Author's response to review by Nicholas Holschuh

January 12, 2025

Dear Dr. Holschuh,

Thank you for your detailed and constructive feedback on our manuscript. We appreciate your positive remarks on the quality and significance of our data and analyses, as well as your recognition of the broader implications of our findings for understanding radar scattering and ice sheet microphysical processes. It is gratifying to know that you value our work.

Your suggestions for improving the manuscript's accessibility and readability have been especially helpful. We acknowledge your critique of specific sections, including the discussion of the previously published model. We included these details with the intention of making the text more accessible and understandable for readers unfamiliar with the prior work. However, we also understand that this approach has unintended consequences, as it may lead to misinterpretations and potential errors in the literature in addition to disrupting the narrative of the paper. Thank you for raising awareness of this important aspect of scientific communication. We agree that clarity and proper attribution are essential, and we have carefully addressed your comments in the detailed, point-by-point response below.

Once again, we thank you for your valuable insights.

On behalf of all authors,

Tamara

1   Technical comments

1. Shortening the Discussion of the Forward Model (Pages 6-12)

In the revised manuscript we have removed the detailed model description and instead refer the readers directly to Fujita et al. (2006). In addition to that, we have also removed the detailed discussion on the forward model, and the comparison between forward model, turning circle and synthesized response from Section 3 to the Supplementary Material, which contains all information to reproduce Fig. 3 and Fig. 4. With these changes, Section 3 is now considerably shorter and focuses on the method of determining the orientation and strength of scattering vs. birefringence effects.

2. Paragraph Structure and Focus

We appreciate your suggestion to simplify and streamline paragraphs to make the manuscript more accessible, particularly for a broader glaciological audience. We have revised the manuscript and tried

to shorten and simplify sections which contain unnecessary details or which are repetitive, and we restructured others for a more logical line of thought. We hope that this way the revised manuscript is more accessible to a wider glaciological audience.

2 Line-Item Corrections:

- **Page 1, Line 3-8:** Something to consider – I think these three sentences (starting "Although both...") can be made more succinct and combined into one, which gets the reader to your results more quickly.

  Thanks. We have revised the text as follows **(line 3–5 in revised manuscript)**:

  We use curve-fitting techniques to evaluate the relative contributions of anisotropic scattering and birefringence in quad-polarized ground-based RES measurements from the Northeast Greenland Ice Stream (NEGIS), identifying their dominance and orientation across depths of 630–2500 m.

- Page 2, Line 23-32: As I mentioned in the technical comments, I think it makes more sense to pull these sentences out of your first paragraph (which is meant to provide the highest level introduction and motivation) and move them to after line 67. That first paragraph could naturally end with the sentences spanning 32-34, and I think it will improve the flow of the introduction.

  We have removed these details here and moved the overview of previous polarimetric observations further down the section, line 62–73 in revised manuscript.

- **Page 3, Line 56:** I think "affecting the overall return power" is a slightly confusing addition to this sentence. It is clearer if it reads "Volume scattering is caused by small-scale inhomogeneities in the physical properties of the glacier, such as air bubbles, dust particles, or impurities."

  We have removed "affecting the overall return power" and streamlined the section for clarity. The revised text now reads (line 40–45):

  Anisotropic scattering describes the directional dependence of ice's scattering properties, which causes variations in signal intensity based on the orientation of the antenna. Scattering of radio waves in ice sheets arises from two main mechanisms: volume scattering, driven by small-scale inhomogeneities within the ice, and surface scattering, caused by reflections at internal interfaces (e.g., Langley et al., 2009; Drews et al., 2012). Volume scattering includes contributions from air bubbles, dust, impurities, and anisotropic features such as elongated air bubbles or small-scale fluctuations in horizontal permittivities related to COF (Drews et al., 2012).

- **Page 3, Line 58:** The phrase "also observed in the optical range" assumes that the reader knows that you've been thinking about scattering in the microwave range up to this point, which isn't explicitly stated (at least in this paragraph). Might be useful to rephrase to something like "Anisotopric volume scattering is significant in the optical range..." or something like that, and then make clear in the next sentence that you are talking about RES data: "Surface scattering of radio waves occurs..."

  We have removed the reference to the optical range, as it is not relevant to this paper. Additionally, we clarified at the start of the paragraph that we are focusing exclusively on radio waves (line 40–42):

  *Scattering of **radio waves in ice sheets** arises from two main mechanisms: volume scattering, driven*

by small-scale inhomogeneities within the ice, and surface scattering, caused by reflections at internal interfaces (e.g., Langley et al., 2009; Drews et al., 2012).

- **Page 3, Line 69-72:** These two sentences present another example of content that feels unrelated to the larger paragraph, and belongs somewhere else in the paper. If I were to summarize the purpose of this paragraph, it is to introduce the limitations of previous radar approaches to estimating ice fabric. But the sentences stating "The COF type" and "In-situ measurements" don't contribute to this idea at all. Without realizing it, these seeingly minor narrative culs-de-sac really increase the difficulty in reading and understanding a work, so minimizing them really helps me as a reader.

  We have removed these two sentences from this paragraph to improve focus and readability.

- **Page 4, Line 98-124:** Thanks!

- **Page 6, Line 132:** For the reasons I described in the general comments, I think lines 132-184 and 199- 207 should be removed in favor of a clear set of references to the original equations in Fujita et al., 2000, 2006, and Ershadi et al., 2022. I think you want to use this space to emphasize to the reader why, exactly, you did this analysis, and be as brief as possible. Something like "Here we show that synthesis from quadpol data faithfully reproduces the results of the turning circle, and both datasets capture the primary features we would expect based on forward-modeling the radio-wave propagation problem using direct fabric measurements collected from the EGRIP core." Much of the rest of the description of the forward model results (lines 215-237) doesn't feel clearly situated in the narrative. The reader doesn't know yet why they should care about any of the things you observe. I would cut them or move them to later in the document when they become relevant for interpreting the sources of scattering.

  We have significantly shortened Section 3 to improve focus and narrative flow. Specifically, we have:

  1. Removed the detailed description of the used model.

  2. Relocated Fig. 3 and Fig. 4 to the Supplementary Information, along with the necessary details to reproduce the model results.

  3. Condensed the text to focus on the key explanation of the curve-fitting method, illustrated with the modeled, turning circle, and synthesized response.

  These changes streamline the section, focusing on why the analysis was conducted, while less immediately relevant details are provided later in the manuscript or in the Supplementary Information.

- **Page 9, Line 181:** While I think this section should be cut entirely, if you keep this text, this sentence is missing a concluding clause.

  This section has been removed.

- **Page 14, Line 261-264:** Why do you estimate the eigenvalue difference from travel-time differences rather than integrated phase difference from your synthesized coherence phase, using the orientation where you know you are aligned with the COF? That would let you account for lack of alignment of your driving direction and the COF. It would at least be interesting to compare those results.

We chose to estimate the eigenvalue difference using travel-time differences instead of the integrated phase difference because of the challenges presented by strong anisotropy in our study area. The coherence method, which works well in areas with low anisotropy (as shown in Jordan et al., 2022; Young et al., 2021), becomes problematic in ice streams with strong anisotropy where scatterer depth differences between polarization directions exceed the radar's range resolution, leading to loss of coherence. For our ultra-wideband radar system, the coherence method can in theory only be applied above 350 m depth, where the scatterer-depth difference is within range resolution (Zeising et al., 2024). As recently suggested by Zeising et al. (2024), reducing bandwidth through zero-padding could perhaps extend the depth at which the coherence method can be applied for the data presented here, but requires data re-processing and would considerably impact signal strength.

Given these challenges, we decided that using travel-time differences was the most effective approach for providing context to the COF orientation derived from anisotropic scattering, while also showing the limitations of that method. We have clarified this reasoning in the revised manuscript (line 187–192):

The integrated phase difference can in theory be used to derive horizontal eigenvalue differences, a method which works reliably in low-anisotropy areas (e.g. Jordan et al., 2022; Young et al., 2021). However, for strong COF anisotropy the depth differences between reflections of opposite polarization directions exceeds the radar's range resolution, leading to loss of coherence (Zeising et al., 2024). Although the phase coherence method could not be applied successfully to our dataset for deriving COF eigenvalues, reprocessing the data to reduce radar bandwidth might improve its applicability in future efforts, albeit at the cost of signal strength (Zeising et al., 2024).

- **Page 15, Fig 8:** You don't present any of the radargrams crossing the eastern shear margin, but there seems to be a lot of variability in the eigenvalue differences presented (I assume, associated with the folding there?). It would be worth interpreting for the reader those signals, because they appear really coherent across the flow-perpendicular lines. Is it that the system has limited capacity to capture the folding because of its narrow beam pattern, and so those are artifacts? I didn't see any mention of focusing in your processing steps either – how might that affect interpratation of those signals? I can't tell if I should think of them as interesting signal or artifactual in nature, a result of the limitations of the system, and it would be nice to clarify that somewhere for the reader.

Thank you for this detailed observation. Indeed, the observed changes in anisotropy appear to align with folds mapped by Jansen et al. (2024) (now added to the figure background). To clarify the nature of these signals, we have re-evaluated the calculation of the eigenvalue difference ($\Delta\lambda$) to ensure that it reflects real features rather than processing artifacts.

In the original analysis, eigenvalue differences were derived by automating the process to cross-correlate traces in a sliding window, identifying the lag with maximum alignment. $\Delta\lambda$ was then calculated only for correlations exceeding 0.6. However, in regions with steeply inclined layers, reflections are sparse, and high correlations can occasionally result from noise rather than true signals. Similarly, if there are too few reflections or if the reflections are too shallow, the eigenvalue difference becomes difficult to estimate accurately, leading to potential over- or underestimation of anisotropy.

To address these limitations, we have refined the criteria for deriving $\Delta\lambda$:

1. Correlations must exceed 0.6, and reflections must have amplitudes above the noise floor, as

derived from areas near the shear margins where no reflections are visible.

2. Eigenvalue differences are now calculated only when there are at least 10 reflections, with at least one extending deeper than 1200 m.

These stricter criteria improve the reliability of the results and reduce the influence of noise or insufficient reflections. The eigenvalue differences in near the shear margin are not considered very reliable with this new criteria and their variability an artifact of the strong folding in that area. We have clarified these points in line 194–201 of the revised manuscript:

The eigenvalue difference was determined using an automated process that measures the travel-time difference between the HH and VV traces. Specifically, the cross-correlation of each trace pair was calculated within a 20 m sliding window to estimate the time delay between signals. Linear regression was then applied to correlated reflections to obtain the depth-averaged apparent eigenvalue difference (for method details see Gerber et al., 2023). The uncertainty of this method increases when only shallow reflections are available or when the number of reflections is low. To ensure reliability, we included only results where at least ten internal reflections could be correlated with a correlation coefficient above 0.6, and where at least one reflection lies below 1200 m depth. Results were discarded where these criteria were not met, particularly in areas with steeply dipping internal layers near shear margins.

For completeness, we also added radargrams of the remaining lines as HH-VV power difference to the Supplementary Information.

- **Page 15, Fig 8:** I'm also interested in that really intense eigenvalue difference found between A-2 and A-3 at the southern end of your survey. Is that real? That looks like (in Figure 9) a region where there is very low signal.

Eigenvalue differences near the shear margin are indeed expected to increase due to the dynamic nature of the ice in this region, as shown by models and shallow ice cores (Gerber et al., 2023). However, as you correctly noted, the eigenvalue difference observed between A-2 and A-3 at the southern end of the survey is based on only a few relatively shallow reflections. This makes it less reliable compared to areas where more and deeper reflections are available.

As mentioned in our response to your previous comment, we have tightened the criteria for confidently deriving eigenvalue differences. Under these stricter criteria, this region does not meet the necessary confidence threshold. Consequently, this part has been removed in the updated figure to ensure the presented data reflect only robust results.

- **Page 16, Line 271-272:** How should I think about "flow parallel" vs. "flow perpendicular" when thinking about anisotropic scattering? Is it fair to just thinkg about it as "the principle fabric axes are shifted 15 degrees clockwise(?) from the modern flow field"? I spent a long time trying to figure out if I should care that it is perpendicular rather than parallel, and I realized I wasn't exactly sure what that distinction meant.

Your interpretation is correct: the principal fabric axes are shifted approximately 5–15° clockwise from the modern surface flow field, which has previously been assumed to be aligned based on reconstructed orientation of the EastGRIP ice core (Westhoff et al., 2021).

The terms "flow-parallel" vs "flow-perpendicular" are not essential for understanding this concept since eigenvectors are per definition perpendicular to each other. We have removed "flow-perpendicular" to avoid potential confusion and here simply modified the text as follows (line 202–203):

The scattering direction in the ice stream center is oriented between 95° and 105° clockwise from the surface flow direction at depths of 1000 m–1500 m where scattering is strongest.

Please also note that we corrected 75–85° to 95-105° clockwise rotation (75-85° would be anticlockwise).

To clearly distinguish between observations and interpretation, we moved the description of the implications of this observation to Section 5.3 (line 369–380):

The direction of anisotropic scattering can give independent indication of the COF orientation when other scattering origins than COF can be ruled out. The 180° periodic signal in the ice-stream center is rotated clockwise from the flow direction by 95–105°, suggesting that the eigenvectors are not perfectly aligned with the surface flow as has been commonly assumed (Westhoff et al., 2021; Gerber et al., 2023), a conclusion which was also reached by Nymand et al. (2024) using double reflections to derive COF orientation from the same dataset as this study. A notable rotation of the scattering direction is also shown in the second half of profile A (55–100 km in Fig. 4, Fig. 6), located in the vicinity of the shear margin, where the scattering axes are rotated 20–70° clockwise, with a tendency towards stronger rotation at larger depths. Here again, Nymand et al. (2024) found similar results. The COF rotation here explains the apparent decrease in horizontal anisotropy derived from travel-time differences, which does not represent the true anisotropy in case of misalignment of COF axis and radar wave polarization. The agreement between scattering COF orientation derived by Nymand et al. (2024) further supports our conclusion that scattering can be attributed to COF orientation, which has the advantage of being independent of depth and strength of anisotropy, and observable at any orientation of the quad-polarized measurement.

- **Page 16, Line 293-294:** I find this phrasing a bit confusing – without echoes, how do you know it is isotropic? A bit more detail on your thinking here would be helpful.

  Thank you for pointing this out. In this context, we were using the term "isotropic" to indicate the absence of anisotropy. However, we understand that this phrasing can be confusing. To avoid misunderstanding, we have removed the term "isotropic scattering" here and similar contexts elsewhere.

- **Page 17, Fig 9:** I think it would be helpful for the reader if you highlight and describe those features due to birefringence seen from 50-100 km in the shallow part of Profile A and from 20-40 in the shallow part of Profile B. I think those signals are really interesting and represent what most folks had been looking for in co-polarized data up to this point, so it is useful to highlight and describe them here.

  We added further description of these beat signatures in line 221–227 of the revised manuscript:

  Sections A3–A4, B2–B3 and B5–B6 show alternating signatures of positive and negative power difference, particularly pronounced in Holocene ice, which is a result of birefringence-induced beat signatures: Birefringence causes a rotation of the polarization ellipsoid in and out of the profile plane, so the power alternates between being higher parallel (VV) and perpendicular (HH) to the profile direction. These birefringence-induced beat signatures are indicative of the misalignment of radar antennas and COF

principal axes. The fact that strong beat signatures are mostly visible at shallow depths might be due to the loss of coherence at larger depths.

- **Page 18, Section 5.1:** This section justifies one of the major conclusions of the paper, but sections 5.1.1/2/3 would benefit from a slight reorganization and additional paragraph breaks. Right now, these sections have paragraphs with 10-15 sentences which tend to wander back and forth between ideas. The cleaner the structure you can provide for the reader, the better. I was going to suggest some specific paragraph breaks, but I think sentences need to get moved around in a way that makes the ideas more clearly separable before you subdivide.

We have tried to reorganize this section for more clarity. The line of thoughts now is as follows:

air bubbles: 1) background of bubble formation and distribution in the Greenland ice sheet, 2) how bubbles elongate and how they are expected to be oriented in and around NEGIS, 3) how this might affect radar return power, which 4) is not consistent with our observations and that's why we reject this as the main cause for anisotropic scattering.

layer roughness: 1) observation and expected orientation of layer roughness in the NEGIS, 2) how it potentially affects scattering, 3) why it is not consistent with our observations.

COF fluctuations: Having ruled out the above mechanisms we explain in this section how we imagine COF fluctuations causing the observed scattering with an additional illustration (a suggestion by reviewer 2) including two example scenarios: 1) inside the NEGIS, where COF is known from the EastGRIP ice core and 2) in the folded units north of NEGIS.

- **Page 19, Line 360-361:** Can you substantiate this statement with a bit more detail? I found this statement counterintuitive, and was having trouble understanding why the opposite wouldn't be true.

Thanks for being critical here. This statement was supported by the referenced studies. However, as reviewer 2 pointed out, those are based on radars with considerably larger beamwidth, and other studies have suggested the opposite for subglacial cylindrical channels. So it actually is not exactly clear how anisotropic roughness would affect the data from our system. We have clarified this in the text (lines 275–294 of the revised manuscript) but ultimately rule out interface roughness as the primary cause of anisotropic scattering for the reasons provided:

The effect of directional interface roughness on radar return power is complex. Interface roughness can transition radar signals from specular reflection to more diffuse scattering and wave depolarization when roughness amplitudes are comparable to the radar wavelength (Peters et al., 2005; Giannopoulos and Diamanti, 2008). Studies with side-looking radars have shown that higher backscatter occurs perpendicular to the folding axis, as folds act as corner reflectors (Bateson and Woodhouse, 2004; Bartalis et al., 2006). However, for a nadir-looking radar system with a much narrower beamwidth, this anisotropic scattering mechanism may not operate in the same way. Instead, stronger co-polarized scattering might occur parallel to the folding axis, depending on fold size and radar characteristics (Scanlan et al., 2022).

Despite the unclear relationship between folds and anisotropic scattering, we can rule out directional interface roughness as the major source of anisotropic scattering for the following reasons: First, if directional interface roughness results from ice dynamics, particularly lateral strain, we would expect

*layers outside the ice stream to be smoother, with less pronounced anisotropic scattering. Indeed, the scattering amplitude is generally slightly higher inside the ice stream than outside (Fig.**??**). However, this pattern is not consistent. For example, scattering amplitudes outside NEGIS in profile B exceed the amplitudes in the ice-stream interior particularly downstream of EastGRIP and in profiles which are not in the ice-stream center (Fig.**??**a–c). Although roughness outside the current ice stream might be remnants of previous ice-dynamic configurations, the spacial distribution of scattering amplitudes is difficult to be explained by roughness alone, particularly the lower amplitudes towards ice-stream margins where folding amplitudes are known to increase (Jansen et al., 2024). Second, while scattering differences between ice from different climate periods could stem from variations in folding amplitudes associated with viscosity differences, the reversed directionality of anisotropic scattering between Holocene and Wisconsin ice north of the NW shear margin would imply an exceptionally distinct strain history between these ice units if attributed to ice-flow-induced interface roughness, which is unrealistic.*

- **Page 20, Line 389-390:** These two sentences both start with "additionally". I would restructure this paragraph in general (noting my comment on section 5.1), but try to avoid that kind of repitition.

  Thanks, we restructured this part, and checked for repetitions similar to this.

- **Page 21, Line 407-415:** This paragraph is really important context for the reader to have, and because it is not a discovery of this paper, I think it should come much earlier in the paper. It might be most appropriate when you introduce the forward model and the 90/180 degree periodicity. Highlighting that the two methods for azimuthal variability have distinct implications for fabric before the reader spends a long time obeserving the differences in fabric would be very helpful.

  Thanks for this suggestion. We moved it to the introduction to provide the context as early as possible, line 52–61 of the revised manuscript.

- **Page 21, Line 416-417:** Again, I find this phrasing odd – "echo free zone characterized by . . . scattering". What exactly do you mean here?

  Agreed. We have changed it to simply 'echo-free zone'.

- **Page 22, Line 439-444:** I'm not sure the conclusion you're drawing here (that because the Eemian ice at NEEM was overturned the signal in the polarimetric data here indicates local ice is overturned) is correct. My mental model of how you get changes in sign of the HH- VV power difference is the following:
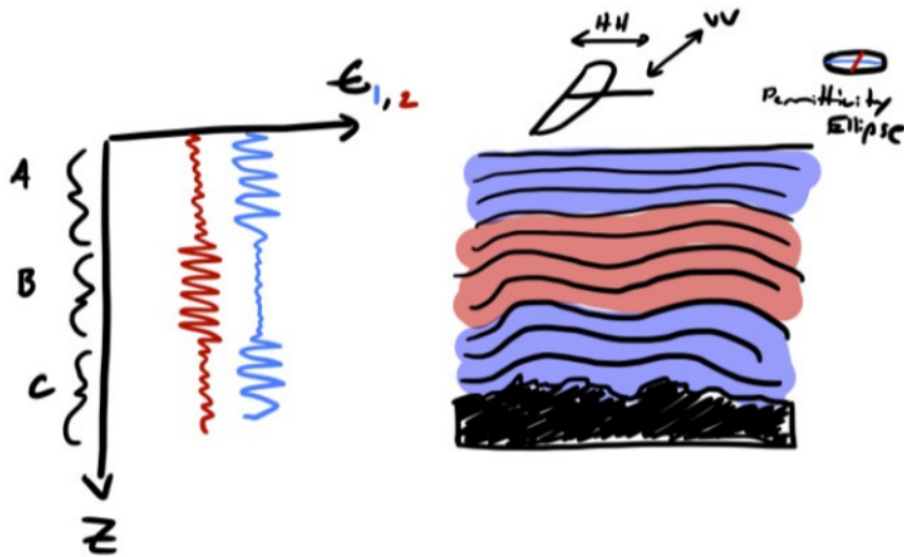
When you have greater variability in the permittivity associated with one polarization than another, you have greater scattering, and therefore a higher power. But imagine you took a section of B and inverted it. You wouldn't then produce a change in the sign of the HH-VV difference; that would actually require a 90 degree rotation of ice in B. Is that consistent with your mental model, or am I missing something? I would say either explain in more detail how inversion would manifest in anisotropic scattering or remove this section here (and the reference to it in the abstract).

Thank you for this comment. To clarify, we do not suggest that the anisotropic scattering is reversed *because* of overturned ice, but rather that the observed change in the scattering properties is indicative of different ice units, which could be ice formed under conditions similar to the Holocene (e.g., Eemian ice). If that is the case, it follows that the stratigraphy must be disrupted (and perhaps overturned) because Eemian ice is significantly older than the traced isochrones. As you correctly point out, an inversion of ice strata in section B would not lead to a change in the sign of the HH-VV difference and this is not what we wanted to indicate.

We have now clarified this point in the manuscript by more clearly distinguishing the change in scattering properties as an indicator of a different ice unit, rather than attributing it directly to the effect of folded ice (line 344–351 in revised manuscript):

In Fig. 8, we proposed a potential mechanism for the reversed scattering pattern, though we do not claim to fully explain the formation of these COF differences. Ice from colder periods, like the Wisconsin, tends to have higher impurity content and smaller crystals, promoting easier deformation compared to ice from warmer periods like the Holocene and Eemian (Paterson, 1991; Cuffey et al., 2000; Faria et al., 2014a,b). The folding of ice itself does not inherently produce a 90° rotation of COF needed to invert the scattering signature. However, changes in the regional ice dynamics could have altered the local strain regime to which the COF adjusts accordingly. The rate and manner of this adjustment may differ between ice units, with Wisconsin ice, having generally higher impurity content and smaller grains,

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potentially adjusting more rapidly or distinctly than Holocene ice, which could explain the observed scattering differences.

- **Page 23, Line 481:** "proved" rather than "proofed"

done.

- **Page 23, Line 489:** At present, you don't fully explain why this is the case for the shear margin – do you think it is because the fabric variability is orientation independent and therefore anisotropy in scattering is weak, or because the effects of birefringence are particularly strong? A bit more discussion of this somehwere in text would be helpful

Our current interpretation is that the shear margins exhibit limited anisotropic scattering due to the steeply inclined layers, which result in reduced radar return power overall. Although birefringence is a contributing factor, we find that anisotropic scattering is still more significant than birefringence in the shear margin, except at shallow depths.

We have added a short paragraph in line 381–385 of the revised manuscript:

It is worth noting that care should be taken in areas of strongly folded internal stratigraphy, in our study area particularly in the vicinity of the shear margins. Here, the overall return power is decreased because of steep internal layers. Hence the smaller scattering amplitudes do not necessarily imply smaller COF fluctuations with depth, but may simply reflect the overall decreased return power. Although birefringence effects are mostly found to be dominant near the shear margins and at relatively shallow depths, anisotropic scattering still dominates in most of the analyzed cases.

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