

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

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

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

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

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

30 2. And, concerning tradeoffs, see work by Levins, mainly “The Strategy of Model-building in
31 population biology” (1966). I note that the subject of modeling is different, but Levins’ thesis
32 applies to the modeling of complex systems generally and is thus related to the discussion in
33 3.1.1.

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

35 Concerning the disagreement about models being “good enough” for current research
36 problems—an article deals with similar disagreement about the value of different modeling
37 systems in relation to different sets of research questions by Lloyd, Bukovsky, and Mearns
38 (2020). The authors here argue that the reason for disagreements about the value of regional
39 versus global models is because they have different research questions and the
40 representational features of the models are different. So they don’t take the representational
41 features of one type of model to be valuable for their questions, and vice versa. Wonder if
42 something similar here is going on, thus this frame might be useful...and might even be useful
43 for analyzing the lack of unanimity in the responses to the questions that were asked. They
44 have different interests, are asking different questions, and have different local epistemologies

45 (Longino 2002 and Morrison 2021). (Where the authors talk about identity, this seems akin to
46 local epistemologies.)

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

49 Regarding identity and local epistemologies (LE): We agree that considerations about LE are
50 relevant to the paper and this will be considered in the next version of the manuscript.
51 However, we draw our analysis on research identity from numerous studies on academic and
52 research identities in the field of education studies (e.g. Valimää,1998; Clegg, 2008;
53 Fitzmauritz, 2013; Borluag et al., 2023). For example, what philosophers of science call LE are
54 akin to disciplinary identity in education studies (e.g. Dressen-Hammouda, 2008). Therefore,
55 rather than being akin to identity, we believe that LE are *part of* identity construction, i.e. they
56 are the “*processes of identification with diverse groups and communities*” in the McCune
57 (2019) definition quoted in the manuscript, as are values. In addition, considering our findings
58 in terms of LE would imply that all participants within the same group would agree (as per the
59 examples in the cited papers “regional” vs “global” climate modellers), which is not the case.
60 There was a lot of within group disagreement, which we will made clearer in the next version
61 of the manuscript by attributing quotes to specific groups.

62

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

66 Please see above regarding “identity”.

67 Regarding the content-context distinction, we believe it is necessary, because, as written in the
68 conclusion of the paper, “*while the written history narrated by our publications does record the*
69 *arguments presented here in the content section, it does not record what is presented in the context*
70 *section*”. Our paper was submitted to The Cryosphere (TC), a journal which, as far as we are aware,
71 has never published a research paper based on qualitative methodologies. We chose to submit the
72 paper to TC because the TC readership is the audience we want to engage with our paper because,
73 as written L569-570 of the paper “*it is an insider’s job. It is a reflective exercise which, we hope, will*
74 *be the start rather than the end point of the conversation*”. As shown by Reviewer 1’s comments, we
75 must expect that some of the readership will be unfamiliar with these methodologies, therefore we
76 must ensure that they recognise some of the findings (Content) or they may disengage with the
77 broader discussion (Context and Moving forward).

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

80 We agree. The Staddon excerpt was echoed in Section 4.3 “Values and positionality” where it was
81 referenced again, although not explicitly “*Values are another construct to a researcher’s identity, but*
82 *the prevailing notion linking value-free science with objectivity and impartiality (Pulkkinen et al.,*
83 *2021) presents obstacles to achieving greater transparency in bridging the gap between our personal*
84 *identities and our professional decisions.* “. We will either reference it more explicitly the second
85 time or remove it.

86 Again, I think these responses are a function of differences in the context in which these individuals
87 conduct research and the local epistemologies they are part of. For example, with “I’m sick of

88 modelers who think the world is a computer screen” this is a rejection of the attitude of being
89 focused on the modeling world as opposed to the empirical world, which can be reduced to
90 differences in one’s scientific ontology and epistemic values. And “these models spend so much
91 time...” this can be interpreted as someone who is more of a pluralist about models and their
92 application, as opposed to part of the paradigm by which models are seen as fit-for-purpose for a
93 limited number of intentionally chosen applications....in other words, it’s not necessarily the
94 “identities” of the researchers that come out in these quotes, but rather, the diversity of local
95 epistemologies that can be found in Arctic modeling, and the disagreement that arises from this
96 diversity. I appreciate the information in the intro to section 3 but think you could do more to shed
97 light on the significance of sharing these sorts of quotes from your interviews. A different frame for
98 your discussion might add depth and significance.

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

102

103 In the same vein as the above comments, I think philosophical discussions can help to frame your
104 results. For example, the somewhat reductive interpretation of the quote at the beginning of 3.2.1.:
105 prioritization is a feature of scientific practices, including modeling, being driven by human interests,
106 and certain elements of the complex systems we investigate being more or less important relative to
107 those interests. While resources are limited, human beings are also inherently value-driven, and if
108 they don’t perceive something as related to their interests, they will deprioritize it, and yes, the
109 practical constraints make this more apparent, but aren’t the sole cause of prioritization in science.
110 There are an infinite number of questions we could ask, and we will see value in some and ignore
111 others. I think this is what the quote is getting at you have chosen here, with the “we have other
112 priorities” AND “we don’t have resources”, i.e., there are two reasons for not tackling the problem,
113 one is, it is inconsistent with what they care about in modeling, and second, there aren’t resources,
114 and these compound one another. Longino’s discussions of modeling complex systems in her 2002
115 book would be helpful here. This is an example of one place in the manuscript where the
116 interpretation of qualitative evidence can be aided by appealing to philosophical discussions from
117 the philosophy of science in practice (i.e., Longino and others have done empirical studies to draw
118 their conclusions, it’s not “armchair” analysis).

119 [We agree with Reviewer 2. These considerations will be addressed in the new additional subsection.](#)

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

122 [We will provide more context around the comments on short-termism.](#)

123 I am a bit confused about the discussion of the anchoring bias...it appears a bit vague in what the
124 bias is in itself, and I am not sure that the explanation in the first paragraph makes it clear what it is.
125 I think it is the judged adequacy of the models, based on historical model features and development,
126 in relation to some purpose, which can shift when one’s interests or research questions change
127 (which the authors hint at in lines 375–379). I think this is what is being said also in the case that
128 community efforts can lead to shifts in these anchors...community comparison projects foster
129 interdisciplinary discourse on model capabilities and limitations, which can presumably highlight
130 inadequacies in relation to priority research questions. This section could be clearer, especially with
131 respect to what it is about the existing models that function as a reference point for judging the
132 value of different future development efforts. The section should also conclude with a clear

133 summary of the argument the authors seek to make given the statement in the first paragraph:
134 “anchoring contributed largely to the absence of Arctic snow processes in existing models”.

135 [The interpretation of Reviewer 2 is correct and we will clarify this in subsection 3.2.3.](#)

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

140 References:

141 Levins, R. (1966). The strategy of model building in population biology. *American scientist*, 54(4),
142 421-431. (see also, for updated discussions: Weisberg, M. (2006). Forty years of ‘the strategy’: Levins
143 on model building and idealization. *Biology and Philosophy*, 21, 623-645. and Matthewson, J. (2011).
144 Trade-offs in model-building: A more target-oriented approach. *Studies in History and Philosophy of
145 Science Part A*, 42(2), 324-333.)

146 Lloyd, E. A., Bukovsky, M., & Mearns, L. O. (2021). An analysis of the disagreement about added
147 value by regional climate models. *Synthese*, 198(12), 11645-11672.

148 Longino, H. E. (2002). *The fate of knowledge*. Princeton University Press. (See chapter 8 for local
149 epistemologies and differences between different investigative communities, which is relevant to
150 your discussion.)

151 Morrison, M. A. (2021). *The models are alright: A socio-epistemic theory of the landscape of climate
152 model development*. Indiana University.

153 Parker, W. (2014). Values and uncertainties in climate prediction, revisited. *Studies in History and
154 Philosophy of Science Part A*, 46, 24-30.

155 Parker, W. S., & Winsberg, E. (2018). Values and evidence: how models make a difference. *European
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168 Higher Education*, 38, 613-622, DOI: 10.1080/03075079.2011.594501, 2013.](#)

169 [Välilmaa, J., Culture and Identity in Higher Education Research. *Higher Education*, 36, 119–138, 1998.](#)