**Response to Editor and Reviewers:**

We thank the editor and reviewers for their thoughtful feedback to our manuscript, “Antarctic Radiative and Temperature responses to a doubling of CO2”. Below, please find our response to the feedback from both reviewers. The original comments are in black, and our responses are in blue font. Our main adjustments aimed to improve validation, limitations and our description of the model, include more citations and details on previous work, and provide clearer figures.

**Reviewer #1 Comments:**

This paper addresses an interesting problem in climate science: the surface warms in the Antarctic despite a negative (cooling) CO2 radiative forcing measured at TOA. The failure of the TOA forcing to predict the direction of surface temperature change poses a challenge for climate scientists to explain. This paper attempts to reconcile the problem from a surface energy balance perspective and uses a simple model to test their interpretation. Overall, I find the paper interesting to read and potentially a valuable contribution, as it brings this interesting problem to more attention and proposes a theory to explain it. However, a few issues need to be rectified, especially concerning the validity and relevance of the modeling results.

Thank you for the helpful comments, questions, and suggestions on citations. We have addressed the comments below, focusing largely on including new citations as well as more in depth descriptions of the model, including on its limitations and a comparison to observations.  
Main comments  
  
1. Clarity and validity of the model   
Paragraph after line 100. Is cloud included in the calculation? What vertical resolution is used, especially close to surface? Is shortwave included? What surface albedo is used in shortwave?... In general, there lacks sufficient description and validation of the simulation of the local climate, especially concerning radiative fluxes. Potential biases due to parameter choice, as well as the use of monthly mean profile, should be noted and discussed.  
We’ve added clarifications about clouds, which are not included in the calculation (paragraph after line 101); vertical resolution, which varies with height (line 99-100); inclusion of shortwave and the surface albedo (paragraph after line 101). Additionally, we’ve added in discussions about the use of monthly mean profiles and other limitations in line 104.

Similarly, concerning the advective and turbulent heating, there ought to be proper illustration and validation of the relevant modelling components. For instance, the profiles of heating rates due to different components, as well as the TOA and surface energy flux values.   
In order to better validate the model, we have included a comparison of the surface and top of atmosphere long and short wave fluxes to monthly observations from CERES from 2001-2019 (lines 108-109 and Figure 1d.). We have also clarified the closure of the energy budget (line 160). Regarding comparison to advective and turbulent heating profiles, since our advective profile is the residual heating needed based on observed temperature profiles, comparisons to any observations would similarly just be made through calculations of the residuals due to temperature profile changes. This would, therefore, be almost the same calculation as is performed to calculate the advection itself.   
Another issue is that the method and model descriptions miss necessary references of the ideas and equations. A few specific notes are given below:  
Paragraphs starting at Line 103: Concerning the forcing dependency on CO2, it is proper to note the idea that explains the forcing as resulting from the vertical displacement of the radiating level and thus naturally dependent on the lapse rate (Huang & Bani shahabadi, 2014, doi:10.1002/2014JD022466). This is a fundamental reason for the negative forcing in Antarctic, as well as sometimes found in winter Arctic when strong inversion occurs (i.e., when lapse rate reverses the sign) (Huang et al. 2016).   
This point has been addressed in lines 116-121, where we discuss the dependence of the forcing on the vertical displacement of radiating level, as well as pointing out the three main factors that contribute to uncertainty in determining the exact explanation behind the shape of the GHESFC from this work alone.  
Paragraph after Line 115  
Regarding the "effective height for emission": If the vertical resolution is coarse, the model can't resolve the change in the emission height, although strictly and theoretically speaking it should be sensitive to CO2 perturbation. With RRTMG, one could analyze the forcing, as well as related band averaged radiative properties (e.g., optical depth), in different bands. One suggestion is to contrast CO2 band 630-700 cm-1 to the surrounding bands (500-630, 700-820 cm-1) to validate these approximations.    
Thank you for this helpful suggestion. Although it would be interesting to look at the different CO2 bands, the current use of RRTMG in ClimLab does not allow us to contrast different wavelengths of CO2 bands, so we cannot analyze the forcing through this method. We’ve clarified the language in this part (now the paragraph after line 127) to make it clearer that the assumption regarding the effective height for emission means that we can use the surface temperature as a proxy for the emission temperature, which should resolve the issue pointed out that a model may not resolve changes in emission height, as we are not aiming to resolve that in this simplified equation.  
Line 117: 10.4 cm-1 - reference to this number? What frequency is it pertinent to?   
The reference to Jeevanjee, 2020 for this number is now made clearer, and the frequency is added in line 129.  
Line 137: this temperature jump phenomenon was documented as early as Manabe & Stricker 1964.   
A reference has been added in the paragraph after line 149. The temperature jump phenomenon is slightly different between the two scenarios, as in Manabe and Stricker, the temperature jump is not physically realizable/stable without the addition of convection/turbulence.  
Eq. 6: reference and rationale to this formulation, including the k(z) expression and choice of d=100m?  
The rationale for this formulation is that the turbulence will decay away from the surface, particularly in the stably stratified atmosphere of the Antarctic. We have clarified this more clearly in the paragraph after the equation (paragraph after Eq. 6, line 149). Additionally, we have discussed the fact that we tested other scale factors (d = 500m), which did not have a substantially impact on the results of the model (paragraph of line 149).   
  
Eq. 4: Pierrhumbert (2010, Principles of planetary climate) should be cited as the original formulation of this group of idea that derives the log relationship from spectrally averaged perspective, i.e., based on such radiative properties as bandwidth; this is distinctive from the idea that is based on the spectrally resolved radiance and interprets the forcing as resulting from the displacement of emission level (Huang & Bani Shahabadi, 2014).   
Thank you for pointing this out—a relevant description and references have been added in the paragraph preceding equation 4.  
  
2. Relevance of the model and results   
Given that this is a single point-based toy model, how much can the results be related to the reality? Some proper discussions are necessary to note what findings are robust and generalizable and what may be subject to caveats. For example, the limitation(s) of fixing advective heating, which in reality may vary and lead to surface warming. And also fixing the turbulent parameter K0 and limiting the turbulent flux to the lowest ~100m.   
We’ve discussed the limitations of a fixed heigh scale, as well as briefly discussing some of the sensitivity tests we have performed to test the impact of changing the height scale on the results in the paragraph after line 149. Additionally, we have added in a discussion on the limitations due to a fixed advective heating profile starting in line 163.   
Additional comments  
  
Paragraph after Line 100: What values are included in the "range of CO2 values"?  
Exact values of CO2 that are used are now provided in the paragraph after line 101.  
Fig 1 and other line plots: it would be easier to use log scale on x-axis for visualizing and testing the log relationship.   
We’ve adjusted the plots that have varying CO2 levels to have log scales on the x-axis.  
Line 160. It would be relevant to compare to Wang & Huang (2020, doi:10.1175/JCLI-D-19-0032.1), whose fig 4b showed qualitatively similar results with more sophisticated single column simulation. This result seems to corroborate the statement here and helps to verify robustness in this result.   
Thank you for pointing this out—we’ve added a citation to Wang & Huang for the discussion on the response to CO2 (line 183).  
  
  
**Reviewer #2 Comments:**  
  
This paper explains why, even though adding CO2 increases outgoing longwave radiation (OLR), it still warms the surface in Antarctica.   
  
They show that, if CO2 is varied independently, the top-of-atmosphere (TOA) greenhouse effect (GHE) decreases for all seasons except the austral winter months, whereas the surface GHE increases for all months. Using the fact that the Antarctic atmosphere is in radiative-advective balance and that the coupled boundary layer and surface system can only lose heat by upward radiation, they derive a simple expression for surface temperature change that depends on CO2 concentration and initial surface temperature. They then use a radiative-advective-turbulent single column model to verify the accuracy of their simple derivation.  
  
I have a few minor comments but think the paper is very good and I recommend publication after addressing my comments.  
Thank you for the helpful comments, questions, and suggestions on citations. We have addressed the comments below, focusing on improving the clarity of our figures and equations.  
Comments  
  
Fig 1: Can you comment on why GHE sfc is much higher in December than the rest of the year. Is it just that surface temperature is higher in December (equation 3)?  
We’ve added this explanation into the caption of figure 1, which is as you describe, due to the higher surface temperatures during December.  
L. 95 CO2 → O3  
We’ve modified this to O3.   
L. 138: Why are there only nine positive values? Are these the months with an inversion?  
These are all but the summer months (December-February), which have weaker inversions. This has been clarified in line 153.   
Eq 10: what is t? And is it not dFadv/dz?  
We’ve defined t as time and have clarified this in Eq 10. In the model, we are calculating a heating rate and prescribing a flux based on that, so the direction of the advection is not of particular importance. Since the advection term has no directionality, we assume the majority of it would be due to meridional heat transport, therefore we have it in the y direction.   
L.167: Shouldn't it be delta GHEsfc? It does say "relative to \*no\* CO2" but other components contribute to GHEsfc such as water vapor, so I am a bit confused here.  
Since we define GHEsfc relative to the no CO2 scenario, this is already part of the calculation. We maintain the same water vapor content across the different levels of CO2.  
Fig 3b : I find this graph hard to read. Maybe you can bundle the months by 3? DJF, JJA, etc.  
We’ve modified this to bold the months highlighted in other graphs, and have also moved it to Figure 1 in order to address questions brought up by other reviewers.  
Can you comment on the relevance of these results to the Arctic?

We’ve added a brief comment on the relevance of these results to the Arctic in line 218-226, particularly noting the role of the GHEsfc, as well as differences between the Arctic and Antarctic feedbacks that may need to be incorporated to translate between these two regions.