Comment on egusphere-2022-157
Anonymous Referee #1

Referee comment on "Interactions between thresholds and spatial discretizations of snow: insights from wolverine habitat assessments in the Colorado Rocky Mountains" by Justin M. Pflug et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-157-RC1, 2022

This is a well-written manuscript that clearly articulates an outstanding need in the field of snow-wildlife interactions (understanding the impact of spatial discretizations and thresholds on habitat estimates), and presents compelling data to fill that need. Overall, the authors have assembled a laudable manuscript that will make a needed and meaningful contribution to the field. It does, however, require a few changes prior to publication. I outline the more substantive/broad changes first, followed by detailed comments.

Major comments:

Use of the terms “wolverine habitat” and “wolverine habitable area.” These are used inappropriately throughout the manuscript. I outline specific recommendations for changes in detailed comments below, but broadly I request that the authors change WHA to “potential wolverine denning area (PWDA)” or similar. Wolverines may (and do) inhabit areas without snow that falls within these defined thresholds, and “wolverine habitable area” implies a level of certainty about this region and the relationship of wolverines to snow that we lack ecological data to support. It seems that the authors are broadly cognizant of this (i.e., through the examination of a range of thresholds), but the terminology is misleading.

More information about snow modeling methods. The reference to Xue et al. (1991) on line 128 does not sufficiently explain how this model works. Please provide more detail regarding the process used to derive SWE from MERRA-2 forcing (i.e., prior to data assimilation and applying the Liston (2004) subgrid method). Additionally, please expand the detail regarding how SNOTEL sites and satellite data were assimilated into SWE.
estimates (for example, what satellite data were used?).

Abstract. The results regarding differences between the three spatial discretizations presented here are somewhat confusing due to terminology (see specific comments below). I would recommend introducing the terminology used in the main text (D480, D90, and S480) on lines 25-26, and then referring to them as such for the remainder of the abstract.

Detailed comments:

Line 26 – Does “Relative to the 480 m grid cells” refer to both (1) and (3) in the previous sentence, or only (1)?

Lines 28-30 – Does “grid cells with subgrid representations of snow heterogeneity” only refer to (3) on lines 25-26?

Line 31 – Does “simulations without subgrid snow heterogeneity” refer to (1) and (2) on line 25?

Lines 61-62 – Why limit this to North American wolverines? This misleadingly implies a different association with snow among palearctic wolverines, for which there is no evidence. Please change to “wolverines (Gulo gulo)”.

Line 64 – Several of the studies cited in the previous sentence, and several not cited, have explicitly identified this problem and attempted to address it by representing snow at resolutions expected to be biologically relevant. Consider citing a few such examples here: “(but see Mahoney et al. 2018, Glass et al. 2021, Liston et al. 2016).”


Lines 77-80 – Please provide more detail regarding where these thresholds (SWE and date) originated. At what spatial and temporal resolution were they originally linked to wolverine denning? How many dens were used to make this association? Which of the cited studies identified these thresholds? Given this paper’s focus on the importance of transparency regarding thresholds, it seems particularly important to address spatial and temporal resolution these thresholds are thought to represent, and therefore any reason they may result in biased estimates of wolverine denning area when applied to other spatial discretizations.

Line 83 – Consider replacing “wolverine habitat” with “wolverine dens” to avoid conflating the two. “Wolverine” does not need to be capitalized here.

Lines 92-93 – Consider “We focus on”

Lines 114-115 – Please clarify that this region does not currently support a reproductive population of wolverines.

Lines 117 – Comma in “Fig, 2.”

Line 123 – It is unclear why this section (SWE Reanalysis) is included under “Domain and data” rather than “Methods”; this presents meaningful data processing and I would generally expect it to appear under methods, though perhaps this is a journal-specific guideline. I would defer to the editor on this.

Lines 124-139 – What satellite data are used here? “Satellite-observed snow cover disappearance” is mentioned, but it is unclear what the source of these data are, or what spatiotemporal resolution they represent.

Lines 156 – Consider replacing “wolverine habitat” with “denning wolverines” (also at lines 222, 347, 393).
Lines 193-194 – Please reiterate here that CoV is derived from reanalysis-based SWE values at 90 m resolution for overlapping pixels.

Lines 244 – At what spatial and temporal resolution do the three spatial discretizations “resolve similar SWE”?

Lines 267-268 – I would recommend removing or clarifying this sentence; “had a relationship with terrain” is too vague to convey meaning.

Lines 272-273 – This would seem to imply that the 90 m reanalysis product is capable of simulating these processes (wind-drifting, preferential deposition, cornice formation, and avalanching), but I don’t expect that’s actually the case for the model developed by Xue et al., 1991. Please remove or clarify, and expand the description of the Xue et al. (1991) model used here.

Lines 321-323 – This is a valuable point, and I think it can be taken one step further. Since the 11% mean WHA increase by S480 in low snow years is never matched by the D90 WHA estimates, it would seem not only that this difference will be “of greater importance,” but that the subgrid methodology will increasingly overestimate WHA as the climate continues warming. That is, S480 is biased high for low-snow years (if we assume D90 as the most accurate estimator of WHA considered here), and that bias will only become more pronounced with continued warming.

Line 323 – Please replace “annual wolverine habitat” with “potential wolverine denning area” or similar. Suitable habitat fluctuates on generational, not interannual, timescales; instead, wolverine habitat in Colorado would likely be defined by having enough years (i.e., some proportion) with sufficient snow for wolverines to successfully reproduce.

Line 325 – Period in “…season. as wet…”

Line 327 – I think the authors mean “comparing SWE on a static date,” since a static date is inherent to WHA.

Line 340 – Present tense: “This is…”

Line 349 – Change to “(i.e., the elevation at which equal WHA existed at higher and lower
elevations)” if that is what’s intended.

Line 352 – Perhaps “elevational” or “altitudinal” is more accurate than “topographical.”

Lines 352-353 – Please replace “annual location of wolverine habitat” with “potential wolverine denning area” or similar (see comment on line 232).

Line 355 (Figure 7) – It is unclear why D480 is represented twice. I would recommend removing one panel and replacing it with a map indicating elevation for the same area.

Line 360 – Does “higher than the snow line” mean across the entire study area? Please clarify.

Lines 364-367 – More information is needed regarding the combination of models and observations used for reanalysis (as indicated by my confusion on lines 272-273 as well). Please expand section 2.2 (SWE Reanalyses) to more thoroughly present the model used to simulate SWE based on meteorological reanalysis forcing. It is unclear based on lines 124-149 how the modeling process used here could account for processes like sloughing and avalanches.

Line 372 – “Wolverines” does not need to be capitalized here.

Lines 386-389 – More caution is warranted in presenting the results of S480, particularly at low elevations. Liston (2004) specified that the CoV used to parameterize the lognormal curve should be derived prior to any melting in the grid cell, but the authors imply that mid-winter melting did take place at these elevations, and was likely responsible for driving up the CoV (lines 268-271). I can’t help but wonder if applying Liston’s method to low elevation grid cells is inappropriate for this reason, and has led to inflated CoV values that consequently overestimate WHA at low elevations. This notion seems to be independently supported by the poor match of S480 and D90 at low elevations. Regardless, since the within-cell variability of S480 is derived from (and therefore essentially a simplification of) D90, it seems inappropriate to present the deviation of S480 from D90 as anything other than an artifact introduced by Liston’s lognormal subgrid representation approach, or the inappropriate use thereof.

Lines 409-411 – Consider adding “and is consistent with the observed range in wolverine parturition dates (Inman et al. 2012)”

Line 410 – It’s unclear what “dates of observed wolverine habitat” refers to. Other studies have proposed threshold dates based on alignment with certain aspects of wolverine biology; please clarify what’s intended here.

Lines 428-436 – This is a valuable point, and its discussion should be expanded a bit. Specifically, the years experiencing spring snowfall highlight the inappropriateness (in at least those cases) of using a single date threshold to measure habitable area for wolverines. If all snow melts by April 15, but then a spring storm dumps 1.5 meters on May 14, what good is it to wolverines? I suggest that the authors mention a few alternate metrics of snow as denning habitat that could be more useful (i.e., representative of what a wolverine uses/needs) than these thresholds (e.g., number of days between late February (parturition) and early May (den abandonment) when SWE exceeds a certain value).

Lines 442-443 – Please rephrase as “…may decrease the sensitivity to changes in SWE and date thresholds.” Including “uncertainties” implies that subgrid representations can be used to account for uncertain thresholds, but identifying accurate thresholds is a biological problem (i.e., determining how much snow wolverines actually need and when), and it shouldn’t be addressed by altering the snow modeling process.

Lines 459-460 – This sentence is misleading. Snowfall a few days after May 15 does not “boost wolverine habitat” (see my comment on lines 428-436). The problem that 2015 demonstrates is that single-date thresholds do not accurately depict wolverine denning habitat, not that this particular date is inadequate. Please remove or rephrase.

Line 495 – Please replace “wolverine habitat” with WHA or PWDA.

Lines 495-498 – “…which allowed for more gradual changes to wolverine habitat with small changes in SWE” seems to imply that lower sensitivity to changes in thresholds is desired, but sensitivity in itself is not a metric of performance. We should strive for the most accurate, not the least sensitive, snow representation.

Line 499 – Please replace “wolverine habitat” with WHA or PWDA.
Overall nice work and I look forward to seeing this published!