

1 PLEASE NOTE THAT ALL LINE NUMBERS IN THE REVISED MANUSCRIPT REFER TO THE
2 VERSION WITH TRACK CHANGES

3 Response to Reviewer 1

4 Review of “Exploring the decision-making process in model development: focus on the
5 Arctic snowpack” by Menard et al.

6 We thank the Reviewer for taking the time to review our manuscript. Please find our
7 answers in blue font.

8 *All comments in this review from L12 to L116 concern the methodology used in our*
9 *manuscript. All points until L116 are now addressed in the Methods Section, which was*
10 *expanded to provide more detailed information about the qualitative research methods used*
11 *in our manuscript.*

12 This is a somewhat unusual manuscript, submitted as a “Research article” for consideration
13 in The Cryosphere. The “unusual” aspect is that, where most research articles focus on
14 measurement data and/or simulations, this study reports on interviews conducted with
15 experts in the field of Arctic snow, [L96-97] *“to understand why decisions made by modellers*
16 *all over the world and over the past decades have not led to more (or is it “any”?) progress in*
17 *Arctic snowpack modelling, ...”*

18 It is true that *“[quantitative] research articles focus on measurement data and/or*
19 *simulations”*. However, qualitative research does not. Indeed, it is “unusual” for a
20 manuscript using qualitative methodologies to be submitted to a journal that predominantly
21 publishes research using quantitative methodologies. For this reason, we contacted the TC
22 editorial board prior to submission to check whether a manuscript using qualitative
23 methodologies to investigate decision-making in snow modelling could be considered for
24 peer-review. The editorial board confirmed that qualitative methodologies as applied to
25 cryosphere topics are within the remit of the journal.

26 We suspect that the many comments in this review questioning the methodological
27 soundness of our study may stem from Reviewer 1's unfamiliarity with qualitative
28 methodologies. We did expect that some TC readers would be unfamiliar with qualitative
29 research, which is why we did explain our process throughout the manuscript, but now
30 realise more information will be needed. For example, we described our approach in the
31 Methods section and referenced a number of papers examining the qualitative
32 methodologies we used in our manuscript (e.g. Braun and Clark, 2006; DiCicco-Bloom and
33 Crabtree, 2006; Lincoln 1995; Rapley, 2011). We as well mentioned in the Introduction and
34 Conclusion that our approach was borrowed from Science and Technology studies (L99 and
35 L563-568).

36
37 Nevertheless, the comments from Reviewer 1 made us realize that, in our revised version,
38 we will need to provide more information about our methodology and stress in greater
39 depth the value and complementarity of qualitative research. We will for example quote
40 Fossey (2002) in the introduction to set the tone of our work: *“Restricting oneself to any*

41 *single paradigm or way of knowing can result in a limitation to the range of knowledge and*
42 *the depth of understanding that can be applied to a given problem situation'. (...) Thus,*
43 *research needs to draw on different perspectives, methodologies and techniques to generate*
44 *breadth of knowledge and depth of understanding. Qualitative research is a broad umbrella*
45 *term for research methodologies that describe and explain persons' experiences, behaviours,*
46 *interactions and social contexts without the use of statistical procedures or quantification.*
47 *(...) One of the major criticisms is that within the positivist paradigm [i.e. scientific research*
48 *based on quantitative methodologies] it is assumed that an objective reality, or truth, exists*
49 *independent of those undertaking the inquiry and the inquiry context. Two research*
50 *paradigms that inform qualitative research methodologies, namely the interpretive and*
51 *critical research paradigms, place emphasis on seeking understanding of the meanings of*
52 *human actions and experiences, and on generating accounts of their meaning from the*
53 *viewpoints of those involved"*

54 Here, issues identified are the somewhat troublesome transferability of modeling
55 approaches between lower latitudes and polar regions, limited data availability from the
56 arctic suitable for model development, parameterization development, and calibration and
57 validation. Other issues are the historical underrepresentation of arctic snow in snow model
58 development environments, lack of attention and (thus) funding for the problem, and
59 inadequate approaches.

60 Major comments:

61 1. I think it is an interesting concept to access knowledge that is normally not finding its
62 way to the broader community in the form of manuscripts. However, I think there are
63 some methodological problems that devalue this manuscript from a "Research article"
64 to only an opinion piece.

65 1. First of all, the selection of participants was seemingly done very subjectively,
66 and is not transparent for the reader. The only procedural aspect mentioned
67 here is [L118] "CM, SR and IM compiled a shortlist of participants". I wish that
68 there would have been some objective criteria, for example a random pick of
69 first authors on papers that mention "snow", "arctic" and "modeling" in the
70 abstract that were published over the last ten years, based on a database like
71 Scopus, ISI knowledge or google scholar.

72 Participant selection abided with qualitative methodologies: "Quantitative
73 research requires standardization of procedures and random selection of
74 participants to remove the potential influence of external variables and ensure
75 generalizability of results. In contrast, subject selection in qualitative research is
76 purposeful; participants are selected who can best inform the research
77 questions and enhance understanding of the phenomenon under study (...)
78 Decisions regarding selection are based on the research questions, theoretical
79 perspectives, and evidence informing the study." (Sargeant, 2012)

80 Based on this, we explained our reasoning for selection in the introduction L96-
81 104: "The aim of this study is to understand why decisions made by modellers all
82 over the world and over the past decades have not led to more (or is it "any"?)

83 *progress in Arctic snowpack modelling (...)* Therefore, to address our aim, we will
84 *investigate the construction of snow models through interviews with the*
85 *individuals who shape their content and present the results of this investigation*
86 *in their own words".* Nevertheless, we recognise that TC readers may welcome
87 more information about our selection process, which we will provide in the
88 revised version in the Methods section.

89 For completeness, we note that that the quoted sentence was truncated. The
90 full sentence is "*CM, SR and IM compiled a shortlist of participants, both within*
91 *and outside CHARTER, who consider the snowpack structure important for their*
92 *research".*

93 2. Second, the manuscript relies heavily on quotes from the interviews. The full
94 interview transcripts are, understandably, not released. Thus this could
95 potentially result in heavy cherry-picking of quotes by the first three authors.
96 Apparently, the interview transcripts have been coded using NVivo, but it is not
97 clear how this has further been used. It is not clear what attempts were made
98 for objective analysis of the interview transcripts.

99 Information about our methods is found between L152:156: "*The transcripts*
100 *were analysed by conducting a thematic analysis (Braun and Clark, 2006;*
101 *Rapley, 2011), which consists in identifying codes (semantic content or latent*
102 *features in interviews) and collating them into overarching themes. Iterative*
103 *coding was conducted in NVivo, a qualitative data analysis software that*
104 *facilitates the classification and analysis of unstructured data. Three iterations*
105 *were necessary to identify all codes and to classify codes into themes".* We
106 recognise that the TC readership may welcome more information about
107 thematic analysis and will provide more information in the revised manuscript;
108 readers particularly interested in the methodology are invited to consult the
109 two papers cited above. Until then and briefly: The questions asked during the
110 interviews were to understand the decision-making process of the participants.
111 Following the interviews and using the transcripts, one or multiple codes (or
112 "labels") were attributed to each statement by CM. Codes were then merged
113 and grouped into themes. This process was repeated three times to ensure
114 thorough codification. These themes were then addressed separately in each
115 subsection in Sections 3. The quotes that best illustrated the themes were then
116 included in the manuscript.

117

118 3. Third, I'm concerned that quotes from the interviewed scientist are published,
119 without fact-checking if this is true. This results in a few false statements, for
120 example that "*CROCUS is an avalanche model*" [L237], or that [L282-
121 283] "*[Models] are limiting the number of [snow] layers for computational*
122 *stability and efficiency",* which for Crocus or SNOWPACK, for example, would be
123 trivially easy to adjust. [L373-375] "*In my sense, large scale climate modellers*
124 *aren't sufficiently aware of snow. (...)* There are so many people who don't care
125 *about that".* The first part of this statement is an opinion. The second part is

126 stated as a fact. *“There are so many people who don't care about that”*. I would
127 like to see evidence for that.

128 We had attempted to explain the nature and purpose of the quotes L173-177 in
129 the manuscript: *“The working title of this study in the participant information
130 sheet was “A multi-perspective approach to snow model developments”, thus
131 implicitly alluding to the fact that, by approaching a single issue from multiple
132 angles, this study sought to elicit diverse responses. This certainly turned out to
133 be the case. Most significantly, no opinion was unanimous; every statement
134 made by each participant was contradicted by a statement made by another
135 participant.”* We also addressed the fact that we were eliciting opinions *“By
136 opting for the semi-structured interview format, our aim was to use a medium,
137 the conversation, in which using “I” was natural. While all participants provided
138 important information related to their field – information that is presented in
139 Section 3.1– they also ventured where few scientists do, at least in their
140 publications: they offered opinions”*. Reviewer 1’s comment makes us realize
141 that the nature of the statements may not be clear to all readers and we will
142 therefore clarify in the next version of the manuscript that all quotes are
143 opinions and that as some quotes contradict each other, none of the quotes are
144 endorsed by all authors and, consequently, readers will inevitably disagree with
145 some quotes.

146 Regarding “fact-checking” and “truth”, as explained in the manuscript and
147 above, what we are interested in is how the opinion of decision-makers - in
148 other words their truth based on their experience, expertise and perspective -
149 inform their decisions. We also stress that the word “fact” is loaded in social
150 sciences, including Science and Technology studies (we refer Reviewer 1 to
151 Fleck, 1935, referenced L565), which understands facts as being constructed.

152 Regarding the Crocus quote, we invite Reviewer 1 to read the full paragraph in
153 which it figures to understand the context in which it is cited: *“Issues of scale
154 are further complicated by the fact that some models are being repurposed and
155 operate at scales that they were not intended to. Examples include context-
156 specific models being used at large scale (‘a lot of snow models are being used
157 now in land surface schemes as broadly applicable snow models for all snow
158 climate classes. But, I mean Crocus, it's an avalanche model, right?’)”. We hope
159 that, as we will be more explicit about quotes expressing opinions in the revised
160 version, there will be no more room for misinterpretation. We will also revise
161 this paragraph e.g. “some participants believed that some models were being
162 repurposed and operated at scales that they were not initially intended to”; this
163 will help the reader understand that the quote means that the participant does
164 not think that a model initially developed as an avalanche forecasting model
165 (Brun et al., 1989) should not be broadly applied for all snow climate classes.*

166
167 *We have further clarified that quotes are opinions in the first paragraph of*
168 *Section 3; as such none of the co-authors nor readers will agree with all of them.*
169 *We further discussed the importance and necessity of this plurality of opinions*

170 (and strategies) for model development in a new sub-section (4.2) that was
171 motivated by Reviewer 2's comments.
172

- 173 • “[Models] are limiting the number of [snow] layers for computational stability
174 and efficiency”, which for Crocus or SNOWPACK, for example, would be trivially
175 easy to adjust. [L373-375].

176 As mentioned above L134-139, we will clarify that all quotes are opinions. We
177 also invite Reviewer 1 to review the context in which this quote is cited: “Ten
178 participants began the interview by providing some background about snow
179 model developments, using this as a historical justification for Arctic snowpack
180 properties not being included in snow models”.

181 See L164-168 of this response.
182

- 183 • “In my sense, large scale climate modellers aren't sufficiently aware of snow. (...)
184 There are so many people who don't care about that”. The first part of this
185 statement is an opinion. The second part is stated as a fact. “There are so many
186 people who don't care about that”. I would like to see evidence for that.

187 As mentioned above L134-139, we will clarify that all quotes are opinions.

188 See L164-168 of this response.
189

- 190 4. Lastly, the Interview consent form states: “Access to the interview transcript will
191 be limited to the research team: Dr Menard, University of Edinburgh; Dr Sirpa
192 Rasmus, University of Lapland; Dr Ioanna Merkouriadi, Finnish Meteorological
193 Institute.” Yet the list of co-authors further encompasses the majority of
194 interviewed scientists. I cannot see how this can be objective. I think
195 interviewees have the right to review their quotes, such that they can verify
196 that no misunderstandings or misrepresentations have occurred. But I fail to
197 understand how the interviewees can also be co-author. On the one hand, they
198 have no access to the other interview transcripts, thus cannot reliably judge if
199 this was a proper reporting of what was said in the interviews, but more
200 importantly, as author they have direct impact on which quotes from them are
201 selected, and how they are presented. That means that this manuscript
202 basically has become a vehicle to get their own opinions across, which I think
203 doesn't align with what is expected for a “Research article”. On top of that, they
204 obviously have full access to their own interview, but not to the other
205 interviews. I cannot see how this can properly result in a good co-authorship,
206 when the majority of underlying data is inaccessible to the co-author. I cannot
207 see a scenario where this leads to proper scientific conduct for a peer-reviewed
208 “Research article”. Unfortunately, I don't see how these methodological flaws
209 can be corrected, and I think the manuscript should be rejected as a peer-
210 reviewed “Research article”. It may find an outlet as an opinion piece.

- 211 i) This quote from Reviewer 1 “as author they have direct impact on which
212 quotes from them are selected, and how they are presented. That means
213 that this manuscript basically has become a vehicle to get their own
214 opinions across” somewhat contradicts this one in their epilogue:
215 “Maybe the interviewees expressed themselves somewhat awkwardly

216 *because they also felt like they were in an informal private conversation.*
217 *It is also very possible that context or tone went missing in the*
218 *transcription and the quote selection for the manuscript". If the*
219 *participants "expressed themselves somewhat awkwardly" during the*
220 *interviews, but then could turn the manuscript into a "vehicle to get*
221 *their opinions across", would the participants/co-authors not have, in*
222 *that case, removed any "awkward" quotes?*

224 In addition, the participants were well aware that the interviews were
225 not "informal private conversations". As mentioned L130-131 in the
226 manuscript "*participants were emailed with a request for participation*
227 *that included a participant information sheet and consent form". All*
228 *participants had to return the signed consent form prior to being*
229 *interviewed. The consent form states that the interviews were recorded*
230 *and transcribed and that quotes from the interviews may be used in*
231 *future publications.*

232 **No further comment.**

233

234 *ii)* As mentioned in the participant information sheet (PIS) and the
235 interview consent form, the methodology used in this study was
236 approved by the University of Edinburgh School of GeoSciences (where
237 CM is based) Research Ethics & Integrity Committee. The Committee
238 consulted the PIS in which it is stated that the participants "*will be given*
239 *the choice to remain confidential, to be named as a participant or to be*
240 *a co-author in publications stemming from this study". The Committee,*
241 *therefore, concluded that inviting participants to become co-authors did*
242 *constitute "proper scientific conduct", perhaps because committee*
243 *members are familiar with qualitative methodologies and knew that it is*
244 *becoming increasingly customary to invite participants to co-author the*
245 *research they participated in (e.g. see Given, 2008; Pope, 2020;*
246 *Farbotko et al, 2021; Doering et al., 2022; Warman et al., 2024)*

247 **Now clarified L212-215 in the revised version.**

248 2. I also struggled with understanding the modeling environment that the authors were
249 considering. I found that the manuscript paints a picture of this environment that
250 simply didn't resonate with me. For example, when I read: [L549-553] "*Yet, models are*
251 *a product of one or multiple modelers' vision. This was reflected in the interviews*
252 *during which many participants often mentioned the name of the model creator or lead*
253 *developer instead of, or as well as, the model's name. The research identity of many*
254 *modellers is, whether they want it or not, intertwined with their model; inviting authors*
255 *to reflect about their positionality would allow modelers to regain control over their*
256 *own narrative and research identity."* My personal experience is completely different.
257 Thinking about the snow model I work with most, and which is widely used and
258 recognized in various cryosphere communities, basically all major model developments
259 in the last 15 years were done by PhD students and PostDocs, most of whom have
260 since moved on. So their "research identities" stretch way beyond "their model". I
261 think when asked, very few of the PhD students would describe the model as "their

262 model". In fact, even though they contributed most significantly to model
263 developments, I doubt they will describe their role as a "modeller". The model I'm
264 mostly familiar with, has almost no dedicated, long-term model developers or code
265 maintainers. The large majority of recent code changes (last 15 years) has been done
266 by people with contracts lasting shorter than a few years. The original "model
267 creators", in the meantime, have taken up different research fields, retired or have
268 taken up other roles in academia. For the model I work with most, no "lead developer"
269 can be identified. Thus, I struggle to agree with this proposed narrative of "model
270 creators" or "lead developers" as well as supposedly the concept of "their model" at
271 face value. It needs to be supported by data and analysis. For example by analyzing
272 model code repositories and investigating how many people contributed how much to
273 the code, and in what role. That would give the necessary underpinning of this
274 narrative. I'm now curious if the model ecosystem I work with is the exception, or the
275 rule. It could also signal a bias in the selection of participants for the interviews.

276 • We are glad that the manuscript is making Reviewer 1 reflect on their own
277 experience because it suggests that we have reached one of our goals stated in
278 the Conclusion: the novelty in this paper is that *"it is a reflective exercise which,
279 we hope, will be the start rather than the end point of the conversation"*.
280 However, while Reviewer 1's experience could contribute future conversations
281 and similar research studies, it does not mean that it cancels the participants'
282 experience. When participants talked about a snow model, they did often
283 mention the name of the model's creator e.g. Glen's or the Liston model
284 (SnowModel), Richard's model (FSM), Marie's model (Ivori), or Dave Lawrence
285 when mentioning CLM; these are the (qualitative) data we base our analysis on.
286 The exception was Crocus; although some of its developers were named, no one
287 was singled out and it was generally referred to as "simply" Crocus.

288 **Now clarified in the last paragraph of Section 4.4.**

289
290 • *"For example by analyzing model code repositories and investigating how many
291 people contributed how much to the code, and in what role."* This would be
292 assuming that all model code repositories exist, are well maintained and that
293 protocols about comments were instigated since the birth of the investigated
294 models and have been respected since. Based on Menard et al. (2021) who
295 found that up-to-date and well-maintained model documentation was rare, we
296 would be reluctant to conduct such an analysis.

297 **No further comment.**

298 3. Further, it is written: [L96-97] *"The aim of this study is to understand why decisions
299 made by modellers all over the world and over the past decades have not led to more
300 (or is it "any"?) progress in Arctic snowpack modelling, ..."*. Given that my personal
301 experience is that most development is done by researchers on PhD or other short-
302 term contracts, I think a lot of issues were mentioned that they have no control over,
303 like funding or the historical legacy of models. In contrast, very little was reported on
304 the experiences and choices made by PhD students or other short-term contracted
305 researchers over the course of their model development efforts. I think it plays a role
306 here that those researchers seem to be absent from the pool of interviewees.

307 Thank you for this comment. It is exactly because PhD students and casualised
308 researchers have no control on funding or the historical legacies of the models that
309 they were not interviewed. The aim of our study was to interview those who decide or
310 influence model developments. This is what we meant when we wrote: “we will
311 investigate the construction of snow models through interviews with the individuals
312 who shape their content”. We recognise that what we meant needs clarifying and will
313 revise the wording for the next version of the manuscript.

314 **We expanded the last paragraph of Section 1 to further clarify our aim.**

315 4. I found that the manuscript was lacking context. It feels like it is assumed that the
316 readers understand the problems with snow models in the Arctic. There is very little
317 substantiation of these problems (basically restricted to L86-95).

318 We kept this part of the introduction short because we wanted the participants to
319 explain, in their own words, what *they* thought were the problems with modelling the
320 Arctic snowpack. We (CM, SR and IM) did not want to dominate the narrative by
321 explaining in detail what *we* thought were the problems. We will make this clearer in
322 the introduction, but also ensure that sufficient information is provided for the reader
323 to have enough context.

324 **This is now explicitly addressed in the Methods Section L171-179.**

325 In my opinion, it fails to properly introduce the problems to the reader. Furthermore, I
326 found context lacking in what the past decades have seen in model development and
327 projects focusing on Arctic snowpacks. In modern-day science, which is highly project-
328 driven, national funding agencies are one of the major sources of funding for model
329 development. There is a lot of emphasis in the manuscript on lack of funding, lack of
330 long-term perspective, focus on other regions than the Arctic, as well as a strong
331 sentiment that these “modellers” supposedly live in their own world.

332 We find this interpretation misleading and incorrect. We particularly refer Reviewer 1
333 to 3.2.2 Adaptability e.g. “*Although much literature argues that there is a conflict
334 between academic freedom and solution-based or applied science (e.g. Henkel, 2005;
335 Winter, 2009; Skea, 2019 etc), we found instead that adaptability and shifting priorities
336 was integral to the participants” or “interdisciplinary collaborations were the key
337 motivation for model development, demonstrating the participants’ adaptability”*. For
338 clarity, we will replace “participants” with “modellers” in the above sentence.

339 **Done (L427).**

340 I would have expected to read much more about efforts undertaken by the Arctic snow
341 community to support model development. How many proposals did they submit?
342 How many were funded? How much of these funds was allocated for model
343 development? I would have expected to see more hard data on this. Also more
344 concrete information about decision making. I.e., if a proposal contains a modeling
345 component, what model is selected and why? How is decided where to focus energy
346 on model development?

347 Please see L93-110 above to see how the themes and sections in the manuscript
348 address our aim. Some of these questions were addressed during the interviews, but
349 not all answers are in the manuscript and, when they are, the information is scattered

350 throughout the manuscript and, therefore, may be difficult to decrypt. We will be
351 clearer in the revised version of the manuscript. We also wish to highlight that, while
352 mixing quantitative and qualitative research methods can provide important
353 information, this study is qualitative and therefore the interview transcripts are our
354 “hard data” and quantitative answers were not specifically sought during the
355 interviews

356 **No further comment.**

357 Right now, the manuscript comes across as a lot of complaining and finger-pointing.

358 The manuscript does not aim to complain or finger-point. It simply describes the
359 research environment in which the participants evolve and which shapes and - given,
360 amongst others, limited data availability and limited funding opportunities - constrains
361 their decision-making. Many quotes reflect a much more collaborative environment
362 than Reviewer 1 believes we describe (e.g. Section 3.2.2, L299-301, L380-385), but we
363 agree that they can seem isolated and lost. We will ensure that explanatory text is
364 clearer.

365 **Section 3.2.2 in general and some quotes in particular were expanded (e.g. L394-402).
366 Nevertheless, as explained above, the manuscript does not “complain or finger-point”,
367 it rather “describes the research environment in which the participants evolve and
368 which shapes and (...) constrains their decision-making”.**

369
370 , but a bit more reporting on one’s own activities, including some concrete and
371 objective data on funding, money spent, etc., would be expected given the goal set
372 forth by the authors. Here, for example: [L116-118] *“In these discussions, it became
373 clear that the current snow models fell short in representing all the Arctic snowpack
374 processes needed by project collaborators.”* We will provide more detail about what
375 was needed in CHARTER.

376 **More detail is now provided in the first paragraph of the Methods section.**

377

378 and then expect there to be a free, open-source model that fits one's needs, with
379 proper documentation and an email address that you can send all your questions to
380 with efficient response times. That is just an unrealistic expectation. These “unrealistic
381 expectations” are not described in the manuscript and Reviewer 1’s comment is
382 somewhat misleading. In my opinion “modeling” should be an undertaking done by the
383 community as a whole, where everyone contributes knowledge, expertise, skills, data,
384 etc.

385 **No further comment.**

386 5. [L182] *“I’m sick of modelers who think the world is a computer screen”* This quote is a
387 confirmation for me about the big problem of accessibility to fieldwork, combined with
388 the “hero”-status attached to fieldwork (Nash et al, 2019). Many research positions
389 including fieldwork ask for previous fieldwork experience, or, alternatively, “outdoor
390 experience”. Particularly back-country skiers, (alpine) climbers, and hikers have an
391 edge in securing snow-related fieldwork. And we know that “the outdoors” notoriously
392 lacks diversity (e.g., Winter et al., 2020, Ho and Chang, 2022). Fieldwork is mostly

393 accessible for PhD students, or senior scientists with previous fieldwork experience.
394 Model developers often lack access to participating in fieldwork, and people without
395 access to fieldwork mostly concentrate on doing modeling work. It's important to note
396 here that even when possibilities arise, fieldwork is not a safe environment for
397 everyone (Marín-Spiotta et al., 2020), and that could be prohibitive for participation.
398 The fact of the matter is that many researchers will never go to the field for a variety of
399 reasons, which may require rethinking of the status of fieldwork (e.g., Bruun et al.,
400 2023).

401 This quote was taken from a conversation during which I (CM) told the participant that
402 some large-scale modellers had told me anecdotally (i.e. prior to the interviews
403 conducted for this research) that improving the representation of snow in ESMS was
404 much less important than improving clouds. The quote was a manifestation of the
405 participant's frustration with such claims. We will either provide more context or
406 remove the quote.

407 **More context is now given L604-611 and more broadly in Section 4.2.**

408

409 We are well aware of the accessibility issues and of the much-needed enormous
410 progress to make field work more accessible, diverse and inclusive. Had this theme
411 emerged in our data analysis, we would have addressed it, but it did not. Undertones
412 of endorsing the hero status did emerge in one conversation and were coded as such,
413 but in order for codes to be included in the final themes, they had to be identified in
414 multiple conversations, which, in this instance, was not the case. We will provide this
415 methodological information in the Methods section in the revised manuscript.

416 **Clarified L189-196.**

417

418 The message delivered in this manuscript is mostly one-directional: [L96-97] *"The aim*
419 *of this study is to understand why decisions made by modellers all over the world and*
420 *over the past decades have not led to more (or is it "any"?) progress in Arctic snowpack*
421 *modelling"*, combined with the statement *"I'm sick of modelers who think the world is a*
422 *computer screen"*. There are more than 80 quotes in the manuscript and it is
423 misleading of Reviewer 1 to isolate one quote and to claim that this is our message.
424 Reviewer 1 expects "proper scientific conduct" (L193 above) to be adopted by the
425 authors; we expect the same from the reviewers.

426 **No further comment.**

427

428

428 6. I so wish the authors would have written "by the Arctic snow community" instead of
429 "by modellers". I found this diversity, equity and inclusion aspect overwhelmingly
430 missing from the manuscript. I will further detail my sentiments here in the "Epilogue"
431 below. We will reword our aim or/and provide clarification regarding the different uses
432 of "modeller", aligning with Reviewer 1's Minor comment #3.

433 **The aim was reworded. We also clarified what was meant by "modeller" L156-158 and**
434 **modified some instances where "modeller" was used instead of "scientist" or "participant"**
435 **and vice versa.**

436

437 Minor comments:

438 1. Several statements and wordings are vague.

439 ○ [L96-97] *“The aim of this study is to understand why decisions made by*
440 *modellers all over the world and over the past decades have not led to more*
441 *(or is it “any”?) progress in Arctic snowpack modelling.”* See also my major
442 concern #3. I think more effort is needed to document and quantify the
443 progress that has been made, such that it can be objectively concluded
444 whether or not this constitutes “progress”. As it stands, this statement
445 carries little weight. In fact, the problems with snow modeling in the Arctic
446 are poorly introduced in the manuscript. Only L88-95 discuss this aspect, but
447 only very marginally.

448 This was already answered. See L292-297 above and L561-567 below of this
449 reply.

450 No further comment.

451 ○ [L294-296] *“When I speak to large scale modellers about rain on snow, the*
452 *feedback is usually ‘we are aware that something needs to be done, but we*
453 *have other priorities and we don’t have resources for this’. It’s not*
454 *straightforward.”*

455

456 I think I understand what this is about because of my expertise, but for
457 reaching a broader audience, it should be made explicit. Please specify what
458 the issues with rain-on-snow are. Is it the precipitation phase separation rain
459 vs snow, is it the runoff from a snowpack, is it the formation of ice lenses?
460 Also, academia is almost fully project driven, so why not write a proposal or
461 provide funding otherwise for a model developer to work on improving the
462 “rain-on-snow” problems in a model? I think this also relates to my major
463 concern #3, listed above, regarding missing context.

464 We will clarify.

465 More context about rain on snow is provided in the Methods section L129-
466 132. Please note that the quote about rain on snow is to illustrate the
467 broader point of the paragraph, which is about prioritisation.

468 Regarding *“why not write a proposal or provide funding otherwise for a*
469 *model developer to work on improving the “rain-on-snow” problems in a*
470 *model?”*, this question is directly addressed in the paragraph about funding
471 that immediately follows the quote mentioned by Review . The answer is in
472 the manuscript: *“really good and important science will not always be funded*
473 *because there’s not enough money to go around”* . That there is no funding
474 for something does not always mean that funding has not been sought; it
475 sometimes mean that it has not been found. We have clarified this by adding
476 to this quote: *“We’ve had trouble getting funding to do the work”, [but]*

477 *“really good and important science will not always be funded because there's*
478 *not enough money to go around”.*

479 ○ [L372-373] *“the first thing it would do is alert the modelers to the difficulties*
480 *that they have in the Arctic that, in the absence of these evaluations, they*
481 *wouldn't even know about...”* Please provide examples. The statement
482 suggests that the interviewee knows about difficulties that the modelers
483 supposedly don't know about. I deem it inadequate to publish a paper with
484 statements like that, without sufficient backing up of examples, preferably
485 using peer-reviewed literature. As mentioned previously, we are interested in
486 how the opinion of decision-makers - in other words their truth based on
487 their experience, expertise and perspective - inform their decisions. This
488 quote is about the need to implement a Tundra-SnowMIP and is consistent
489 with one of the aims of the previous SnowMIP i.e. ESM-SnowMIP, which was
490 to “identifying previously unrecognized weaknesses in these models”
491 (Krinner et al., 2018)

492 **No further comment.**

493 ○ [L310-311] *“I mean, the idea that you're going to create an arctic snow model*
494 *in a PhD is...?!”*

495 This is an incomplete sentence, and I'm not sure what I need to fill in at the
496 “...?!”. Please add some explanation here.

497 **We will. Done**

498 ○ [L537-538] *“Some users of [our model], they probably don't know what*
499 *they're doing, and sometimes a paper comes where I say ???”*

500

501 Please fill in the “???” here. With my social background, I think I understand
502 what “???” and “?!” is supposed to indicate, but for non-native English
503 speakers, I think there is a risk here that they don't get the implicit message.

504 **We will. The quote was removed.**

505 2. There were a few quotes that I think are wrong, and I wonder if there should not be an
506 editorial comment that the statement is deemed inaccurate.

507 ○ For example, looking at the publications involving Crocus over the last 10 years,
508 I don't think the statement [L237] *“But, I mean Crocus, it's an avalanche model,*
509 *right?”* is accurate.

510 ○ Similarly, [L282-L284] *“[Models] are limiting the number of [snow] layers for*
511 *computational stability and efficiency so they are not respecting the way in*
512 *which the snow pack is actually built up i.e. in episodic snowfall events, which*
513 *will form different layers (...)”*. For models like Crocus and SNOWPACK, it is
514 trivially easy to avoid a limiting number of snow layers. I think it is important to
515 make an editorial remark, since otherwise, false information gets propagated.

516 **This was already answered. See L122-159 above.**

517 3. Extensive use of the term “Modeller”: I’m not sure the word “modeler” is meaningful.
518 Even the authors seem to have an ambivalent definition, defining it both as “model
519 developer” [L127] as well as “with expertise in modeling” [L128]. I think there is a
520 substantial difference between both. Note that in L132, both SPM and LSM “modelers”
521 are defined as “model developers”. Personally, I think labeling someone as a “modeler”
522 often attaches an identity to an individual, where this is not justified. It also has unclear
523 meaning. Is it someone who uses the model, or someone who develops for the model,
524 or is it someone who maintains the model code? Is someone who has used a model
525 once in their research career already a “modeler”, or is it someone who uses models in
526 more than, let’s say, 50% of their research? I would rather like to see more exact
527 wording being used, specifically focusing on the role someone has. Like “model user”,
528 “model developer” or “model maintainer”. I think IPCC rightfully avoids the word
529 modeler (referring to L546). But thinking about roles avoids attaching an identity to a
530 researcher, while allowing to encapsulate the common situation where researchers can
531 take up different roles during their career, or even within a single project.

532 We will provide more exact wording.

533 See answer L429-431.

534 4. [L427-428]: “We argue that efforts to represent Arctic snowpack processes would pave
535 the way in the research areas highlighted below for new interdisciplinary
536 collaborations”. What follows are three rather specific research directions. Not that I
537 want to argue about their relevance, it is just missing context why those three are
538 listed, who has set these priorities? Did this come out of the interviews as well?

539 They did. We will clarify.

540 We clarified in the first paragraph of Section 4.1.

541

542 Epilogue

543 I also would like to stress that the manuscript contained quite some material that to me
544 came across as somewhat “aggressive”. I would like to make the authors aware that it left
545 me with the impression of a poorly working field, with a lack of communication,
546 collaboration and a missing cooperative mindset.

547 We note Reviewer 1’s concern.

548 As mentioned L169-172 of this response, we provided more context to some of the quotes
549 (see also for example L707-717 in the revised manuscript). As mentioned in other parts of
550 this answer, Section 3.2.2 Adaptability was always about the importance of collaboration in
551 snow modelling and about how adaptable the snow modelling community is. To further
552 emphasise this point, we provided more context in this section regarding the IVORI project,
553 which was borne out of interdisciplinary conversations and collaborations. It was first
554 mentioned in the previous version of the manuscript in Section 3.2.3. to highlight the
555 differences of opinions about the value of starting models from scratch.

556 A new Section (4.2. A plurality of strategies) emphasises that the differences of opinions
557 expressed in this manuscript are necessary because they “provide different representational
558 perspectives (Morrison, 2021) to investigate the same phenomenon” (L623-625 in the
559 revised manuscript).

560 Finally, as quotes were not attributed to the group the quoted participant belonged to,
561 there may have been an impression that many quotes were criticism of snow modellers
562 from non-snow modellers. This was not the case and we have now attributed each quote to
563 specific group, thus making it clearer that most quotes are reflections upon one’s own
564 community.

565 Below, Reviewer 1 expressed concerns about the manuscript not fostering a healthy,
566 welcoming, open environment and objects to specific quotes being used. Reviewer 1 also
567 accuses us of “heavy cherry-picking”, of making up data (“these sorts of things apparently
568 have been said in the interviews”), of misleading the participants (“Maybe the interviewees
569 expressed themselves somewhat awkwardly because they also felt like they were in an
570 informal private conversation”), of having no consideration for equ(al)ity, diversity and
571 inclusion. These are very strong accusations of data falsification, manipulation and selection
572 i.e. of instances of research misconduct. We hope that Reviewer 1 understands that they
573 were mistaken, now that we have clarified that (1) these quotes are not presented as
574 “truths” but as opinions that contribute to informing decisions, (2) these quotes illustrate
575 the themes that were identified during the thematic analysis, (3) that the themes are about
576 decision making and therefore serve to answer our research question, and (4) qualitative
577 data (here the quotes) *are* data and that this is a research paper which followed established
578 methodologies. As mentioned above, we will revise the manuscript to ensure that this
579 process is clear to all readers.

580 We would also like to mention that the review process is not an “open” environment either.
581 While Copernicus publications are leaders in the peer-review process and have dramatically
582 improved reviewing by making it open-access, reviewers still can, as is the case for Reviewer
583 1, remain anonymous; a choice we, of course, respect. Nevertheless, there is a power
584 imbalance in single-blinded reviews (see e.g. Manchikanti et al., 2015; Parmanne et al.,
585 2023) and with power comes responsibility. We trust that this responsibility includes not
586 accusing authors of misconduct until having given them the opportunity to prove otherwise.

587 **No further comment.**

588 Examples:

589 [L182] “I’m sick of modelers who think the world is a computer screen”
590

591 In fact, many scientists have no other choice but to focus on modeling, since fieldwork in
592 polar regions is generally poorly accessible (Nash et al., 2019, Karplus et al., 2022). I know
593 scientists who would give an arm and a leg to go to the field just once, and probably doing
594 so would increase the quality of their model development efforts considerably. The phrasing
595 of this statement suggests that the scientist never considered that they could have made an
596 effort to bring the “modelers who think the world is a computer screen” in closer contact

597 with the real world, instead of saying that they are “sick” of them. This was already
598 addressed L366-378 of this reply.

599 [L184-185] *“The[se] models spend so much time doing things that aren't very important for*
600 *lots of applications that they're kind of worthless”*

601

602 Claiming that work done by fellow scientists is worthless, because it doesn't fit one's own
603 needs, is detrimental to a healthy, open and welcoming academic atmosphere I think.

604 We wish to clarify that almost half of the quotes used in this manuscript are from modellers
605 reflecting on their own practice and community (hence L569 “the novelty here is that it is an
606 insider's job. It is a reflective exercise”). As mentioned L159 in the manuscript, we decided
607 not to indicate which quotes came from which group unless necessary to improve
608 understanding of the context within which they were cited. We understand thanks to
609 Reviewer 1's comments that we must revise this decision and be clearer about which group
610 the quotes came from. We hope that it will make it clearer that the manuscript is not a
611 criticism of modellers, but a reflective process that includes modellers and other members
612 of the Arctic snow community.

613 **See L561-565 of this response.**

614

615 [L537-538] *“Some users of [our model], they probably don't know what they're doing, and*
616 *sometimes a paper comes where I say ???”*

617

618 First of all, I'm not really sure what I have to fill in at the “???” , but I assume it is some
619 negative sentiment. In these cases, reaching out to those users can be of great help to the
620 users, and would foster exchange of knowledge, and, again, an open and welcoming
621 academic environment.

622 We make it clear in Section 3.2.2 Adaptability that modellers do collaborate extensively.
623 Reviewer 1's comment proposes a solution to an issue that our manuscript identified. As we
624 wrote in the Conclusion, *we hope that this reflective exercise will be the start rather than the*
625 *end point of the conversation*. For example, the EDI issues in fieldwork that Reviewer 1
626 highlighted are only starting to be tackled because recent papers have exposed these issues
627 and those who want to change the system now have academic papers to back their
628 initiatives. As highlighted in the Conclusion, we argue that our manuscript serves a similar
629 purpose. It addresses issues that are well-known but have remained hidden in the literature.
630 Visibility is key to changing practices and our manuscript contributes to making some of the
631 issues more visible in order to address them.

632 **No further comment.**

633

634 [L374-375] *“In my sense, large scale climate modellers aren't sufficiently aware of snow. (...)*
635 *There are so many people who don't care about that”*

636

637 I find this quite the accusation that those people don't care. Please provide evidence that
638 they don't care, for example from reviews of proposals and/or manuscripts. Did papers in
639 fact get rejected, because reviewers claim that snow is irrelevant? See L532-538 in the
640 manuscript for examples provided by other participants of how snow is treated in some

641 manuscript using large scale models. Quote attribution in the revised manuscript now show
642 that this quote is from a large scale modeller. We also modified the text L532-538 (now
643 L707-717) to clarify how the review process at times overlook snow in large scale models.
644 I'm skeptical that that is the case.

645 [L96-97] *"The aim of this study is to understand why decisions made by modellers all over*
646 *the world and over the past decades have not led to more (or is it "any"?) progress in Arctic*
647 *snowpack modelling, ..."*

648

649 I understand that the phrasing "(or is it "any"?)" is catchy, but it comes across a bit as
650 dismissive towards publications from, let's say, the last 10 to 20 years, documenting
651 improvements in modeling approaches, some of which are cited in the manuscript. I would
652 strongly encourage more precise wording. We agree. As mentioned, L384-385 of this reply,
653 the aim will be reworded. Done.

654 Which objective has not been achieved (yet)? The statement that directly precedes *"The*
655 *aim of this study etc"* answers this question *"No ESM, so far, simulates these Arctic*
656 *snowpack processes"*. As already stated above, we will ensure that sufficient information is
657 provided for the reader to have enough context, but we maintain this statement to be
658 accurate with regards to the representation of the snow profile of Arctic snowpacks, vapour
659 fluxes and ice crust formation in ESMs. We welcome references from Reviewer 1 that could
660 inform us otherwise.

661 No further comment.

662

663 All the points below have already been made by Reviewer 1 and addressed by the authors
664 multiple times. There will, therefore, be no further comments. No further comment.

665 No further comment.

666

667 Also, this phrasing implies that "modelers" are to blame for the supposedly slow progress. In
668 fact, the manuscript discusses very few decisions made by "modelers" (interpreted by me
669 here as model developers). And also in light of the sentences I have listed above, I think this
670 is unfair. There seems to be a lack of healthy collaboration in the field. I am also aware that
671 there is also a big issue with accessibility (diversity and inclusion) to fieldwork, that in my
672 opinion plays a role here.

673 There are also funding agencies, and hiring decisions that I think are to blame for a lack of
674 resources for model development. Some of those are addressed in the manuscript, some of
675 those are not. But it would have been better to phrase the aim of the study as: *"The aim of*
676 *this study is to understand why decisions made by the Arctic snow community all over the*
677 *world and over the past decades have not led to more progress in Arctic snowpack*
678 *modelling, ..."*

679 I put this feedback as "Epilogue", because for me, it is not relevant to whether or not the
680 manuscript could be published as a scientific research article, but I hope the authors
681 become aware that including statements like these, unfortunately left me with the

682 impression that the field of Arctic snow is a somewhat unhealthy environment, with some
683 missing collaborative mindset. In a way, I think it's already a problem that these sorts of
684 things apparently have been said in the interviews, but maybe this was simply the heat of
685 the moment. Maybe the interviewees expressed themselves somewhat awkwardly because
686 they also felt like they were in an informal private conversation. It is also very possible that
687 context or tone went missing in the transcription and the quote selection for the
688 manuscript.

689 One could argue that it may be important to report about such sentiments in the field, since
690 it can signal problems hindering progress. However, it would require proper context,
691 including identifying this as a problem, and proposing pathways forward to resolve such
692 conflicts. I think that the authors should seriously consider the purpose, and effect, of
693 including statements like these in the manuscript.

694 In my opinion, it doesn't reflect well on the Arctic snow community, and I refuse to believe
695 that this is the message the authors wanted to get across.

696
697
698

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- 743 All other references can be found in the manuscript.

1 Response to Reviewer 2

2 We thank Reviewer 2 for supporting this manuscript and for providing us with detailed comments.
3 We also thank them for deepening our knowledge on philosophical literature on decision-making in
4 modeling. Having now read the suggested papers, we agree that they are important to our paper
5 and that they will help frame our discussions further.

6 This paper covers an important topic—how decisions are made in the context of modeling of Arctic
7 snowpack. By providing insight into the influences on decisions throughout the modeling process the
8 paper contributes to a minimally understood feature of modeling practice, as in most cases, the
9 decision-making is not explicitly documented, nor are the reasons that justify particular decisions.
10 However, there are some issues with the manuscript that need to be addressed:

11 I urge the authors to consider some of the philosophical literature on decision-making in modeling,
12 which mainly concerns climate models but applies to the discussions and perhaps the
13 interpretations of some of these qualitative findings.

14 1. Several philosophers working on issues in climate science have detailed how values (i.e.,
15 interests) influence decision-making through the course of model development, but none of
16 that literature is referenced here despite the high relevance to the topic of discussion. I
17 recommend looking at Parker and Winsberg (2018), Parker (2014), and Morrison (2021),
18 specifically chapter 3 of the latter. The research by these scholars discusses how interests
19 (subjective preferences) and features of the modeling context (pragmatics) influence decision-
20 making in the course of climate modeling, including those choices of determining modeling
21 purposes and priorities, what and how to represent features of the target system, the
22 suitability of observations and metrics for model assessment and validation, etc. The authors
23 might also consider looking at the Shackley article “Epistemic Lifestyles in Climate Change
24 Modeling” (2001). I suggest adding elements from these papers to the paragraph starting in
25 line 64 or including an additional paragraph to capture the discussions in philosophy on these
26 topics. You might also find that the insights from certain papers are relevant to specific
27 sections as well (for example, Parker 2014 for section on data available and resources.

28 We will add these references throughout our paper. We will add a subsection in Section 4 that
29 will frame our findings within topics discussed in these and other papers suggested below by
30 Reviewer 2.

31 We added most of the papers cited by Reviewer 2 (e.g. Levins, 1966; Longino, 2002; Winsberg,
32 2012; Morrison, 2021) in this review. We added others throughout our revised manuscript,
33 which we found relevant during our readings prompted by this review (e.g. Parker, 2011;
34 Rudner, 1965; Walmsley, 2021; Winsberg, 1999, 2021). We also included an additional
35 subsection “4.2 A plurality of strategies”, in which many of the topics addressed in this review
36 are discussed.

37 2. And, concerning tradeoffs, see work by Levins, mainly “The Strategy of Model-building in
38 population biology” (1966). I note that the subject of modeling is different, but Levins’ thesis
39 applies to the modeling of complex systems generally and is thus related to the discussion in
40 3.1.1.

41 We will consider Levins’ strategies in the new subsection in Section 4.

42 Done (Section 4.2).

43 Concerning the disagreement about models being “good enough” for current research
44 problems—an article deals with similar disagreement about the value of different modeling
45 systems in relation to different sets of research questions by Lloyd, Bukovsky, and Mearns
46 (2020). The authors here argue that the reason for disagreements about the value of regional
47 versus global models is because they have different research questions and the
48 representational features of the models are different. So they don’t take the representational
49 features of one type of model to be valuable for their questions, and vice versa. Wonder if
50 something similar here is going on, thus this frame might be useful...and might even be useful
51 for analyzing the lack of unanimity in the responses to the questions that were asked. They
52 have different interests, are asking different questions, and have different local epistemologies
53 (Longino 2002 and Morrison 2021). (Where the authors talk about identity, this seems akin to
54 local epistemologies.)

55 We will add references and discussions by philosophers of science in the additional subsection
56 to help reframe our discussion.

57 See answer above L31-36.

58 Regarding identity and local epistemologies (LE): We agree that considerations about LE are
59 relevant to the paper and this will be considered in the next version of the manuscript.
60 However, we draw our analysis on research identity from numerous studies on academic and
61 research identities in the field of education studies (e.g. Valimää,1998; Clegg, 2008;
62 Fitzmauritz, 2013; Borluag et al., 2023). For example, what philosophers of science call LE are
63 akin to disciplinary identity in education studies (e.g. Dressen-Hammouda, 2008). Therefore,
64 rather than being akin to identity, we believe that LE are *part of* identity construction, i.e. they
65 are the “*processes of identification with diverse groups and communities*” in the McCune
66 (2019) definition quoted in the manuscript, as are values. In addition, considering our findings
67 in terms of LE would imply that all participants within the same group would agree (as per the
68 examples in the cited papers “regional” vs “global” climate modellers), which is not the case.
69 There was a lot of within group disagreement, which we will made clearer in the next version
70 of the manuscript by attributing quotes to specific groups.

71 We added the references above at L60-61 to Section 3. A discussion about identity and local
72 epistemologies is now included in Section 4.2.

73

74 Appreciate the content-context distinction, however, I wonder if you can separate them, and would
75 appreciate more consideration of the way research context, understood more generally than
76 “identity” in the paper, shapes perception of modeling practice, etc.

77 Please see above regarding “identity”.

78 Regarding the content-context distinction, we believe it is necessary, because, as written in the
79 conclusion of the paper, “*while the written history narrated by our publications does record the*
80 *arguments presented here in the content section, it does not record what is presented in the context*
81 *section*”. Our paper was submitted to The Cryosphere (TC), a journal which, as far as we are aware,
82 has never published a research paper based on qualitative methodologies. We chose to submit the
83 paper to TC because the TC readership is the audience we want to engage with our paper because,
84 as written L569-570 of the paper “*it is an insider’s job. It is a reflective exercise which, we hope, will*
85 *be the start rather than the end point of the conversation*”. As shown by Reviewer 1’s comments, we
86 must expect that some of the readership will be unfamiliar with these methodologies, therefore we

87 must ensure that they recognise some of the findings (Content) or they may disengage with the
88 broader discussion (Context and Moving forward).

89 **No further comment.**

90 I am also not sure whether the analysis from Staddon (2017) on the distinction between professional
91 and personal is fitting here

92 We agree. The Staddon excerpt was echoed in Section 4.3 “Values and positionality” where it was
93 referenced again, although not explicitly “*Values are another construct to a researcher's identity, but*
94 *the prevailing notion linking value-free science with objectivity and impartiality (Pulkkinen et al.,*
95 *2021) presents obstacles to achieving greater transparency in bridging the gap between our personal*
96 *identities and our professional decisions.* “. We will either reference it more explicitly the second
97 time or remove it.

98 **We moved the reference to Staddon (2017) to Section 4.2. where it is now a better fit.**

99 Again, I think these responses are a function of differences in the context in which these individuals
100 conduct research and the local epistemologies they are part of. For example, with “I’m sick of
101 modelers who think the world is a computer screen” this is a rejection of the attitude of being
102 focused on the modeling world as opposed to the empirical world, which can be reduced to
103 differences in one’s scientific ontology and epistemic values. And “these models spend so much
104 time...” this can be interpreted as someone who is more of a pluralist about models and their
105 application, as opposed to part of the paradigm by which models are seen as fit-for-purpose for a
106 limited number of intentionally chosen applications....in other words, it’s not necessarily the
107 “identities” of the researchers that come out in these quotes, but rather, the diversity of local
108 epistemologies that can be found in Arctic modeling, and the disagreement that arises from this
109 diversity. I appreciate the information in the intro to section 3 but think you could do more to shed
110 light on the significance of sharing these sorts of quotes from your interviews. A different frame for
111 your discussion might add depth and significance.

112 We agree with Reviewer 2. We will return to the quotes used in the intro to section 3 in Section 3.2
113 or 4 and will frame them within a broader discussion about the constructs of researcher identity
114 (which include values and local epistemologies; see above for details).

115 **This is now discussed in Section 4.2, where some quotes e.g. “I’m sick of modelers who think the**
116 **world is a computer screen” are discussed within the context of research and disciplinary identities,**
117 **and local epistemologies.**

118

119 In the same vein as the above comments, I think philosophical discussions can help to frame your
120 results. For example, the somewhat reductive interpretation of the quote at the beginning of 3.2.1.:
121 prioritization is a feature of scientific practices, including modeling, being driven by human interests,
122 and certain elements of the complex systems we investigate being more or less important relative to
123 those interests. While resources are limited, human beings are also inherently value-driven, and if
124 they don’t perceive something as related to their interests, they will deprioritize it, and yes, the
125 practical constraints make this more apparent, but aren’t the sole cause of prioritization in science.
126 There are an infinite number of questions we could ask, and we will see value in some and ignore
127 others. I think this is what the quote is getting at you have chosen here, with the “we have other
128 priorities” AND “we don’t have resources”, i.e., there are two reasons for not tackling the problem,
129 one is, it is inconsistent with what they care about in modeling, and second, there aren’t resources,

130 and these compound one another. Longino’s discussions of modeling complex systems in her 2002
131 book would be helpful here. This is an example of one place in the manuscript where the
132 interpretation of qualitative evidence can be aided by appealing to philosophical discussions from
133 the philosophy of science in practice (i.e., Longino and others have done empirical studies to draw
134 their conclusions, it’s not “armchair” analysis).

135 We agree with Reviewer 2. These considerations will be addressed in the new additional subsection.

136 Done.

137 The comments on short-termism are incredibly important, appreciate their explicit inclusion, and
138 wonder if more can be said about the implications of this current paradigm in funding procedures...

139 We will provide more context around the comments on short-termism.

140 We expanded the section on short-termism (L385-402).

141

142 I am a bit confused about the discussion of the anchoring bias...it appears a bit vague in what the
143 bias is in itself, and I am not sure that the explanation in the first paragraph makes it clear what it is.
144 I think it is the judged adequacy of the models, based on historical model features and development,
145 in relation to some purpose, which can shift when one’s interests or research questions change
146 (which the authors hint at in lines 375–379). I think this is what is being said also in the case that
147 community efforts can lead to shifts in these anchors...community comparison projects foster
148 interdisciplinary discourse on model capabilities and limitations, which can presumably highlight
149 inadequacies in relation to priority research questions. This section could be clearer, especially with
150 respect to what it is about the existing models that function as a reference point for judging the
151 value of different future development efforts. The section should also conclude with a clear
152 summary of the argument the authors seek to make given the statement in the first paragraph:
153 “anchoring contributed largely to the absence of Arctic snow processes in existing models”.

154 The interpretation of Reviewer 2 is correct and we will clarify this in subsection 3.2.3.

155 Done.

156 In conclusion, this is a valuable study and provides significant empirical insight into understudied and
157 implicit components of modeling of climate features generally. However, I think work needs to be
158 done with the framing of the findings from the study and their discussion. I strongly suggest bringing
159 in philosophical work on modeling to help add depth and detail to the discussion.

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