

1 **Reply to Anonymous Referee #1 comment on egusphere-2023-2523,**  
2 **doi:10.5194/egusphere-2023-2523 (Sippel et al.)**

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4 [In the following the reviewer comments appear in black with the author responses in blue.](#)

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6 This study has investigated the likelihood of a re-occurrence of the extremely cold European  
7 winter of 1963 under present climate conditions and what such a winter would look like. To  
8 investigate this, the authors employ a range of techniques, some involve accounting for the  
9 dynamical features which gave rise to the extremely cold winter, while others are standard  
10 statistical methods, e.g. extreme value analysis. All the methods gave approximately similar  
11 answers, that such an event could occur today but is less likely, and if it did occur, the  
12 temperatures would be approximately 1.5 degrees warmer than the original 1963 event.

13 The paper provides a clear and thorough assessment of the theoretical occurrence of the 1963  
14 winter under today's climate. It is an interesting study, and would have been more interesting if  
15 they had found that the winter could not occur today. However, since the conclusion is that an  
16 extremely cold winter from the past is less likely to occur and would be warmer if it did occur  
17 under today's warmer climate is not especially groundbreaking. The paper does raise the issue  
18 of mal-adaption by society towards warmer winters, and I think this is very valuable. I also think  
19 the comparison of the methods will be of interest to the community. Overall, I would  
20 recommend the paper be published with minor revisions.

21 [We thank the reviewer for the positive evaluation of our study, and we provide a more detailed  
22 response to the issues raised below.](#)

23  
24 **General Issues**

25 1. While the result that the cold winter can still occur and would be warmer is not wholly  
26 surprising, the paper does makes a good point that society may be adapting under the  
27 assumption of warmer winters while extremely cold winter are still very possible. I think  
28 this could be raised up in the paper. Perhaps introducing the idea as part of the  
29 motivation for the work so it is in the reader's minds as they go through.

30 [Important point. We will provide a more solid case for a risk of mal-adaptation in the  
31 Introduction.](#)

32 2. It is a potentially provocative finding that the unconditional statistical method of using a  
33 fitted GEV gave as good a result as methods which incorporated knowledge of the  
34 dynamics. Many statistical based studies are criticized for not including knowledge of the  
35 dynamics of a situation, and here we have a case where this knowledge provided no  
36 additional benefit, and for an extreme event no less. Obviously, this is a single case and it  
37 could be random chance that the statistical method did so well. Given that the authors  
38 have used so many different methods, I think it would be a nice addition to the paper for  
39 the authors to briefly comment on how the methods compare, especially the  
40 unconditional statistical method with the methods incorporating knowledge of the  
41 dynamics.

42 [There is indeed an important debate that discusses different conditional and unconditional  
43 approaches to event attribution. We agree with the reviewer that the overall similar results may  
44 occur by chance. But it may be noted nonetheless: The statistical method is fitted based on a long  
45 1900-2023 period, with thus probably a relatively small dynamical trend over this long time  
46 scale \(hence the thermodynamical part likely dominates also the unconditional analysis, which](#)

47 may partly explain the overall similar results). Interestingly, if the statistical method would be  
48 fitted on a shorter 1950-2023 period, the warming estimated obtained for a 1963-like winter  
49 would be much larger, because of the dynamical trend on this decadal time scale. We will explain  
50 this in more detail in the revised manuscript.

51 3. The title is a little sensationalist and I'd suggest changing it to match the style of the WCD  
52 journal.

53 Thank you for this feedback. We will consider changing the title in a revised manuscript.

54 4. The second paragraph in the Results section discusses the failure of models to show  
55 pronounced forced changes in atmospheric circulation. This is not a topic that is really  
56 investigated by this study. CESM2 simulations were run to perform the model boosting  
57 analysis, but there is no assessment of how or why or to what extent CESM2 fails to show  
58 forced changes. The discussion reads more like a commentary on the failure of models in  
59 this particular aspect, and does feel connected to the rest of the study. If this is a main  
60 motivation or theme in this study, then this failure of models should be introduced in the  
61 introduction and its implications on the use of model data in this study needs to be  
62 assessed, not merely commented on. Without expanding on the issue raised in this  
63 paragraph and incorporating it more fully into the study, I'd recommend removing the  
64 paragraph, more specifically, sentences from line 204 to 212.

65 Thank you for highlighting this important point. Indeed it is not the goal of this study to  
66 investigate the ability or failure of models with respect to simulating multi-decadal trends in  
67 atmospheric circulation. There are many other studies that look at this phenomenon, and we cite  
68 them (Blackport and Fyfe 2022, Faranda et al., 2023). But we also stress that it is currently not  
69 understood whether the observed circulation-induced trend is indeed forced or not. For this  
70 precise reason (uncertainty in forced dynamical changes), the conditional storyline approach  
71 was conceived (Shepherd ). In the context of our study, CESM2 shows only a very small  
72 ensemble mean circulation-induced trend over the historical period (Deser et al., 2023, Fig. 10c).  
73 Hence, we use CESM2 to explore the worst-case in the sense that the observed circulation-  
74 induced warming trend would be unforced, which would imply that very cold atmospheric  
75 circulation patterns, such as 1963, may still recur. We will make this point (and its implications)  
76 more clear in the revised paper.

77 5. Figure captions for figures 2, 3, and 6 need to be expanded to better explain what is being  
78 shown in the figures. This is especially true of figures 3 which is important for the  
79 paper.

80 We will expand substantially on the clarity and content of figure captions in a revised  
81 manuscript.

82

83 Minor Issues

84

85 24: You use the expression "led to". This suggests some precession in time, that the pressure  
86 anomalies occurred and then afterwards, there was a negative NAO anomaly. Perhaps change to  
87 "resulted in" or "comprised".

88 Correct, we did not mean precession in time, but indeed "resulted in" or "associated with".

89 65-90: The dynamical adjustment method. Is it reasonable to separate the dynamic and  
90 thermodynamic influences on surface temperature in such a linear way? Consider adding a  
91 sentence or two discussing the caveats or limitations of this assumption/approach.

92 Important point. In the dynamical adjustment literature, this separation in both component  
93 appears quite standard (Smoliak et al., 2015, Deser et al., 2016, Deser et al., 2023). But of course  
94 it needs to be acknowledged that this is an assumption that may bear some caveats for the  
95 interpretation.

96 78: "The first dynamical adjustment approach (dark blue line in Fig. 1) uses ERA5 to train the  
97 regression model, and the spatial pattern of sea level pressure (SLP) over a circulation domain  
98 over Europe and the North Atlantic." Possibly it is just my reading of the sentence, but it feels a  
99 little awkward. The 'and' feels like it is part of the regression statement. Consider changing this  
100 to "along with".

101 82: Start a new paragraph at "We use a second method..." It provides a cleaning break when  
102 reading. Possibly change to "We also use a second..."

103  
104 169-183: Please revise the structure of this paragraph. It jumps straight into what the method  
105 leads to and only describes the method itself towards the end of the paragraph. I suspect this is  
106 one of those cases where the author is so familiar with the method, he forgets that the reader  
107 may not understand what he is talking about from the beginning.

108 OK, thanks for highlighting these (all 3 comments above). We will clarify the text in a revision.

109 174-181: Please explain in the paragraph why you use the single coldest winter in December for  
110 the first boosting, but two coldest Januarys for the second boosting.

111 Our aim was to test (in the context of a "storyline") whether a winter of the 2020's could be even  
112 as cold as the observed 1963 winter. Therefore we started with the 1st to 15th December of the  
113 coldest December in the 2020's (there was a colder December in the 2000's which we did not  
114 boost). We then realized that most ensemble members of this coldest December in the 2020's  
115 were going back to the climatologically expected "normal" range in the following January, but  
116 there were two ensemble members that showed exceptionally cold (but almost identically cold)  
117 conditions in the following January. Because resources allowed it, we decided to boost both of  
118 these events, which yield relatively similar "coldest DJF of the 2020's" conditions.

119 176: Please write the dates out in full. Using "01.12 to 15.12" could be written as "1st to 15th  
120 December".

121 180: Again, write the dates out in full.

122 219: Remove the double brackets.

123 OK, thanks (to all 3 above).

124 245: The return period of the 1963 event was 119 years. For such an event to occur today, the  
125 return period would be 371 years. However, the uncertainty for the occurrence today is 97 to  
126 7680 years. That is to say, the return period of the event today may still be 119 years at the 95%  
127 level. This should be commented on.

128 We agree with the reviewer. The large uncertainty is due to the relatively small sample size in  
129 the observations (around 100 years), and thus is inherent to the statistical GEV approach.

130 251-252: "This indicates, incidentally, that the storyline approach is not providing larger effects  
131 of climate change compared to the probabilistic approach, or possibly exaggerating these effect."  
132 This sentence is confusing. Does this mean the storyline approach is 'not providing larger effects'?

133 or is exaggerating effects (i.e. to make larger)? These seems to suggest storyline approach either  
134 doesn't make the effects larger or it does make them larger. Please rephrase the sentence.

135 We agree with the reviewer that this sentence is confusing. Our intent was to say that based on  
136 this particular event, we did not see larger (climate change) effects based on the storyline  
137 approach.

138 259: This is a good point about mal-adaptation. I think it is a shame that this only now appears in  
139 the paper and wasn't raised in the introduction.

140 We change the introduction to reflect this point.

141 270: Change "similarly" to "similar".

142 Figure 2: Subplot a needs a grey line in the legend to explain what the grey lines are, and caption  
143 could explain how they relate to the blue line. You said (d) shows difference between (c) and (d),  
144 think you meant (b) and (c).

145 Figure 3: This figure needs a lot more explanation in the caption. Possibly, this could be done in  
146 part in the paragraph on lines 239-260 where the figure is discussed. Instead of simply making a  
147 statement and referencing (Fig.3), instead reference (Fig.3a red line). This would make it easier  
148 for the reader to connect the point your are discussing with the specific feature in Figure 3.

149 Figure 6. This needs more explanation in the caption.

150 OK, we will do so (on all 4 points above).

151

## 152 References

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