

## Comment on egusphere-2025-3870 (referee #1)

*We would like to thank reviewer 1 kindly for taking the time to read our manuscript and for the constructive feedback, which helped us to improve the study. In the following, we addressed all comments point by point and point out respective corrections. Please find our answers in blue italic font.*

Review of

Impact of spatial resolution on large-scale ice cover modelling of mountainous regions, Werner et al.

v2

It's great to see that the authors have addressed all of the outstanding issues raised during the first round of review. This is now a really solid contribution to the literature. I read through again and found no real major issues to comment on, though there are a number of minor comments. The only broader comment to make is that, while much improved, the text does still read a bit long. If the authors feel inclined to do so I would suggest carefully reviewing some of the results and discussion in particular to see if sentences can be removed, shortened, merged, or otherwise tightened.

*Thank you very much for this assessment and the valuable comments below, which we hope we have addressed in our modifications. In line with comments on lines 224, 317, 386, we slightly shortened some text passages by merging sentences, omitting fill words, and focusing on main points. Additionally, we tried to address all comments without adding too many words.*

Figures:

Fig. 3 - is this hysteresis? To me hysteresis is a future path dependent on its history, which all panels show.

*Yes, we agree and removed the second figure title.*

Fig. 4 - nice figure!

*Thank you very much.*

Fig. 6 - again, nice figure, though same issue with 1000. Quite unexpected that you get these 'rugged' surface features?? Even with low resolution I really wouldn't anticipate finding this -- do you think it could be a result of IGM rather than Stokes?. You have space to include a key for the red arrow and green stars in the figure, too, to save the reader flipping to the caption

*Thank you very much, we added a key for the red arrow and green star to the figure. In line with your comment on line 450, we updated the corresponding sentences in the discussion (lines 444-448). We would like to point out that these artefacts are not only observed in IGM-simulations, but also found in simulations using PISM (Jouvet et al., 2023; Seguinot et al., 2018). We clarified this now (see lines 446-448 and response to comment on line 450).*

Minor comments:

14 - I think the fact that your ice-field is the alps is very much worth including in the abstract

*Thank you for this comment, we added that information: “Here, we examine the influence of spatial resolution by modelling a growing and retreating **exemplary ice field in the European Alps** at resolutions ranging from 50 m to 2 km using the Instructed Glacier Model (IGM).”*

16 - to 'model resolutions up to 300 m are comparable...'?

*Thank you for this suggestion. We would like to focus on the comparison between the model results at different resolutions and not on the comparisons of resolutions themselves. This might be a linguistic detail which we do not apply consistently throughout the rest of the manuscript. However, in the abstract, we would like to keep this distinction.*

20 - 'rate of temperature forcing' is not super clear, nor is the rest of this sentence really -- could be simply omitted ?

*We changed the sentence to: “This hysteresis between climate forcing and glacial response at coarse resolutions is only partially decreased **with slower temperature change.**”*

36 - 'shown to influence mass loss from simulations of continental-scale ice sheets'?

*We agree and changed the sentence to: “The horizontal resolution of numerical ice models has been shown to influence **mass loss in simulations of continental-scale ice sheet** and estimated sea-level contributions...”*

39 - it bothers the pedant within me that the studies in this string of sentences are not in chronological order (and in fact, neither are the references on l38! Please have some kind of system for this throughout, probably date then author)

*Thank you for this comment. On the [cryosphere website](#), we found the following information regarding the order of citations:*

*In terms of in-text citations, the order can be based on relevance, as well as chronological or alphabetical listing, depending on the author's preference.*

*We decided to order the citations based on relevance. In this sentence (lines 37-39), we cite Cuzzone et al. (2019) first, because we believe they most clearly demonstrate the link between resolution differences and representation of bedrock topography. In their analysis, they distinguish between two regions in Greenland with low and high bed complexities, respectively, and find correspondingly small and large differences between resolutions. The other references also consider the ability to resolve bedrock topography as an explanation for resolution effects; however, their analysis is less focused on this link.*

43 - 'in agreement' to 'in best agreement', as we will never get it perfectly correct

*Yes, we changed the sentence accordingly: “...Aschwanden et al. (2016) find that a resolution of ~600 m is required to model most Greenland outlet glaciers in **best** agreement with observational data.”*

46 - the Alpine Ice Sheet would be considered an ice sheet today. It's large enough, but it still has the steeper slopes that we don't associate so much with Earth's two extant ice sheets.

*In the manuscript, we distinguish between ice sheets, which are characterized by the formation of a central ice dome with outward radial ice flow, and mountain glaciers and ice fields, that are substantially controlled by bedrock topography (Benn and Evans, 2014). This distinction is common in the literature (e.g., Gardner et al., 2013; Hugonnet et al., 2021; Zemp et al., 2019, 2025; Intergovernmental Panel on Climate Change (IPCC), 2023). In the European Alps, some ice domes existed, but most of the ice field, including its interior, was heavily influenced and directed by the underlying topography (e.g., Leger et al., 2025). The presence of protruding nunataks all the way towards the centre of the ice mass, evidenced by trimline data (e.g., Leger et al., 2025; Kamleitner et al., 2022; Wirsig et al., 2016; Mey et al., 2016), further demonstrates the ice field's characteristics. The glacier system in the European Alps during the LGM has been often referred to as the “Alpine Icefield” in recent publications (e.g., Imhof et al., 2019; Jouvett et al., 2023; Leger et al., 2025).*

*Today, the glaciated area in the European Alps is much smaller than during the LGM and the steep mountain glaciers are remnants of the LGM-ice field. Comparing glaciated areas, prominent examples for modern-day ice fields, including the Juneau Icefield (e.g., Davies et al., 2022) or Southern Patagonian Ice field (e.g., Meier et al., 2018), are larger in area than the present-day glaciated area in the European Alps (Cook et al., 2023; Millan et al., 2022).*

*Therefore, in this study we distinguish between ice sheets and ice fields, that are substantially controlled by bedrock topography, whereby we regard the European Alps glacier system (present-day and during LGM) as part of the latter.*

89 - I appreciate the intention of this sentence, but it feels like the 'ought' is bearing a lot of weight. I think what you're trying to get across is that errors are likely similar with changing resolution under full-Stokes and Blatter-Pattyn? Any way to adjust this to keep the meaning, while making it a bit more thorough?

*Thank you for pointing this out. We changed the text passage to: “IGM enables us to overcome this computational bottleneck by using deep learning based on the higher-order **Blatter-Pattyn model (Blatter, 1995)**. Although deviations between a full Stokes ice physics implementation and IGM are expected, our focus in this study is on the impact of spatial resolution on bedrock topography and ice model results, **which we expect to be on the same order across different mechanical models.**”*

108 - 'extensive ice sheet'?

*In line with the comment on line 46, we consider the glacial system in the European Alps as well as our regional model domain an **ice field** rather than an ice sheet, and applied no changes to this sentence.*

118 - can you add the exponent you use, and if hydrology is included ?

*We added the Weertman exponent value to the sentence: “Ice flow at the ice-bedrock boundary is described by a non-linear Weertman friction condition (e.g., Schoof and Hewitt, 2013) **with an exponent of four.**” In general, we refer to the supplementary table S1 for all model parameters.*

*The hydrology components are described in the next paragraph (lines 131-140) on the enthalpy module, where we clarified the control of ice temperature on ice flow via the Arrhenius factor by changing the sentence structure: “The enthalpy module follows an energy-conservative formulation by Aschwanden et al. (2012). It distinguishes between cold and temperate ice: For cold ice at temperatures below the pressure-melting point, enthalpy is proportional to pressure-adjusted temperature, while for temperate ice, enthalpy includes an additional component incorporating the creation of water content through energy transfer. **The Arrhenius factor in the Blatter-Pattyn model for ice flow depends on the pressure-adjusted ice temperature and is calculated via the Glen-Paterson-Budd-Lliboutry-Duval law (Cuffey and Paterson, 2010).** The sliding coefficient follows the Mohr-Coulomb sliding law (Cuffey and Paterson, 2010) and accounts for basalt melt and meltwater production when the pressure melting point is reached.”*

125 - why were they performed more often? Also, what time steps are used, or is this the retraining?

*Generally, a higher retraining frequency ensures that the deviation of the emulator from the analytical solver remains small (see lines 126-128). The retraining for the highest-resolution simulations at 50 and 100 m was performed more often to ensure convergence with the Blatter-Pattyn model. At these fine resolutions, bedrock slopes and velocities are higher than at coarser resolutions, leading to more complex longitudinal stresses and lateral shear margins on ice flow. Therefore, the emulator needs more training to resolve the more complex topography at finer resolutions properly. We added this aspect to the updated manuscript (see below).*

*Please note that the time step in IGM is adaptive and not static, so it changes over the course of the model runs. The fraction of model years in this sentence refers to the retraining frequency, as indicated in the previous sentence (line 123): “For adjustments to new ice field states attained through run time, we retrained the physics-informed neural network multiple times within a single model year (every  $\sim 0.18$  years). The on-the-fly retraining in the highest-resolution simulations at 50 and 100 m was performed more often (every  $\sim 0.05$  years) **to resolve the more complex topography with its steeper slopes at finer resolutions (Fig. 1).** The retraining frequencies were found to be high enough to ensure that the emulator’s deviation from the analytical solver remains below  $5 \text{ m yr}^{-1}$  in ice velocity for test cases on the Aletsch and LGM-Rhône glaciers (see Figs. 4-7 in Juvet and Cordonnier, 2023).”*

139 - a citation would be useful here, particularly as we can actually measure GHF now that the Alps are largely ice free

*We added the references to this sentence: “At the glacier bed, boundary conditions depend on the temperature at the bottom ice layer and bed surface, where we set the geothermal heat flux to 0.065 W m<sup>-2</sup> across the entire model domain, **which is in the range of reported values in the European Alps (Goutorbe et al., 2011).**”*

185 - suggest putting this as m.w.e.

*We adopted this suggestion and changed the unit for precipitation from mm to m w.e. throughout the manuscript.*

210 - I'm not quite following this sentence

*We changed the structure of the sentence and combined it with part of the next sentence to give a concrete example, which should add clarity: “**Elevation differences between** variously resampled DEMs and the reference DEM at 50 m resolution vary with Strahler order, **with the highest discrepancies at intermediate Strahler orders (3–4) (Fig. 2).**”*

215 - 'median difference values' to 'median elevation difference values'

*We changed “median difference values” to “median elevation difference values” in line 213.*

216 - 'indicating' to 'resulting from'. As you're controlling all variables here I think you can be more direct about the causation

*We adopted this suggestion: “Elevation differences in large valleys of high streams orders (6–7) are small and strictly positive, **resulting from** resampling-induced elevated valley floors.”*

224 (Section 3.2) - this is reading much better now, but I expect you could still go from three paragraphs to two here

*We reduced the word count from 603 to 582 (~ 4 %) in section 3.2 by merging sentences and omitting fill words. We refrain from reducing the number of paragraphs in this section. Although it would be possible to merge paragraphs 1 and 2, we think that the clear distinction between paragraph 1, which describes the strong increase in ice volume with negligible increase in ice area with resolution, and the hysteresis effects in paragraph 2, helps the reader to maintain focus and understand the individual points better.*

237 - I wanted to hold off on this, but I think you should be putting a comma in all 4-digit numbers. <https://service-manual.ons.gov.uk/content/numbers/writing-numbers>

*Thank you for this comment. Please note that we were not able to find a convention for 4-digit numbers on the [cryosphere submission website](#). Going through recently published papers highlighted as [Editor's choice](#), we found different ways of handling those numbers, including no comma or space separation in 4-digit numbers equivalent to our notation (e.g., Åkesson et al., 2025; Dutrievoz et al., 2026; Law et al., 2026).*

*We believe that including a comma in a 4-digit number might decrease readability, especially for lists of numbers. For example, the list of elevation bands would read as 0–1,000, 1,2000–2,4000, >2,4000 m a.s.l., and the list of resolutions shown in Fig. 4 read as 50, 500, 1,000 m, which seems a bit clumsy.*

*On the other hand, we agree that comma separation can increase readability and continued to use this notation for numbers with five or more digits.*

238 - In these shorter results sentences, it would still be useful to put in a figure reference, even if that's a subpanel i.e. Fig. 3a

*We added the reference to the corresponding figure subpanel: “Larger and persistent volume differences between the warming (3000–5000 years) and cooling phases (1000–3000 years) in the coarse resolution simulations occur for temperature forcings stronger than -3 K and thus during large parts of the model run (years ~1750–4250) (Fig. 3c).”*

240 - a slightly difficult sentence. It will capture it more accurately because it's higher resolution, so I would word this the other way around, i.e. quicker response to warming is more accurate because this is what the high resolution runs show

*Thank you for this suggestion, we changed the structure of the sentence accordingly: “Overall, we find that fine resolutions capture changes in temperature forcing more accurately, **indicated by** shorter delays in reaching the maximum ice volume as well as smaller volume differences between the warming and cooling phases.”*

253 - At around

*We agree and adopted this suggestion: “At around 2000–2500 years, thick glaciers begin to form in large valleys...”*

268 - perhaps I'm a little tired, but I had to read this opening sentence a few times to get with the program

*We agree that this sentence might be hard to follow and changed its structure and added a reference to Fig. 4: “The ice volume **deviations that depend on resolution and altitude (Figs. 3, 4)** arise from spatially non-uniform ice thickness patterns.”*

279 - I'm not really following the last sentence here

*We modified the sentence to add clarity: “This indicates that, **considering each individual pixel, ice thickness differences do not increase monotonically between 50 and 2000 m resolution.**”*

307 - 'thicker glacier' doesn't read super well

*Thank you for this comment, we changed the sentence to: “At ~1500 m a.s.l., **increased ice thickness and area** lead to over 160 % more ice volume at resolutions coarser than 800 m compared to the 50 m run.”*

317 - As with Section 3.2 I suggest making further cuts on this and focussing on the points you feel are most relevant to the reader, as a lot of the nuance here is captured already in the figures

(which is good, that's what figures should do)

*We omitted two sentences related to ice thickness which contained too detailed trends and numbers. Thereby we reduced the word count in the paragraphs related to Fig. 8 from 471 to 383 (~19 %).*

329 - This paragraph useful though ! I also think the changing surface roughness seen in Fig. 6 is really worth mentioning

*Thank you for this suggestion. We refrain from adding a reference to Fig. 6 in this paragraph, as we see no close connection between lower mean ice velocities and the rugged ice surfaces at coarse resolution, and doing so would increase the word count.*

351 - K/kyrs to K/kyr. This sentence not super clear. 'coarser resolution results depend more'... what variable changes? Why the dependence?

*We changed the unit from kyrs to kyr throughout the manuscript. Additionally, we agree that the word "depend" was misleading and changed the sentence to: "A comparison of modelled ice volume under fast ( $\pm 4$  K/kyr) and slow ( $\pm 2$  K/kyr) temperature forcing shows that **different cooling rates strongly affect** coarser resolution results, while results at fine resolutions are rather comparable (Fig. 9)."*

365 - easy to slot in an actual value here

*The values corresponding to this sentence are given in the following sentence. We replaced the period by a colon to make this clear: "The delay in reaching the maximum ice volume with respect to temperature forcing is reduced with the slower temperature change: In the 1000 m runs, the lag drops from 420 to 250 years, driven by continued ice growth at low altitudes, while glaciers at mid and high altitudes are more sensitive to temperature change and start to retreat earlier."*

368 - slightly clumsy sentence that maybe should be in the caption of Fig. 9 and not here (if needed at all)

*Thank you for the suggestion, we shortened the sentence and moved it to the caption.*

280 - here and throughout, better to refer to 'the ice sheet' or similar, rather than just 'ice'

*In accordance with our reply to the comment about line 46, we used the term "ice field": "Our results show that the **ice field** is generally thinner and faster-flowing at resolutions of 300 m and finer, in contrast to rather thick glaciers at resolutions coarser than 800 m..."*

386 - these two sentences really belong in the results (or just omitted), I would just crack straight on with the discussion points

*Thank you for this comment. We omitted the first sentence and combined the second sentence with the following one to discuss the interactions of coarse-resolution DEMs with glacier thickness at high altitudes more directly: "**Despite the reduced high-altitude area, coarse-***

*resolution DEMs with their gentle slopes (Fig. 1) tend to prevent glaciers from directly flowing into the ablation area, and thereby support ice thickening at high altitudes (Figs. 5, 7, 8)."*

393 - well, thicker ice is a very effective insulator trapping GHF, hence lakes beneath Antarctica

*Yes, we added this mechanism to this text passage: "The shift from generally frozen bed conditions at fine resolution simulations to temperate basal ice at coarser resolutions is a consequence of different mechanisms, including colder climatic conditions in the fine resolution DEMs due to higher surface elevations. **In contrast, the thicker ice at coarser resolutions is an effective insulator, trapping geothermal heat**, while increased basal pressure lowers the pressure melting point, increasing the possibility for basal melting."*

395 - I don't think the 'likely' is necessary here

*We agree and dropped the "likely": "In the valleys, ice flow at fine resolutions undergoes strong lateral confinement by steeper and narrower valley walls that forces the glaciers into a more constricted flow path."*

409 - 'seems augmented' is a bit unclear

*Thanks for pointing this out. We replaced "seems augmented": "Conversely, the delay in reaching maximum ice volume **is exaggerated** in coarser simulations and results are more affected by different temperature forcing rates."*

421 - this sentence also a little unclear. Also, interesting to speculate as to why you find this

*Thank you for this comment. We consider the contrast between ice sheets like Greenland and Antarctica and the topographically-controlled ice field we modelled as the explanation for different resolution thresholds and emphasized this contrast: "Similar to Williams et al. (2025) and Rückamp et al. (2020), we identify a spatial resolution that is sufficiently fine to produce results close to a fine-resolution reference run. However, in contrast to the km-scale resolution adequate for **ice sheets** in Greenland and Antarctica (Williams et al., 2025; Rückamp et al., 2020), we find that it requires a resolution of 300 m or finer to model the exemplary ice field in the **topographically complex** Western Alps. At such fine resolutions, the characteristic alpine topography is preserved with high accuracy (Figs. 1, 2, 8)."*

423 - this sentence, and the one at 425 don't really stand up on their own, or relate particularly well to the rest of the paragraph

*Thank you for pointing this out. We combined the sentences about the fine resolutions. Additionally, we believe that the changes we made to address the previous comment on line 423 help to understand the importance of the role of the complex topography in our model domain. With these changes, we believe that the contrast between the finest resolution results that are substantially controlled by the underlying topography, and the coarse resolution outputs, which are increasingly controlled by surface elevation in terms of ice thickness rather than bedrock relief (lines 426-429), is clearer: "However, in contrast to the km-scale resolution adequate for ice*

*sheets in Greenland and Antarctica (Williams et al., 2025; Rückamp et al., 2020), we find that it requires a resolution of 300 m or finer to model the exemplary ice field in the topographically complex Western Alps. **At such fine resolutions**, the characteristic alpine topography is preserved with high accuracy (Figs.1, 2, 8) **and** minor differences in elevation and slope effectively result in similar modelling results (Leger et al., 2025; Williams et al., 2025; Rückamp et al., 2020). However, the non-linear variations in model outputs **at coarser resolutions** cannot be explained exclusively by gradual changes in DEMs with resolution (Fig. 8 a, b)."*

428 - I suppose this gets a bit philosophical, but I think you can drop some of these cautious statements. You are controlling all the variables, and while IGM is not exactly full-Stokes (or even Stokes) physics, I think you can be firmer in your statements about what is happening

*Thank you for this comment. We changed the sentence to: "We **find** that at ~800 m resolution, the topographic constraints become sufficiently weakened so that further coarsening has a minor additional effect."*

446 - good point

*Thank you very much.*

450 - as before, interested why, and if this is an IGM artefact or not

*We discussed potential reasons for the rugged ice surface at coarse resolution among the author team and considered possible mechanisms. However, we could not identify a definitive cause of these artefacts and investigations in future studies are needed. We would like to point out that the artefacts are not only observed in simulations using IGM, but also in outputs from Seguinot et al. (2018) and Jouvet et al. (2023) (also shown in Fig. 5 in Leger et al., 2025), who used PISM (lines 446-448). We clarified this in the updated manuscript: "Specifically, checking if surfaces of glacier flowing over mountain summits are realistically represented might be an initial indicator, as models at coarse resolutions might show imprints of bedrock topography onto the ice surface (Fig. 6). Similar features can be observed in simulations of glaciations in the European Alps at 1–2 km resolution **using PISM** (Fig. 5 in Leger et al., 2025; supplementary data in Jouvet et al., 2023 and Seguinot et al., 2018), **implying that these artefacts are not related to IGM.**"*

460 - latter half of this sentence a bit confusing

*We changed the structure of this sentence to add clarity: "**Deviations between modelled velocities in a Greenland ice stream region using the Blatter-Pattyn compared to a full Stokes model are less than ~10 m yr<sup>-1</sup>, as investigated by Rückamp et al. (2022), and therefore smaller than the resolution-induced differences of up to several tens of meters per year found in our analysis.**"*

480 - the conclusion is slightly repetitive, but I appreciate it can be challenging to tie things up in with a fresh perspective

*Thank you for this comment. We acknowledge that the conclusion might feel repetitive because the main points have already been presented and discussed before. However, we think that the*

*conclusion ties those points together in a concise summary and makes sure that the reader catches all important points.*

## Comment on egusphere-2025-3870 (referee #2)

*We would like to thank reviewer 2 kindly for taking the time to read our manuscript and for the constructive feedback, which helped us to improve the study. In the following, we addressed all comments point by point and point out respective corrections. Please find our answers in blue italic font.*

I would like to congratulate the authors on their work. Overall, I believe the manuscript has improved considerably. The authors have implemented the requested changes and clarified several aspects of the study. In addition, the shift in focus suggested by Reviewer 1 and adopted by the authors has, in my opinion, strengthened the paper and made the overall contribution clearer.

That said, there are still a couple of small details that I would like to point out. I would not even classify them as minor revisions, but I believe they are worth considering.

*Thank you very much for your assessment.*

First, in line 531, the DOI of the reference Duncan et al. (2003) is incorrect. It contains typographical errors and therefore does not work.

*We apologize for this error and updated the DOI in the references manually. Our reference software Zotero seems to have problems displaying some characters in the DOI link.*

Second, in lines 76–77 it is mentioned that the work of Leget et al. (2025) significantly reduces the mismatch between trimlines and the modelled ice surface elevations to approximately 150 m. My understanding is that this reduction is relative to the results presented in Mey et al. (2016). However, when reviewing this paper from Mey et al., I noticed that Figure 4 shows an RMS value of about 140 m. Therefore, either I am misunderstanding the comparison or there is something here that I am not getting. Could the authors please clarify this point?

*Thank you for pointing out this misunderstanding. For the reduction of mismatch between modelled ice surface elevations and trimlines by Leger et al. (2025), we gave the absolute value, i.e., the mean difference between the observations and the model, and not relative to other model results. We changed the structure of these sentences to make this clearer. Additionally, we specified that the value refers to the best-scoring simulation in Leger et al. (2025), as this was not clear before and the value in our previous version referred to the group of NROY-simulations: “In the European Alps, comparisons between modelled ice surface elevations and trimlines suggest ice thickness overestimations during the Last Glacial Maximum (LGM, ~24,000 years ago) in models based on a km-scale resolution, e.g., by up to 1000 m in the Rhône Valley (Jouvet et al., 2023; Seguinot et al., 2018). **A better match to trimline records** was achieved by Mey et al. (2016) by adjusting the sliding coefficient in a steady-state model **at 3 km resolution**. Recent work by Leger et al. (2025) significantly reduced **the Alps-wide mean mismatch between***

*modelled ice surface elevations and trimlines to ~130 m in the best-scoring simulation of a 100-member ensemble by using a higher spatial resolution of 300 m.”*

Finally, in the abstract—particularly between lines 20–25—I feel that there is some of information presented in a somewhat enumerative way, but it is not very well integrated. It reads more like a list of ideas than a cohesive piece of text. I believe this section could be improved to make the abstract flow more naturally.

*Thank you for this comment. In accordance with the comment on line 20 by referee 1, we changed these lines to: “Raised valley floors at coarse resolutions result in slower-flowing ice with increased thickness and glacial response times. **The resulting hysteresis** between climate forcing and glacial response at coarse resolutions is only partially decreased **with slower temperature change**. Seemingly stable model results at coarse resolutions may be misleading and accurate glacier geometries might arise from parameter choices that compensate for poorly resolved topography. **Similar** non-linear and altitudinal-dependent resolution effects are likely in mountain regions worldwide and emphasize the need for model advances to enable simulations at sufficiently high spatial resolutions to accurately resolve glacier dynamics.”*

Other than that, I would once again like to congratulate the authors on the excellent work done, and I hope that these final details are not too problematic to address.

*Thank you very much for the valuable comments, which we hope we have addressed in the above-mentioned modifications.*

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