Report on review comments of version 2 of "Stochastic Foundations of Atmospheric Thermodynamics: Deriving the Laws from Maximum Entropy and Implications for Earth's Climate"

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Introductory notes

The manuscript with the above title was initially submitted on 13 June 2026 to the journal *Geosciences* (Manuscript ID: geosciences-4406555), following an invitation from the journal. It received reviews by Reviewers #1, #2 and #3. The decision, as stated in the decision letter of 15 July 2026, was the following

> Based on the evaluations of the reviewers and the Editorial Board Member, we have decided that your manuscript may be of interest to our readers and has the potential to be published in Geosciences. However, significant revisions or new data are required to warrant further processing of your manuscript; therefore, at this stage, it will be rejected to allow you to make your revisions.

After revision, it was resubmitted on 2026-08-02 to the same journal (Manuscript ID: geosciences-4508810) along with detailed responses to review comments. It was again reviewed by three reviewers: Reviewers #1 (same as in the first review round), #2 (Reviewer #3 in the first review round) and #3 (new reviewer).

The decision on the revised manuscript suggested major revisions as seen in next pages of this report.

Below I reproduce all the review comments of the second review round without any editing except numbering them by Reviewer, along with my detailed responses and explanations of how I have addressed them. Because some of the new comments cross-reference those of the first review round, I also include as an annex the review report of the first round without any change. In the revised manuscript, the numbering of review comments starts with 'RR' followed by a number, while that of the first round starts as 'R' followed by a number. The two numberings are individual.

Because of the numerous review comments, I had to conduct additional analyses and expand the paper accordingly. In general, I have tried to keep the main paper as concise as possible and hence most of the additional analyses have been placed in Appendices. The original version did not contain appendices. The first revision contained four appendices. The current second revision contains six appendices.

I have also tried to make this Report self-contained and therefore I include in it parts from the revised paper (but not the whole text of the Appendices to keep the Report relatively concise).

Key:

> Review comment.

Response.

> Quotation from revised manuscript (possibly with slight differences due to last moment changes).

> Text from other sources.

Note: The numbering of references included here is sequential and does not correspond to that of the paper.

Editor

E1.

Major

Dear Prof. Koutsoyiannis,

Your manuscript has been reviewed by experts in the field and we request that you make major revisions before it is processed further. Please revise your manuscript according to the reviewers' comments and upload the revised file within 10 days. Please click on the "Peer Review Reports" below to find the reviewers' comments and the version of your manuscript to be used for your revisions.

I appreciate the processing of my manuscript and the reviews I received, and I am thankful to the Editors for the positive decision. I have revised the manuscript addressing all review comments, as detailed below.

Decision letter

E2.

Please Note

Please try to add more references from the last five years.

Please revise the manuscript found at the above link according to the reviewers' comments and upload the revised file within 10 days. Note the following check-list:

(I) Ensure all references are relevant to the content of the manuscript.

(II) Highlight any revisions to the manuscript, so editors and reviewers can see any changes made.

(III) Provide a cover letter to respond to the reviewers' comments and explain, point by point, the details of the manuscript revisions.

(IV) If the reviewer(s) recommended references, critically analyze them to ensure that their inclusion would enhance your manuscript. If you believe these references are unnecessary, you should not include them.

(V) If you found it impossible to address certain comments in the review reports, include an explanation in your appeal.

I have followed all suggestions in the check-list, as well as the request to add more references from the last five years, which are highlighted in the annotated manuscript that accompanies the submission.

Reviewer #1

RR1.1.

Review Report Form

Open Review	(x) I would not like to sign my review report ( ) I would like to sign my review report
Quality of English Language	( ) The English could be improved to more clearly express the research. (x) The English is fine and does not require any improvement.

	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	(x)	( )	( )	( )
Is the research design appropriate?	(x)	( )	( )	( )
Are the methods adequately described?	(x)	( )	( )	( )
Are the results clearly presented?	(x)	( )	( )	( )
Are the conclusions supported by the results?	(x)	( )	( )	( )
Are all figures and tables clear and well-presented?	(x)	( )	( )	( )

Comments and Suggestions for Authors	My comments are listed in the attached file: Referee Report 2. peer-review-58206386.v2.pdf
Submission Date	02 August 2026
Date of this review	05 Aug 2026 11:43:17

I am grateful, once again, to the Reviewer for the very positive assessment of the paper.

RR1.2.

Recommendation

The author was responding in an extraordinary way to the reports of the three reviewers. In a significantly extended manuscript he integrated nearly all suggestions of the reviewers 1 and 3. He gave additional explanations for clarification in the running text, in additional new appendixes, and in a longer separate file as reply to the reviewers.

Concerning the question what is responsible for the atmospheric radiative effect and the formation of the atmospheric lapse rate, the author has another understanding than I do and obviously also reviewer 3 has. But this is legal and the basic principle how science works.

I already recommended acceptance of the previous manuscript, all the more this is true for the revised manuscript.

I have no words to thank the Reviewer for the positive recommendation, for accepting the legitimacy of "another understanding" that differs from hers/his, and particularly for her/his statement: "But this is legal and the basic principle how science works."

I wish such views were more common among scientists; they would greatly facilitate the pursuit of the truth, novelty and innovation.

Reviewer #2

RR2.1.

Review Report Form

Open Review	( ) I would not like to sign my review report (x) I would like to sign my review report
Quality of English Language	( ) The English could be improved to more clearly express the research. (x) The English is fine and does not require any improvement.

	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	(x)	( )	( )	( )
Is the research design appropriate?	( )	(x)	( )	( )
Are the methods adequately described?	(x)	( )	( )	( )
Are the results clearly presented?	(x)	( )	( )	( )
Are the conclusions supported by the results?	( )	( )	(x)	( )
Are all figures and tables clear and well-presented?	(x)	( )	( )	( )

Submission Date	02 August 2026
Date of this review	06 Aug 2026 21:33:28

I am thankful to the Reviewer for upgrading the marking of the paper (in comparison to the earlier round, see Annex, Comment R3.1) with regards to some of the journal's questions. However, I am surprised for the low marking regarding the question "Are the conclusions supported by the results". I looked again at the conclusions, and I cannot find anything that is not supported by the analyses made, nor anything that needs change. Here is the relevant bulleted list of conclusions from the relevant section (section 6) of the paper:

- A novel foundation of entropy has been proposed, grounded in stochastic principles and four postulates that naturally incorporate the principle of maximum entropy as an ontological (physical) law of nature, rather than merely an epistemological tool.
- The resulting entropy expression, uniquely derived, coincides with the classical Planck-Shannon form but rests on firmer mathematical and conceptual grounds, free from the circularities and limitations of previous formulations.
- This framework unifies probability, uncertainty, and change: entropy is uncertainty quantified, and its maximization drives the irreversible evolution of natural systems.
- In hydrometeorology and atmospheric thermodynamics, the approach allows deductive derivation of key physical laws and relations (temperature, ideal gas law, specific heats, dry and moist isentropic profiles, Clausius-Clapeyron

equation, Planck's radiation law) directly from stochastic principles and conservation of energy.

- The stochastic view reveals the atmosphere as a system governed by coexisting molecular and macroscopic randomness across scales, explaining why its temperature profile tends toward (but is not strictly) isentropic rather than isothermal.
- Overall, stochastics emerges not as a modelling convenience but as the actual physics underlying natural processes. This consideration offers clarity (saphenia), predictive power, and deeper insight into the workings of a fundamentally uncertain world.

Section 6 does not conclude that the conventional radiative-convective ordering is refuted. It concludes that the proposed postulates recover the standard thermodynamic relations and that the tropospheric profile is interpreted as a competition between molecular (isothermal) and macroscopic (isentropic) maximum-entropy states. In the revised manuscript, distinctive tests of that interpretation are given in Appendix A.6.

RR2.2. I appreciate the author's detailed and thoughtful responses to the my original comments. The revised manuscript has clarified several points, particularly regarding the intended interpretation of the stochastic entropy framework and the distinction between equilibrium and constrained nonequilibrium systems. I also appreciate the author's willingness to explicitly state that the manuscript challenges aspects of the established radiative-convective equilibrium framework rather than attempting to reconcile with it.

I appreciate the Reviewer's positive attitude in considering my explanations.

RR2.3. However, after considering the author's responses and revisions, my fundamental concerns remain unresolved. The central issue is not whether the proposed framework is mathematically consistent or whether it can reproduce familiar thermodynamic results. Rather, the key scientific question is whether the proposed framework provides a more fundamental or predictive explanation of physical phenomena than existing statistical mechanics and atmospheric physics.

Throughout the manuscript, the author argues that entropy maximization, together with appropriate constraints, generates the observed physical behavior. However, many of the essential physical elements appear to enter through carefully chosen constraints, empirical parameters, or other forms of externally supplied information. The fundamental issue throughout the manuscript is the distinction between reproducing known physics through entropy maximization and deriving known physics from entropy maximization. The former demonstrates consistency; the latter would constitute a new foundational theory. At present, the manuscript demonstrates consistency but does not establish derivation.

The Reviewer writes that derivation "would constitute a new foundational theory," and that the manuscript "demonstrates consistency but does not establish derivation." I agree that consistency is shown. Section 5 is the place where I argue that the thermodynamic laws follow from the postulates and the conservation constraints. I do not claim that this already constitutes "a new foundational theory"; that judgment lies outside the paper.

RR2.4. For example, the author's clarification regarding molecular independence in the statistical-mechanical section is helpful. I agree that the independence of molecular velocity distributions can emerge from entropy maximization under an expectation-value energy constraint. However, this raises a deeper foundational question: why is this particular constraint physically fundamental? The distinction between microcanonical and canonical descriptions is not merely mathematical but reflects physical assumptions regarding the system and its interaction with the environment. Traditional statistical mechanics devotes considerable effort to demonstrating ensemble equivalence and connecting the statistical description to microscopic physics. In the present framework, the entropy maximization procedure appears to require the appropriate constraints to already be known. Thus, the framework does not yet demonstrate that entropy maximization determines the physics; rather, it demonstrates that entropy maximization produces the expected results once the relevant physics has been incorporated into the constraints.

For parsimony I avoided use of the notions and terms of microcanonical and canonical descriptions in the paper. Despite being fundamental in statistical mechanics, they are not necessary in the proposed framework.

To address and reply to Reviewer's "deeper foundational question", I have added the following paragraphs in the end of section 3.1:

> In addition to the principle of maximum entropy, which is regarded here as a first principle, we also use as first principles the conservation of mass, momentum and energy. In our framework, these take the form of constraints for the entropy maximization. The framework is formulated in such a manner that can work for any number of molecules, including the case of a single molecule. In the latter case, even if the molecule is treated in isolation, the fact is that it interacts (collides) with other molecules. Hence its state variables, such as velocity and energy, are not fixed but vary in time. Naturally, we should consider them as uncertain quantities represented as stochastic variables. This is also a necessary requirement to define the entropy of its state—without uncertainty there is no entropy. Accordingly, the conservation laws do not apply to the molecule's instantaneous state, but to the expectation thereof. As we will see below, there is no reason to change this setting when we move from one to many molecules. We always formulate conservation laws in terms of expectations or the respective variables. This may sound odd within the classical mechanical thinking, but it is in full consistency with the ontological treatment of uncertainty. It is similar to the treatment in quantum mechanics, where, for instance, there is no single classical-like value of energy that could be "strictly conserved" from one instant to the next. What is strictly conserved is again the expectation of energy.
>
> For further clarification, our framework considers the mass of each molecule as fixed and therefore mass conservation switches to probabilities adding up to 1—the standard requirement of a normalized measure, which probability is. For simplicity, when we examine a gas mass in a container, we choose a reference frame for which the container is motionless (all velocity coordinates have zero expectation). This allows us to omit from the constraints the momentum conservation, while it is always necessary to use the energy conservation (in addition to probability normalization). This does not affect the

value of entropy, which is independent from the choice of the refence frame [1] (Digression 6.A).

RR2.5. A related foundational issue concerns the treatment of indistinguishable particles and the Gibbs paradox. The author argues that indistinguishability is unnecessary and that the correct extensive form of entropy can be recovered through the proposed entropy formalism without invoking this assumption. The modern resolution of the Gibbs paradox is understood through the microscopic state counting of identical particles, which in quantum mechanics is expressed through the indistinguishability of identical particles. Thus, indistinguishability reflects a fundamental property of quantum mechanics, namely that permutations of identical particles do not correspond to physically distinguishable states. The resolution of the Gibbs paradox in statistical mechanics therefore arises from the microscopic structure of the state space, rather than from a particular normalization or conditioning procedure applied to entropy.

These comments concern the Gibbs paradox and the status of indistinguishability. I have not added this debate to the manuscript: it lies outside the atmospheric application and is treated in my book [1,section 5.2] and in my reply R3.20 of the first round. The derivations in the present paper do not rely on accepting or rejecting indistinguishability as a physical principle—which I do not.

For completeness of information, here is a relevant passage from my book [**1,** section 5.2]:

> [I]n the case of equation (5.4) [$W_{\mathrm{MB}}(M,N) = W/N! = M^N/N!$, where $W_{\mathrm{MB}}(M,N)$ is the number of possible ways of placing N particles into M boxes (representing particle states or locations) according to the so-called Maxwell-Boltzmann model], […] even simple inspection shows that it cannot be consistent. For example, taking $M = 3$ boxes and $N = 2$ particles, from (5.4) we obtain a number of possible arrangements $W_{\mathrm{MB}}(3,2) = 4.5$, which is absurd—this number cannot be non-integer (see additional problems in Koutsoyiannis, 2013a). By increasing the number of particles, i.e. $N = 9$ for $M = 3$, W_{MB} becomes lower than 1 ($W_{\mathrm{MB}}(3,9) = 0.054$), which is even more absurd, indicating a probability greater than 1 ($1/W_{\mathrm{MB}} = 18.44$).

[It is reminded that to demonstrate inconsistency in a mathematical property even a single counterexample suffices.]

Also for completeness of information, in my book [**1**, section 5.2] the Bose–Einstein and Fermi–Dirac counting formulae (which are not mathematically inconsistent) are recovered from stochastic dependence rather than from indistinguishability. However, that argument is independent of the present paper and is not required for the atmospheric results.

I find it strange that science struggles for unifying theories while it accepts dichotomies of logic for the microcosmos and the microcosmos. As I wrote, I do not accept them and work for a unifying logic.

RR2.6. The author's derivation of an extensive entropy expression is an interesting mathematical result, but it does not yet demonstrate that the proposed framework reproduces the physical origin of indistinguishability. In particular, the introduction of a spatial partition to obtain the appropriate extensive form appears to reproduce the thermodynamic result through an imposed construction rather than derive why identical particles should be counted as a single microscopic configuration. Thus, the framework recovers the expected entropy form but does not yet demonstrate that the underlying statistical mechanics emerges from entropy maximization alone. A successful replacement of traditional statistical mechanics would need to explain why the microscopic counting of states, including the treatment of identical particles, follows naturally from the proposed axioms.

As I explained in my response to the previous comment, it was not my aim that “the proposed framework reproduces the physical origin of indistinguishability”. Rather, my aim was the opposite: to abandon the hypothesis of indistinguishability and replace it with typical logic.

In other words, the paper does not aim to reproduce the standard resolution of the paradox. Instead, it uses a formulation in which the paradox does not arise.

RR2.7. The same issue arises more strongly in the atmospheric applications. The derivation of Clausius-Clapeyron relations, lapse rates, and atmospheric temperature structures is an interesting demonstration of consistency. However, these results are already consequences of established thermodynamics, the equation of state, hydrostatic balance, and phase equilibrium. Therefore, agreement with ERA5 observations does not uniquely validate the proposed framework, because conventional atmospheric models based on the same physical constraints reproduce the same structures. A stronger demonstration would require showing that the stochastic framework predicts atmospheric behavior that cannot be obtained from existing theory or that it requires fewer fundamental assumptions.

I am glad that the Reviewer found that my derivations are “an interesting demonstration of consistency”. I concede that agreement with ERA5 does not uniquely validate the proposed framework. However, I do not agree that the same mean structures are already fixed by the conservation laws, the equation of state, hydrostatic balance and phase equilibrium. Those relations are satisfied by many vertical profiles. A variational or selection principle is still required to choose among them. In the conventional radiative-convective construction the selection is imposed by radiative transfer. In the present paper the bounding profiles are selected by entropy maximization at the molecular scale (isothermal) and at the parcel scale (isentropic). The two routes can produce similar climatological means for different reasons.

Therefore, for validation (or falsification) of the proposed approach (also following a constructive comment by Reviewer #3—Please see my reply to comment RR3.6), I have now included in the new Appendix A.6 about further testing:

Appendix A.6: Falsifiable empirical benchmarks

[Please see the full appendix in the revised manuscript.]

That my approach “requires fewer fundamental assumptions” is already demonstrated in the paper and repeatedly explained in my responses to review comments of both the previous and the current review rounds. To repeat it once more:

- In a deductive mode, the proposed method only uses the principle of maximum entropy as an ontological principle (stated in just one line) and the conservation laws of classical mechanics.

- My approach does not use the Laws of Thermodynamics. Instead, it derives them.
- When deduction becomes impossible, my approach uses induction based on observations, which is an integral part of both stochastics and physics (particularly geophysics).

RR2.8. The discussion of moist thermodynamics illustrates this issue. The author has appropriately revised the treatment of liquid water by acknowledging collective molecular modes rather than treating liquid water as simply a gas with additional degrees of freedom. However, the value of the degree-of-freedom parameter remains empirically determined. The stochastic framework does not predict why $\beta B = 18$; rather, this information is supplied from physical measurements, after which entropy maximization determines the equilibrium relation. Thus, the framework requires molecular-scale physical information before the entropy principle can be applied, rather than deriving that information from entropy itself. This is consistent with the standard role of statistical mechanics, where entropy determines the statistical distribution once the microscopic constraints are known. It does not demonstrate that entropy itself generates the underlying molecular physics.

I agree with the Reviewer that the "value of the degree-of-freedom parameter remains empirically determined". Determining the number of degrees of freedom of each species and phase is outside the present scope; the values are taken from the cited sources. In that respect the framework has the same structure as standard statistical mechanics: the microscopic constitution (masses, degrees of freedom, phase constraints) is given, and entropy maximization then determines the macroscopic equilibrium relations, including the Clausius–Clapeyron form.

The paper's claim is therefore not that entropy generates molecular structure. It is that, given that constitution, the same ontological maximum-entropy principle used elsewhere in the manuscript yields the moist relations without taking the thermodynamic laws as additional primitives.

RR2.9. Relative to the original manuscript, the author's interpretation of radiative-convective equilibrium (RCE) remains the most unresolved issue. In the revised manuscript, the author has explicitly indicated that he reverses the conventional causal ordering of RCE. He argues that (1) entropy maximization precedes and produces convection, (2) convection establishes the lapse rate, and (3) radiation subsequently adjusts to maintain energy balance. This is a legitimate hypothesis to investigate. However, a reversal of a well-established causal framework requires evidence beyond reproducing the mathematical consequences of a prescribed lapse rate.

I am glad that the Reviewer found my reversal of "the conventional causal ordering of RCE" to be "a legitimate hypothesis to investigate". I believe it is impossible to convince everyone, with a single paper, that "the conventional causal ordering" is wrong. Therefore, it has not been the purpose of the paper to dismiss the conventional hypothesis. Rather, the purpose is (a) to question the conventional hypothesis, (b) to formulate an alternative hypothesis, and (c) provide arguments supporting the alternative hypothesis. Those three objectives are what the revised manuscript is meant to deliver.

As I already mentioned in my reply to comment RR2.7, in the current revised version, further testing of the alternative hypothesis is proposed, and to this aim the new Appendix A.6 proposes possible falsifiable empirical benchmarks for further testing. I am currently conducting relevant work on this, and I hope others would also wish to test the hypotheses.

RR2.10. The central mathematical relation in the manuscript is actually consistent with standard greenhouse theory. In the standard greenhouse view, the lapse rate determines the vertical temperature difference, but the temperature scale is established by the planetary energy balance. Earth's emission temperature is determined by planetary radiative balance. Earth's emission altitude depends upon the infrared optical depth, and the lapse rate determines the temperature difference between the surface and emission level. In this view, convection establishes the energy balance, and convection adjusts the atmospheric lapse rate toward a stable profile. The manuscript demonstrates that changing the lapse rate changes the surface temperature, but it does not demonstrate that the lapse rate develops independently of radiative forcing.

I agree with the Reviewer that "Earth's emission temperature is determined by planetary radiative balance". The long-term, planetary-mean energy balance fixes an effective radiating temperature of ~255 K. That quantity is an average: emission temperature varies with location, altitude and time (Table 2 and Appendix A.3). This balance remains a useful global accounting relation, but does not by itself determine the lapse rate, nor whether radiation or convection sets the lapse rate.

As in the reply to Comment RR2.9, the paper questions the conventional causal order, formulates an alternative, and gives arguments for it (see sections 4.4 and 4.5, Appendices A.3 and A.4). In particular, Figure A3 shows a large seasonal range of $L_S - L_{TOA}$ with almost no corresponding change in the lapse rate. That is offered as evidence that the lapse rate is not controlled by the concurrent longwave contrast, not as a refutation of the standard energy-balance identities. Whether the lapse rate is established independently of radiation is the hypothesis to be tested by the benchmarks in Appendix A.6.

RR2.11. The discussion of Manabe and Strickler remains problematic for the same reason. The author correctly acknowledges that his RRTM calculations and the Manabe and Strickler calculation solve different problems. However, this admission removes the basis for claiming inconsistency between the two results. In Manabe and Strickler, the temperature profiles are calculated based upon energy transport via radiation alone. The present calculations are calculated based upon a prescribed temperature profile. These are not equivalent boundary-value problems. A radiative equilibrium solution cannot be evaluated by calculating radiative flux divergences for arbitrarily prescribed temperature profiles.

I appreciate the Reviewer's explanations about this issue. I agree that the two calculations are not the same problem. Manabe and Strickler obtain a temperature profile from radiative transfer alone. The RRTM runs used here take a prescribed linear profile and diagnose the energy imbalance. I do not claim that the two calculations have been shown to be numerically inconsistent.

In response to these remarks, in the current revised manuscript I have made a major overhaul, with substantial revisions to the part of the paper related to the discussion of Manabe and

Strickler. In this overhaul I also considered the comment RR2.21 of the same Reviewer, in which she/he suggests removing the related discussion, as well as the related comment RR3.4 by Reviewer #3, in which she/he suggests enhancing the related discussion.

Given the above rather diverging opinions, I proceeded with the thought that a golden mean would be to remove the discussion from the body of the paper and gather the related material into an appendix:

> **Appendix A.5: Comparison of the proposed framework with the notion of radiative equilibrium**

The new appendix contains (a) several changes so as to be aligned with the Reviewer's #2 suggestions, and (b) additional analyses to cover, as much as possible, the requests of Reviewer 3. It investigates whether linear profiles that envelope the Manabe–Strickler curve are close to energy balance in this RRTM. They are not. That does not replace a free radiative-equilibrium calculation.

[Please see the full appendix in the revised manuscript. Please also see my reply to comment RR3.4 by Reviewer #3.]

RR2.12. Similarly, the isothermal atmosphere argument does not establish that thermodynamic equilibrium and radiative equilibrium coincide. RRTM determines radiative fluxes for a specified atmospheric state; it does not determine the atmospheric state that evolves under radiative forcing. The fact that an imposed isothermal atmosphere produces approximately zero radiative flux divergence does not demonstrate that an Earth-like atmosphere would naturally evolve toward that state. Radiative equilibrium is not the same as thermodynamic equilibrium, and there are examples of non-isothermal atmospheric layers that maintain radiative equilibrium (such as Earth's stratosphere).

I agree with the Reviewer that the paper "does not establish that thermodynamic equilibrium and radiative equilibrium coincide" in general. But it shows, based on RRTM runs, that the isothermal atmosphere is also in a state of near radiative equilibrium. For that imposed isothermal profile the diagnosed radiative divergence is near zero, so radiation alone would not drive it away from isothermality. The paper's thesis is that convection is the agent that does this, forcing it toward the isentropic profile.

The example of the stratosphere brought up by the Reviewer is different, because there the dominant mechanism is radiation. In the stratosphere there is no regular convection, no weather, and almost no clouds, storms, or turbulence. Indeed, in the stratosphere "there are examples of non-isothermal atmospheric layers that maintain radiative equilibrium" (as the Reviewer states). But there are also examples of isothermal atmospheric layers that maintain radiative equilibrium (the layer between the troposphere and the altitude of 20 km in the temperate zone and an even thicker layer in the polar regions). These are explained in the beginning of section 3.11:

> The standard atmosphere, i.e., a profile representative of year-round, midlatitude conditions (ICAO [2]), is depicted in Figure 6. It is seen that in the mesosphere, the temperature decreases with the increase of altitude as there is removal of heat from the atmosphere (cooling) due to emission of longwave radiation (see section 4). In the upper

stratosphere, the pattern is opposite (inversion) because of the absorption of solar radiation, caused by the ozone layer reacting with ultraviolet radiation. The lower stratosphere is a 9 km thick layer at the isothermal state. There, the presence of ozone is weak, and the air absorbs just as much heat as it radiates away, keeping the temperature uniform.

In the troposphere the temperature drops as the altitude increases. While in the established theory the latter behaviour is again attributed to radiation, here it is suggested that it is driven by a different mechanism, namely convection. This convection is strong near the surface and dies off at the level of the tropopause, thus not affecting the upper atmospheric layers, where radiative action drives the observed profiles.

In the current revision, the following addition (from Appendix A.6) is relevant to this review comment:

To further test the present stochastic maximum-entropy framework, in comparison to the established one, a first possible falsifiable empirical benchmark could be based on whether suppressed convection increases or decreases the distance from molecular isothermality. A distinctive, testable prediction of the proposed framework is that the tropospheric temperature profile systematically approaches the isothermal molecular equilibrium (the state that maximises entropy at the microscopic level) as the intensity of macroscopic convective motions declines. Because residual convection, advection and latent-heat release never vanish completely, pure isothermality is an asymptotic reference rather than an attainable end-state. The observable signature is therefore a continuous reduction of the conditional mean lapse rate (and a corresponding narrowing of the temperature profile difference from isothermality) with decreasing convective activity. This relation can be examined with existing high-resolution soundings, radio-occultation profiles and reanalyses by stratifying the data according to independent measures of convective intensity (such as convective available potential energy—CAPE) while controlling for radiative forcing and moisture content. Polar-night and inversion regimes provide natural promising cases.

The appropriate conventional counterpart is that of the pure radiative equilibrium that does not impose any fixed critical lapse rate through convective adjustment. Under those conditions, the temperature profile is free to evolve according to radiative cooling rates, and is under no theoretical obligation to approach isothermality. Rather, in this counterpart framework an increase in the distance from isothermality is expected, accompanied by a steepening of the near-surface lapse rate. The so-called radiative-convective equilibrium, which also includes convection, is another candidate counterpart. We note though that this is not an equilibrium state despite the term used for it [3].

If, after the stated controls, the observed conditional mean lapse rate shows no systematic drift toward zero as convective intensity decreases, but rather remains constant or steepens toward the non-adjusted pure radiative model, then the claim that the tropospheric profile reflects a competition between molecular (isothermal) and macroscopic (isentropic) maximum-entropy states is falsified.

[Please see the full appendix in the revised manuscript.] So, future research (which is underway on my part but can also be conducted by others) will show which of the hypotheses, the mainstream (conventional), the alternative proposed in the present paper, or none of the two, is verified (or falsified).

> **RR2.13.** I appreciate the clarification that the author does not claim that the real atmosphere reaches thermodynamic equilibrium. Rather, the author proposes that entropy maximization occurs subject to constraints that prevent equilibrium. However, the physical origin of those constraints remains the central question. Solar forcing, infrared emission, rotation, gravity, and surface boundary conditions are not consequences of entropy maximization; they are properties of the physical system. Therefore, the framework does not derive atmospheric behavior from entropy alone but from entropy maximization combined with externally specified physical constraints.

I fully agree with the Reviewer. The proposed framework does not derive atmospheric behaviour from entropy alone but from entropy maximization together with externally specified constraints (conservation laws imposed on expectations, and constitutive quantities such as molecular mass and degrees of freedom).

> **RR2.14.** Furthermore, the argument that convection is an approximately reversible adiabatic process does not imply that the entire atmosphere evolves through reversible entropy maximization. Individual parcels may approximately conserve entropy during ascent and descent, but the convective circulation is an irreversible process involving mixing, turbulence, phase changes, and entropy production. The relevant thermodynamic description of a forced climate system requires accounting for entropy production associated with irreversible processes. Maximizing entropy subject to imposed constraints does not explain the origin of those constraints or the irreversible processes that maintain the system away from equilibrium.

I generally agree with the Reviewer. I do not think that the paper says anything to the contrary.

The following passages are indicative.

In section 3.8, I clearly state that the system is kept away from equilibrium:

> In reality, the Earth is not in equilibrium and hence the atmosphere is not isothermal. The non-equilibrium state is caused by perpetual change occurring on all scales due to diverse mechanisms affecting radiation processes, both shortwave and longwave: day and night, cloud formation and disappearance, summer and winter, albedo's spatial and temporal variation (also affected by biosphere changes), Sun's dynamics and astronomical processes, orbital variations (Milanković cycles), tectonic and volcanic activity, and numerous other irregular changes by internal processes and external forces [4].

In section 4.3, I state the main reason why the system is maintained away from equilibrium:

> Given that the amount of energy the Earth receives from the Sun is equal to the amount it radiates back to space, it is not correct to say the Earth gains energy from the Sun. Rather, it is correct to say that the Sun provides the Earth with a source of low entropy.

Indeed, solar radiation is at higher frequencies than terrestrial (see Figure 8 and Figure 9) and entropy is a decreasing function of frequency (see equation (A29) in Appendix A.2). The difference between the lower entropy of solar radiation and the higher entropy of terrestrial radiation is main reason why the atmosphere is maintained away from equilibrium.

Individual parcels may be treated as approximately reversible and adiabatic; the circulation as a whole is not. Mixing, turbulence and phase change produce entropy. In the paper those processes belong to the perpetual change that keeps the column away from molecular equilibrium (section 3.8), with the Sun supplying a low-entropy flux (section 4.3).

RR2.15. I agree with the author that consensus theories should not be accepted solely because they are widely accepted. However, overturning an established theory requires demonstrating either a fundamental inconsistency in the existing framework or a clear predictive advantage of the alternative. At present, the manuscript demonstrates that entropy maximization can reproduce many known thermodynamic relationships and that prescribed lapse rates influence radiative fluxes. It does not demonstrate that the conventional radiative-convective equilibrium framework is incorrect or that the proposed framework provides superior explanatory power.

Again, I agree with the Reviewer. However, I do not think that a single paper in a scholarly journal would be enough to overturn established theories. The purpose of the paper is less ambitious than this, as already explained in my reply to Comment RR2.9.

RR2.16. The manuscript contains an interesting and ambitious attempt to reformulate thermodynamics from a stochastic entropy perspective. The statistical-mechanical development provides an alternative interpretation of entropy and demonstrates that entropy maximization can reproduce many familiar results under appropriate constraints. However, the manuscript has not demonstrated that these results arise from fewer assumptions than traditional statistical mechanics or that the proposed framework provides new predictive capability.

As I already mentioned in my reply to Comment RR2.7, the proposed framework requires fewer assumptions. Furthermore, I believe that this framework uses the minimal possible set of assumptions, a single variational principle plus conservation laws. New predictive content, as distinct from reproduction of known relations, is stated in the benchmarks of Appendix A.6.

RR2.17. The atmospheric application extends beyond what has been demonstrated. The manuscript does not establish that radiative-convective equilibrium theory is incorrect or that convection, rather than radiation, determines Earth's greenhouse warming. I would encourage the author to first establish the predictive and foundational advantages of the entropy framework before applying it as an alternative explanation of atmospheric climate physics. Additional comments are attached.

I think the paper **does** already provide what the reviewers asks for. It “first establish[es] the predictive and foundational advantages of the entropy framework”. The foundational advantages are:

- The elevation of the principle of maximum entropy from epistemological to ontological, thus integrating it into the fabric of physics;
- The consistent set of postulates, producing the entropic expression (the postulates and the derivation of the Planck–Shannon formula are not those of Jaynes, Shannon or Planck).

Both these are 100% novel and are highlighted even the Conclusions section:

- A novel foundation of entropy has been proposed, […] that naturally incorporate[s] the principle of maximum entropy as an ontological (physical) law of nature, rather than merely an epistemological tool.
- The resulting entropy expression, uniquely derived, […] rests on firmer mathematical and conceptual grounds, free from the circularities and limitations of previous formulations.

The predictive advantages of the entropy framework are clearly demonstrated by predicting the atmospheric thermodynamic and radiative behaviour, as well as the Laws of Thermodynamics per se, by the proposed framework. In other comments, Reviewer #2 recognized those predictions (e.g. Comment RR2.19).

In addition, the atmospheric causal order is a predictive application of that foundation. I do not claim that this application has already replaced radiative-convective theory. Distinctive tests are given in Appendix A.6.

RR2.18. Specific Responses

Regarding R3.3

The author has satisfactorily addressed my concern regarding Eq. (16) and statistical independence. Given the stated constraints, the factorization of the probability distribution follows from entropy maximization rather than being introduced as an additional assumption. This clarification improves the presentation.

However, my original concern regarding the choice of constraints remains. The statement that *"the energy constraint is expressed in terms of the expectation of the total kinetic energy"* appears to correspond to the canonical ensemble rather than the microcanonical ensemble. The author then states that *"if we used a fixed value instead, dependence would appear."* I agree with this statement, but it raises the more fundamental question of why the expectation constraint should be regarded as the physically fundamental one. Is this dictated by the entropy axioms themselves, or is it an independent modeling choice?

The manuscript already acknowledges that its interpretation differs from traditional statistical mechanics. In the conventional development, considerable effort is devoted to explaining the physical circumstances under which the microcanonical, canonical, and grand canonical ensembles arise and why they become equivalent in the thermodynamic limit. In contrast, the present framework appears to begin directly with an expectation constraint. I do not view this as a mathematical flaw, but rather as a deliberate selection of one ensemble. The physical justification for that selection is still unclear.

This issue is related to another statement in the response:

"Had we used a fixed value instead, this would create dependence among different molecules, as well as departure of the probability distribution of the molecules' velocities from the normal distribution."

If the resulting distribution depends upon whether the constraint is imposed as a fixed total energy or as an expected total energy, then the entropy formalism by itself does not uniquely determine the equilibrium distribution. Rather, the equilibrium distribution depends on the physical constraints imposed on the maximization problem. This is precisely the viewpoint traditionally emphasized in maximum-entropy methods: entropy maximization is conditional on the available physical information. The manuscript, however, appears to suggest that entropy itself determines the physics. I still do not see how the proposed framework derives the appropriate physical constraints rather than assuming them.

My second concern regarding the conditioning argument also remains. The manuscript argues that conditioning on a particular spatial partition restores the extensivity of entropy. While this successfully reproduces the desired thermodynamic form, it is still not clear why this particular partition is physically distinguished. In traditional statistical mechanics, the corresponding correction arises from the indistinguishability of identical particles, since permutations do not generate new microstates. The present framework offers a different interpretation, but it has not yet demonstrated why the proposed conditioning is uniquely required by the underlying physics rather than being one possible choice that reproduces the correct result,

This comment is already addressed as explained in my replies to Comments RR2.4 and RR2.5. The conservation laws are imposed as constraints on expectations.

RR2.19. Regarding R3.5

I agree that it is a meaningful contribution to demonstrate that many standard results of atmospheric thermodynamics—including the ideal gas law, hydrostatic balance, the Clausius-Clapeyron relation, and the dry and moist adiabatic lapse rates—can be recovered from the proposed stochastic entropy framework without assuming the traditional laws of thermodynamics as axioms. If successful, this would provide an alternative conceptual foundation for classical thermodynamics.

The broader question remains whether the stochastic framework actually requires fewer or weaker assumptions than traditional statistical mechanics. The manuscript argues that entropy maximization, together with an appropriate choice of constraints, leads successively to the Maxwell distribution, the definition of temperature, the ideal gas law, hydrostatic balance, and ultimately the familiar results of atmospheric thermodynamics. However, each stage of this chain still depends on physically motivated constraints and additional assumptions. The manuscript does not yet demonstrate that these assumptions arise uniquely from the entropy postulates themselves or that they are fundamentally weaker than those employed in conventional statistical mechanics.

For the same reason, I do not believe the ERA5 comparison fully validates the proposed framework. Agreement with observed atmospheric temperature profiles demonstrates that the recovered atmospheric thermodynamics is physically reasonable. However, essentially the same agreement is obtained by conventional atmospheric models based upon classical thermodynamics, hydrostatic balance, and realistic moist processes. Consequently, the ERA5 comparison does not uniquely support the proposed stochastic formulation; rather, it demonstrates that the proposed framework is compatible with existing atmospheric thermodynamics.

Ultimately, the relevant scientific question is not simply whether the framework reproduces established results, but whether it offers a genuine explanatory or predictive advantage. A new theoretical foundation should ideally (1) be internally consistent, (2) remain consistent with more fundamental physical theories, and (3) explain existing phenomena with fewer assumptions or generate new, testable predictions. While the present manuscript largely satisfies the first criterion, it has not yet convincingly demonstrated the second and third.

I am glad that the Reviewer found the framework internally consistent (her/his point 1).

Regarding consistency with "more fundamental physical theories" (her/his point 2), I cannot imagine something more fundamental than the definition of entropy, as proposed in the paper.

As I already mentioned in my reply to other comments, starting with Comment RR2.7, the proposed framework requires fewer assumptions (her/his point 3).

Regarding whether or not "the ERA5 comparison fully validates the proposed framework", I think that **full** validation is out of the capabilities of science and therefore Popper's system relies on falsification rather than validation.

Regarding "testable predictions", as I already mentioned in my reply to comment RR2.7, in the current revised version, further testing of the alternative hypothesis is proposed and to this aim the new Appendix A.6 proposes possible falsifiable empirical benchmarks for further testing. Please also see my reply to comment RR3.6 (by Reviewer #3).

Regarding whether "these assumptions arise uniquely from the entropy postulates themselves" my reply is that there are no additional assumptions except the physical constraints that correspond to conservation laws or consequences thereof. There may be parameters derived from measurements (e.g. the masses of different molecules) but these do not classify as "assumptions".

RR2.20. Regarding R3.6

The revised manuscript satisfactorily addresses my original concern regarding the interpretation of the additional degrees of freedom of liquid water by clarifying that they represent collective low-frequency intermolecular vibrational modes rather than kinetic degrees of freedom. This is a physically more reasonable interpretation.

However, a broader issue remains. The manuscript still does not explain why the liquid phase should be characterized by βB = 18. This quantity is introduced from experimental knowledge rather than derived from the stochastic framework itself. The author argues that the framework accommodates data-based inference, but from my perspective the derivation actually requires such inference. For the Clausius-Clapeyron relation, the effective degrees of freedom are first supplied from observations, after which entropy maximization produces the equilibrium relation. Thus, experimentally determined parameters remain essential inputs to the derivation.

A similar concern applies to the parameter ξ. My original comment noted that ξ appears to absorb much of the intermolecular physics associated with hydrogen bonding, configurational structure, and the energetic differences between the liquid and vapor phases. The current response does not resolve this issue. Entropy maximization itself does not determine ξ; rather, the underlying molecular physics must first establish its value before the entropy formalism can be applied.

This illustrates what I view as the broader conceptual issue throughout the manuscript. Traditional statistical mechanics does not claim that entropy alone determines physical behavior. Rather, the physics specifies the microscopic states, interactions, and constraints, while entropy determines the corresponding equilibrium probability distribution. In the present framework, maximum entropy likewise reproduces the known thermodynamic relations only after physically motivated constraints and experimentally determined parameters are supplied. Consequently, I remain unconvinced that the manuscript has demonstrated that entropy maximization uniquely determines the physics, rather than providing an alternative mathematical framework for organizing physical assumptions that have already been introduced.

I do not think that the paper claims "that entropy maximization uniquely determines the physics". Entropy maximization is the powerful variational principle, which determines the probability distribution of the final state, and hence the direction on which things will evolve—the arrow of time. Any variational principle needs constraints to determine non-trivial solutions. In this respect "physically motivated constraints and experimentally determined parameters" are necessary.

The conservation of energy, for instance, is necessary to include as a constraint. The paper does not say that this is derived from the principle of maximum entropy. (It could be derived by another variational principle, the principle of stationary action, but this outside the scope of the paper). Additionally, the paper does not refute the necessity of "experimentally determined parameters". Rather, it highlights the importance of induction, and its relationship with stochastics, on the one hand, and real-world data, on the other hand. The quotations from the paper in the Reviewer's next comment, clearly state those.

Please also see the replies to Comments RR2.4, R3.6, RR2.7 and RR2.8 ($\beta_B = 18$ is an empirical input, not a prediction of entropy maximization in this paper).

RR2.21. Regarding R3.7

The author does understand the standard radiative-convective equilibrium (RCE) picture in which solar radiation generates an unstable lapse rate and convection redistributes energy, leading to an adjusted lapse rater. The author hypothesizes the opposite: namely that entropy maximization (along with the constraints of mass, momentum, and energy conservation) establishes an isentropic lapse rate via moist convection, and the atmospheric lapse rate leads to an adjustment in radiation. To establish the hypothesis, the author must show that the stochastic framework is a superior explanation for the observed atmospheric lapse rate, and I don't think this has been demonstrated. I believe that the author must show that his proposed framework is uniquely supported by the governing conservation laws.

The author states:

"An isothermal atmosphere would lead to an average surface temperature of 255 K (see Table 2 and its description). It is the temperature gradient that makes the surface warmer than this. This may sound paradoxical if viewed on a local scale, e.g., above a warm ground where convection removes heat. However, it should be viewed on a macroscopic basis including the entire air column and not as a local phenomenon. Convection does not impact the average temperature of the atmospheric column, which remains close to 255 K and this is indeed the case as seen in Figure 10. A temperature equal to the column average one appears at about 5.7 km. As illustrated above, if we take this "stationary point" (the intersection of the isothermal at T = 255 K and an inclined profile) and apply a slope of 6.5 K/km to the inclined profile, then we find that the surface temperature will be close to 288 K. The warming (from ~255 K to ~288 K) is a necessary consequence of the lapse rate, which in turn appears because of convection. Indeed, there would not be lapse rate without convection."

It is mathematically true that increasing the lapse rate necessarily increases the surface temperature. **However, this does not explain why the column-averaged temperature is 255 K?** The author appears to treat this as a starting point, which is consistent with the overall framework. The author previously stated that:

"… While theoretical reasoning does provide qualitative explanations on what is observed in vertical atmospheric temperature profiles, it does not suffice for quantitative modelling. The processes are too complex to describe within a simple entropic framework in an independent column approximation. Convection (vertical heat transfer), advection (horizontal heat transfer by wind from warmer areas, also affected by Coriolis forces and cell formation), radiation, cloud formation, precipitation and several other processes are acting together and their final outcome cannot be predicted by deduction, unlike the analyses of previous sections. However, a stochastic framework, on which entropy and its maximization principle are founded, also supports induction, i.e., inference based on data."

Hence, the column-averaged temperature functions as an experimentally determined parameter that is essential to the rest of the results of this section, leading to the conclusion that the column averaged temperature determines Earth's energy balance.

However, modern atmospheric physics shows that 255 K is the effective radiating temperature determined by planetary energy balance. The mathematical relation: $Ts = Te + \Gamma ze$ is the standard greenhouse relation, and the modern theory states that the emission level (fixed by radiation) and the lapse rate (maintained by convection) produce the surface temperature. **In the modern theory, the lapse rate is part of the greenhouse effect, not an alternative to it. The author should explain why the modern theory cannot be correct**.

A similar statement can be made about Manabe and Strickler. I'm glad that the author acknowledges that the pure RCE of Manabe and Strickler is a different problem than the calculations in the paper. However, it is not enough to state that the "*comparison remains informative*". To advance the theory, one should show RRTM contradicts Manabe and Strickler.

However, as the author admits, this cannot be done since RRTM and Manabe and Strickler are not solving the same problem.

I still find the isothermal argument unconvincing. The author argues that since $dR/dz \approx 0$ for an isothermal atmosphere, thermodynamic equilibrium is the same as radiative equilibrium. However, this does not follow from RRTM, which is simply calculating radiative fluxes for a prescribed atmosphere. The goal of RRTM is to determine the radiative fluxes for a given temperature profile; it cannot determine the temperature profile that matches radiative equilibrium.

In the appendix, the author argues that since seasonal changes in radiation lead to small changes in lapse rate, then radiation must adjust to the lapse rate. This does not follow. Modern theory states that the lapse rate remains stable because convection rapidly adjusts the atmosphere toward moist neutrality on timescales that are much smaller than the seasonal timescales. Thus, stable lapse rates do not distinguish between convection as a response to radiation from radiation as a response to convection. Both frameworks would predict constant lapse rates.

There's also a logical issue in the author's framework. Currently, the author argues that entropy maximization in the atmosphere leads to convection, which then establishes the lapse rate, and then radiation restores balance. However, it is without dispute that convection is an energy transport mechanism (not a generator of energy). This means that there must be a source of energy entering the convective system. The upward convective flux should arise from solar energy absorbed at the surface, and the outgoing longwave radiation should balance the absorbed solar radiation (via energy conservation). Thus, global energy balance should be independent of convection, and radiation must provide the boundary conditions for Earth's energy balance.

I think the only way that the debate in this paper can be resolved is if (1) the author solve the coupled radiative-convective problem in which both the temperature profile and the radiative fluxes evolve simultaneously and (2) provide observational evidence of large-scale convection that matches his prediction (which can be inferred from vertical heat and moisture transport). Thus far, the manuscript demonstrates that a prescribed lapse rate influences the radiative fluxes (which is well-understood in the standard theory), but it does not show that the lapse rate itself originates independently of radiative forcing. At best, this framework offers an alternative interpretation of calculations that are also consistent with the standard theory, which is not enough to overturn the modern theory of RCE.

In general, I would implore the author to consider removing the discussion of RCE and the greenhouse effect to another paper and to focus the attention on validating the novel framework.

I appreciate the Reviewer's assessment that "The author does understand the standard radiative-convective equilibrium (RCE)". Further, I appreciate the Reviewer's recognition that I hypothesize the opposite (to RCE). Since I hypothesize the opposite, I had to provide arguments that support the opposite.

I also appreciate the Reviewer's opinion that "Modern theory states that the lapse rate remains stable because convection rapidly adjusts the atmosphere toward moist neutrality". But I do not follow this "modern theory" for several reasons. One of them is that it is not true that the atmosphere adjusts toward moist neutrality. I explain this in the following text (section 3.11):

> Clearly, the atmospheric profiles discussed here are induction-based. The most impressive finding of this induction is the near constancy of the lapse rate, which remains close to the standard of 6.5 K/km across the troposphere and in all areas. While this signifies a tendency toward the isentropic profile in the troposphere, we cannot maintain that the troposphere is isentropic—whether dry or moist. This becomes clear from the polar areas where the dry and moist isentropic profiles almost coincide with a lapse rate of 9.8 K/km. The latter coincidence can be explained by the low presence of water vapour, which also substantially reduces the radiative activity. However, in the polar areas the actual rate, instead of tending to 9.8 K/km, is lower even than the standard of 6.5 K/km (Figure 7). Yet the local profiles clearly reflect the connection with convection: In the polar regions and midlatitude winters, the atmosphere is closer to the isothermal profile than in tropics, because of the lower level of convection, whose full absence would necessarily lead to an isothermal atmosphere. Furthermore, in tropical and midlatitude areas the actual lapse rate is almost constant, as implied by equation (26), rather than following the patterns seen in Figure 5. Hence, a common argument that it is the moist isentropic state that determines the lapse rate in the troposphere is deemed not accurate. Additionally, most of the time the atmosphere is not saturated and therefore the moist isentropic profile is not applicable. As already mentioned, the climatic system is too complex to describe by invoking a single mechanism.

I think this extract gives a couple of reasons related to the Reviewer's phrase "why the modern theory cannot be correct" (polar lapse rate lies below both dry and moist isentropes, and the air is usually unsaturated) but I give more reasons in other parts of the paper.

Regarding the explanation about "why the column-averaged temperature is 255 K", Table 2 gives a detailed account (based on detailed calculations) of the equilibrium temperature, which turns out to be 255 K—and this does not differ from the commonly assumed value. Up to this point, this is not an "experimentally determined parameter", but one determined from theory.

However, in standard interpretation this is thought to be the surface temperature without the "greenhouse effect". I believe this interpretation is wrong and I explain this below Table 2 (section 4.3):

> In calculating the equilibrium temperature of 255 K we used the irradiance and albedo at the TOA, and thus we considered the Earth as a whole. On the other hand, at any location (defined by its geographical coordinates (φ, λ)) we do not have a unique Earth temperature across altitudes z because the atmospheric column is not isothermal.
>
> Since calculating the equilibrium temperature we considered the Earth as a whole, it is reasonable to assume that this value represents the average temperature over the atmospheric column. As inferred from Figure 7, the column average temperature equals the actual temperature at an altitude of 5-7 km, which roughly corresponds to the atmospheric pressure level of 500 hPa.

Hence, the "experimentally determined parameter" is the column average, and this agrees with the theoretical prediction of 255 K".

As per the concluding part of this comment (and as I already explained in my reply to Comment RR2.11), I found it as a golden mean not to remove the discussion from the body of the paper but to move the related material into an appendix:

Appendix A.5: Comparison of the proposed framework with the notion of radiative equilibrium

The new appendix contains (a) several changes so as to be aligned with the Reviewer's #2 suggestions, and (b) additional analyses to cover, as much as possible, the requests of Reviewer 3.

[Please see the full appendix in the revised manuscript. Please also see my reply to comment RR3.4 by Reviewer #3.]

Finally, seasonal near-invariance of lapse rate may be compatible with both pictures if convection is fast. Figure A3 is therefore not a proof of causal order; it is one of the arguments referred to Appendix A.6. I also agree that convection transports energy rather than creating it, and that the planetary energy balance is set by absorbed solar and outgoing longwave radiation. The disagreement is only about what sets the lapse rate, not about that budget. A coupled integration in which $T(z)$ and the fluxes evolve together is not performed here; that limitation is stated.

RR2.22. Regarding R3.8

I appreciate the author's clarification that the manuscript does not treat the atmosphere as relaxing toward thermodynamic equilibrium, but rather as a constrained entropy maximization problem in which additional constraints prevent equilibrium. This is an important distinction. However, the fundamental question remains: what determines these constraints? The relevant constraints for Earth's atmosphere (solar forcing, infrared emission to space, rotation, gravity, surface exchange, and phase changes) are not consequences of entropy maximization; they arise from the physical properties of the Earth system. Therefore, the framework does not derive atmospheric behavior from entropy alone, but rather applies entropy maximization after the governing physical constraints have already been specified. In contrast, for traditional statistical mechanics, entropy maximization determines the equilibrium distribution after the physical constraints are specified. The constraints themselves originate from conservation laws and microscopic physics. Therefore, applying entropy maximization to a nonequilibrium atmosphere requires demonstrating that the relevant nonequilibrium constraints emerge uniquely from the proposed framework rather than being supplied externally.

A related issue concerns the interpretation of convection. The author writes

"Therefore when there is convection, the macroscopic motion is mostly reversible adiabatic (where adiabatic means without heat exchange), i.e., isentropic. Ergo, the atmospheric temperature profile departs from the isothermal and tends to the isentropic."

The fact that individual air parcels approximately undergo reversible adiabatic motion does not imply that the atmospheric circulation is an isentropic process. The convective system as a whole is irreversible because of turbulent mixing, diffusion, radiative exchange, and phase changes. Therefore, the existence of an approximately adiabatic lapse rate does not demonstrate that convection arises from entropy maximization. Rather, it reflects the tendency of convection to reduce unstable temperature gradients in a continuously forced system.

I continue to believe that entropy production is the appropriate thermodynamic framework for this problem. Earth's atmosphere is maintained away from equilibrium by continuous solar forcing and radiative cooling to space. The relevant quantity is therefore the entropy production associated with irreversible processes, rather than the maximization of entropy in an isolated system.

With regards to non-equilibrium systems, the author writes:

“Contrary to classical equilibrium thermodynamics, the proposed framework can also deal with nonequilibrium cases. For the latter cases, we just put an additional constraint expressing the reason why the systems are not allowed to reach equilibrium.”

This raises a deeper concern. The physical mechanisms that prevent equilibrium are precisely the processes that determine atmospheric evolution: radiative forcing, convection, latent heating, and circulation. If these mechanisms are introduced as constraints, and entropy maximization is then applied to recover atmospheric structure, the explanatory power of the entropy principle remains unclear. The framework must demonstrate that the relevant nonequilibrium constraints emerge uniquely from the proposed theory rather than being supplied from existing atmospheric physics.

I have repeatedly responded to similar comments related to whether or not “the atmospheric circulation is an isentropic process”; for example, see reply to Comment RR2.21, which explains why it is **not**.

I agree that those constraints are specified from the physics of the Earth system, not generated by entropy maximization. Treating physical mechanisms that prevent equilibrium as constraints is the intended structure of the framework, which I regard as a strength. In addition to the constraints related to conservation laws, which typically result in equilibrium states, the framework can accommodate other constraints, when there is no equilibrium. As already noted in the paper (in one of the extracts quoted above by the Reviewer) the framework can accommodate as a constraint “the reason why the systems are not allowed to reach equilibrium”. Whether we know that reason from theory or from observation is indifferent to the framework. The methodology does not condemn observation, measurement, and induction. Rather, the framework uses all available information, including constraints taken from observation, because a constraint is an entropy reduction.

For these reasons, given the scope of the paper, there is no need to invoke entropy production, and I do not adopt it as the primitive in this paper. The primitive is maximization of entropy subject to the stated constraints, including constraints that keep the column away from molecular equilibrium.

RR2.23. Regarding R3.9

I appreciate the author's clarification that the goal of the manuscript is not to reconcile the proposed framework with modern radiative-convective equilibrium theory, but rather to challenge that framework. I wholeheartedly agree that scientific progress often comes from challenging consensus. However, a new theory that contradicts a well-established framework must demonstrate not merely that it is mathematically consistent, but that it provides superior explanatory or predictive capability.

The purpose of my original comment was not to argue that the proposed entropy framework must conform to existing theory, but rather to point out that the same entropy arguments developed in this manuscript are naturally consistent with nonequilibrium thermodynamics and the conventional greenhouse interpretation. The production of entropy associated with the transformation of high temperature solar radiation into lower-temperature terrestrial radiation is precisely the type of process expected in a forced-dissipative climate system. Greenhouse gases influence this entropy production by modifying the radiative pathway through which Earth balances incoming and outgoing energy.

Therefore, the manuscript must demonstrate why this interpretation is incorrect. At present, the manuscript demonstrates that a prescribed atmospheric lapse rate affects radiative fluxes, but it does not demonstrate that radiation is merely responding to a thermodynamically determined lapse rate rather than contributing to the determination of that lapse rate through radiative-convective equilibrium. Likewise, the manuscript has not demonstrated that the entropy framework provides predictions that differ from or outperform existing atmospheric theory.

I fully agree that consensus theories can contain errors. However, the relevant scientific question is not whether the proposed interpretation differs from consensus, but whether the evidence presented is sufficient to establish that the existing framework is incorrect. At present, I do not believe that this has been demonstrated.

As I already noted in my reply to Comments RR2.9 and RR2.15, the paper is less ambitious than providing conclusive and irrefutable evidence so that everyone would agree that “this [incorrectness of the existing framework] has been demonstrated”. Rather, the paper aims are to question the conventional causal order, to formulate an alternative, to give arguments for it, and propose furthers tests.

RR2.24. Regarding R3.20

I appreciate the author's clarification that the manuscript intentionally avoids the assumption of indistinguishability and that a more complete discussion is provided elsewhere. However, I believe the importance of this issue extends beyond the Gibbs paradox itself.

In modern statistical mechanics, indistinguishability is not merely an assumption introduced to correct classical entropy counting. It is a consequence of quantum mechanics: identical particles do not correspond to separate physical labels, and particle permutations do not generate distinct quantum states. The resulting correction in classical statistical mechanics is therefore connected to the underlying structure of the quantum state space.

Consequently, removing indistinguishability represents a significant change in the connection between microscopic physics and macroscopic entropy. A framework that replaces indistinguishability must therefore provide an alternative physical principle explaining why permutations of identical particles do not correspond to additional states.

The proposed conditioning procedure successfully recovers the extensive entropy form, which is an important result. However, the current argument appears to establish that an appropriately chosen entropy measure reproduces the thermodynamic result, rather than demonstrating why that entropy measure is uniquely required by microscopic physics. **The question remains whether the partitioning/conditioning procedure emerges from a fundamental principle or is selected because it produces the known thermodynamic behavior**.

Therefore, the issue is whether the proposed framework provides a physically deeper replacement for the quantum-mechanical origin of indistinguishability. Without such a demonstration, the framework should be interpreted as an alternative mathematical formulation of entropy rather than a replacement foundation for statistical mechanics.

The author argues that this procedure is preferable because it produces the correct intensive and extensive forms. However, this appears to be a selection criterion based on the desired thermodynamic outcome rather than a derivation from microscopic physics. In traditional statistical mechanics, extensivity emerges because indistinguishable particle permutations do not correspond to independent physical states. In the proposed framework, extensivity appears to emerge because the entropy measure is transformed or conditioned appropriately. **Therefore, the question is whether it explains why that correction must occur from physical principles.** At present, it appears that the framework replaces the indistinguishability assumption with a different assumption concerning the appropriate entropy normalization or conditioning.

This does not necessarily invalidate the proposed framework. However, it changes the interpretation of the result: the manuscript demonstrates that entropy maximization with a suitable entropy definition can reproduce the thermodynamics of identical particles, but it has not yet demonstrated that the stochastic entropy framework uniquely requires this definition.

I have already replied to those issues (related to indistinguishability) in my replies to other comments, beginning with Comment RR2.5. I do not regard indistinguishability to be a physical principle. Extensivity is obtained by conditioning on the spatial partition. In that bookkeeping the Gibbs paradox does not arise. The present manuscript does not deal with the quantum-mechanical account of the state space, but as mentioned in my reply to Comment RR2.5, stochastic dependence can replace indistinguishability.

RR2.25. Regarding R3.26

I appreciate the author's clarification that represents the energy required to disrupt intermolecular interactions and that the additional degrees of freedom in liquid water represent collective molecular modes rather than additional kinetic degrees of freedom. However, this clarification reinforces my original concern. The value of ξ is not derived from the stochastic entropy framework; it is an empirical representation of the microscopic molecular physics of the liquid phase.

The issue is therefore not whether the proposed expression can reproduce the Clausius-Clapeyron relation or fit experimental vapor-pressure data. It clearly can. The issue is whether the phaseequilibrium behavior of water emerges from entropy maximization alone. In the present formulation, entropy maximization determines the equilibrium distribution only after the relevant physical parameters governing the liquid and gas phases have already been specified.

This is consistent with the traditional role of statistical mechanics, where entropy determines macroscopic equilibrium once the microscopic Hamiltonian and degrees of freedom are known. However, it does not demonstrate that entropy maximization itself determines the microscopic physics. The success of a constant approximation therefore demonstrates the usefulness of the parameterization but does not establish that the stochastic framework provides a fundamental derivation of moist thermodynamics.

I have already provided replies about the Clausius-Clapeyron relation and ξ. Please see my replies to Comment RR2.20, as well as those to Comments RR2.7 and RR2.19. Like β, ξ is a constitutive input, not a prediction of entropy maximization in this paper.

RR2.26. Regarding R3.33

The manuscript correctly identifies that the lapse rate contributes to the difference between surface temperature and effective emission temperature. However, this does not demonstrate that convection is the cause of atmospheric warming. The effective emission temperature and emission altitude are determined by radiative transfer and planetary energy balance. Convection modifies the vertical temperature structure connecting the emission level to the surface but does not determine the radiative boundary condition. Therefore, the manuscript has not shown that convection replaces radiative forcing as the fundamental mechanism of the greenhouse effect.

I do not claim that "convection replaces radiative forcing" as the mechanism of Earth's energy balance. Absorbed solar radiation and outgoing longwave radiation still set the planetary budget. The paper's claim is about the origin of the lapse rate that converts a column-mean / emission temperature into the surface temperature. That claim is argued in sections 4.4–4.5 and Appendix A.6.

See also replies to Comments RR2.9 and RR2.23.

Reviewer #3

RR3.1.

Review Report Form

Open Review	(x) I would not like to sign my review report ( ) I would like to sign my review report
Quality of English Language	( ) The English could be improved to more clearly express the research. (x) The English is fine and does not require any improvement.

	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	( )	(x)	( )	( )
Is the research design appropriate?	( )	(x)	( )	( )
Are the methods adequately described?	( )	(x)	( )	( )
Are the results clearly presented?	( )	(x)	( )	( )
Are the conclusions supported by the results?	( )	(x)	( )	( )
Are all figures and tables clear and well-presented?	( )	(x)	( )	( )

Submission Date 02 August 2026

Date of this review 16 Aug 2026 18:03:02

I thank the Reviewer for the careful evaluation and for the very constructive specific comments, which to address I made substantial additions to the manuscript.

RR3.2. This manuscript delivers an innovative, mathematically rigorous stochastic axiomatic foundation for entropy that unifies probabilistic and thermodynamic entropy, deductively deriving core gas, radiation and atmospheric thermal laws solely from the maximum entropy (ME) ontological principle, supported by ERA5 reanalysis, radiosonde, satellite spectral data and RRTM radiative modelling; the novel re-interpretation of atmospheric vertical lapse rates as convection-governed with radiatively active gases only serving a stabilizing Le Chatelier-style adjustment mechanism, plus empirical evidence of dual warming/cooling atmospheric radiative effect (ARE) under positive/negative lapse rates, constitutes valuable geophysical insight, yet the work carries substantial conceptual, modelling, contextual and presentation shortcomings requiring major revision.

I am glad that the Reviewer found that the paper constitutes a valuable geophysical insight. I thank her/him for the constructive suggestions, which I have followed as I explain below.

RR3.3. The paper strongly rejects the standard "greenhouse effect" label and frames ARE as an "anti-greenhouse" mechanism, creating severe misalignment with decades of mainstream climate and atmospheric physics literature; adopt dual standardized terminology and soften polemical framing to improve cross-field readability without discarding the core dual-warming/cooling empirical argument.

I tried to follow this suggestion and lower the tone as much as I could. For example, I have rephrased the text which contains the phrase in question, hopefully in a lower tone. Now it reads:

> Because the dominant mechanisms operating in the atmosphere are in several respects opposite to those of a greenhouse, the traditional label "greenhouse effect" is arguably inaccurate. It carries several potential drawbacks: a mismatch of mechanisms (convection versus radiation and the implication of a static barrier), an oversimplified view of complex processes, and an uneven emphasis on particular gases [4]. As first highlighted by Aristotle, sapheneia (clarity and accuracy) of concepts and terminology is essential in science. In this respect, the expression "anti-greenhouse effect" would be more accurate in describing the behaviour of the atmosphere, namely the fact that the convection establishes a lapse rate and thereby a surface temperature above the column-mean temperature, whilst a greenhouse warms mainly by suppressing convection. For these reasons, as already mentioned, the present work uses the terms *radiatively active gas* (RAG) and *atmospheric radiative effect* (ARE). To improve cross-field readability, the conventional expression "greenhouse effect" is retained only sparingly and is always placed in quotation marks.

Several other modifications have been made throughout the paper. Dual terminology is used from section 4.2 onward. The empirical dual warming/cooling result is unchanged.

RR3.4. The rebuttal to classic Manabe & Strickler (1964) radiative-convective equilibrium relies exclusively on linear constant-lapse RRTM test cases; add non-linear radiative equilibrium simulation runs and controlled sensitivity sweeps varying radiatively active gas concentrations to quantify radiative forcing magnitudes and strengthen the critique of canonical theory.

To address this comment, I have made additional analyses which I give in a new appendix:

> **Appendix A.5: Comparison of the proposed framework with the notion of radiative equilibrium**

[Please see the full appendix in the revised manuscript.] Please note that I have not added a free nonlinear radiative-equilibrium integration or concentration sweeps of radiatively active gases: the online RRTM used here only accepts a linear temperature profile. Appendix A.5 instead compares linear envelopes of the Manabe–Strickler curve and diagnoses the energy imbalance for those profiles; the limitation of the comparison is stated there.

RR3.5. No quantitative sensitivity analysis of anthropogenic trace gas perturbations is included; add brief targeted RRTM or column model tests to illustrate how changing RAG loading impacts lapse stability and surface temperature, to bolster the manuscript's real-world climatic significance.

To address this comment, I have augmented Figure 12 adding results of new RRTM runs for modified ARE due to changes in RAGs and clouds. The related text is copied below.

> For sensitivity analysis additional RRTM runs were conducted and two sets of results are also shown in Figure 12. For the first additional set, the assumption of double CO_2

concentration (800 ppm, where the default value is 400 ppm) was made, as is typical in climatic sensitivity studies. In isothermality no change is seen, while in positive and negative lapse rates the doubling of CO_2 concentration results in increase and decrease of surface temperature, respectively (and the opposite behaviour in TOA temperature). For $\Gamma = 6$ K/km and 12 K/km, the temperature increase is 2.5 K and 3 K, respectively.

For the second additional set, the CO_2 concentration was kept constant at the default value of 400 ppm, but a relatively small fraction, 20%, of high clouds was assumed (against 0 in the default parameter set). The behaviour is similar, but the changes much higher: 5.5 K and 12 K for $\Gamma = 6$ K/km and 12 K/km, respectively (2.2 and 4 times greater than the effect of doubling of CO_2 concentration, respectively). Overall, the sensitivity analysis shows that the presence of NC-RAGs does affect the temperature profile, but other factors such as lapse rate and clouds are more important.

RR3.6. The author treats the ontological maximum entropy axiom as a universal physical law but provides no testable, unique observational predictions that distinguish this stochastic framework from established radiative-convective thermodynamics; design and clearly state falsifiable empirical benchmarks for the proposed entropy foundation.

To address this comment, I have added a paragraph in the main text and a detailed new appendix in the end. The paragraph at the end of section 4.5 reads:

Yet the proposed stochastic framework, which is different from the established radiative-convective thermodynamic framework needs to be further tested. Tests should also include comparisons of the two approaches based on testable observational predictions. For this reason, some possible falsifiable empirical benchmarks for such testing are discussed in Appendix A.6.

The appendix reads:

Appendix A.6: Falsifiable empirical benchmarks

It contains three falsifiable benchmarks: (i) approach to isothermality as convection is suppressed; (ii) timescale of lapse-rate adjustment versus the longwave field; (iii) other planet profiles. It also discusses the possible conventional counterparts.[Please see the full appendix in the revised manuscript.]

RR3.7. The molecular independence and dilute gas assumptions central to all derivations are insufficiently highlighted early in the text, with minimal discussion of breakdowns under turbulence or dense fluid conditions; add a dedicated subsection addressing turbulence limits to molecular stochastic independence and outline necessary framework adaptations for complex atmospheric turbulent regimes.

To address this comment, first I modified the last paragraph of section 3.2, which now reads:

Dependence can also appear in dense systems, where the free path between collisions, λ, is of the order of molecular size, d. Such systems are characterized by macroscopic coupling and cross-dependencies, which are strong, persistent, and highly complex. These systems cannot be represented by the above setting, which is appropriate for $\lambda/d \gg 1$ so that the systems be regarded as collections of isolated particles that only interact during brief, instantaneous collisions. Instead, when a fluid becomes dense ($\lambda/d \to 1$), molecules are in a constant state of overlapping contact. Apparently, the study of such systems should require additional constraints and more involved treatment. However, the Earth's atmosphere is characterized by ratios λ/d of the order or several hundreds and can therefore be represented as a dilute gas. Hence the study of dense systems is out of our scope of typical atmospheric applications. Additional information about this issue is given in [**1**]. The molecular-independence assumption is also challenged by turbulence, in which strong velocity correlations develop inside coherent structures. This issue is examined in Section 3.12.

Second, and more important, I have rewritten section 3.12 to discuss turbulence. It now reads as follows:

3.12 Macroscopic motion, cells or air parcels, and turbulence

Once entropy is viewed as quantification of uncertainty, we may think it relevant to maximize it not only at a molecular level but also at a macroscopic level. As clarified in section 2.2, entropy is a function of the probabilities of the events that form a partition. Many different partitions could be formed, which have different entropies and, when these are maximized, different values of the variables involved will be obtained. Hence, we would expect that the maximization of a coarser partition would give a different result from that of the finest partition.

The entropy maximization at the finest, i.e. molecular, scale gave accurate results for atmospheric thermodynamics, virtually eliminating macroscopic uncertainty, but these do not explain everything that occurs in the atmosphere. Has Nature restricted entropy maximization to a unique unit or scale, the molecular one? Or does she employ a multitude of scales for this maximization, considering herself different partitions on which to maximize uncertainty? The second possibility appears to be the case. Indeed, there are macroscopic atmospheric structures, named *cells* or *air parcels*, which move as structures (convection), while being continually transformed in shape and physical characteristics. This makes their study much more difficult. While molecules belong to a few species only, each having a fixed and stable structure, the clusters that form the cells are temporary, lack a clear shape and a fixed scale, and instead span a hierarchy of spatiotemporal scales, starting with a spatial scale of mm to m and a temporal scale of min, and reaching the horizontal scale of a third of a hemisphere, the vertical scale of the entire troposphere, and the seasonal time scale. The hierarchy includes *Rayleigh-Bénard cells*, *small thermals* or *plumes*, *mesoscale convection cells*, *deep convective cells*, *mesoscale convective systems*, and *global circulation cells*. The entire hierarchy is linked to macroscopic motion: without it the clusters do not form at all, in contrast to the microscopic motion, which is intrinsic.

In this respect, it looks reasonable to accept that (a) Nature does not adhere to a unique scale or unit for maximizing entropy, (b) the atmospheric system composition includes units of different sizes and (c) there are different physical mechanisms acting on multiple scales. It is important to recognize that there is an antagonism between scales: maximizing entropy on one scale reduces it on another. A numerical illustration for this, based on a toy model, is given in [1] (Digression 6.L). Because simultaneous maximization at all scales is impossible, a balance across many scales would be the alternative. It is recalled that entropy means uncertainty, and entropy maximization results in maximum uncertainty. In this respect, we could assume that Nature may "sacrifice" some part of uncertainty on one scale in order to increase uncertainty on other scales.

The physical mechanisms responsible for the emergence of clusters (coherent flow structures) on different scales are twofold: viscosity and, primarily, turbulence. Inside these coherent structures the velocities of neighbouring molecules (or fluid elements) are strongly correlated, so the assumption of molecular independence no longer holds.

At the finest macroscopic scales, viscous packets emerge within a flow due to local velocity gradients. The flow disrupts the spatial equilibrium of intermolecular electromagnetic bonds (liquids) or biases the random thermal transport of molecular momentum (gases). Both manifest macroscopically as a resistance to sliding motion (viscous shear stress) between adjacent fluid layers.

At larger scales and higher velocities, the flow mechanisms are no longer molecular in origin. Instead, they arise from macroscopic inertial transport driven by the stochastic structure of the velocity field. Specifically, these turbulent stresses manifest as covariances between fluctuating velocity components in different directions (Reynolds stresses). Turbulence spans a wide range of spatial and temporal scales (encompassing the entire hierarchy of cells mentioned above) and is sustained by a massive energy cascade. This cascade continuously transfers kinetic energy from coarser to finer macroscopic scales, until it reaches the dissipation range where viscosity ultimately converts it into microscopic kinetic energy (heat).

Because both viscous and turbulent motions emerge from the collective statistical behaviour of countless particles, they are mathematically intractable to model as isolated molecules via standard molecular thermodynamics. Instead, they are effectively analysed within the continuum framework of fluid dynamics.

A natural adaptation of the presented framework is to treat the air parcel (or coherent turbulent structure) as the new elementary unit and to maximise entropy subject to the appropriate macroscopic constraints (mass, energy, and possibly Reynolds stresses or other moments of the velocity field). Developing the corresponding parcel-scale maximum-entropy theory lies beyond the present scope but constitutes a logical extension for complex turbulent regimes.

Even though continuum dynamics is beyond the scope of this study, the above analysis helps us to understand the different mechanisms and dynamics on different scales, and to explain and interpret the observed profiles discussed in section 3.11. As shown in sections 3.1-3.9, if there were only molecular motion, the atmospheric profile would tend to the isothermal, driven by the principle of maximum entropy at the molecular scale.

The coexistence of molecular and macroscopic motion at multiple scales determines a nonzero vertical temperature gradient, reducing the entropy at the molecular level. Indeed, macroscopic motion cannot maintain a constant temperature. Rather it drives the actual vertical temperature profile away from the isothermal. An air parcel that has been warmed at the surface will move upward so fast that no heat can be removed from it, or added to it. Hence the process is reversible and adiabatic, i.e., isentropic. The lapse rate will then tend to the isentropic lapse rate, Γ_I, the dry one if the atmosphere is not saturated, or the wet one if it is saturated.

Since the entropy maximization at the microscopic (molecular) level results in $\Gamma = 0$, while that at the macroscopic level the lapse rate is pushed toward $\Gamma = \Gamma_I$, it is reasonable to expect that the actual Γ would be in between 0 and Γ_I, and the standard lapse rate $\Gamma =$ 6.5 K/km is therefore unsurprising. But as we have already explained, we do not expect to find this by deduction, due to the complexity of the entire climatic system and the fact that this standard value is the result of an integration over the globe. Hence it is necessary to rely on induction, which is fully consistent with the stochastic approach adopted here.

Acknowledgment: This Report benefited from comments by Grok (xAI) on clarity and structure.

Appendix added on 2026-09-15: Final verdict of Reviewer 2

Review Report Form

Quality of English Language
( ) The English could be improved to more clearly express the research.
(x) The English is fine and does not require any improvement.

	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	(x)	( )	( )	( )
Is the research design appropriate?	(x)	( )	( )	( )
Are the methods adequately described?	(x)	( )	( )	( )
Are the results clearly presented?	(x)	( )	( )	( )
Are the conclusions supported by the results?	(x)	( )	( )	( )
Are all figures and tables clear and well-presented?	(x)	( )	( )	( )

Comments and Suggestions for Authors

I thank the author for responding to the concerns I raised in my previous two reviews. The manuscript has been substantially expanded, and the author has addressed nearly all of my suggestions and comments. The new appendices are also very helpful in strengthening the claims presented.

The author and I hold a strong and fundamental disagreement regarding the nature of radiative-convective equilibrium and its implications for the atmospheric greenhouse effect. I have previously suggested that the author focus this manuscript on establishing the benefits of the stochastic framework and treat the work on radiative-convective equilibrium in a separate paper. I do not believe there is anything further I can usefully add to the manuscript in its current form. It will ultimately fall to the broader scientific community to evaluate the merits of these claims and to determine whether the proposed framework is robust enough to challenge decades of research in atmospheric physics.

Nevertheless, the paper contains sufficient original scientific contribution to warrant publication in its present form.

Submission Date 02 August 2026

Date of this review 31 Aug 2026 13:23:43

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Annex
Report on review comments of version 1

Report on review comments of "Stochastic Foundations of Atmospheric Thermodynamics: Deriving the Laws from Maximum Entropy and Implications for Earth's Climate"

by Demetris Koutsoyiannis (dk@itia.ntua.gr)

Introductory notes

The manuscript with the above title was submitted on 13 June 2026 to the journal *Geosciences* (Manuscript ID: geosciences-4406555), following an invitation from the journal. It received reviews by Reviewers #1, #2 and #3. The decision, as stated in the decision letter of 15 July 2026, was the following

> Based on the evaluations of the reviewers and the Editorial Board Member, we have decided that your manuscript may be of interest to our readers and has the potential to be published in Geosciences. However, significant revisions or new data are required to warrant further processing of your manuscript; therefore, at this stage, it will be rejected to allow you to make your revisions.

Below I reproduce all the review comments without any editing except numbering them by Reviewer, along with my detailed responses and explanations of how I have addressed them.

Because of the numerous review comments, I had to conduct additional analyses and expand the paper accordingly. In addition, I followed the reviewers' suggestions to add Appendices with proofs of some key theoretical aspects. In general, I have tried to keep the main paper as concise as possible and hence most of the additional analyses have been placed in Appendices.

I have also tried to make this Report self-contained and therefore I include in it parts from the revised paper (but not the whole text of the Appendices to keep the Report relatively concise).

Key:

> Review comment.

> Copy from the original (round 1) manuscript—possibly with reviewer's highlights.

Response.

Quotation from revised manuscript (possibly with slight differences due to last moment changes).

Text generated by AI systems for the sake of independent assessments in response to the negative assessment of Reviewer #2.

Note: The numbering of references included here is sequential and does not correspond to that of the paper.

Reviewer #1

R2.1.

Review Report Form

Open Review (x) I would not like to sign my review report
( ) I would like to sign my review report

Quality of English Language ( ) The English could be improved to more clearly express the research.
(x) The English is fine and does not require any improvement.

	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	(x)	( )	( )	( )
Is the research design appropriate?	(x)	( )	( )	( )
Are the methods adequately described?	(x)	( )	( )	( )
Are the results clearly presented?	(x)	( )	( )	( )
Are the conclusions supported by the results?	(x)	( )	( )	( )
Are all figures and tables clear and well-presented?	(x)	( )	( )	( )

Comments and Suggestions for Authors: See review report

peer-review-57401378.v2.pdf

Submission Date: 13 June 2026

Date of this review: 06 Jul 2026 20:00:03

I am grateful to the Reviewer for the very positive assessment of the paper.

R1.2.

Summary

The paper proposes an axiomatic, stochastic foundation of entropy that aims to unify probabilistic and physical entropy. Entropy is defined as quantified uncertainty with four postulates leading to a maximum-entropy principle framed as an ontological physical principle.

Based on this framework and the fundamental governing principle of nature, the author derives the classical laws of thermodynamics for gases and as atmospheric applications presents the Planck and Stefan–Boltzmann equations as direct consequences within the same axiomatic scaffold.

It is further shown that molecular-scale entropy maximization yields an isothermal equilibrium state; gravity does not change that molecular equilibrium but differentiates it from isentropic macroscopic parcel behavior. Radiatively active gases interacting with shortwave and longwave radiation are then considered to explain observed non-equilibrium atmospheric vertical profiles.

Significance and novelty

All derivations are satisfying well-known physical principles, and the atmospheric implications are quantitatively demonstrated. Therefore, the presented framework could offer a unifying approach across statistical mechanics, classical thermodynamics, and atmospheric physics, with didactic and possibly methodological value for parameterizations. It offers new insights into molecular uncertainty, emergence of macroscopic structures and radiation in shaping Earth's climate. It also suggests a broader stochastic view of nature and atmospheric processes.

I am grateful to the Reviewer for the nice summary of the paper, and I am glad to read her/his assessment of its significance and novelty. I thank her/him for the constructive attitude of the review comments.

> **R1.3.**
> **A few suggestions for clarity**
>
> – The four postulates are clear and look consistent with the Shannon description of entropy. But it would be helpful for the reader to clarify the scope and limits of postulate 3, which effectively enforces separability of all relevant partitions. So, it would be important to know, how far this holds for real gases with attractive forces during collisions or for more complex systems with coupled degrees of freedom and non-separable entropies.

In the revised manuscript (section 2.2) I have clarified that the postulates (including Postulate 3) are not connected to particular systems, such as gases and collisions in them, but should be (and are) general:

> It is evident that the above postulates are abstract and general in order to characterize uncertainty and its maximization in the most abstract and broad way possible. No reference is made to any specific field of application, let alone to the specific constraints of a particular system. As already noted in the Introduction, the definition of entropy should be independent of constraints.

Related to postulate 3 is also my reply to Comment R3.16.

In terms of caveats in the applicability of the entropic framework to more complex systems, I have now clarified (in section 3.2) that these would need additional constraints, which however are out of the scope of the paper:

> Dependence can also appear in dense systems, where the free path between collisions, λ, is of the order of molecular size, d. Such systems are characterized by macroscopic coupling and cross-dependencies, which are strong, persistent, and highly complex. These systems cannot be represented by the above setting, which is appropriate for $\lambda/d \gg 1$ so that the systems be regarded as collections of isolated particles that only interact during brief, instantaneous collisions. Instead, when a fluid becomes dense ($\lambda/d \to 1$), molecules are in a constant state of overlapping contact. Apparently, the study of such systems should require additional constraints and more involved treatment. However, the Earth's atmosphere is characterized by ratios λ/d of the order or several hundreds and can therefore be represented as a dilute gas. Hence the study of dense systems is out of our scope of typical atmospheric applications. Additional information about this issue is given in [1].

In short, interactions (attractive forces, coupled degrees of freedom, etc.) are accommodated by additional constraints in the maximisation problem; they do not alter the form of the entropy functional derived from the four postulates.

Please see also my reply to Comment R3.3.

R1.4.

– The author distinguishes molecular equilibrium (isothermal) from macroscopic isentropic parcels. It would clarify their applicability, how certain constraints may select isothermal vs. adiabatic profiles when additional conserved quantities are enforced, e.g., when under gravity a barometric pressure profile with non-isothermal solution is expected or when adiabatic parcel considerations lead to a dry adiabatic lapse rate. It would also be good to see how radiation effects can modify an expected isothermal atmosphere in absence of convection (see lines 538 and 539) and an isentropic atmosphere with convection.

I thank the Reviewer for these remarks. In response, I have added the following paragraphs in section 3.11:

> It is stressed that the standard and local profiles presented are not theoretically derived (by deduction) but were compiled from long-term term observations. In this case, while theoretical reasoning does provide qualitative explanations on what is observed in vertical atmospheric temperature profiles, it does not suffice for quantitative modelling. The processes are too complex to describe within a simple entropic framework in an independent column approximation. Convection (vertical heat transfer), advection (horizontal heat transfer by wind from warmer areas, also affected by Coriolis forces and cell formation), radiation, cloud formation, precipitation and several other processes are acting together and their final outcome cannot be predicted by deduction, unlike the analyses of previous sections. However, a stochastic framework, on which entropy and its maximization principle are founded, also supports induction, i.e., inference based on data.
>
> Clearly, the atmospheric profiles discussed here are induction-based. The most impressive finding of this induction is the near constancy of the lapse rate, which remains close to the standard of 6.5 K/km across the troposphere and in all areas. While this signifies a tendency toward the isentropic profile in the troposphere, we cannot maintain that the troposphere is isentropic—whether dry or moist. This becomes clear from the polar areas where the dry and moist isentropic profiles almost coincide with a lapse rate of 9.8 K/km This coincidence can be explained by the low presence of water vapour, which also substantially reduces the radiative activity. However, in the polar areas the actual rate, instead of tending to 9.8 K/km, is lower even than the standard of 6.5 K/km (Figure 7). Yet the local profiles clearly reflect the connection with convection: In the polar regions and midlatitude winters, the atmosphere is closer to the isothermal profile than in tropics, because of the lower level of convection, whose full absence would necessarily lead to an isothermal atmosphere.

R1.5.

– The present manuscript refers the reader to Ref. 3 for full proof of deriving Planck's law and the Stefan–Boltzmann equation from maximum-entropy. Nevertheless, also for this research article, a shorter outline of the key steps, constraints, and the exact form of the maximized functional would be desirable, e.g., as Appendix.

In response to this Reviewer's comment, I have added an appendix with the following title:

> Appendix A.2: Derivations for Planck's Law for blackbody radiation

Also, in response to related comments by Reviewer #3, I have added another appendix with the following title:

Appendix A.1: Proof of theorem 2 for the derivation of the entropic formula

Two more new appendixes with additional information are also contained in the revised manuscript (see below).

> **R1.6.**
> – Also the barometric formula and isothermal equilibrium under gravity are explicitly derived from the proposed max-entropy concept and then could be checked how additional radiative constraints might perturb the profile.

I thank the Reviewer for this remark. The revised manuscript contains two new appendices dealing with this remark, entitled:

Appendix A.3: Atmospheric radiative cooling as an emergence of Le Chatelier's principle

Appendix A.4: Plausibility that the atmospheric profile of thermodynamic equilibrium is also at radiative equilibrium

Specifically, radiation constraints are not easy to formulate and quantify, because the emissivity of the atmosphere varies substantially depending on many factors. This is explained in the following text taken from Appendix A.3:

However, while emissivity of Earth's surface ϵ_S is almost constant and close to 1 (typically between 0.95 and 0.97), the bulk atmospheric emissivity ϵ_A is not a physical constant. Rather, it increases with rising temperature for several reasons, thereby enhancing the adaptivity still further. One such reason is the spectral shift, described by Wien's displacement law (equation (A37)). As temperature increases, $\lambda_{\max}$ decreases and the spectrum shifts toward smaller wavelengths. As illustrated in Figure 9 of the main text, this shift brings a larger portion of the spectrum into the region where water vapour is strongly radiatively active, thereby increasing the effective emissivity. Yet at lower temperatures (high altitudes) it is the carbon dioxide that becomes more active, as the emission band is centred at its thermal emission peak (14-15 μm). Still the relationship of emissivity with temperature is dominated by the presence of water.

Another reason originates from the geometric distribution of the probability that a photon of frequency ν is emitted. According to equation (37) (setting $j = 0$) this equals $1 - P_0 = \exp(-h_\mathrm{p}\nu \,/\, k_B T)$, which entails that the probability increases with increasing temperature. Also, at higher temperatures molecules have high kinetic energy and collisions are more frequent. A large percentage of molecules are constantly bumped into excited states, giving a higher emissivity for higher temperatures.

An additional reason is related to the fact that the saturation water vapour pressure increases almost exponentially with temperature. For the same relative humidity, the concentration of water molecules tends to be higher at higher temperatures. As water is the strongest RAG, this results in higher bulk emissivity.

This should be combined with another factor which is not directly related to temperature but it becomes relevant in the presence of a positive lapse rate: the rise in temperature goes together with the rise in pressure. At higher pressure, collisions are more frequent, again resulting in higher bulk emissivity at higher temperatures.

As a combined result of all these factors, in the standard atmosphere the atmospheric emissivity drops from over ~0.80 near the ground to ~0.10 near TOA, while at the same time the temperature drops from an average of 288 K near the ground to less than 230 K at the tropopause.

This is the actual reason why I used RRTM for this part of the paper, rather than seeking analytical solutions. Yet in Appendix A.4 I attempted an analytical solution for a special case, the isothermal profile. The analysis I present there confirms the RRTM results that the isothermal profile remains stable if we also consider the radiation processes.

R1.7.

– Interesting could be a radiative–convective column illustrating how shortwave/longwave interactions within this maximum-entropy framing yield realistic near-surface temperatures and lapse rates.

The radiative–convective behaviour is examined with RRTM calculations and discussed in the new Appendices A.3 and A.4 (see also the reply to Comment R1.6); an analytical treatment is possible (albeit with simplifying assumptions) only for the special isothermal case, as seen in Appendix A.4.

R1.8.

– In Subsection 4.4 the Atmospheric Radiative Effect (ARE) is only considered as the result of suppressed convection. But it is clear from measurements and calculations that longwave radiation as backradiation can be observed and contributes to the energy balance at the Earth's surface (see, e.g., *Pat Arnott*, 2008: University of Nevada, Reno, ATMS 749; Harde, 2013, http://dx.doi.org/10.1155/2013/503727, Fig. 22). Therefore, only a combination of convection, evaporation and radiation can be responsible for the temperature at ground and the lapse rate that is forming.
Without IR-active gases and thus, without their radiation to space (also to the ground), convection cannot contribute to any cooling, and in this sense convection is suppressed; then all absorbed solar radiation (or the Sun's feed with low entropy) has to be reradiated by the surface with temperatures as correctly specified for different conditions in Table 2. But can a construct without radiatively active gases still be called ARE?

I thank the Reviewer for this comment and the reference, which I cited in the revised manuscript.

I generally agree with the Reviewer's thesis that a combination of convection, evaporation and radiation can be responsible for the temperature at ground and the lapse rate. But I believe this thesis is not popular in the climate literature, where the role of radiation (and in particular that produced by CO_2) is overemphasized. Hence, I believe there is a need to highlight the importance of the other factors. In the revised manuscript I have described the roles of the different factors, as I perceive them, in the following manner (section 4.4):

In our framework, ARE is not absent, but constitutes the final link of the chain shown in Figure 11. Specifically, ARE is necessary for the maintenance and stability of the lapse rate, which is created by convection. This stability requires an energy source at the bottom and a sink at the top. Therefore, in a fully transparent atmosphere (without the ARE), while the perpetual changes listed in section 3.8 would still occur and would produce convection, that convection would vary in time and space, and the lapse rate would likely become more variable and could frequently change sign locally. In that imaginary case, atmospheric cells, such as convective systems and global circulation cells, as well as the evaporation–condensation–precipitation processes would likely have a more complex structure and an enhanced role in energy redistribution within the atmosphere. In the real atmosphere ARE properly adapts the energy balance so that the lapse rate remains at the value determined by thermodynamics, rather than imposing a different value of its own. The adaptability of radiation with respect to lapse rate will become more evident in section 4.5, where it will be seen that the net effect of ARE on the surface temperature (without considering convection) can be either warming (for the most common case of positive lapse rate) or cooling (in the case of atmospheric inversion). In this sense the radiative adjustment functions as the restorative mechanism described by the notion of Le Chatelier's principle. This principle states that, if a dynamic balance is disturbed by changing conditions, the system shifts its state to counteract that change and restore balance. Further clarification and justification of the proposed logical chain is provided in Appendix A.3, also using real-world atmospheric data.

> **R1.9.**
>
> – It is also not clear, when the author writes: "Manabe and Strickler's results imply that convection decreases the surface temperature while RRTM results and all other thermodynamic and radiative analyses presented show the opposite" (lines 799-801).
> Shall this be understood that convection increases the surface temperature and radiation reduces it? Convection is generally observed when a warmer parcel is lifting up and colder air is flowing in, thus reducing the surface temperature in agreement with Manabe and Strickler. Independent, with or without convection, is the surface radiating and carrying energy away (Stefan-Boltzmann). From RRTM calculations it normally follows how much of this surface radiation and local thermal energy is reradiated by the IR-active gases in upward- and downward-direction. And indeed, this gives a surplus with IR-actives gases in the atmosphere compared to no RAGs.

I thank the Reviewer for this interesting remark, which prompted me to describe my line of thought in the following manner, both verbally, in section 4.4, and schematically with the new Figure 11:

In contrast, in the atmosphere, the surface warming relative to the effective radiating temperature occurs through the establishment of the lapse rate via convection. Without convection, the temperature profile would be isothermal (as is in a real greenhouse). An isothermal atmosphere would lead to an average surface temperature of 255 K (see Table 2 and its description). It is the temperature gradient that makes the surface warmer than this. This may sound paradoxical if viewed on a local scale, e.g. above a warm ground where convection removes heat. However, it should be viewed on a macroscopic basis including the entire air column and not as a local phenomenon. Convection does not impact the average temperature of the atmospheric column, which remains close to 255 K and this is indeed the case as seen in Figure 10. A temperature equal to the column average one appears at about 5.7 km. As illustrated above, if we take this "stationary point" (the intersection of the isothermal at $T = 255$ K and an inclined profile) and apply a slope of 6.5 K/km to the inclined profile, then we find that the surface temperature will be close to 288 K. The warming (from ~255 K to ~288 K) is a necessary consequence of the lapse rate, which in turn appears because of convection. Indeed, there would not be lapse rate without convection. Convection occurs in a reversible adiabatic manner, thus pushing the atmosphere toward the isentropic profile. In an isentropic profile, temperature decreases with increasing altitude (see Figure 3)—hence the lapse rate. This line of reasoning is summarized in the schematic of Figure 11. Note that this ordering is the reverse of the classic Manabe and Strickler (1964 [2]) pure-radiative versus radiative-convective comparison (see section 4.5); the difference arises because here the lapse rate is set by the entropy maximization framework rather than by radiative equilibrium.

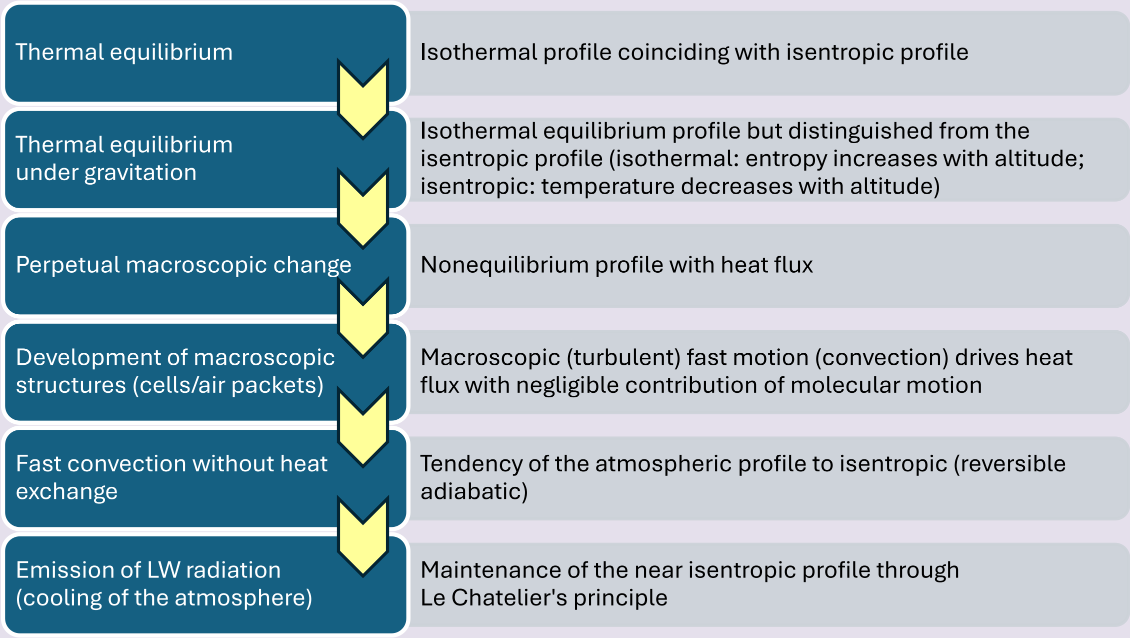

Figure 11. *Logical chain for the formation of the atmospheric temperature profile according to the proposed approach.*

As per the role of radiation, this is described in my reply to the previous comment R1.9 and in Appendix 1.3.

R1.10.

Minor comments and corrections

- Line 78: … he arrived as entropy ..
- Line 118: … rather than following Jaynes …
- Eq. (6): Not all readers know the meaning of E, should be defined?
- Line 222: β is used as background density and later as degrees of freedom. Should at least be mentioned on first appearance.
- Line 224: f(x) should also be noted.
- Eq. (9): Wouldn't it be better to write:

$$-\int_{-\infty}^{\infty} f(x)\cdot ln\left[\frac{f(x)}{\beta(x)}\right]dx \ ?$$

- Line 248: See line 222.
- Line 259: std[$\underline{x}$]/E[$\underline{x}$] should be explained.
- Eq. (30): The agreement of this equation with measurements looks excellent, but gives it sense to specify e_0 to 5 decimal places?
- Line 575: Better: "Thus, in order to remain in steady state, a system must receive energy equal to …"
- Line 646: Better: "… is partly reflected and absorbed when reaching the Earth"
- Line 661: … corner of ~~in~~ Table 2, …
- Fig. 10: Is the ordinate with p = 400 hPa correct? Shouldn't this be 500 hPa?
- Line 721: …incoming and reflected sunlight → better is "scattered" sunlight.

I thank the Reviewer for the attentive reading and the spotting of these errors or imperfections, which I corrected in the revised manuscript (except the "arrived at" which I think was grammatically correct). In particular, I changed the symbol of the background measure from β to b.

R1.11.

Recommendation

The manuscript is well written with helpful historical context to probabilistic and physical entropy. As the proposed ambitious principle of maximum-entropy presupposes some deeper background in thermodynamics and stochastics, the author refers to his book (Ref. 3), which can well act as supplementary material. Nevertheless, some shorter outline of the key steps and constraints, e.g. when deriving Planck's law could further complete this very valuable work.

It would also help to avoid misinterpretations, when the author reconsiders some of the phrasing and explanations listed as *Suggestions for clarity*. However, it's to the author to decide how far this and other mentioned suggestions may be integrated adequately.

With only minor corrections the manuscript is strongly recommended for publication in Geosciences.

I hope I addressed the Reviewer's comments and suggestions successfully, including the suggestion to add extra information (in the new appendices).

I appreciate the Reviewer's strong recommendation for publication in *Geosciences*, and I hope that it becomes a catalyst for the final publication of the manuscript.

Reviewer #2

R2.1.

Review Report Form

Open Review ( ) I would not like to sign my review report
(x) I would like to sign my review report

Quality of English Language (x) The English could be improved to more clearly express the research.
( ) The English is fine and does not require any improvement.

	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	( )	( )	(x)	( )
Is the research design appropriate?	( )	( )	(x)	( )
Are the methods adequately described?	( )	( )	(x)	( )
Are the results clearly presented?	( )	( )	(x)	( )
Are the conclusions supported by the results?	( )	( )	(x)	( )
Are all figures and tables clear and well-presented?	( )	( )	(x)	( )

Comments and Suggestions for Authors: my best recommendation is not to consider the present manuscript in any form any more. I personally find it hard to find any original contributions, and I believe it enough to return it in this manner based on this ground.

Submission Date 13 June 2026

Date of this review 10 Jul 2026 12:14:03

I appreciate the Reviewer's negative assessment of my paper, which in a way served as an incentive to improve the manuscript in its revised form.

R2.2. my best recommendation is not to consider the present manuscript in any form any more. I personally find it hard to find any original contributions, and I believe it enough to return it in this manner based on this ground.

The first half of the manuscript (Sec. 2) consists of a review of new developments of statistical mechanics. However, the materials therein are not directly relevant for geoscioences, thus I do not comment on this part directly. If the main originality of the work is found in this part, the author should send this manuscript to a journal of theoretical physics. This journal is not relevant to publish these materials.

Thus, the originality of the present manuscript must be judged by how the author applies those new theoretical developments to atmospheric science or geosciences in general. In my

> own judgment, the response to this question is negative: I do not see any direct or indirect use of those new developments applied in the later sections.
>
> In Sec. 3, the author tries to re-construct (atmospheric) thermodynamics from the scratch. However, the manuscript even does not explain why this is needed. The lead sentence of Sec. 3 merely says: "To find the state of the atmosphere we maximize entropy at the lowest level: the uncertainty of molecules in motion."
>
> Worse than that, I simply fail to identify any original insights. For example, Eq. (17) is an already well-know statistical definition of the entropy for the ideal gas. Secs. 3.3-3.8 read like copys from a textbooks. In Sec. 3.9, the author derives some standard atmospheric thermodynamics profiles that can easily found in any standard textbooks.
>
> As a whole, I simply fail to identify the main question addressed in this manuscript; it is very difficult to identify any originality, due to many well-known materials copied and pasted throughout the text, without any obvious link to other parts of the manuscript: the text essentially consists of a collection of segments of diverse materials without any clear mutual link. The last section of conclusions simply prove my judgments above: I only see textbook knowledge of statistical mechanics and thermodynamics listed therein. It is obvious that the author did not provide any insights on any processes in the atmosphere or any parts of the geosystem.
>
> I believe these judgments also supports my recommendation stated above.

Given the strongly negative overall assessment by Reviewer #2, here I focus on demonstrating the originality of the work, in particular in the atmospheric applications developed in sections 3.10–6, which the review does not discuss.

I believe that the paper is original, and highly novel and innovative. However, to avoid basing my arguments on my self-assessment, I am copying assessments by others, namely the two other reviewers and two AI systems, which may help defend my thesis.

The other reviewers' comments are included in full in this Report. Here I highlight a few phrases, which may oppose Reviewer's #2 judgment exemplified by her/his phrase: "I only see textbook knowledge of statistical mechanics and thermodynamics listed therein." The highlighted phrases point out either novel items in the manuscript or aspects at which the manuscript contrasts with existing knowledge.

Reviewer #1

> - The paper proposes an axiomatic, stochastic foundation of entropy that aims to unify probabilistic and physical entropy.
> - **Significance and novelty**
>   […] the presented framework could offer a unifying approach across statistical mechanics, classical thermodynamics, and atmospheric physics, with didactic and possibly methodological value for parameterizations. It offers new insights into molecular uncertainty, emergence of macroscopic structures and radiation in shaping Earth's climate. It also suggests a broader stochastic view of nature and atmospheric processes.

> - With only minor corrections the manuscript is strongly recommended for publication in Geosciences.

Reviewer #3

> - This is a very ambitious work, and it succeeds in recovering many familiar results from a common entropy-maximization framework – such as the Maxwell velocity distribution, the ideal gas law, the laws of blackbody radiation, and atmospheric lapse rates. In that sense, the paper demonstrates that the proposed framework is mathematically consistent over a broad range of classical problems.
> - […] the application of this framework to atmospheric physics conflicts with well-established radiative-convective equilibrium theory because the manuscript does not distinguish between equilibrium entropy maximization and the behavior of a continuously forced, nonequilibrium system.
> - This would be a genuinely novel approach, but I think it should be very clearly stated.
> - Modern atmospheric theory interprets convection as a response to radiative instability […] In contrast, the manuscript argues that convection itself creates the atmospheric temperature profile while radiative effects merely respond to the prescribed lapse rate.
> - The interpretation of Figure 11 is almost the opposite of the consensus in modern atmospheric physics.

The AI systems were asked to provide a reply to Reviewer #2. Their independent AI analyses of the review reached similar conclusions. Selected quotations are given below and the full replies to this review they suggested are provided in the Appendix of this Report (with full conversations and at the links indicated).

Claude

- I believe the review addresses only the first half of the paper (through Section 3.9) and does not engage with Sections 3.10 through 6, which contain the material most directly relevant to atmospheric science and geosciences and, I would argue, the paper's principal original contributions.
- Section 2 […] exists only to establish the entropy definition that every subsequent physical result in the paper depends on […] It is a foundation, not the paper's content.
- Sections 3.10 through 4.5 apply the entropy-maximization framework to derive, among other things: a closed-form expression for the saturation vapour pressure […] the moist and 'wet isentropic' atmospheric temperature profiles […] Planck's Law and the Stefan-Boltzmann Law […] an analysis […] of how the vertical temperature (lapse-rate) profile determines whether the atmospheric radiative effect warms or cools the surface […] a reassessment of the physical mechanism of the 'greenhouse effect' […]
- None of this is textbook material, and none of it is mentioned in the review.
- [T]he recommendation to reject rests on the claim that the paper contains no application of the new framework to geoscience beyond textbook reproduction of

known results, and that claim does not address roughly half of the manuscript (Sections 3.10–6).

Grok

- I respectfully disagree with the assessment that the manuscript lacks sufficient originality and should not be considered further. […] The work is not a mere compilation of textbook material but a coherent application of a new axiomatic entropy foundation to derive (rather than assume) key laws and explain observed behaviors.
- This provides a rigorous, uncertainty-based definition fully consistent with the principle of maximum entropy as an ontological principle. While this part has a theoretical physics flavor, it is foundational to the atmospheric applications that follow and enriches the geosciences by grounding thermodynamics in stochastics.
- [T]he framework derives these deductively from entropy maximization + conservation of energy, yielding new clarity on key distinctions (e.g., molecular isothermal equilibrium vs. macroscopic isentropic air parcels under gravitation).
- It further provides: Quantitative support via RRTM simulations […] Explanations for moist/dry lapse rates, phase changes, and non-equilibrium profiles driven by convection and radiation. A parsimonious view of the atmospheric radiative effect (ARE) and critique of ‘greenhouse’ terminology […]
- The manuscript’s strength lies in its unification: thermodynamics emerges from stochastics, providing deeper understanding of uncertainty, emergence, and non-equilibrium processes in the atmosphere.

Reviewer #3

R3.1.

Open Review	(x) I would not like to sign my review report ( ) I would like to sign my review report
Quality of English Language	(x) The English could be improved to more clearly express the research. ( ) The English is fine and does not require any improvement.

	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	(x)	( )	( )	( )
Is the research design appropriate?	( )	(x)	( )	( )
Are the methods adequately described?	( )	(x)	( )	( )
Are the results clearly presented?	( )	(x)	( )	( )
Are the conclusions supported by the results?	( )	( )	(x)	( )
Are all figures and tables clear and well-presented?	( )	(x)	( )	( )

Submission Date	13 June 2026
Date of this review	14 Jul 2026 01:23:10

I thank the Reviewer for the careful evaluation and for the constructive comments that follow.

R3.2. In this manuscript, the author attempts to reformulate statistical mechanics and atmospheric thermodynamics using an entropy framework derived from stochastic uncertainty rather than the traditional foundations of statistical mechanics based upon information theory. This is a very ambitious work, and it succeeds in recovering many familiar results from a common entropy-maximization framework – such as the Maxwell velocity distribution, the ideal gas law, the laws of blackbody radiation, and atmospheric lapse rates. In that sense, the paper demonstrates that the proposed framework is mathematically consistent over a broad range of classical problems.

However, the manuscript repeatedly overstates what has actually been demonstrated. In many cases, standard thermodynamic relations are recovered only after additional assumptions are introduced, and several foundational arguments remain incomplete. More importantly, the application of this framework to atmospheric physics conflicts with well-established radiative-convective equilibrium theory because the manuscript does not distinguish between equilibrium entropy maximization and the behavior of a continuously forced, nonequilibrium system. My full comments are attached, but the most significant comments are given below.

I am grateful to the Reviewer for the summary of the paper, and I am glad that she/he found it ambitious. I am impressed by the number, depth and detail of her/his Comments and her/his constructive attitude, and I express my special thanks for this.

I do not think that assumptions additional to those explicitly stated are introduced, as I clarify in reply to the general and the specific comments below.

Indeed, the proposed framework conflicts with established theories, and I have not hidden it even in the original manuscript. Rather, I explicitly state these conflicts. In the revised manuscript, I have provided additional support for my analyses that are against the established theories, including in the new Appendices A.3 and A.4.

I regard the questioning of established theories, when supported by new analyses, as a legitimate path to scientific progress.

> **R3.3.** The statistical-mechanical development is generally the strongest part of the paper, but several important derivations require clarification. For example, the extension from a single-particle distribution to the many-particle distribution in Eq. (16) implicitly assumes statistical independence of the particles. In traditional statistical mechanics, this result follows as a theorem, whereas in this paper, it is presented as a postulate. If this is indeed the intended argument, it should be stated explicitly rather than left implicit. Likewise, the author is able to recover the extensive nature of entropy, but this is done by conditioning entropy on a specific spatial partition. The manuscript does not explain why this particular conditioning is uniquely required by the underlying physics.

I am glad that the Reviewer found the statistical-mechanical development to be the strongest part of the paper. However, I respectfully disagree with the remark about an implicit assumption. What the Reviewer regards as an implicit assumption is resulting from entropy maximization for the given constraints. The following paragraph has been added now which clarifies this issue (section 3.2):

> It is stressed that the independence in the motion of different molecules is a result of entropy maximization under the given constraints, and not an assumption. It is also stressed that, similar to the case of one molecule, the energy constraint is expressed in terms of the *expectation* of the total kinetic energy of all molecules and not as a *fixed* value of the total kinetic energy. Had we used a fixed value instead, this would create dependence among different molecules, as well as departure of the probability distribution of the molecules' velocities from the normal distribution. However, as N would tend to infinity, the difference would tend to disappear (because of ergodicity, i.e., equality of the average of infinitely many variables with the true average) and independence to reappear. Practically this happens for N as low as 100. Hence, even in the case of a fixed value constraint, the dependence could be totally neglected in practical problems, where N is of the order of Avogadro's number.

> **R3.4.** A similar issue arises in the definition of temperature. The manuscript correctly derives an intensive variable θ-1=∂Φ/∂E having dimensions of energy, which is entirely analogous to temperature in traditional statistical mechanics. However, the subsequent identification S=kθ and T=θ/k is introduced rather than derived. This distinction is important because one of the principal accomplishments of conventional statistical mechanics is proving that the entropy derivative coincides with the empirical Kelvin temperature established by thermodynamics. In the present framework Boltzmann's constant remains a bridge between the information

entropy and physical entropy, implying that the thermodynamic temperature scale has not yet emerged naturally from the proposed axioms.

I respectfully disagree that “Boltzmann's constant remains a bridge between the information entropy and physical entropy”. In my opinion, both the unit of Kelvin and Boltzmann’s constant are arbitrary (and unnecessary) choices. The bridge between entropy, a purely probabilistic concept, and physics relies on the principle of maximum entropy, which I attempt to elevate from epistemological to ontological concept. It cannot rely on an arbitrary numerical constant.

I accept that the proposed framework does not derive the empirical kelvin scale from the axioms. But it should not do so, as the identification remains a conventional correspondence. The advantage I claim is conceptual economy: once the correspondence is made, all subsequent thermodynamic relations follow from entropy maximization without further postulates.

I have now expanded the related part in section 3.3, which now reads as follows:

> Here the temperature θ has energy units and differs from the classical temperature in physics, T, measured in kelvins. The unit of kelvin was an arbitrary choice (a historical accident) which in our framework is rendered an energy unit: 1 K = 13.81 yJ. The fact that the kelvin unit and the Boltzmann constant are both arbitrary choices that need each other to be made consistent, is expressed even in the definition of the kelvin, according to the SI Brochure [3]:
>
> > “[The kelvin] is defined by taking the fixed numerical value of the Boltzmann constant, k, to be 1.380 649 × 10^{-23} when expressed in the unit J K^{-1}, which is equal to kg m^2 s^{-2} K^{-1} […]
> >
> > $$1\ \mathrm{K} = \left(\frac{1.380\,649\ \times\ 10^{-23}}{k}\right)\ \mathrm{kg\ m^2 s^{-2}}$$
> >
> > […] The effect of this definition is that one kelvin is equal to the change of thermodynamic temperature that results in a change of thermal energy kT by 1.380 649 × 10^{-23} J.”
>
> Although, according to SI, the kelvin is derived from Boltzmann’s constant, the historical course was the opposite, and this constitutes the historical accident. First Kelvin defined the unit in 1848 [4] and later in 1900, for consistency with the arbitrary Kelvin scale, Planck [5] introduced the constant, which he named Boltzmann-Drude constant.
>
> Yet the two quantifications are equivalent: Our definition of entropy through theorem 2 (equation (2)) allows for any arbitrary multiplicative constant k, which here for parsimony we set $k = 1$. If we took it $k = k_\mathrm{B}$ = 1.38 × 10^{-23} J K^{-1} (i.e., equal to the Boltzmann’s constant) we would find the classical entropy, S, in lieu of Φ^*, while the derivative $\partial S/\partial E$ would equal $1/T$. The two quantifications are therefore related by the proportionality relationships:

$$S = k_{\mathrm{B}}\, \Phi^*, \qquad T = \frac{\theta}{k_{\mathrm{B}}} \tag{1}$$

R3.5. The most significant revisions are needed in Sections 3 and 4. Sections 3.8 through 3.11 successfully reproduce familiar atmospheric structures (including the Clausius-Clapeyron relation along with the dry and moist adiabatic lapse rates) but these are already well-established consequences of the first law of thermodynamics, hydrostatic balance, and the ideal gas law. It's unclear what additional explanatory power the stochastic entropy framework provides. Likewise, the ERA5 comparisons in Figure 7 demonstrate agreement with observed atmospheric profiles, but they do not uniquely validate the proposed entropy formulation because essentially any atmospheric model incorporating hydrostatic balance, realistic thermodynamics, and convection reproduces similar large-scale temperature structures.

I did not dispute the fact that the Clausius-Clapeyron relation, the dry and moist adiabatic lapse rates, etc., are familiar and well-established consequences of the laws of thermodynamics. The contribution of the paper is that it derives them without assuming the laws of thermodynamics. Actually, as described in Section 5, it derives the laws of thermodynamics from the stochastic approach proposed, rather than assuming them as first principles.

Figure 7 demonstrates agreement with observed atmospheric profiles, but this agreement is not perfect at all. The purpose of including the figure is to stress the need to use observational data—the ultimate judge of theories. In complex systems like the atmosphere, one cannot expect to develop perfect models of deterministic nature. The stochastic framework developed in the paper offers the option of induction, based on data, in addition to that of deduction, using standard mathematics and apodeictic syllogism.

This is expressed in the revised manuscript in the following paragraph (section 3.11):

> It is stressed that the standard and local profiles presented are not theoretically derived (by deduction) but were compiled from long-term term observations. In this case, while theoretical reasoning does provide qualitative explanations on what is observed in vertical atmospheric temperature profiles, it does not suffice for quantitative modelling. The processes are too complex to describe within a simple entropic framework in an independent column approximation. Convection (vertical heat transfer), advection (horizontal heat transfer by wind from warmer areas, also affected by Coriolis forces and cell formation), radiation, cloud formation, precipitation and several other processes are acting together and their final outcome cannot be predicted by deduction, unlike the analyses of previous sections. However, a stochastic framework, on which entropy and its maximization principle are founded, also supports induction, i.e., inference based on data.

R3.6. Similar concerns arise with the discussion of moist thermodynamics in Section 3.10. The proposed vapor-pressure equation agrees well with observations, yet the derivation appears to represent liquid water as a collection of additional kinetic degrees of freedom. This treatment neglects configurational entropy and the cooperative molecular structure characteristic of liquids, while the parameter governing bond breaking effectively absorbs much of the underlying intermolecular physics. Consequently, it is difficult to conclude that the vapor-pressure relation emerges solely from entropy maximization rather than from empirical tuning of appropriate constants.

I agree with the Reviewer: the additional degrees of freedom in liquid water are not kinetic, of course. I had missed noting that in the original manuscript. I have now rephrased the related paragraph and added a reference. Now it reads (section 3.10):

> To find the equilibrium state between the liquid and gaseous state of water, we maximize the uncertainty of single molecule with respect to: (a) its phase (whether gaseous or liquid); (b) its location in space; and (c) its kinetic state, i.e., its velocity and other coordinates corresponding to its degrees of freedom: $\beta_A = 6$ in gaseous phase, $\beta_B = 18$ in liquid phase [6], where the additional degrees of freedom in the liquid phase are not translational or rotational, but are due to local clusters with low energy vibrational modes that can be thermally excited, reflecting the collective, low-frequency intermolecular modes of water molecules.

The fact that the number of degrees of freedom of liquid water is an experimental value does not invalidate the proposed stochastic framework. Rather it better illustrates it, as stochastics does not exclude induction (inference based on data) when deduction is impossible or too difficult.

R3.7. The most significant conceptual difficulty appears in the discussion of atmospheric radiative-convective equilibrium. Throughout Sections 4.4 and 4.5 the author repeatedly reverses the established causal relationship between radiation and convection. Modern atmospheric theory interprets convection as a response to radiative instability: solar heating and infrared radiative emission establish a radiative temperature structure that is often steeper than the dry adiabatic lapse rate, and convection subsequently redistributes heat upward until the atmosphere approaches convective neutrality. In contrast, the manuscript argues that convection itself creates the atmospheric temperature profile while radiative effects merely respond to the prescribed lapse rate. This same misunderstanding appears in the comparison with Manabe and Strickler (1964). Their classical calculation addressed the question: What temperature profile develops if radiation alone transports energy? The resulting radiative-equilibrium atmosphere possesses an unrealistically steep lapse rate that is unstable to convection. By contrast, the present manuscript prescribes a family of linear lapse rates and examines the corresponding radiative fluxes. These are fundamentally different boundary-value problems. Consequently, the manuscript's conclusion that RRTM contradicts Manabe and Strickler (1964) is not justified because the two studies do not examine the same physical problem.

The Reviewer is right that I reverse the "***established causal relationship between radiation and convection***". The standard view, which the Reviewer accurately summarizes, holds that solar heating and infrared cooling first establish a radiatively determined temperature profile that is steeper than the dry adiabatic lapse rate; convection then acts as a response that redistributes heat until the atmosphere approaches convective neutrality. In the framework developed here the order is inverted: the thermodynamic tendency toward maximum entropy under the constraints of mass, momentum, energy and gravity establishes a near-isentropic (or moist-isentropic) lapse rate through macroscopic convective motions; radiation, being highly adaptive through the temperature dependence of emissivity and the distribution of water vapour, functions as the restorative agent that maintains global energy balance (Le Chatelier's principle).

In the revised version I have made substantial changes to better present my own understanding and support my thesis that is opposite to the established one.

First, in section "4.4 Is the atmosphere a greenhouse?" I have rephrased and expanded the related text, and added Figure 11 as follows:

> In contrast, in the atmosphere, the surface warming relative to the effective radiating temperature occurs through the establishment of the lapse rate via convection. Without convection, the temperature profile would be isothermal (as is in a real greenhouse). An isothermal atmosphere would lead to an average surface temperature of 255 K (see Table 2 and its description). It is the temperature gradient that makes the surface warmer than this. This may sound paradoxical if viewed on a local scale, e.g., above a warm ground where convection removes heat. However, it should be viewed on a macroscopic basis including the entire air column and not as a local phenomenon. Convection does not impact the average temperature of the atmospheric column, which remains close to 255 K and this is indeed the case as seen in Figure 10. A temperature equal to the column average one appears at about 5.7 km. As illustrated above, if we take this "stationary point" (the intersection of the isothermal at $T = 255$ K and an inclined profile) and apply a slope of 6.5 K/km to the inclined profile, then we find that the surface temperature will be close to 288 K. The warming (from ~255 K to ~288 K) is a necessary consequence of the lapse rate, which in turn appears because of convection. Indeed, there would not be lapse rate without convection. Convection occurs in a reversible adiabatic manner, thus pushing the atmosphere toward the isentropic profile. In an isentropic profile, temperature decreases with increasing altitude (see Figure 3)—hence the lapse rate. This line of reasoning is summarized in the schematic of Figure 11. Note that this ordering is the reverse of the classic Manabe and Strickler (1964 [2]) pure-radiative versus radiative-convective comparison (see section 4.5); the difference arises because here the lapse rate is set by the entropy maximization framework rather than by radiative equilibrium.

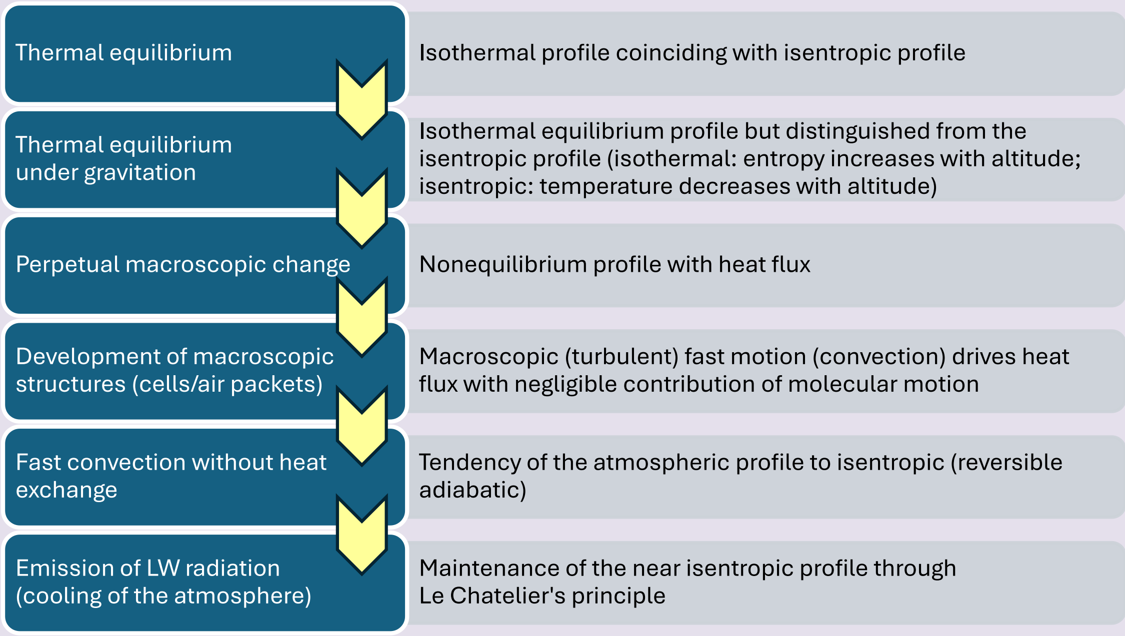

Figure 11. *Logical chain for the formation of the atmospheric temperature profile according to the proposed approach.*

Due to the opposite mechanisms driving the atmosphere from those in a real greenhouse, what has been called "greenhouse effect" for the atmosphere should have better been called an "anti-greenhouse effect".

As first highlighted by Aristotele, sapheneia (clarity and accuracy) of concepts and terminology is essential in science. The adoption of the wrong term "greenhouse effect" has several misleading consequences: mechanism mismatch (convection vs. radiation and implication of a static barrier); oversimplification and non-holistic view of complex processes; and unjustified emphasis on particular gases [30]. Here, as already mentioned, the terms *radiatively active gas* (RAG) and *atmospheric radiative effect* (ARE) are used.

In our framework, ARE is not absent, but constitutes the final link of the chain shown in Figure 11. Specifically, ARE is necessary for the maintenance and stability of the lapse rate, which is created by convection. This stability requires an energy source at the bottom and a sink at the top. Therefore, in a fully transparent atmosphere (without the ARE), while the perpetual changes listed in section 3.8 would still occur and would produce convection, that convection would vary in time and space, and the lapse rate would likely become more variable and could frequently change sign locally. In that imaginary case, atmospheric cells, such as convective systems and global circulation cells, as well as the evaporation–condensation–precipitation processes would likely have a more complex structure and an enhanced role in energy redistribution within the atmosphere. In the real atmosphere ARE properly adapts the energy balance so that the lapse rate remains at the value determined by thermodynamics, rather than imposing a different value of its own. The adaptability of radiation with respect to lapse rate will become more evident in section 4.5, where it will be seen that the net effect of ARE on the surface temperature (without considering convection) can be either warming (for the most common case of positive lapse rate) or cooling (in the case of atmospheric

> inversion). In this sense the radiative adjustment functions as the restorative mechanism described by the notion of Le Chatelier's principle. This principle states that, if a dynamic balance is disturbed by changing conditions, the system shifts its state to counteract that change and restore balance. Further clarification and justification of the proposed logical chain is provided in Appendix A.3, also using real-world atmospheric data.

I accept that the pure-radiative-equilibrium calculation of Manabe an Strickler and the calculations presented in this paper (prescribed thermodynamic lapse rates, RRTM radiative fluxes) are different problems. The comparison remains informative, however, because it highlights the contrast between a radiatively determined, convectively unstable profile and a thermodynamically constrained profile whose radiative consequences can be examined directly. The new observational analysis in Appendix A.3 (and specifically Figure A3) further shows that large seasonal variations in the surface-to-TOA LW radiation difference leave the tropospheric lapse rate essentially unchanged—consistent with radiation adjusting to a thermodynamically set lapse rate rather than determining it.

Furthermore, in the revised manuscript, I have rewritten a large part of section "4.5 How are the vertical temperature gradient and ARE linked?" to more clearly present my thesis that Manabe and Strickler's results are inconsistent to those of RRTM, as seen in the superadiabatic case and more vividly in the isothermal case. The modified text follows:

> The profile with the steepest gradient (superadiabatic case) in Figure 13, $\Gamma = 12$ K/km, results in a very positive $\mathrm{d}R_S/\mathrm{d}z$ near the surface and a negative $\mathrm{d}R/\mathrm{d}z$ in the upper atmospheric layers. Without convection, we will have $\mathrm{d}T/\mathrm{d}t < 0$ in the troposphere and $\mathrm{d}T/\mathrm{d}t > 0$ in the stratosphere, i.e. a clear tendency to the isothermal profile. This conclusion stands in contrast to the classical claim by Manabe and Strickler (1964 [2]) who argued that, in absence of convection, a "pure radiative equilibrium" would produce a very steep lapse rate. According to their claim, this would lead to a surface temperature of 332.3 K due to the "greenhouse effect". However, the RRTM result does not suggest that a very steep gradient can represent a form of stable "pure equilibrium" as Manabe and Strickler maintained.
>
> One may counter than Manabe and Strickler's "pure radiative equilibrium" is not associated with a linear vertical temperature profile as are the RRTM results. However, the asymptotically constant surface temperature for high lapse rates (clearly visible in Figure 12) and the general behaviour of the RRTM-derived radiative flux gradient $\mathrm{d}R/\mathrm{d}z$ (seen in Figure 13) make it paradoxical to suggest that a further increase in surface temperature, as suggested by Manabe and Strickler, would achieve radiative equilibrium without convection. On the contrary, all analyses converge on the conclusion that increasing the lapse rate enhances convection. A more detailed examination of this issue is not particularly meaningful, as it refers to idealized conditions irrelevant to reality, and therefore lies beyond the scope of this paper.
>
> Most interesting and enlightening are the RRTM results for the isothermal state ($\Gamma = 0$). We observe in Figure 13 that in this case $\mathrm{d}R/\mathrm{d}z$ is close to zero for the entire troposphere. A small negative $\mathrm{d}R/\mathrm{d}z$ appears in the lower atmospheric levels, which however is not deemed to be significant, as the accuracy of the model is not perfect. Also, an isothermal atmosphere would block convection and hence radically reduce evaporation. In RRTM

the default value of humidity is 80%, and this was not modified in our model runs. If taken lower, the negative $\mathrm{d}R/\mathrm{d}z$ disappears. For $\mathrm{d}R/\mathrm{d}z = 0$ we have $\mathrm{d}Q/\mathrm{d}z = 0$ implying no heat transfer and no convection. Therefore, the isothermal state is stable both with and without RAGs. In other words, for the isothermal case ($\Gamma = 0$), RRTM confirms that the thermodynamic equilibrium in the troposphere ($\mathrm{d}Q/\mathrm{d}z = 0$) is also a radiative equilibrium ($\mathrm{d}R/\mathrm{d}z \approx 0$). The plausibility that the atmospheric profile of thermodynamic equilibrium is also at radiative equilibrium is also studied in Appendix A.4 by means of simple and transparent calculations rather than sophisticated models. These simple calculations are consistent with RRTM results for an isothermal atmosphere.

It is stressed that RRTM is a purely radiative model and does not model or make dynamical assumptions about macroscopic atmospheric motion, including convection. Hence, its results are not affected by the presence or the absence of convection. Rather, the absence of convection is signified by the condition $\mathrm{d}T/\mathrm{d}z = 0$ in the temperature profile, i.e., by the isothermal atmosphere at about $T = 255$ K. Once RRTM results in $\mathrm{d}R/\mathrm{d}z \approx 0$ for this condition, the same condition $\mathrm{d}T/\mathrm{d}z = 0$ also signifies a radiative equilibrium. This is again opposite to the above mentioned claim by Manabe and Strickler that absence of convection would produce a "pure radiative equilibrium" with a very steep lapse rate and a surface temperature of 332.3 K. Furthermore, Manabe and Strickler's results imply that convection would decrease the surface temperature while RRTM results, as seen in Figure 12 suggest that the surface temperature increase is accompanied by increased lapse rate, which is also associated with increased convection intensity. That is, increased convection increases, rather than decreases, the surface temperature. Hence Manabe and Strickler's and RRTM results are inconsistent to each other. All other thermodynamic and radiative analyses presented in this work are also in support of the RRTM results.

Whether this reversal of the established ordering constitutes a "misunderstanding" is a separate question. In my view the terms "understanding" and "misunderstanding" are used too casually in scientific discourse. According to my understanding, these terms describe processes that are not algorithmic (and thus provable and objective). This has been articulated by Roger Penrose in his books "The Emperor's New Mind" [7] and "Shadows of the Mind" [8]. Different people may have different types of understanding which is reflected in their own works. My humble ambition is to present my own understanding.

In addition to the above revisions, I have added two new appendices (already mentioned in the above revised text):

Appendix A.3: Atmospheric radiative cooling as an emergence of Le Chatelier's principle

Appendix A.4: Plausibility that the atmospheric profile of thermodynamic equilibrium is also at radiative equilibrium

These contain detailed calculations and processing of real world data that provide further explanations and support for the key findings of the paper. For brevity of this Report, the Appendices are not reproduced here but can be found in the paper.

> **R3.8.** More generally, the manuscript repeatedly treats the atmosphere as though it were relaxing toward thermodynamic equilibrium. However, modern atmospheric physics views Earth's atmosphere as a forced-dissipative system in which solar radiation (at low entropy) is continuously supplied and terrestrial radiation (at high entropy) is radiated to space. Rather than assuming entropy maximization within a closed system, it would be more appropriate to examine Earth's atmosphere through the entropy production rate. In this view, radiation continually generate entropy while maintaining the atmosphere far from equilibrium. Convection is part of this irreversible adjustment process rather than an independent mechanism that competes with entropy maximization.

I do not think I repeatedly treated the atmosphere as if it were relaxing toward thermodynamic equilibrium. Rather, I clarified that this would only be the case if there were no mechanisms producing change. I also stressed that the thermodynamic equilibrium is the isothermal state, but this is never the case in the real atmosphere. Section 3.8 ("Equilibrium and non-equilibrium states of an air column") includes the following text:

> In reality, the Earth is not in equilibrium and hence the atmosphere is not isothermal. The non-equilibrium state is caused by perpetual changes occurring on all scales due to diverse mechanisms affecting radiation processes, both shortwave and longwave: day and night, cloud formation and disappearance, summer and winter, albedo's spatial and temporal variation (also affected by biosphere changes), Sun's dynamics and astronomical processes, orbital variations (Milanković cycles), tectonic and volcanic activity, and numerous other irregular changes by internal processes and external forces [9].
>
> In absence of equilibrium, there is heat transfer. While in solids heat is transferred by conduction, in fluids (atmosphere and oceans) heat transfer is dominated by convection, i.e. mass flow. When mass flows, conduction can be neglected as its power is orders of magnitudes smaller than in convection. This is particularly the case for air, whose thermal conductivity is 25 times smaller than in water.
>
> Therefore when there is convection, the macroscopic motion is mostly reversible adiabatic (where adiabatic means without heat exchange), i.e., isentropic. Ergo, the atmospheric temperature profile departs from the isothermal and tends to the isentropic.

I agree that entropy production is a powerful and natural concept for open, forced systems. In the present work I have deliberately restricted the analysis to entropy maximization under constraints (including constraints that keep the system away from equilibrium) in order to explore how far that single principle can be pushed before additional concepts become necessary. The analyses presented show that, indeed, entropy and the principle of maximum entropy are sufficient for the objectives of the paper. This I made clear in section 3.7 ("Interactions of bodies"):

> Contrary to classical equilibrium thermodynamics, the proposed framework can also deal with non-equilibrium cases. For the latter cases, we just put an additional constraint expressing the reason why the systems are not allowed to reach equilibrium.

[…]

It is stressed that in these non-equilibrium cases we apply precisely the same entropy maximization framework with additional constraints, and we do not involve additional concepts (e.g. the concept of entropy production or the hypothesis of maximum entropy production; e.g. [10-12]).

R3.9. Ironically, the proposed entropy framework appears capable of supporting the conventional greenhouse explanation that is firmly resisted throughout the paper. The positive entropy production associated with converting high-temperature solar photons into low-temperature terrestrial photons provides a natural nonequilibrium thermodynamic description of the climate system. Greenhouse gases modify this radiative pathway by changing the effective emission altitude, thereby altering both the energy balance and the entropy budget. I think this interpretation would align the new framework with modern atmospheric physics while preserving many of the manuscript's novel ideas.

I thank the Reviewer for these remarks, expressing her/his views about how the novel ideas could be reconciled with the existing views. I hope that she/he can tolerate the fact that my views are different. Also, my aim is different: to **highlight the differences** in my findings from the "consensus in modern atmospheric physics" rather than align the two (where the phrase in quotations is from another remark by Reviewer #3). Of course, there may be errors in my work, but there may be errors in the "consensus science" too. And of course, discovering and correcting errors is the way science progresses.

R3.10. Overall, this paper is original and mathematically sophisticated, and it successfully reproduces many classical thermodynamic results from a unified entropy-based framework. However, its strongest claims are not yet adequately supported. In particular, the manuscript would benefit from (i) more rigorous derivations of the many-particle distribution and temperature scale, (ii) a clearer distinction between extensivity and indistinguishability in the Gibbs paradox, and (iii) a substantially revised discussion of atmospheric thermodynamics that distinguishes thermodynamic equilibrium from radiative-convective equilibrium. Addressing these issues would bring this manuscript closer to publication.

I am really glad that the Reviewer found the paper original and mathematically sophisticated. I hope that the revised version is sufficiently improved in terms of supporting my claims. With respect to points (i)-(iii), my brief reply is this:

- Point (i) is addressed as explained in my reply to Comment R3.3.
- Point (ii) is addressed as explained in my reply to Comment R3.20 below.
- Point (iii) is addressed as discussed in full detail in my replies to the general Comment R3.7 and the specific Comments below, beginning with R3.30.

Reviewer #3 specific comments on the manuscript PDF

R3.11.

> Deterministic laws are essentially conservation laws (for mass, momentum, energy, etc.). Conservation does not cause change. There should be a quantity that is not con-

This statement should be substantiated or clarified because there are many counter-examples to the idea that determinism are essentially conservation laws.

For example, time-dependent external forces in Newtonian mechanics are deterministic while violating energy and momentum conservation. Dissipative systems (including Navier-Stokes equations) are deterministic but not conserve various quantities directly. There appears to be a conflation of symmetries and invariants with the existence of deterministic evolution equations.

I thank the Reviewer for this insightful remark. Indeed, the statement was not quite accurate. I have now rephrased it as follows (section 1):

> The fundamental deterministic physical principles are primarily either conservation laws (for mass, momentum, energy, etc.) or derived from conservation laws.

In my view, dissipative systems, if seen deeply at the microscopic level, are not deterministic, but stochastic. The Navier-Stokes equations, even though widely regarded as deterministic, are fundamentally stochastic. When turbulence is present (i.e. in almost all real cases except trivial ones) the equations are clearly stochastic. Specifically, in the Reynolds-averaged Navier–Stokes equations, apparent (Reynolds) stresses arise from the fluctuating velocity field. These are covariances of fluctuating velocities, that is, inherently stochastic terms: To define covariance, one needs to represent flow velocities as stochastic processes. Even in viscous flows, the Navier-Stokes equations contain the viscosity term, which is the source for dissipation. However, dissipation cannot result from first principles at the microscopic level. The viscous term is absent from the underlying reversible microscopic dynamics and appears only after macroscopization. In turn, macroscopization is tightly connected to entropy increase, and in the framework proposed in the paper entropy is a purely stochastic concept.

R3.12.

> etc.). Conservation does not cause change. There should be a quantity that is not con-

This is somewhat incomplete because conservation laws are directly connected to dynamics by Noether's theorem. Conservation laws do not cuase change but they clearly constrain possible changes.

I thank the Reviewer for this insightful remark. I agree that the original phrasing "Conservation does not cause change" is incomplete. As the reviewer correctly notes, Noether's theorem establishes a direct link between continuous symmetries and conservation laws, and those conservation laws impose constraints on the admissible dynamical trajectories.

My intended point was that conservation laws themselves are not the source of dynamical evolution. Rather, while they restrict the space of possible changes, they do not generate them. To make this clearer and to incorporate the reviewer's observation, I have revised the sentence to (section 1):

> Conservation laws cannot be responsible for change; rather they constrain the changes that are dynamically allowed.

I believe this wording now accurately reflects the constraining role of conservation laws.

R3.13.

> etc.). Conservation does not cause change. There should be a quantity that is not conserved—and this is entropy. This is the driving force of change as it tends to take its maximum allowed value. This tendency is mathematically expressed by the principle of max-

This should be clarified since there is no requirement in modern physics that suggests that a system must possess a non-conserved quantity because conserved quantities exist.

In contrast, there are mnay examples of systems that are fully described by conserved quantities (i.e. harmonic oscillator, two-body systems, etc.).

Also, the phrase "entropy is the driving force of change" should be clarified. Traditional statistical mechanics begins with microscopic dynamics, which leads to probability distributions to entropy and to thermodynamics. This phrase gives the impression that you are stating that stochastic processes are the foundation, which leads to entropy to probability and to physical laws. If this is your intention, you should state this clearly as part of what you are attempting to demonstrate.

Following this comment I have reworded these phrases and expanded the context also to include time reversibility and irreversibility. I hope the revised version is more accurate (section 1):

> Because those allowed changes are time-reversible, the irreversible evolution observed in macroscopic systems requires a quantity that is not conserved—and this is entropy. The tendency of entropy to attain its maximum value under the constraints imposed by the conservation laws is the driving force of change.

My wording about which the driving force of change is may seem an exaggeration, but I am not the only one making that claim. I quote here Peter Atkins who is renowned in modern physical chemistry.

From his book *Galileo's Finger: The Ten Great Ideas of Science* [13]:

> *A question anyone might forget to ask is why anything happens at all. Deep questions are often thought wrongly to be naive questions; yet deep, seemingly naive questions properly investigated can expose the heart of the universe. That is certainly true of this particular question, for we shall see that by pursuing the answer we are led into a full understanding of the driving force of all change in the world. We shall come to understand the simple events in everyday life, such as the*

> *cooling of hot coffee, and we shall see at least the ankle of the explanation of the most complex events of everyday life, such as birth, growth, and death. The answer to our question about the origin of change lies in the field of science known as thermodynamic, the study of the transformations of energy particularly of heat into work.*

From his book *Four Laws that Drive the Universe* [14]:

> *The second law is of central importance in the whole of science, and hence in our rational understanding of the universe, because it provides a foundation for understanding why any change occurs. Thus, not only is it a basis for understanding why engines run and chemical reactions occur, but it is also a foundation for understanding those most exquisite consequences of chemical reactions, the acts of literary, artistic, and musica creativity that enhance our culture.*

It is in this sense that I use the expression 'driving force of change'.

The difference is that in my paper the Second Law is not taken from classical thermodynamics, but derived from the principle of maximum entropy, which in turn is posited in stochastic terms.

Otherwise, I agree that there are systems fully described by conserved quantities. However, the initial conditions in such systems should also be considered. Without a factor that set the initial condition away from a stationary point, there would be no motion/change. For example, a pendulum has a stable stationary point where the mass hangs straight down (angle $\theta = 0$). If the initial condition is $\theta = 0$, then the pendulum will remain unchanged over time. Therefore, something must disturb the pendulum from the stationary point in order to cause motion. In the real world there is dissipation, and so the pendulum will end at that stationary point (the most probable state) and remain there forever if there is no force to disturb it again.

R3.14.

> reliability and consistency of the results it yields. Furthermore, contrary to the usual epistemological interpretation, the principle is here advanced as an ontological—hence physical—principle.

This is an important argument that should be made explicit. Traditional physics claims that entropy increases in irreversible processes, but it appears that the claim in this section is stating that the existence of entropy increase is the fundamental reason that irreversible processes occur. This would be a major shift from thinking of entropy as a measure of macroscopic multiplicity to a fundamental causal agen tof nature.

This would be a genuinely novel approach, but I think it should be very clearly stated.

I thank the Reviewer for highlighting this point and for recognizing its significance. I agree that the claim needs to be stated more explicitly.

In the revised manuscript I have expanded the relevant passage (also by moving text from below) so that the contrast with the conventional view is clear. After affirming that the principle of maximum entropy is advanced here as an ontological (physical) principle, I now compare it directly with Jaynes's classic epistemological formulation and note his insistence that probabilistic and thermodynamic entropies are distinct. I then state that, in the present approach, the two are treated as equivalent. The relevant text now reads (section 1):

> Furthermore, contrary to the usual epistemological interpretation, the principle is here advanced as an ontological—hence physical—principle.
>
> This difference in status can be seen clearly by comparing the formulation given above with the classic statement of the principle by Jaynes [15]. Jaynes introduced the principle of maximum entropy in 1957 not as a physical (ontological) principle, but as one for logical inference (epistemological):
>
> > *"in making inferences on the basis of partial information we must use that probability distribution which has maximum entropy subject to whatever is known."*
>
> Interestingly, Jaynes (2003 [16]) insisted that probabilistic and thermodynamic entropies are different. In contrast, the approach proposed here treats the two as equivalent: the same maximization principle is regarded as governing both the assignment of probabilities and the irreversible change in physical systems. The tendency of entropy to attain its maximum value under the prevailing constraints is taken as the principle that supplies the direction of irreversible evolution once the description has been reduced from the underlying time-reversible dynamics. Additional key features of the proposed approach are the explicit connection between uncertainty and entropy, with the latter being the quantification of the former, and the recognition of uncertainty as a physical reality, also connected with change.

This expansion makes both the departure from the traditional interpretation and the precise scope of the claim explicit. Hopefully, it more clearly supports that the paper does advance a non-standard position.

R3.15.

troposphere). As a scientific term, *entropy* was introduced by Rudolf Clausius in 1865 [8], who understood the connection of entropy with change and its tendency to increase until it reaches a maximum. However, his classical thermodynamic definition is circular. He defined entropy, S, for a reversible processes by $dS = \delta Q/T$, where Q is heat and T temperature, and where a reversible process is one in which this equality holds—otherwise $dS > \delta Q/T$. Furthermore, he did not connect entropy with probability or uncertainty.

This is not a fair interpretation of Clausius' argument. Logically, Clausius began with empirical observations of heat engines, and he knew about the concept of reversibility based upon the Carnot theorem. Clausius also knew about Kelvin's statement of the second law, which leads to the Clausius inequality. From this, he is able to show that dQ_rev/T = 0 for a reversible cycle. Thus, he concludes that there must exist a state function satisfying S which satisfies dS = dQ_rev/T.

Thus, Clausius did not assume entropy. He proved that an integrating factor existed - leading to his definition of entropy. In other words, entropy was introduced after a discussion on reversible processes as the state function that exists because reversible heat satisfies the Clausius integral.

> I think the true heart of your criticism is the logical starting point of classical thermodynamics, not Clausius' work.

This is a fair and useful comment. I have now changed the wording to refer to classical thermodynamics rather than Clausius' work. The related paragraph now reads (section 1):

> Even the colloquial meaning of the name *entropy* is related to change; it is etymologized from the Greek word *εντροπία*, a synthesis of *εν* (= in) and *τροπή* (= change; cf. *troposphere*). As a scientific term, *entropy* was introduced by Rudolf Clausius in 1865 [17], who understood the connection of entropy with change and its tendency to increase until it reaches a maximum. However, he did not connect entropy with probability or uncertainty. His pioneering work led to the development of classical thermodynamics, in which, however, the definition of entropy is rather circular. Specifically, the thermodynamic entropy, S, is defined for a reversible process by $\mathrm{d}S = \delta Q/T$, where Q is heat and T temperature, and where a reversible process is one in which this equality holds—otherwise $\mathrm{d}S > \delta Q/T$.

> **R3.16.**
>
> ***Postulate 3, Non-interaction****: There is no interaction between any two elements of a partition:* $\partial^2\Phi(\mathbb{A})/\partial P(A_i)\partial P(A_j) \equiv 0, i \neq j$.
>
> This is posited for consistency with the principle of maximum entropy. Had this second derivative been nonzero, the knowledge of $P(A_i)$ would provide some information on $P(A_j)$ and therefore would decrease uncertainty—without a reason.
>
> It should be noted that P(A_i),...,P(A_n) are not independent due to probability normalization. Or said in another way, knowledge of P(A_i) provides a constraint on P(A_j) by probability normalization.
>
> Also, if the mixed partial derivatives vanish, this implies that entropy must be a sum of one-variable functions. This seems to rule out entire families of entropy functions that would be included in Shannon or Gibbs entropy.

I thank the Reviewer for these remarks. It is true that the probabilities $P(A_1), \dots, P(A_n)$ are not independent once the normalisation constraint is imposed. The non-interaction postulate, however, is stated for the *unconstrained* partial derivatives. Constraints—including normalization (mass conservation) and any other physical constraints, e.g. energy conservation—are introduced afterwards through Lagrange multipliers. I have therefore added an explicit clarification immediately after the statement of the principle of maximum entropy (section 1):

> Such a reason, if it exists, would be expressed mathematically as a constraint, as we will see below, but the definition of entropy should be independent of constraints.

Mathematically speaking, the postulate of no interaction between any two elements of a partition: $\partial^2\Phi(\mathbb{A})/\partial P(A_i)\partial P(A_j) \equiv 0, i \neq j$ clearly refers to unconstrained partial derivatives over a set $U \subset \mathbb{R}^n$ that is open and connected, while containing the simplex $S \coloneqq \{x \in (0,1)^n : \sum_i x_i = 1\} \subset U$. We do not posit any postulate for partial derivatives constrained on that simplex. Generally speaking, the derivatives $\partial^2 f/\partial x_i \partial x_j$ are called *ambient partial derivatives*.

They are standard partial derivatives evaluated within a surrounding (*ambient*) space or coordinate system, measuring how a multivariable function changes due to elementary changes $\mathrm{d}x_i, \mathrm{d}x_j$ along the coordinate axes x_i, x_j while holding all other ambient coordinates constant (hence violating the constraint that the point (x_i, x_j) should strictly belong to the simplex). Ambient partial derivatives are standard in differential geometry and multivariable calculus.

Furthermore, in an optimization framework, the Lagrangian to be maximized for any specific problem will contain the terms $\Phi - \lambda L$, where λ is a Lagrange multiplier and $L \coloneqq \sum_{j=0}^{n} P(A_j) - 1$. If we take joint partial derivatives for the term L, we obtain $\partial^2 L / \partial P(A_i) \partial P(A_j) \equiv 0$ and hence the noninteraction on Φ is inherited by the Lagrangian $\Phi - \lambda L$.

I think the above information provides further explanation on Postulate 3. However, I preferred to omit it from the manuscript in order not to burden the general reader with elaborate mathematical details (ambient versus constrained derivatives, etc.). I believe that the brief addition, "the definition of entropy should be independent of constraints" is satisfactory.

As per Reviewer's phrase on a risk to "rule out entire families of entropy functions that would be included in Shannon or Gibbs entropy", I do not see how this could be the case. Rather, the requirement of vanishing mixed partials selects precisely the additive functionals of the Shannon–Gibbs type; it does not exclude them. And, as clearly stated in the Conclusions (second bullet):

> The resulting entropy expression, uniquely derived, coincides with the classical Planck-Shannon form but rests on firmer mathematical and conceptual grounds, free from the circularities and limitations of previous formulations.

R3.17.

> ***Theorem 1***: *There exists a univariate function $\varphi(P)$ such that*
>
> $$\Phi(\mathbb{A}) = \sum_{i=1}^{n} \varphi\big(P(A_i)\big) \qquad (1)$$
>
> ***Theorem 2***: *The function $\varphi(P)$ in equation (1) is uniquely determined as*
>
> $$\varphi(P) = -k\, P \ln P \qquad (2)$$
>
> *where $k > 0$ is a constant.*

Whereas Theorem 1 is a straightforward result from multivariable calculus (using Postulate 3), Theorem 2 is not a trivial derivation, and it directly depends upon Postulate 4. The full proof should be given in an appendix.

Following this suggestion, in the revised manuscript I have given the full proof of Theorem 2 in Appendix A.1.

R3.18.

> a universal constant, i.e., the Planck constant h_{p} = 6.626 × 10^{-34} J·s, whose dimensions are [L^2 M T^{-1}]. If we combine it with a reference mass m_0 (such as the particle's or the proton's mass), we observe that the quantity $(m_0/h_{\mathrm{p}})^3$ has the required dimensions [L^{-6} T^3],

In quantum statistics, the appearance of Planck's constant arises because of the quantization of phase space (i.e. usually in reference to the continuum approximation). Here, it appears that you chose a universal constant that matches the dimensions. This is mathematically sufficient, but physically less motivated.

The Reviewer is right. Indeed, I used dimensional analysis, which is a powerful method in physics and engineering. I did that deliberately, because it is the simplest and a clearcut method for this derivation. The derived equation is consistent with the Sackur-Tetrode form.

R3.19.

> motion coordinates.With mass, momentum and energy constraints expressed in terms of expectations of the related stochastic variables, the entropy maximizing distribution is found to be the product of the distribution functions of the individual molecules:
>
> $$f(\boldsymbol{Z}) = \left(\frac{\beta m_{\mathrm{G}}}{4\pi\varepsilon}\right)^{\beta N/2} \frac{1}{V^N} \exp\left(-\frac{\beta}{2\varepsilon}\sum_{j=1}^{N}\sum_{i=1}^{\beta}\frac{m_i \underline{u}_{ij}^2}{2}\right), \qquad 0 \le x_{ij} \le a \qquad (16)$$
>
> This clearly suggests: (a) independence of the motion of different molecules, (b) uniform distribution for their location and (c) multivariate normal distribution for their ve-

I think this should be clarified. The generalization to many particles in Eq. (16) is correct if the molecules are independent. I suspect that the argument is based upon Postulate 4. If this is the case, then this should be made more explicit. In traditional statistical mechanics, independence at maximum entropy is a theorem that is proved from its axioms, but here it appears as an axiom.

I have already addressed this remark as specified in my reply to Comment R3.3. Furthermore, the way entropy is founded (whether based on my postulates or on traditional statistical mechanics) does not produce any modification once the resulting entropic formula is the same, provided that the same constraints are used.

R3.20.

> The standardized entropies $\varphi^*(\varepsilon, v)$ and $\Phi^*(E, V, N)$ are intensive and extensive, respectively, while the original ones, $\varphi(\varepsilon, v)$ and $\Phi(E, V, N)$ do not have these properties. However, the descriptions by either the original or the standardized entropies are precisely equivalent, once the probability calculus for each case is correct. The original quantities are more effective, and hence preferable, in explaining behaviours regarded as paradoxical (e.g., the Gibbs paradox [23]).

Whereas I think that you solved the problem of making entropy properly extensive, I don't think you've addressed the indistinguishability and Gibbs paradox issues.

> Here, it appears that your procedure produces ln(V/N) by conditioning on a partition. This gives a very different physical interpretation. In traditional statistical mechanics, this is explained by noting that identical molecules correspond to the same microstate under permutations. In contrast, your argument appears to be that entropy should be conditioned on a spatial partition of size N.
>
> This leads to the quuestion of why the molecule should be discretized into N bins. In traditional statistical mechanics, this is due to the permutation of identical particles. In this framework, it appears to be related to a choice of reference partition.

I understand the Reviewer's residual concern that the indistinguishability and Gibbs-paradox issues may not have been fully resolved. I think the Gibbs paradox is a result of the indistinguishability assumption. I never thought that this assumption stands; if it stood, it would imply different rules of logic for the microscopic and the macroscopic world, which for me is hard to accept. Therefore, I devoted time and effort to derive the correct results based on assumptions that do not include indistinguishability. I think it is clear in the paper that I do not use indistinguishability. This also resolves the Gibbs paradox, as described in another paper I published about this specific topic [18]. I cite this paper (numbered [25] in the original manuscript), and I think this suffices for the present paper; it would be too much to give the details. For the Reviewer's convenience, I copy below (but not include in the paper) a related text (Digression 6.E) from my book [1], which contains a summary of the paper.

About the question that the Reviewer raises, of why the molecule should be discretized into N bins, the answer is simple. Discretizing into N bins serves the purpose of obtaining intensive and extensive entropic forms. These are derived just for convenience. The original quantification, without any discretization, can derive precisely the same final results. This is stated in the following paragraph of the manuscript (end of section 3.3):

> The standardized entropies $\varphi^*(\varepsilon, v)$ and $\Phi^*(E, V, N)$ are intensive and extensive, respectively, while the original ones, $\varphi(\varepsilon, v)$ and $\Phi(E, V, N)$ do not have these properties. However, the descriptions by either the original or the standardized entropies are precisely equivalent, once the probability calculus for each case is correct. The original quantities are more effective, and hence preferable, in explaining behaviours regarded as paradoxical (e.g., the Gibbs paradox [18]).

Appendix to this reply (the cited sections, equations and additional references can be found in [1])

Digression 6.E: The Gibbs paradox

We consider two identical parts of a box separated by a diaphragm, each containing N identical particles with energy E in a volume V. The entropy in each part is $\Phi(E, V, N)$. Since independence between the two parts can be assumed, the total entropy is $2\Phi(E, V, N)$. If we remove the diaphragm, the entropy becomes $\Phi(2E, 2V, 2N)$ and according to (6.28) with $\alpha = 2$, there is an increase of entropy equal to $2N \ln 2$. The same result is obtained from (6.47) by replacing N with $2N$ and N_A and N_B with N. If we reinsert the diaphragm, entropy will become again $2\Phi(E, V, N)$ and there will be a decrease of entropy, $-2N \ln 2$.

On the other hand, if we describe the process in terms of standardized entropies, before we remove the diaphragm, the standardized entropy in each part will be $\Phi^*(E, V, N)$ and totally $2\Phi^*(E, V, N)$. After the removal of the diaphragm, the standardized entropy becomes $\Phi^*(2E, 2V, 2N)$ and according to (6.34) with $\alpha = 2$, this is again $2\Phi^*(E, V, N)$. So no change of standardized entropy is seen. When no change in entropy occurs, the process is reversible. This, however, is incorrect, as can be understood if in the two compartments there were, e.g., different monatomic gases before the diaphragm was removed. Apparently, after its removal, there will be a mixture and the mixing cannot be reversed. These have been regarded as contradictions to thermodynamics, known as the Gibbs paradox (Jaynes, 1992; Swendsen, 2008). And since mixing happens, there must be an explanation why it does.

This explanation is given by the actual entropy Φ and its interpretation as the quantification of uncertainty. The increase of Φ by $2N \ln 2$ shows that the process is irreversible and there is no contradiction or paradox. The increase of entropy after the removal of the diaphragm, quantifies the fact that we have greater uncertainty about the location of each particle. Initially, we knew each particle's location with "precision" V and after the removal we lost information, with the "precision" becoming $2V$. The increase of entropy objectively expresses the lost information, and reflects the physical fact that the motion of each particle has a larger domain. Likewise, the decrease of entropy after reinsertion of the diaphragm (which can hardly be thought of as a spontaneous natural phenomenon) reflects the gain of information and the decreased uncertainty about the position of a particle.

One may contend that the introduction of the diaphragm gives us no information at all about which part any particular particle might be found in. Such an argument is interesting and in fact can be fully addressed by the proposed probabilistic approach. Specifically, if we do not know whether a particle is in one or the other part, then the entropy of the particle is φ as given by equation (6.39) and is not affected by the diaphragm. However, once the diaphragm is there and once we know that at some time the particle is in a specific part, then the entropy of the particle has been reduced to $\varphi_A < \varphi$ (the leftmost term in (6.39)). The information that the specific particle is in the specific part at a specific time is important because it is transferred to subsequent times. That is, if we know in which part a particle is found and there is a diaphragm, then the entropy is φ_A now and will be the same in the future. In contrast, if the diaphragm is absent, then knowing in which part a particle is found makes the entropy φ_A now, but soon after in the future this information is lost and the entropy becomes φ. Thus, the introduction of the diaphragm is associated with reduction of uncertainty.

According to the proposed approach, this situation does not change if on either side of the diaphragm the gases are different, thus removing the asymmetry (or discontinuity) in the two cases where in the two boxes the gases are different or the same. In both cases, the entropy Φ after the mixing (by removal of the diaphragm) will be by $2N \ln 2$ greater than the sum of the entropies in the initial state. And again, if we consider the mixture of gases as an equivalent single gas (as is standard practice, for instance, in the atmospheric mixture; see section 6.12) the entropy Φ^* will not change, in comparison to the initial state.

If we assumed that the particles were indistinguishable, then there would be no distinction between Φ and Φ^*, which would equal each other. In this case the paradox would hold in both cases. This is another reason why we should reject the indistinguishability hypothesis, additional to those already stated in Chapter 5.

R3.21.

in physics, T, measured in kelvins. The unit of kelvin was an arbitrary choice (a historical 346

This should be handled more precisely. The numerical size of the kelvin is historically conventionally. However, the existence of the absolute temperature scale is not arbitrary.

I did not dispute the existence of a zero point, which defines the "absolute temperature". Rather I state (manuscript, Section 5, Discussion):

> ***Third Law****: it states that the entropy of a system approaches zero as the temperature approaches absolute zero.*

We did not need to assume this law at all. Rather, the law is derived in [1] in a slightly different form, in which as the entropy approaches zero, the energy is not zero but a super-tiny quantity, smaller than the quantum zero-point energy that is imposed by Heisenberg's uncertainty principle. Thus, our stochastic framework is in better agreement with quantum physics than the classical version which implies that zero energy is in principle feasible.

R3.22.

classical entropy S. The proportionality relationships are given through *Boltzmann's constant* k_B = 1.38 × 10^-23 J K^-1:

$$S = k_B\,\Phi^*, \qquad T = \frac{\theta}{k_B} \tag{22}$$

Thus far, the claim has been entropy is fundamentally uncertainty (hence it must be conventional), and thermodynamic entropy is a derived quantity, Boltzmann's constant appears to be defined, rather than reasoned. The conversion between S and Phi indicates that there is still a bridge constant required. Thus, the framework has not produced the thermodynamic entropy scale.

A bridge constant $k_B = 1$ (dimensionless)(could also do the job, with the advantages of (a) being more parsimonious and removing one artificial physical constant from physics; (b) retaining the dimensionless nature of entropy; and (c) rendering the temperature units as energy units.

Please also see my reply to Comment R3.4.

R3.23.

Converting to SI and using the *universal gas constant* ($R_* := k_B N_a =$ 8 314.463 J K^{-1} kmol^{-1}) or the *specific gas constant* $R := R_*/M_0 = k_B/m_0$ (in J K^{-1} kg^{-1}), where $M_0 := N_a m_0$ is the molar mass, we find the classical form of the ideal gas law:

$$pV = nR_*T \;\Leftrightarrow\; \frac{p}{\rho} = RT \tag{24}$$

where n is the number of moles in the volume V and ρ is the density.

See my previous comment on the temperature scales. Eq. (23) shows that a dimensionless entropy naturally generates a temperature with energy units, and the usual Kelvin temperature is obtained by introducing Boltzmann's constant as a conversion constant.

I appreciate the remark. Yet I am a follower of Aristotle and his *principle of parsimony* (also known as *Ockham's razor*). Everything would be described, at least equally well, without introducing Boltzmann's and without conversion. Please notice that in application of the principle of maximum entropy to radiation physics, all expressions include the product $k_B T$, which precisely equals θ. Hence the equations would be simpler.

The reason I am using the kelvin in temperature and entropy is to be consistent with standard physics, not because it is necessary.

R3.24.

> *3.8 Equilibrium and non-equilibrium states of an air column*
>
> The state of an air column is generally affected by gravitation. However, gravitation does not alter the equilibrium state: It remains isothermal, the same as if gravitation were absent—a result verified by simulating molecule collisions under gravitation [25]. What

The results from Section 3.8 and 3.9 are standard results from classical thermodynamics that requires only the ideal gas law, the 1st law of thermodynamics, hydrostatic balance, and constant entropy. It would be helpful to clarify the benefit of the stochastic framework here.

I respectfully disagree. There are infinitely many profiles that satisfy the ideal gas law, the 1st law of thermodynamics and the hydrostatic balance. One of them has constant entropy, the others not. Sections 3.8 and 3.9 describe several profiles, all satisfying these laws, one of which is the isentropic, as well as the connections to each other.

R3.25.

> absent—a result verified by simulating molecule collisions under gravitation [25]. What gravitation does is to distinguish the isentropic state from the isothermal.

This is somewhat unclear. Gravity doesn't produce the distinction between the isentropic state and the isothermal state. Rather, this distinction is established by the type of constraint.

At true equilibrium within the microcanonical ensemble, the maximzation of entropy is subject to fixed total energy, fixed mass, and fixed volume, which leads to the isothermal condition.

For an adiabatic atmosphere, the maximization of entropy is subject to fixed entropy of parcels, hydrostatic balance, and no heat exchange, which leads to the dry adiabatic lapse rate. In this scenario, gravity supplies the mechanical work since

$$dE = -P\, dV - mg\, dz$$

Notice that this is a non-equilibrium constraint.

Without gravitation an isentropic profile is also isothermal. The factor that differentiates the two is therefore gravitation.

I agree that maximization of entropy leads to the isothermal condition, either with or without gravitation. The difference is that that condition is no longer isentropic under gravity.

Faithful to the principle of parsimony, I avoided using concepts like microcanonical ensemble and work, which the Reviewer seems to prefer. I just use entropy maximization under appropriate constraints, and this works well.

R3.26.

The final result for the saturation vapour pressure, e, as a function of the temperature, θ, is:

$$e = e_0 \exp\left(\frac{\xi}{\theta_0}\left(1 - \frac{\theta_0}{\theta}\right)\right)\left(\frac{\theta_0}{\theta}\right)^{\beta_B/2-\beta_A/2-1} \quad (29)$$

where $(\theta_0, e_0 = e(\theta_0))$ is an anchoring point (arbitrarily chosen) and ξ is the amount of energy per molecule to break the bonds between molecules of the liquid phase in order for the molecule to switch to the gaseous phase (evaporation). If converted to SI units

There are many steps missing in obtaining Eq. (29). However, I think the lack of physical justification is important. This argument assumes that liquid water can be represented as a molecule with 18 degrees of freedom under a kinetic theory framework. I do not believe that this is valid because a liquid is not a gas with additional degrees of freedom. The entropy of liquid water contains configurational entropy, collective molecular structures, etc.

The \xi factor seems to hide a lot of physics, such as the configurational complexity of the liquid phase and possibly the enthalpy difference between the liquid and gaseous phase. This appears to be a situation where uncertainty alone is not the driver of change in the system.

I agree, there are many steps missing, but if I included all steps the paper would become book sized. Instead of providing all steps in each derivation, I cite earlier works (the particular earlier works for this case are cited in the quotation from the paper as seen below).

As I already explained in my reply to Comment R3.6, the additional degrees of freedom in the liquid phase are not kinetic.

In my humble view, ξ is the best physical concept I could think of, rather than something hiding the physics. As explained in the paper, ξ is a very simple physical concept: the amount of energy per molecule to break the bonds between molecules of the liquid phase in order for the molecule to switch to the gaseous phase—that is, to remove the non-kinetic degrees of freedom and retain only the kinetic ones. Judging from the impressive comparisons with experimental data, the assumption of a constant ξ proves to work well.

In the revised manuscript I have made some minor amendments to the related text, which now reads (section 3.10):

> The final result for the saturation vapour pressure, e, as a function of the temperature, θ, which was derived in [19] is:
>
> $$e = e_0 \exp\left(\frac{\xi}{\theta_0}\left(1 - \frac{\theta_0}{\theta}\right)\right)\left(\frac{\theta_0}{\theta}\right)^{\beta_B/2-\beta_A/2-1} \quad (29)$$
>
> where $(\theta_0, e_0 = e(\theta_0))$ is an anchoring point (arbitrarily chosen) and ξ is the amount of energy per molecule (which is assumed constant) to break the bonds between molecules of the liquid phase in order for the molecule to switch to the gaseous phase (evaporation). If converted to SI units Equation (29) becomes:

$$e = e_0 \exp\left(24.921\left(1-\frac{T_0}{T}\right)\right)\left(\frac{T_0}{T}\right)^{5.06}, \qquad T_0 = 273.16\ \mathrm{K}, e_0 = 6.117\ \mathrm{hPa} \tag{30}$$

where the point (T_0, e_0) denotes the triple point of water and the constant $\beta_B/2 - \beta_A/2 - 1 = 5$ was slightly modified to 5.06 to agree with the experimental quantity $(c_L - c_p)/R$. The agreement of the equation with measurements is excellent, better than in standard and empirical formulae, and is not limited to the saturation vapour pressure but also extends to the latent heat of water [19,20]. This is a confirmation for the assumption of a constant ξ. The impressive agreement with empirical data shows that by maximizing microscopic uncertainty we almost eliminated the macroscopic one, for the very reason discussed in section 3.1.

R3.27.

> where the point (T_0, e_0) denotes the triple point of water. The agreement of the equation with measurements is excellent, better than in standard and empirical formulae [27,28].

This should be clarified. There are empirical vapor pressure formulae (such as Tetens equation and Magnus equation) and there are thermodynamical formulations (such as the IAPWS formulation). It seems that you are comparing Eq. (30) to empirical atmospheric formulas, not the full thermodynamic standard

Comparisons to empirical Magnus-type equations (including the most recent version of a Tetens-like equation) have been made in my earlier paper [20] and are beyond the scope of the present paper.

R3.28.

> cated and needs integration across the atmospheric column. The integration between any two states 0 and 1 gives the following relationship of the *moist isentropic profile*:
>
> $$\left(1+\frac{\beta_D}{2}-\frac{N_W}{N_D}\frac{\beta_B}{2}\right)\ln\frac{\theta_1}{\theta_0} - \ln\frac{p_1-e_1}{p_0-e_0} + \frac{N_{A_1}}{N_D}\frac{\lambda_1}{\theta_1} - \frac{N_{A_0}}{N_D}\frac{\lambda_0}{\theta_0} = 0 \tag{31}$$
>
> where $\lambda := \xi - (\beta_B/2 - \beta_A/2 - 1)\theta$ is the *latent heat of vaporization* per molecule, p and e are the total pressure and saturation vapour pressure, respectively, N_D and N_W, are the numbers of molecules of dry air and water, respectively, and $N_A \le N_W$ is the number of water molecules that are in gaseous phase. Note that during the transition from state 0 to state 1, N_A does not remain constant because of vapour condensation.

Eq. (31) and (32) are physically reasonable, but the question is whether the stochastic entropy framework improves the moist adiabat. Said in another way, does the stochastic approach determine the latent heat L(T), phase equilibria, and saturation vapor curves without empirical input and tuning?

As I already mentioned in my reply to Comment R3.26, the proposed approach (with constant ξ) represents well the latent heat and the saturation vapour pressure curves. In this respect, it works well in quantification of the moist isentropic profile, without substantial differences from classical thermodynamic equations.

I would like to stress in this respect that the key aim of the paper is to produce everything from stochastics (in particular, maximization of entropy as a stochastic concept), without using the laws of thermodynamics. It is not to dispute the well-tested results of atmospheric thermodynamics.

R3.29.

> profile would tend to the isothermal, driven by the principle of maximum entropy. However, there are macroscopic atmospheric structures, named *cells* or *air parcels*, which move as structures (convection), while being continually transformed in shape and physical characteristics.

This should probably be rephrased. Convection is not an independent competing process against molecular entropy maximization. Convection is itself a consequence of the system being maintained away from equilibrium due to impacts of solar forcing and surface heating.

I appreciate that the Reviewer has this view, which is also the dominant view. On the other hand, I hope that she/he can tolerate the fact that I am presenting in this paper a different view. The text she/he highlighted represents my view. I do not see anything in it that should be rephrased.

I have now articulated better my different view, particularly by adding the new Appendices A.3 and A.4 and in other parts, as mentioned in my reply to Comment R3.7.

R3.30.

> The coexistence of molecular and macroscopic motion at multiple scales determines the nonzero vertical temperature gradient. The macroscopic motion cannot retain constant temperature. Rather it drives the actual vertical temperature profile away from the isothermal. Indeed, an air parcel that has been warmed at the surface will move upward so fast that no heat can be removed from it, or added to it. Hence the process is reversible and adiabatic, i.e., isentropic. The lapse rate will then tend to the isentropic lapse rate, the dry one if the atmosphere is not saturated, or the wet one if it is saturated.

There are two aspects missing here:

(1) The true atmosphere is in radiative-convective equilibrium

(2) Entropy is produced in the atmosphere by radiation, turbulence, diffusion, and precipitation. Therefore, the relevant quantity at this point should be the entropy production rate.

Also, this paragraph gives the impression that you are stating that entropy maximization drives the lapse rate. If the atmosphere were in a maximum entropy state, then it would be isothermal. The adiabatic lapse rate develops because the atmosphere is constrained by the rate of heat exchange, pressure changes, and gravitation. Thus, the formation of the adiabatic lapse rate is a constrained entropy process.

Again my view is different. In my view, the true atmosphere is continually out of equilibrium of any type, due to continuous changes that are described in the paper. (By the way, the term “radiative-convective equilibrium” looks to me as an oxymoron because convection per se reflects non-equilibrium conditions).

I agree with the Reviewer’s statement “If the atmosphere were in a maximum entropy state, then it would be isothermal.” But in my view the lapse rate develops because of convection, as I explain in detail in my reply to Comment R3.7.

R3.31.

Therefore, it is not correct to say that the Sun feeds the Earth with energy. Rather, it is correct to say that the Sun feeds the Earth with low entropy, given that solar radiation is at higher frequencies than terrestrial and, entropy is a decreasing function of frequency.

I understand the sentiment that solar radiation provides high-temperature radiation which is low entropy per unit energy. However, it would be an overstatement to say that the Sun does not provide energy. The standard expression for Earth's equilibrium temperature is based upon energy balance.

It's much easier to say that the Sun supplies energy to Earth in a low-entropy form, and the Earth emits energy to space as higher-entropy infrared radiation.

Thanks for this comment. I have now rephrased the paragraph in the following manner (section 4.3):

> Given that the amount of energy the Earth receives from the Sun is equal to the amount it radiates back to space, it is not correct to say the Earth gains energy from the Sun. Rather, it is correct to say that the Sun provides the Earth with a source of low entropy. Indeed, solar radiation is at higher frequencies than terrestrial (see Figure 8 and Figure 9) and, entropy is a decreasing function of frequency (see equation (A29) in Appendix A.2).

I think my formulation is not quite different from similar ones by Roger Penrose in his book “The Emperor’s New Mind” [7].

R3.32.

4.4 Is the atmosphere a greenhouse?

A *greenhouse* is a structure designed to regulate the temperature and humidity of the environment inside. Such constructions had their origin in Roman times [37], and initially aimed to insulate a garden during the night. When greenhouses became common at the 17th century, they were outfitted with glass. In modern times the cover and walls can be plastic, usually low-density polyethylene (LDPE).

It was thought that the glass's opacity to LW radiation was the mechanism causing warming of the greenhouse, by letting SW radiation in and trapping LW radiation. This is not accurate, as testified by both Roman and modern technology. In particular, LDPE is transparent to LW radiation.

In fact, in a real greenhouse, the mechanism of warming is the blocking of convection, which otherwise would transfer heat from the inside farther away. This is known since more than 100 years ago, by an experiment by Wood (1909) [38], who reported that "the loss of temperature of the ground by radiation is very small in comparison to the loss by convection; in other words […] we gain very little from the circumstance that the radiation is trapped".

The re-telling of Wood (1909) applies for a physical greenhouse, but this does not imply that the atmosphere behaves as a physical greenhouse.

For example, the blocking of convection does warm a physical greenhouse. However, atmospheric convection is a heat transport mechanism, not a warming source. The source of atmospheric warming is absorbed radiation.

This section appears to reverse the causality in which convection functions as a heating source rather than a constraint which prevents the atmosphere from developing an arbitrarily large radiative temperature gradient.

While I appreciate the Reviewer's view that "Wood (1909) applies for physical greenhouse, but this does not imply that the atmosphere behaves as a physical greenhouse", I have difficulties to understand it, let alone to accept it. My view tries to follow the Aristotelian logic, perhaps because of my classical Greek education. In my view, a "physical greenhouse" is just a greenhouse—adding the adjective "physical" is just a pleonasm. The statement "does not imply that the atmosphere behaves as a physical greenhouse", simply means that the atmosphere is not a greenhouse. That's exactly my thesis. And if it is not a greenhouse, why call it a greenhouse?

To expand my position, I articulate in the paper that, while convection is indeed not a warming source per se, its existence is the source of the lapse rate, because convection moves the temperature profile from the isothermal equilibrium state toward the isentropic state, which in turn is associated with the lapse rate. The lapse rate entails that the surface is warmer than the atmosphere. In turn, this means that convection's role in a greenhouse and in the atmosphere is just opposite to each other. The greenhouse works by blocking convection, the atmosphere by enabling convection. For this reason, I state in the paper (section 4.4):

> Due to the opposite mechanisms driving the atmosphere from those in a greenhouse, what has been called "greenhouse effect" for the atmosphere should have better been called an "anti-greenhouse effect".

Therefore, it is true that my paper reverses the causality that is incorporated in mainstream science. However, this does not mean that it reverses the true causality that characterizes the real-world atmosphere. If my argument is right (which I hope), then it is mainstream science that reverses the true causality. I hope that the Reviewer can tolerate that my view and argument get published so that they can be discussed further within the scientific community. The future will show what is right and what is wrong.

R3.33.

greenhouse). An isothermal atmosphere would lead to a surface temperature of 255 K.

This statement is true only for a specific case. A globally isothermal atmosphere in radiative equilibrium has a very different structure.

More importantly, the 255 K temperature is not caused by convection. It is caused by an energy balance between absorbed solar radiation and outgoing longwave radiation. This determines the effective radiating temperature, which is different than the surface temperature since the atmosphere is not transparent to infrared radiation.

I thank the Reviewer for this remark. Indeed my formulation was incomplete and I rephrased it in the revised manuscript (section 4.4):

> An isothermal atmosphere would lead to an average surface temperature of 255 K (see Table 2 and its description).

Table 2 summarizes detailed calculations of the energy balance, in which I took into account radiation balance (according to Stefan-Boltzmann law), Earth's rotation and tilt, and used the trigonometric and astronomic details of Earth's geometry and motion.

Of course, "the 255 K temperature is not caused by convection". Actually, the paper says the exact opposite, that in an isothermal atmosphere there would not be convection, because there would not be temperature differences to trigger heat transport.

R3.34.

cause of not considering clouds). Importantly, the surface temperature T_S increases with Γ for the same atmospheric composition and tends to become constant for high Γ.

The greenhouse effect operates primarily by changing the emission level. For example, increasing water vapor increases the optical depth so that radiation escapes from a higher altitude. From here, T_s = T_e + Gamma*z_e where T_s is the surface temperature, T_e is the temperature at the emission level z_e, and Gamma is the lapse rate.

I appreciate the Reviewer's remark, but this phrase in the paper just reports what is seen in Figure 11, which in turn is a plot of the results of RRTM. The plot is reproduced here below, in the next comment. There is no interpretation in my phrase. The phrase is accurate, as can be verified in the graph and there is nothing to change.

R3.35.

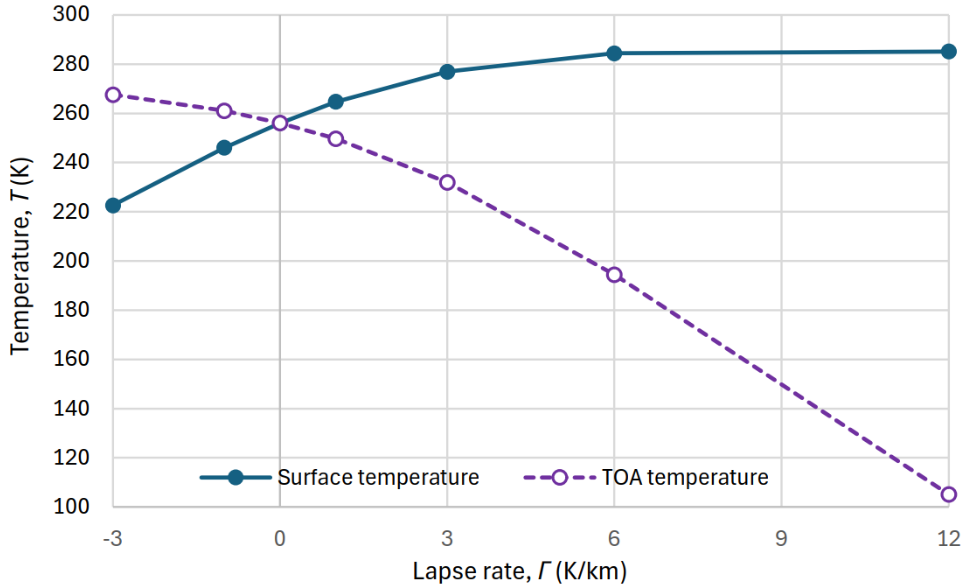

Figure 11. Surface and TOA temperatures leading to energy balance vs. lapse rate, as calculated from RRTM runs using default parameters (without clouds).

The interpretation of Figure 11 is almost the opposite of the consensus in modern atmospheric physics. To see why, consider an atmosphere with an effective emission level z_e. At this altitude, the temperature = the effective temperature T_eff. For Earth's atmosphere, T_eff = 255 K. If we assume a constant lapse rate Gamma, then

T(z) = T_sfc - Gamma*z

At the emission altitude, T_e = T_sfc - Gamma*z_e, which implies that

dT_sfc/d Gamma = z_e

Thus, a larger lapse rate automatically gives a warmer surface as expected. However, the lapse rate does not cause the emission temperature. Rather, radiation balance creates T_e, and the lapse rate determines the temperature differnece between the emission level and the surface.

I thank the Reviewer for this remark. I am afraid that what he calls "interpretation of Figure 11" (in the revised manuscript renumbered as Figure 12) is not an interpretation bur a simple description. Indeed, it is "the opposite of the consensus in modern atmospheric physics".

I agree with the reviewer that "a larger lapse rate automatically gives a warmer surface as expected." However, I disagree with her/his view on what causes the lapse rate. I further explain my view on the cause in my reply to Comment R3.7.

R3.36.

$$c_p \rho \frac{dT}{dt} = -\frac{dQ}{dz} - \frac{dR}{dz} \qquad (44)$$

where R and Q are the total vertical radiation and heat flux, respectively (in units of power per area). The equation holds for an atmosphere without conduction and convec-

Notice that Eq. (44) produces a negative feedback when convection is applied. Solar heating creates a radiative imbalance such that dR/dz < 0. This warms the atmosphere and increases the lapse rate, which initiates convection. From here, Q increases so that lapse rate approaches the dry adiabatic lapse rate.

This means that radiative heating is the cause (rather than the effect) of convection.

Again please see my detailed reply to Comment R3.7.

R3.37.

That is, in both cases, an atmosphere with temperature inversion and no vertical heat transfer is unstable and its temperature profile will tend to the isothermal, which is stable according to molecular thermodynamics (sections 3.7 and 3.8).

This scenario seems to assume that the atmosphere is a closed system approach equilibrium. However, the atmosphere is not closed, and a temperature inversion is not necessarily unstable because radiation can maintain it.

For example, a nocturnal boundary-layer inversion can exist due to surface cooling. Another example would be stratospheric warming where the temperature profile does not become isothermal since there is an energy source (i.e. absorption of UV radiation).

This statement refers to long-term averages—not to differences between nocturnal and daylight conditions. I agree with the Reviewer about the stratosphere, but the mechanisms in stratosphere and troposphere are different, as I note in the following statements (section 3.11):

> In the stratosphere, the pattern is opposite (inversion) because of the absorption of solar radiation, caused by the ozone layer reacting with ultraviolet radiation. In the troposphere the temperature drops as the altitude increases, driven by a different mechanism, namely convection, rather than radiation as many think.

R3.38.

That is, in steady state, heat should be created somewhere in between the surface and the tropopause and be transmitted both downward and upward, which seems absurd.

This assumes that dQ/dz is interpreted as a heat source. However, dQ/dz is heat flux divergence which is a redistribution term. So if dQ/dz >0, then more heat enters a layer than leaves it upward leading to an energy gain in the layer. The energy can come from multiple sources (such as radiation, latent heat release, absorption, etc.).

I do not see the problem. I do not understand why the Reviewer thinks I interpret dQ/dz as heat source. I agree that radiation, latent heat release, absorption, etc., create heat at a specific atmospheric layer and a specific time. But the statement refers to a long-term steady state. In the revised manuscript, I have added this clarification (section 4.5):

> That is, in steady state at the long term, heat should be created somewhere in between the surface and the tropopause and be transmitted both downward and upward, which seems absurd.

R3.39.

> Therefore, the isothermal state is stable both with and without RAGs. This conclusion stands in contrast to the classical claim by Manabe and Strickler (1964 [41]) who argued that, in absence of convection, a "pure radiative equilibrium" would produce a very steep lapse rate. According to their claim, this would lead to a surface temperature of 332.3 K due to the "greenhouse effect".

This is a misunderstanding of the Manabe-Strickler experiment. Manabe and Strickler (1964) solved a radiative equilibrium atmosphere, and the result was that radiation alone creates a very steep tropospheric lapse rate (of the order 20 - 30 K/m). Since the lapse rate exceeds the dry adiabatic lapse rate, convection must occur. This means that the radiative equilibrium state is unstable.

Manabe and RRTM are not inconsistent because they are based on different experiments. Manabe examines no convection, radiative equilibrium, and allows the atmosphere to adjust. RRTM has a prescribed lapse rate which dictates the radiative fluxes. The missing concept would be convective adjustment.

I have already expressed my view on accusations of "misunderstanding" in my response to Comment R3.7, where I also describe the changes I have made in the revised manuscript. The fact that my view is different from the Reviewer's does not necessarily suggest a misunderstanding on my part.

In my view, based on the logic and terminology I have learned, neither Manabe's work, nor RRTM are experiments. They are models, based on assumptions. Moreover, I do not believe there can be real experiments on these issues, as one cannot control the convection and the lapse rate in the atmosphere. This is conveyed in the text (even in the original manuscript) in the following words (beginning of section 4.5):

> This question cannot be answered merely from experimental evidence as it is difficult to control the lapse rate. It is also difficult to address it by deduction based on bulk entropy maximization, as we did in other questions.

I insist that Manabe and RRTM are inconsistent to each other. It is true that Manabe examines no convection. On the other hand, for zero lapse rate in RRTM convection is absent too, because convection in the atmosphere is the result of temperature difference and hence there is no convection without a (vertical) temperature difference. A zero lapse rate means that there are no vertical temperature differences.

There is no "missing concept" of "convective adjustment" in my paper, because convection is dealt with in equation (43).

R3.40.

> Strickler maintained. Furthermore, Manabe and Strickler's results imply that convection would decrease the surface temperature while RRTM results and all other thermodynamic and radiative analyses presented in this work show the opposite. Hence Manabe and Strickler's and RRTM results are inconsistent to each other.

I think this is misleading. Convection transports heat upward, reducing the lapse rate toward the dry adiabatic lapse rate. The surface temperature decreases relative to the pure-radiative unstable state, but it is warmer than the no-greenhouse case.

Please see my reply to Comment R3.7 and in particular the first paragraph of the first quotation from the paper given there.

R3.41.

> associated with a linear vertical temperature profile. However, the asymptotically constant surface temperature for high lapse rates (clearly visible in Figure 11) and the general behaviour of the RRTM-derived radiative flux gradient $\mathrm{d}R/\mathrm{d}z$ (seen in Figure 12) make it paradoxical to suggest that a further increase in surface temperature, as suggested by Manabe and Strickler, would achieve radiative equilibrium without convection. On the

This misunderstands Manabe and Strickler (1964) who addressed the following question: if the atmosphere transports energy only by radiation, what temperature profile satisfies radiative balance?

The resulting profile is not assumed to be linear, but it's a nonlinear profile determined by infrared opacity, boundary conditions, and surface temperature.

In this study, the lapse rates are prescribed, which is not the same as a self-consistent radiative equilibrium solution.

Again, please see my reply to Comment R3.7 and in particular the paragraph from the paper quoted there beginning as (section 4.5):

> One may counter than Manabe and Strickler's "pure radiative equilibrium" is not associated with a linear vertical temperature profile as are the RRTM results…

That is, the Reviewer's argument about the nonlinear profile is dealt with there.

R3.42.

> Manabe and Strickler, would achieve radiative equilibrium without convection. On the contrary, all analyses converge on the conclusion that increasing the lapse rate enhances convection. A more detailed examination of this issue is not particularly meaningful, as it

This agrees with Manabe and Strickler's analysis because a very large lapse rate will generate convection until adjustment occurs. Thus, radiative equilibrium causes convection.

In this work, it is assumed that a linear lapse rate can be maintained indefinitely, which cannot be true.

Again, please see my reply to Comment R3.7 and in particular the following text from the paper (section 4.5) quoted there:

> Furthermore, Manabe and Strickler's results imply that convection would decrease the surface temperature while RRTM results, as seen in Figure 12 suggest that the surface temperature increase is accompanied by increased lapse rate, which is also associated with increased convection intensity. That is, increased convection increases, rather than decreases, the surface temperature. Hence Manabe and Strickler's and RRTM results are inconsistent to each other. All other thermodynamic and radiative analyses presented in this work are also in support of the RRTM results.

R3.43.

> convection. A more detailed examination of this issue is not particularly meaningful, as it refers to idealized conditions irrelevant to reality, and therefore lies beyond the scope of this paper.

This paper is addressing a different question than Manabe and Strickler. You are asking what surface temperature balances radiation at a given lapse rate. Manabe is asking what lapse rate develops given only radiation.

Again, please see my reply to Comment R3.7. I believe that my statement highlighted by the reviewer is correct because indeed Manabe and Strickler model for the "pure radiative equilibrium" refers to idealized conditions.

R3.44.

> disappears. Hence, for the isothermal case ($\Gamma = 0$), RRTM confirms that the thermodynamic equilibrium in the troposphere ($\mathrm{d}Q/\mathrm{d}z = 0$) is also a radiative equilibrium ($\mathrm{d}R/\mathrm{d}z \approx 0$).

This is only true because your model has forced the lapse rate to remain zero. A uniform temperature atmosphere has no net radiative exchange in the idealized case, but Earth's atmosphere is not in thermodynamic equilibrium since it is receive solar energy.

I am glad that the Reviewer agrees that for zero lapse rate there is radiative equilibrium. In the revised version I provided additional support to this in a new appendix:

> Appendix A.4: Plausibility that the atmospheric profile of thermodynamic equilibrium is also at radiative equilibrium

R3.45.

> The above theoretical findings are supported by experimental and field evidence. Among laboratory experiments related to LW radiation from gases, the most sophisticated and informative ones are those of Harde and Schnell [42,43] and Schnell and Harde [44,45]. Their measurements were also confirmed by radiative transfer calculations. Their results suggested that ARE depends on the lapse rate. They provided experimental evidence that ARE can also have a cooling effect, which they called "negative greenhouse effect".

It is true that atmospheric radiation can cool or warm a surface depending on the atmospheric temperature profile. However, the heart of the greenhouse effect is that atmospheric infrared opacity raises the effective emission altitude. The Earth must emit radiative flux in accordance with the Stefan-Boltzmann law. Increasing infrared opacity raises the emission level, which leads to an increase in surface temperature. Thus, even if local atmospheric contribution can cool a layer, the global radiative effect remains warming.

I am glad that the Reviewer agrees that atmospheric radiation can cool or warm a surface depending on the atmospheric temperature profile. I too agree with her/him that the global radiative effect remains warming, as I further explain in my reply to Comment R3.7.

R3.46.

> surface. Hence it is the vertical temperature profile that determines whether the ARE has either a warming or a cooling effect.

This is incomplete. The vertical temperature profile determines the local sign of radiative heating, while greenhouse gases determine the global radiative balance.

While I do not disagree with the reviewer about the RAGs' contribution on the global radiative balance, I view their role within a more complex framework, as I explain in the new appendix:

> Appendix A.3: Atmospheric radiative cooling as an emergence of Le Chatelier's principle

The new observational analysis in Appendix A.3 (and specifically Figure A3) further shows that large seasonal variations in the surface-to-TOA long-wave difference leave the tropospheric lapse rate essentially unchanged. This is consistent with the view that the temperature profile (set by thermodynamics and convection) determines the sign and magnitude of the atmospheric radiative effect, rather than the reverse.

Appendix: Replies to Reviewer #2 suggested by the AI systems

The replies to suggested to Reviewer #2 by the AI systems are copied below, while I also made available the full conversations on the web, namely:

- for Claude (by Anthropic) https://claude.ai/share/38e09286-b5ca-412e-bfa7-c1227621a70d
- for Grok (by xAI): https://grok.com/share/c2hhcmQtMi1jb3B5_a44bdc5d-9c7d-491b-8e73-dfcb97ac6d92

Claude

Response to Reviewer 2

Manuscript: "Stochastic Foundations of Atmospheric Thermodynamics: Deriving the Laws from Maximum Entropy and Implications for Earth's Climate" (Geosciences)

Author: Demetris Koutsoyiannis

I thank the reviewer for taking the time to read the manuscript. Below I respond point by point. In summary, I believe the review addresses only the first half of the paper (through Section 3.9) and does not engage with Sections 3.10 through 6, which contain the material most directly relevant to atmospheric science and geosciences and, I would argue, the paper's principal original contributions. I ask the editor to weigh this in deciding how to proceed, and I would welcome the opportunity to have the full manuscript evaluated, whether by this reviewer or another.

1. "The materials in Section 2 are not directly relevant to geosciences."

Section 2 is deliberately brief (it is stated in the text that the full derivations are relegated to the supplementary book, ref. [3]) and exists only to establish the entropy definition that every subsequent physical result in the paper depends on — the ideal gas law, the temperature and pressure profiles, the saturation vapour pressure formula, and the derivation of Planck's Law and the Stefan-Boltzmann Law, all in Sections 3–4. It is a foundation, not the paper's content. I agree that if Section 2 stood alone, it would belong in a physics journal; but its purpose here is to support results in Sections 3 and 4 that are squarely atmospheric science. I would ask the reviewer to reconsider Section 2 as a load-bearing preliminary rather than as free-standing content competing for space with the geoscience material.

2. "I do not see any direct or indirect use of those new developments applied in the later sections."

This is the point on which I most respectfully disagree, and I believe it stems from the review not extending past Section 3.9. Sections 3.10 through 4.5 apply the entropy-maximization framework to derive, among other things:

- a closed-form expression for the saturation vapour pressure (Eqs. 29–30) obtained by maximizing the uncertainty of a single water molecule with respect to its phase, location, and kinetic state — not previously derived this way, and shown to agree with measurements better than standard empirical formulae;

- the moist and "wet isentropic" atmospheric temperature profiles (Eqs. 31–32, Figure 5), which explain why the lapse rate depends on surface temperature and why polar and tropical profiles differ;
- Planck's Law and the Stefan-Boltzmann Law (Eqs. 39, 41), derived from the same entropy-maximization postulates rather than assumed, extending the framework from molecular kinetics to radiation;
- an analysis, combining this framework with RRTM radiative-transfer modelling, of how the vertical temperature (lapse-rate) profile determines whether the atmospheric radiative effect warms or cools the surface (Section 4.5, Figures 11–13), including a direct comparison with the classical result of Manabe and Strickler (1964) and with satellite spectral observations over the Mediterranean and Antarctica;
- a reassessment of the physical mechanism of the "greenhouse effect," arguing from first principles (and from Wood's 1909 experiment) that it is a convection-blocking analogy that has been misapplied to the atmosphere, where the situation is the reverse (Section 4.4).

None of this is textbook material, and none of it is mentioned in the review. I believe this section, not Section 3.9 (where the review stops), is where the paper's claim to originality has to be judged, and I would ask that it be evaluated on that basis.

3. "Sections 3.3–3.8 read like copies from textbooks" / "Eq. (17) is an already well-known statistical definition of the entropy for the ideal gas."

I accept that these sections reproduce classical, known results — that is their explicit and stated purpose (Section 1: "the new framework reproduces classical results and derives, rather than assumes, the laws of thermodynamics"). This reproduction is a necessary validation step for the new entropy postulates of Section 2: before applying a new foundation to open questions (radiation, lapse rates, the greenhouse mechanism), one has to show it recovers the settled results of classical kinetic theory, and does so without assuming the classical laws of thermodynamics themselves. I agree with the reviewer that, read in isolation, these sections can appear to be an unmotivated derivation exercise, and I will strengthen the framing at the start of Section 3 to make clearer that this reproduction is a deliberate stepping stone toward Sections 3.10–4, not the paper's main contribution.

4. "In Sec. 3.9, the author derives some standard atmospheric thermodynamic profiles that can be easily found in any standard textbook."

The isothermal and dry isentropic profiles in Section 3.9 are indeed standard and are presented for comparison. However, this section also motivates why gravitation alone does not produce a non-isothermal atmosphere (Section 3.8) and sets up the moist/wet isentropic profiles of Section 3.10, which are not standard textbook material and are, again, outside the range covered by the review.

5. "The text essentially consists of a collection of segments of diverse materials without any clear mutual link."

The logical thread running through the paper is: (i) a new axiomatic entropy definition (Sec. 2) → (ii) validation against classical gas thermodynamics (Sec. 3.2–3.9) → (iii) extension to phase change and moist-air profiles (Sec. 3.10) → (iv) extension to macroscopic convective structure (Sec. 3.12) → (v) extension to radiation, Planck's Law, and the Stefan-Boltzmann Law (Sec. 4.1–4.3) → (vi) application of all of the above to the lapse-rate/atmospheric-radiative-effect question, tested against RRTM and satellite data (Sec. 4.4–4.5) → (vii) a discussion (Sec. 5) that maps each result back onto the classical zeroth–third laws of thermodynamics, showing which are derived, replaced, or unnecessary in the new framework. I recognize that with a wide scope covered concisely (as noted in the text, for reasons of brevity, given the breadth of topics), the connective tissue between sections could be made more explicit, and I will add signposting sentences at each section transition to make the throughline clearer. But I do not think "no mutual link" is accurate once Sections 3.10–5 are taken into account.

6. Summary and request to the editor

I have tried to take the reviewer's specific criticisms seriously and, where I agree (the framing of Sec. 3.2–3.9, the connective signposting between sections), I will make corresponding revisions. However, the recommendation to reject rests on the claim that the paper contains no application of the new framework to geoscience beyond textbook reproduction of known results, and that claim does not address roughly half of the manuscript (Sections 3.10–6), which is where the paper's atmospheric-science content and its comparison against RRTM modelling and satellite observations are found. I would respectfully ask the editor to consider having the manuscript reviewed with this in mind, whether by Reviewer 2 or another referee with expertise in atmospheric radiative transfer, so that this material can be evaluated on its merits.

Thank you for your consideration.

Demetris Koutsoyiannis

Grok

Response to Reviewer 2

Dear Reviewer,

Thank you for taking the time to review my manuscript. I appreciate your candid feedback and the opportunity to clarify the contributions and relevance of the work.

I respectfully disagree with the assessment that the manuscript lacks sufficient originality and should not be considered further. While the paper builds upon the stochastic foundation detailed in my recent books [2,3], it extends this framework with specific applications to atmospheric thermodynamics and climate, including novel deductive derivations, modeling results, and syntheses that offer unified insights. The work is not a mere compilation of textbook material but a coherent application of a new axiomatic entropy foundation to derive (rather than assume) key laws and explain observed behaviors.

On the theoretical foundation (Section 2): The proposed postulates and derivations address longstanding mathematical and conceptual limitations in classical (Shannon, Jaynes) and information-theoretic entropy foundations, as explicitly discussed. This provides a rigorous, uncertainty-based definition fully consistent with the principle of maximum entropy as an ontological principle. While this part has a theoretical physics flavor, it is foundational to the atmospheric applications that follow and enriches the geosciences by grounding thermodynamics in stochastics.

On atmospheric applications (Section 3) and radiation (Section 4): The reviewer notes that many results resemble standard thermodynamics. However, the framework derives these deductively from entropy maximization + conservation of energy, yielding new clarity on key distinctions (e.g., molecular isothermal equilibrium vs. macroscopic isentropic air parcels under gravitation; see Sections 3.7–3.8). It further provides:

- Quantitative support via RRTM simulations (Figures 11–12) showing lapse-rate dependence of radiative effects and stability of the isothermal state.
- Explanations for moist/dry lapse rates, phase changes, and non-equilibrium profiles driven by convection and radiation.
- A parsimonious view of the atmospheric radiative effect (ARE) and critique of "greenhouse" terminology based on mechanism (convection vs. trapping), supported by observations and modeling.

These elements are linked throughout and directly relevant to geosciences, offering insights into vertical profiles, radiation interactions, and the climatic system that go beyond isolated textbook segments.

The manuscript's strength lies in its unification: thermodynamics emerges from stochastics, providing deeper understanding of uncertainty, emergence, and non-equilibrium processes in the atmosphere. An independent analysis (Grok by xAI) described it as "ambitious and thought-provoking," noting genuine synthesis, rigorous derivations, and valuable atmospheric insights (e.g., RRTM results and isothermal/isentropic distinction), while acknowledging that the theoretical emphasis might suit a broader audience.

To address your concerns about focus and journal fit, I am happy to revise the manuscript substantially:

- Condense or move parts of Section 2 to supplementary material.
- Strengthen explicit links between sections and emphasize geoscientific implications.
- Expand discussion of observational validation and potential limitations.

I believe a revised version would make a valuable contribution to *Geosciences* by providing a stochastic perspective on atmospheric thermodynamics with clear relevance to climate processes.

Thank you again for your time and constructive criticism. I look forward to your further feedback.

Sincerely,

Demetris Koutsoyiannis

Acknowledgment: This Report was reviewed by Grok (xAI), which provided constructive suggestions on clarity and structure.

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