarity theory" because this expression refers to a body of theory, rather than a particular theory. We have sometimes included articles preceding "internal stresses" and "sea surface tilt" and sometimes omitted them, depending on the context. Either is grammatically correct, but adding an article (i.e. "the") changes the emphasis slightly.

(2) Look for "fee drift" and change to "free drift" (I found it two times).

➔ “fee drift’ is changed to “free drift”. Thank you.

(3) Change "internal (ice) stress" to "internal (ice) stresses" (singular → plural).

➔ We have changed these as recommended to be consistent.

(4) Use adverbs to characterize a verb, e.g. p.3, l.10 (full**y** coupled), caption in Fig. 2: "nominall**y** enhanced", p.16, l.20: "individuall**y** observed".

➔ These typos are corrected. Thank you.

(5) p.3. l.11: You do not exploit the efficiency (I do not see any comparison to a fully coupled model there). Instead, you exploit the performance/quality of the analytical model. Please, rephrase.

➔ We are no longer guilty of exploiting our model’s efficiency.

(6) p.3, l.21: No full stop in "... IOBL was small compared ..."

➔ This is corrected. Thank you.

(7) p.3, l.22: "Consequently, our model largely captures..."

➔ This is corrected. Thank you.

(8) p.5 l.3: "time scale **of** one week"

➔ This is corrected. Thank you.

(9) p.6 l.2: " $\vec{u}_o^*$ is **the** stress velocity"

➔ This is corrected. Thank you.

(10) p.7 l.8f: In order to start with the original intention and finish the sentence with the resulting task, I would rephrase the sentence like

In order to derive a solution for the ice velocity $\vec{u}_i$ *we now solve the previously derived equations (8) and (9) for the stress velocities* $\vec{u}_{io}^*$ *and* $\vec{u}_o^*$.

But it is up to you. (For me it is easier to get the message in that way.)

➔ As the reviewer suggests, this sentence is rephrased.

(11) p.7 l.13-15: In Section 4 you do not show, that your approach provides a close

approximation to the general solution for ice concentrations greater than 50%.

It might be better to formulate, that there are regimes where the model provides a close approximation to the solution for ice concentrations greater than 50%.

➔ We have now rephrased this sentence.

(12) p.7 l.17: What does it mean: "... that the solution be given in full"?

➔ We have now rephrased this sentence.

(13) p.8 l.13: For an easier reading you may insert "due to (16)" in "Note that the right hand side of (11).."

➔ This has been corrected as suggested.

(14) p.9 l.1: $|\vec{u}_{io}^{*}|$ is the wind *speed*.

➔ $|\vec{u}_{io}^{*}|$ is the magnitude of the ice-ocean stress velocity. We believe the reader was referring to $|\vec{u}_{ai}^{*}|$ in this sentence, which we have now referred to as the wind stress velocity magnitude.

(15) p.9 l.5: "for which it would be...". You could also skip the part of the sentence after "100%". There is no new information.

➔ As the reviewer suggests, the later part of the sentence is deleted: "for which it is would be inaccurate to assume φ ≈ 1"→ this is deleted.

(16) p.9 l.6f: I would rephrase to emphasize that you aim to solve this case numerically. For instance by "As there is no closed-form of the analytical solution to the model equations in general, we determine a solution numerically."

➔ Here we outline the procedure to obtain a closed-form solution for the model equations with arbitrary sea ice concentration. However, the written solution would be too complex to yield any insight, so in practice we solve numerically. We have revised the text to make this clearer.

(17) p.9 l.26: upper case: "section". Check for consistency with other Section references, e.g. p.9 l.8, l.12, where you use the abbreviation "Sec."

➔ This is corrected. Thank you.

(18) p.12 l.1: one times "a larger" is sufficient.

➔ This typo is corrected.

(19) In the beginning of this section I would mention the time interval you are intending to consider, otherwise the reader could get lost.

➔ We have considered the reviewer's suggestion, but we feel that the specific details of the time interval to be considered are best left to until the actual analysis is described.

(20) p.15 l.1: "therefore"? I think, setting hi = 1:5m is motivated by the observations. You could just skip the word and add something like "in agreement with the observations".

➔ The sentence is worded this way to emphasize that this thickness is only appropriate over a lengthscale much larger than the individual ice floe, whose thickness is much larger than 1.5m. We have rephrased the sentence to reflect this.

(21) p.15 l.27: I see a linear relation only in the range between 4 - 11 m/s.

➔ We have inserted the word "approximately" here.

(22) p.15 l.20: delete "a" before "constant".

➔ 'a' is deleted. Thank you.

(23) p.16 l.17: swap $K_o^* = 0.1$ and $K_o^* = 0.028$.

➔ This is corrected. Thank you.

(24) p.16 l.21: "factor **of** 10".

➔ This is corrected. Thank you.

(25) p.17 l.17: "ITP-V data are not suitable".

➔ This is corrected. Thank you.

(26) p.18 l.18: change 60° to 50°.

➔ This is changed.

(27) p.18 l.4: This is confusing to me: You wrote on p.17 l.14, that the ITP-V data are not suitable for a comparison, but then you mention, that they compare well with the analytical solution. In Fig. 5 you use the data of ITP-V.

Maybe you could pronounce it in p.17 l.12 that way: "As the shallowest ITP-V data are at 7m depth, we use the solution of the Ekman spiral and compare our theoretical results in that depth against those data."

➔ The ITP-V data are suitable for comparison with the model, but they are not suitable for estimating the IOBL turning angle because the stress velocity is rotated substantially between the ice base and 7m depth, and because this requires an estimate of the magnitude and direction of the interior geostrophic velocity to be made. We have rewritten lines 11-13 on p.18 to reflect this.

(28) p.18f l.29ff: When it is surprising, why do you immediately come up with an answer? Better reformulate.

➔ As the reviewer suggests, this sentence is rephrased as: "The response of the IOBL turning angle to the mixture of sea ice and water ($\varphi \ll 1$) is presented in Fig. 6d".

(29) p.19 l.24: "velocity typically is large compared to".

➔ This sentence is changed following the reviewer's suggestion.

(30) p.20 l.22: "These" change to "The" or "The identified". (In the sentence before you treat a different topic).

➔ "These" is changed to "The".

(31) p.20 l.23: Why do you introduce a new variable (I_c) for sea ice con centration and do not apply the variable (φ) you used throughout the article?

➔ I_c is changed to φ.

(32) p.20 l.24f: dt is not part of the equation, which you refer to in the first part of the sentence. I would make two sentences out of the one.

➔ We believe the reviewer has misread the equation: "dt" is certainly part of it.

(33) p.20 l.26: "climatology mean dI_c from the daily dI_c". Something is wrong here.

➔ We calculated daily changing rate of sea ice concentration dI_c. We believe this is okay to calculate the long-term climatological mean changing rate to get the anomalous changing rate of sea ice concentration.

(34) Section 5.2: you explain: SIC decreases by 7-8%, but in the Figure you use a colorbar range of 6.5%.

➔ In some areas, sea ice concentration decreases more than 7% during the strong southerly events.

(35) p.21 l.17: You use the word "possibility". If you want to indicate, that the content of the sentence before, in l.16f, is something you think that might be the reason (for sth.), then I would say it in the sentence in l.16 (e.g. "We suggest, that ...").

➔ This sentence is rephrased.

(36) p.22 l.13: Please, finish the formerly started sentence in right grammar.

➔ This sentence is rephrased as: “The formation of a summer freshwater layer at the ice base can also reduce the ice-ocean drag coefficient C_{io} (Randelhoff et al., 2014) by changing the shape of the ice base”.

(37) p.23 l.17: "a quadratic drag **law**. "

➔ This is corrected.

(38) p.23 l.22: You wrote: "This makes the model straightforward to interpret". What should be interpreted? Something else than the model or the model itself? If you mean the latter, use passive; else add the object!

➔ The model itself is the object to be interpreted. This grammar is actually correct, but we have changed it at the reviewer’s request.

(39) Fig. 2. You could add "for $\varphi \sim 1$".

➔ As the reviewer suggests, we added this information in the figure caption.

(40) Fig. 5: inconsistency between x axis label and caption, see also p.17, l.26

➔ This typo in the caption is corrected. Thank you.

(41) Fig. 6: inconsistency: *H* is used in the plots, while h_i is used in the text.

➔ H is changed to h_i.

(42) You do not comment on Fig. 6c, if you do not need this graph, delete it.

➔ Fig. 6c is consistent with equation (20) and is briefly mentioned in the text.

(43) Fig. 9: l.13f: This is no sentence (verb is missing).

➔ The verb in this sentence is “is”.