Response to reviewers - Rethinking fractal scaling of sea ice deformation: the dominant role of signed-gradient cancellation

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October 26, 2026

Legend

Original Comments
Responses
Changes to the manuscript

Reviewer #1

Summary

Sea ice moves non-uniformly under the influence of wind, currents, and internal stress. The degree of non-uniformity, or deformation, may depend on the spatial scale over which it is calculated. Previous studies have found a power-law relationship between spatial scale and deformation with a negative exponent (i.e., larger deformation at smaller scales). This paper re-examines the reason for such a relationship. Previous studies have asserted that "localization" is responsible, in which the deformation originates from highly localized (small-scale) discontinuities or non-uniformities in the sea-ice motion. This paper finds that "signed-gradient cancellation" is responsible, in which positive and negative velocity gradients tend to cancel each other more and more at larger and larger scales. The paper also claims that standard invariant measures of deformation are actually not invariant under rotation.

We thank the reviewer for their time, a very thorough review and for highlighting inconsistencies in notation.

Comments

The Introduction presents a good overview of sea-ice deformation and scaling.

Sections 2.2, 2.3, and 3.1 are full of notational problems (and a few math errors), making it difficult to understand or evaluate the main analysis of the paper. Details are given in the Math and Notation sections below.

The analysis is based on synthetic velocity fields defined on a grid shown in Figure 2. On lines 159-160, the authors write: "Note that off-diagonal terms of the deformation matrix (du/dy and dv/dx) have a larger stencil than the diagonal terms and therefore introduce asymmetry into the x- and y-directions." Yes, I can see that in Figure 2. The question is, why use such a strange stencil that introduces asymmetry when a very simple square stencil with no asymmetry could be used instead? Consider a square with velocity components (u,v) defined at the corners. All four velocity derivatives can be calculated for the square without asymmetry. Why not use this very simple and obvious stencil? Doesn't the stencil of Figure 2 introduce an undesirable property into the calculations and results?
We used the C-grid stencil because it is the discretization of choice in many sea ice models (e.g., MITgcm, Losch et al., 2010; SI3, Vancoppenolle et al., 2023; SIS2, Adcroft et al., 2019); the synthetic experiments therefore mirror how the diagnostic is applied to model output on native grids. The location of the velocity vector on the C-grid is more natural for the discretization of the advection equation for mass and concentration, is more direct for coupling with C-grid ocean and atmospheric models, and better represents inertial–plastic compressive waves (Bouillon et al., 2009). For these reasons, members of the CICE Consortium decided to implement a finite-difference C-grid capability in CICE (Lemieux et al., 2024), whose discretization we follow here.
More importantly, the conclusions presented in the manuscript are robust to the choice of stencil. In the experiments of Secs. 3.1-3.3, only diagonal terms, which are identical in both B- and C- grid, are used. In the rotation experiment presented in Sec. 3.4, off-diagonal terms associated with shear are present, and while the B-grid discretization differs from that of the C-grid, the results are nearly identical (Figs. R1–R2).
Finally, the analyses of RGPS and SIREx deformations presented in Secs. 3.5–3.7 is done using line-integral of velocity gradients directly and do not depend on the choice of stencil. We have revised Sec. 2.3 (L160) to clarify this point:

We use the C-grid stencil because it is the discretization of choice in many sea ice models (e.g., MITgcm, Losch et al. 2010; SI3, Vancoppenolle et al. 2023; SIS2, Adcroft et al. 2019); the synthetic experiments therefore mirror how the diagnostic is applied to model output on native grids. Note that the choice of stencil only affects the off-diagonal (shear) terms; the diagonal terms used in Sections 3.1–3.3 are identical on the B- and C-grids. Repeating the rotation experiment of Section 3.4 on an Arakawa B-grid, where all four gradients share the same stencil, yields nearly identical results (not shown). The RGPS and SIREx deformation rates analyzed in Sections 3.5–3.7 are computed using a line-integral approach (Bouchat et al., 2022b) and are therefore independent of the stencil choice.

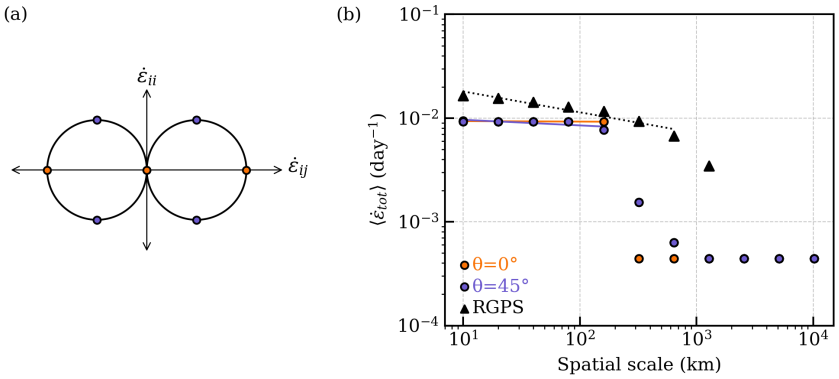


Figure R1: B grid

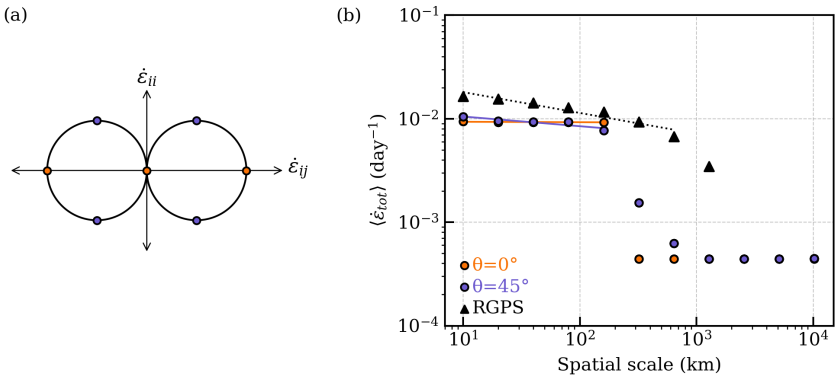


Figure R2: C grid; New Fig. 4

Section 3.4 is titled "Scaling is not invariant under rotation". The Abstract also makes that claim (lines 9-10). The quantity under consideration here is the total deformation,

which I'll call eps tot, given by equation (5). I can assure you that eps tot is invariant under a rotation of the coordinate system – that is a mathematical fact. If a particular gridding scheme of the velocity field (u,v) leads to non-invariance of eps tot under rotation, that is the fault of the gridding scheme. The authors need to make very clear that their result is due to the discrete spatial representation of (u,v) and not due to some property of eps tot. If I am mis-interpreting the authors' argument then that argument needs to be clarified.
This point is well taken. The total deformation is invariant under rotation by construction; but this invariance is lost in the coarse-graining procedure, leading to a different scaling for two identical deformation field, expressed in two different reference axes. We made this point explicit in the revised manuscript (see revised text in blue below; L221-222 and L227). The sensitivity to the stencil (B- versus C-grid) presented above also confirms that the loss of invariance comes from the alignment of the coarse-graining boxes rather than from the stencil used to compute the gradients.
Note that we found a missing $\sqrt{2}$ while carrying the sensitivity test on the B-grid for a grid rotated by 45° (Fig. 4). This does not change the conclusions of the section. The mistake was corrected and the figure updated
The difference arises because coarse-graining interacts differently with the spatial arrangement of signed velocity gradients in each configuration. While $\dot{\varepsilon}_{tot}$ itself is rotation invariant, the coarse-grained values $\langle \dot{\varepsilon}_{tot} \rangle_L$ ($L > \Delta x$) are not.
The non-invariance of scaling under rotation could have consequences for model evaluation since simulated and observed sea ice deformation are computed on different grids (e.g., tripolar, curvilinear, polar stereographic).

At lines 250-251, the authors say that the total deformation (eps tot) at the smallest scale (DELTAx) and largest scale (N) can be derived from the PDFs of the velocity gradients alone, i.e. the spatial arrangement of the velocity gradients doesn't matter. In Marsan et al. (2004), which claimed that eps tot vs. scale followed a power law, they randomly rearranged the velocity gradients and re-did the calculation of eps tot vs. scale, finding non-power-law behavior. From this they concluded that the spatial arrangement of the velocity gradients does matter. How should the results of the present study be reconciled with those of Marsan et al. (2004)?
The spatial arrangement does affect the power law behavior, for instance, whether it is log-linear or not. Under log-linearity, the scaling exponent, taken from the two endpoints, however does not depend on the spatial arrangement. Our results are consistent with Marsan et al. (2004), as the two endpoints of the scaling curve do not depend on the spatial arrangement of the gradients. $\langle \dot{\varepsilon}_{tot} \rangle_{\Delta x}$ is the mean of the grid-scale deformation rates, and $\langle \dot{\varepsilon}_{tot} \rangle_N$ is computed from the domain mean of each signed gradient. Both

are therefore unchanged when the gradients are randomly rearranged (up to some differences in masking/coarse-graining domain). What rearranging changes is how $\langle \dot{\varepsilon}_{tot} \rangle_L$ decays between these two endpoints, and hence whether this decay is log-linear, as shown by Marsan et al. (2004) and Stern and Lindsay (2009, their Fig. 4).
Moreover, this is consistent with Sec. 3.6, where the scale at which $\langle \dot{\varepsilon}_{tot} \rangle_L$ drops off reflects the dominant spacing between regions of opposite-sign velocity gradients; in a randomly rearranged field, this spacing is Δx. We have made this distinction explicit in Secs. 3.5 (L242) and 3.6 (L302) of the revised manuscript.
It follows that, when the scale dependence is approximately log-linear, differences in β between observations and models can be quantified from $\langle \dot{\varepsilon}_{tot} \rangle_{\Delta x}$ and $\langle \dot{\varepsilon}_{tot} \rangle_N$ alone, i.e. they do not depend on the spatial arrangement of the gradients (Fig. 6). The spatial arrangement of the gradients, however, does have an impact on whether the scale dependence is log-linear.
The link between the drop-off scale and the spatial organization of opposite-sign deformation also explains why randomly rearranging the velocity gradients changes the shape of the scale dependence (Marsan et al., 2004; Stern and Lindsay, 2009).

I'm wondering if the authors have simply re-discovered the Divergence Theorem and labelled it "signed-gradient cancellation". A version of the Divergence Theorem states:

Integral over a region (du/dx) dx dy = Integral around the boundary (u) dy.

The left-hand side is the average value of du/dx times the area of the region. The right-hand side is an integral around the boundary of the region. In other words, when calculating the average value of du/dx over a region, all the interior values cancel, leaving only values on the boundary. This can be seen in a square grid, for example where the vertices are labeled A to I:

A B C

D E F

G H I

Suppose u(x,y) is defined at the 9 vertices.

There are 4 grid cells: ADEB, BEFC, DGHE, and EHIF.

The segment BE is common to the first 2 grid cells, the segment DE is common to the first and third grid cells, etc. The finite difference terms of du/dx from the interior segments (such as BE) all cancel, leaving only a sum of u around the outer boundary. That looks like "signed-gradient cancellation" to me. Another way to look at it is that the integral around ADEB (counterclockwise) traces the segment EB while the integral around BEFC (also counterclockwise) traces BE, i.e. in the opposite direction, resulting in cancellation. All the interior segments cancel because they are traced in opposite directions

from adjoining cells. Anyway, I would be curious to know if the authors think that signed-gradient cancellation is something other than a consequence of the Divergence Theorem.
We thank the reviewer for this perspective, which we agree is a useful way to view the mechanism. Averaging the grid-scale velocity gradients over a coarse-graining box is equivalent to computing the gradient at the box scale from the velocities on its boundary, since interior contributions cancel pairwise. Signed-gradient cancellation is the same statement expressed in terms of gradients rather than boundary velocities. This equivalence is exact for complete boxes on a regular grid, and approximate when cells are masked or for the off-diagonal terms, whose stencil extends slightly beyond the box. We clarified this more intuitive perspective in Sec. 3.2 (L188). Just to be clear, we do not argue that this mechanism is new; the contribution of the manuscript is that this cancellation, rather than localization, controls β in practice.
Equivalently, by a discrete form of the divergence theorem, the gradient averaged over a coarse-graining box depends only on the velocities at its boundaries, since interior contributions cancel. Opposite-sign gradients within a box therefore cancel, regardless of their arrangement.

Math

Lines 115-116. The maximum shear strain rate, epsII, is not the determinant of the matrix L in equation (2). Suppose the eigenvalues of L are denoted by e1 and e2, with e1 > e2. Then epsII = e1 - e2. This is not the same as the determinant of L. The divergence epsI = e1 + e2 is the trace, which is correctly written.
The reviewer is correct; we have corrected the definitions as follows.
The divergence ($\dot{\varepsilon}_I = I_1$) and the maximum shear strain rate ($\dot{\varepsilon}_{II} = \sqrt{I_1^2 - 4I_2}$) are written in terms of the first and second invariants (I_1 and I_2) of the 2D sea ice deformation tensor ($\mathbf{L}$), which can be written as (ignoring the rotational component) [...]

Figure 1 caption, last line. The number of boxes is given as: $(N/n - 1)^2$ for n = 1, 2, ..., 10. This is not correct. The correct expression is: $(N/p - 1)^2$ where $p = 2^{(n-1)}$.
The reviewer is correct, we have revised the equation in the Figure 1 caption.
Note that the mean total deformation (colored circles) is calculated over a gradually smaller number of boxes (N_x^2 for $n = 0$, and at most $(N_x/2^{n-1} - 1)^2$ for $n \geq 1$) with increasing scales $n = 0, 1, 2, ..., 10$.

In equation (8), the denominator on the right hand side should be 4*DELTAy, not 2*DELTAy. In equation (9), the denominator on the right hand side should be 4*DELTAx,

not 2*DELTAx. If the authors used 2*DELTA in the calculations then they should re-do the calculations.
This was a typo in the manuscript; the code uses the correct denominator. Eqs. 8-9 are corrected in the revised manuiscript as suggested.

$$\frac{\partial u}{\partial y} = \frac{u_{i+1,j+1} + u_{i,j+1}}{4\Delta y} - \frac{u_{i+1,j-1} + u_{i,j-1}}{4\Delta y},$$
$$\frac{\partial v}{\partial x} = \frac{v_{i+1,j+1} + v_{i+1,j}}{4\Delta x} - \frac{v_{i-1,j+1} + v_{i-1,j}}{4\Delta x}.$$

Notation

Line 103. $L^n = 2^n$ * DELTAx. It took me a long time to realize that the "n" on the left-hand side is a superscript while the "n" on the right-hand side is an exponent. This is confusing. Why can't the "n" on the left-hand side be placed in the more usual subscript position to avoid confusion with exponents?
This is corrected as suggested by the reviewer (see corrected form in the next answer).

Equation (1). The quantity L is not defined.
L is the scale, as defined on line 103. We clarified that this definition applies to both L and L^n, in the revised manuscript. The new sentence now reads L_n (L103-105).
In the case of scaling sea ice deformation, it describes how the domain averaged deformations $\langle \dot{\varepsilon}_{tot} \rangle_{L_n}$ change with scale L, where $L_n = 2^n \Delta x$ for $n = 0, 1, 2, ..., 10$, i.e.,

$$\frac{\langle \dot{\varepsilon}_{tot} \rangle_{L_{n+1}}}{\langle \dot{\varepsilon}_{tot} \rangle_{L_n}} \propto L^{-\beta}, \tag{1}$$

for all n.

Line 144. Apparently Nx = 1024. Why does it need the subscript x?
We thank the reviewer for this remark. N is used as a scale (i.e., 10 240 km), whereas N_x is the number of grid cells (i.e., 1024). The definition of N has been specified following the reviewer's suggestion bellow (L174).)

Line 149. In the equation for u(x), the variable x is used as both the independent variable and the dummy variable of integration.
This is corrected as suggested by the reviewer.
$u(x) = \int_0^x \frac{du}{dx'} dx'$

Line 169. s = k * DELTAx for integer k, and DELTAx = 10 km. Now go four lines down to equation (10). Several problems here:

(i) The summation is over index k, but apparently the right-hand side depends on k through the definition of s.
We thank the reviewer for catching this notation error. In Eq. 10, k was intended as the summation variable and has been replaced by m to avoid conflict with other variables in the manuscript.

(ii) eps0, defined on line 171, should be a function of two variables, but it is written as a function of one variable.
We have now specified this in the revised manuscript (Eq. 10), following the reviewer's suggestion in a comment bellow.

(iii) The argument of eps0 is k*s which is apparently k*k*DELTAx. This seems very strange.
The argument of ε_0 was intended to be $m \cdot s$ rather than $k \cdot s$; the confusion arises from the double use of k noted in the previous comment. Upon reflection, we have removed the argument entirely, as it is an unnecessary source of confusion.

(iv) The upper limit of the summation is Nx*Nx/s. Note that Nx is dimensionless (value = 1024) while s has the dimension of length through DELTAx, so the upper limit of the summation is not dimensionless.
Thanks for catching this notation error. The index $s = k$ is dimensionless. The new sentence (L169-170) and Eq 10 in the revised manuscript now reads:
We first show that power-law scaling does not result from spatial localization using a domain where axial strain is localized along one-grid-cell-wide LKFs with regular spacing $s = k$ grid cells for integer k forming a discrete set of delta-like discontinuities (Fig. 3).

$$\langle \dot{\varepsilon}_{tot} \rangle_{L=\Delta x} = \frac{1}{N_x^2} \sum_{m=1}^{N_x^2/s} |\dot{\varepsilon}_0| = \frac{\dot{\varepsilon}_0}{s}. \tag{2}$$

Equation (11). Same problems as equation (10). Also, "N" on the left-hand side is not defined.
N is the largest scale. The new sentence (L174) and Eq 11 in the revised manuscript now reads:

Similarly, we write the mean total deformation at the largest scale ($N = N_x * \Delta x$) as:

$$\langle \dot{\varepsilon}_{tot} \rangle_{L=N} = \left| \frac{1}{{N_x}^2} \sum_{m=1}^{{N_x}^2/s} \dot{\varepsilon}_0 \right| = \frac{\dot{\varepsilon}_0}{s}. \tag{3}$$

Equation (12). Same problems as equation (11).
Corrected as suggested by the reviewer (Eq. 12).

$$\langle \dot{\varepsilon}_{tot} \rangle_{L=N} = \left| \frac{1}{{N_x}^2} \sum_{m=1}^{{N_x}^2/s} \dot{\varepsilon}_0 \right| = \left| \frac{1}{{N_x}^2} \sum_{m=1}^{{N_x}^2/s} (-1)^m \dot{\varepsilon}_0 \right| = 0. \tag{4}$$

Line 191. Here, $s = 2^n$. Now s is dimensionless. Is this the same s as previously introduced?
The index s is dimensionless. Its definition above was changed to be coherent.

Minor Comments

RGPS data are available "from 1997 to 2008" (line 92) but "we use data from ... 1997 and 2008" (line 99). Just to clarify: only the first and last years of the available data are used, right?
We use all available RGPS observations for every year between 1997 and 2008, whereas SIREx model outputs are only available for 1997 and 2008. This has been clarified in the revised manuscript (L99):
RGPS data are used for all winters (JFM) 1997–2008, whereas SIREx model outputs are only available for JFM 1997 and 2008.

Line 95. "model physics (EVP, VP, EAP, MEB)." These are better described as model rheologies. They should probably be spelled out somewhere (not necessarily here, it would be distracting).
Corrected as suggested by the reviewer (L94-96). The acronyms are defined here since it is the first time they are used in the manuscript.
We also use simulated deformation estimates from the Sea Ice Rheology Experiment (SIREx) models (Bouchat et al., 2022a), covering a wide range of spatial resolutions (therefore LKF density and spatial localization) and model rheologies (Elastic-Viscous-Plastic, Viscous-Plastic, Elastic-Anisotropic-Plastic, Maxwell-Elasto-Brittle).

Line 125. How is the weighting done in the "weighted average"?

The average is taken over non-masked cells only, and each box is assigned an effective length scale fL_n, where f is the fraction of valid cells in the box. This effective length is used as the box's scale in the power-law fit. For example, at $L_1 = 2\Delta x$, if one of the four cells is masked, the effective length scale is $(3/4)L_1$. This is clarified in the manuscript on L112-113 and L125-126.
To minimize biases, only boxes with at least 50% of non-masked grid cells are retained, and an effective length scale fL_n is assigned to each box, where f is the fraction of valid grid cells.
Eqs (4-5), where $\overline{\dot{\varepsilon}_{tot}}$ is the average of non-masked grid cells using the effective length scale (hereafter referred to as $\dot{\varepsilon}_{tot}$).
The sentence following (L125-126) was removed as redundant.

Line 138. What is the "weighting procedure"?
This sentence refers to the data-quality weighting procedure described in Bouchat and Tremblay (2020). The is clarified in the revised manuscript on L138. The new sentence now reads:
The data-quality weighting procedure of Bouchat and Tremblay (2020) has a much larger impact on the estimated scaling than the noise itself.

Line 125. "This correction" – what correction?
This sentence was removed following the reviewer's suggestion above (L125-127).

Lines 126-127. How could the field contain only one strain rate invariant? Is this meant to say that the second invariant equals zero?
Thanks for pointing this out. The new sentence on L126-127 now reads:
Note that if the field contains only one non-zero strain rate invariant (e.g., $\dot{\varepsilon}_I \neq 0$ and $\dot{\varepsilon}_{II} = 0$), then $\overline{\dot{\varepsilon}_{tot}} = |\overline{\dot{\varepsilon}_I}|$.

Line 142. What is meant by the "strain-rate intensity"?
We meant the magnitude of the strain rate ($|\dot{\varepsilon}|$). The wording was changed on L142 of the revised manuscript.

Figure 1. In panel (c), it's very difficult to see the "circles with black contour". Why not just use solid black circles for the mean values?
Corrected as suggested by the reviewer.
See Fig. R3.

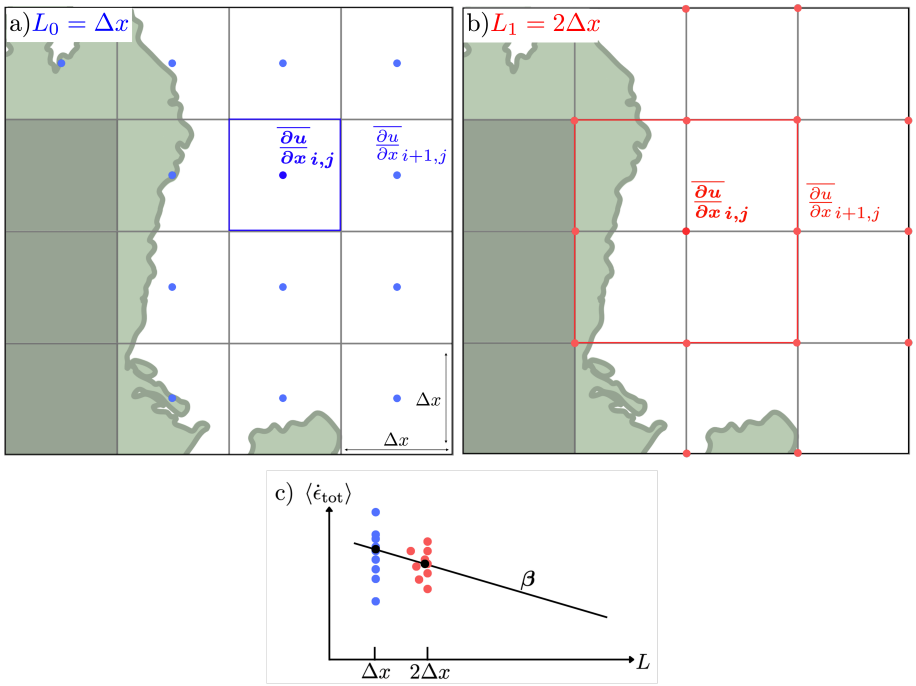


Figure R3: New Fig. 1

Figure 2 caption. The vectors (u,v) are defined on the horizontal and vertical *mid-points* of each grid box, not the vertices.
Corrected as suggested by the reviewer.

Line 171. It would be helpful to explain that |eps0| is actually eps tot for the case where u = function of x only, and v = constant, as in Figure 3.
Corrected as suggested by the reviewer (L171).
For a constant, positive-definite sea ice deformation – $\dot{\varepsilon}_0 = \sqrt{2\partial u/\partial x} > 0$, where $\dot{\varepsilon}_0$ is $\dot{\varepsilon}_{tot}$ when $u = u(x)$ and v is a constant; see Fig. 3a –, the mean total deformation $\langle \dot{\varepsilon}_{tot} \rangle$ at the smallest scale can be written as [...]

Line 243. "To test this hypothesis" – what hypothesis?
The hypothesis that differences in β in observations and models are influenced by $\langle \dot{\varepsilon}_{tot} \rangle_{\Delta x}$ and $\langle \dot{\varepsilon}_{tot} \rangle_N$ alone. The sentence on L243 now reads:
L243: To test the hypothesis that β is influenced by $\langle \dot{\varepsilon}_{tot} \rangle_{\Delta x}$ and $\langle \dot{\varepsilon}_{tot} \rangle_N$, we use the same two-point approximation of β (used in Section 3.1) calculated from the mean total deformation at the smallest (Δx) and largest (N) scales [...]

Lines 254-255. "The departure from linearity of <eps tot>N" – I'm not sure what this refers to. Linearity with respect to what?

By linearity we meant log-linearity, i.e., that $\langle \dot{\varepsilon}_{tot} \rangle$ follows a power law across scales. The largest scale $\langle \dot{\varepsilon}_{tot} \rangle_N$ typically departs from this log-linear behavior, as discussed in L239–242. The text has been corrected accordingly.
The departure from log-linearity of $\langle \dot{\varepsilon}_{tot} \rangle$ with scale introduces a positive bias.

Lines 259-260. Deformation intensity and mean are given as percentages, but eps tot is a dimensional quantity. What is the percentage based on?
The percentages are the contributions of each term in Eq. 18 to the total variance of $\tilde{\beta}$, i.e., each term divided by Var($\tilde{\beta}$), so that the three terms sum to 100% (345% + 161% - 406%). The sentence at L259-260 now reads:
Each term in Eq. 18 is expressed as a percentage of Var($\tilde{\beta}$), so that the three terms sum to 100%. The large contributions of the deformation intensity (345%, $\langle \dot{\varepsilon}_{tot} \rangle_{\Delta x}$) and the deformation mean (161%, $\langle \dot{\varepsilon}_{tot} \rangle_N$) are largely compensated by their covariance term (-406%), resulting [...]

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