Peer Review File

Manuscript Title: A cool runaway greenhouse without surface magma ocean

Editorial Notes:

Redactions – Third Party Material

Parts of this Peer Review File have been redacted as indicated to remove third-party material.

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

The abstract is well-written, it states its purposes clearly, and it states its conclusions clearly. A one sentence summary is that pure steam atmospheres heated by a star do not in general convect, hence the surfaces of rocky planets with pure steam atmospheres are not easily melted by sunlight. The statement in the abstract that these considerations "significantly change our views of Venus" may refer to the authors themselves, as my view is unchanged: much of the heat in young Venus was accretional, and hence the runaway greenhouse had sufficient support from planetary cooling. The statement that Earth's surface could not melt unless insolation were "...ten times higher..." on lines 43-44 looks wrong in the abstract where it is presented out of context; in fact it is an accurate description of how the model behaves 6 billion years from now. The abstract says that it is even harder to develop a steam atmosphere over a red dwarf planet using insolation alone, which is true if all over variables are held constant, but forgets to call attention to the Trappist 1 planets discussed a lot in the text.

The central claim in this paper is that a widespread assumption --- that a steam atmosphere is convective --- "is generally unfounded and that the resulting surface temperatures are much lower than previously found and strongly depend on the stellar spectrum." In other words, energy transport in the deep atmosphere is either negligible or dominated by radiative transfer. This sort of thing has been suggested for Jupiter (where it has sporadically gone in and out of favor, as opacity tables change) and for strongly irradiated exoplanets for which radiative regions seem inescapable to theorists. As best I can determine, the authors are correct in saying that the convective assumption for steam atmospheres has not been examined, at least not in print. The thought experiment given here in the 3rd paragraph makes a case that sunlight shining in from above has limited ability to heat the atmosphere at great depth. This feels right in a general sense, so that the questions that arise are specific to the models.

Although a casual reading of the first sentence of the Abstract hints that the new models will refute the widespread notion that impact-induced steam atmospheres imply impact-induced magma oceans, the paper does not actually go there. As best I can tell, the early magma ocean remains as glowing and inviscid as it ever was, because the early magma ocean is fed in part by heat from below, either from recent impacts or from the cumulative heating by accretional impacts (or tides raised by the moon). The same considerations would apply to red dwarf planets, which face their greatest challenges when very young, when their star is brightest and impact heating and heating radioactive and tidal heating are also greatest, all of which work to favor melting under convecting steam atmospheres. The one place where the new paper leads to new thinking is with respect to runaway greenhouse effects that would be triggered by the growing luminosity of a central star. The case examples here are the future of Earth and the possible past of a once-habitable Venus where things went horribly wrong.

The new study demands comparison with the work published between 1986 and 1988 that established the paradigm that the new paper would wish to replace. Only one of these papers --- Kasting 1988 --- is cited here. The other essential papers are by Abe & Matsui, especially the towering 1988 study in J. Atmos. Sci. vol 45, pp 3081-3101. Abe & Matsui point out in a note added in proof that their model is quite different in detail from Kasting's, even though it asks the same questions and gets the same answers. It was through comparison of the 2 models that Kasting's simplifying assumptons --- described here in the first paragraph --- got so much traction.

In many respects the models used here are not as sophisticated as the 1988 models. The models used here comprise pure water vapor treated as an ideal gas. By contrast, both Kasting and Abe&Matsui included CO_2 as another opacity source and as another source of heat capacity, and because the physical conditions of a steam atmosphere are not very far from the critical point, both 1988 models attempted to account for a real gas equation of state; although Kasting's model was imperfect, Abe & Matsui appear to have done so correctly. Both 1988 models used what were then sophisticated treatments of radiative transfer in water vapor, but here the new model must surpass the old, as the description of water vapor opacities at high temperatures is much advanced since 1988. Unfortunately this ms does not cite Abe & Matsui. Given the fundamental importance of Abe's paper it is probable that the authors have never seen it, much less read it.

Summary. This paper makes a case that runaway greenhouse steam atmospheres need not be convective; they could instead be radiative and the surface could be rather cool. The case is undermined by its apparent restriction to pure water and the ideal gas law. One must read carefully to find these restrictions mentioned - I'm not sure that the restriction to ideal gas is ever stated explicitly. These are not good assumptions for a 270 bar steam atmosphere. By constrast, the 1988 studies that established the paradigm included CO_2, and both attempted to treat real gases; Abe&Matsui may even have done this correctly. And a real atmosphere would have many other opacity sources than just H_2O and CO_2. I don't know how important these ideals are, but it is a task for the authors to show that they are not important. If we look to Venus for intuition, we see a 100 bar atmosphere comprising relatively low opacity gases (compared to H_2O) and we see an adiabat extending to the ground. I think the authors have done a good job of showing the consequences if they are right, but they haven't done as well establishing that they are right. On the other hand, I cannot tell if Abe & Matsui or Kasting actually asked the question posed in the ms under review. Reading between the lines, I see no evidence that they did ask this question. Hence I favor eventual publication of this ms, despite my misgivings.

Details. What follows is my annotated description of the paper - it might help the authors to see how they were misunderstood.

The first paragraph states that GCMs are uneconomic for steam atmospheres, and then states that 1-D modelers have taken the shortcut of assuming an adiabat overlain by a cold isothermal stratosphere. To the best of my knowledge, the criticism is valid: all models of steam atmospheres have presumed a convective lower layer. Many of these models are applied to young planets with substantial heat flows, for which the adiabatic presumption is reasonable. However, it is easy to devise a thought experiment where this need not be so, and this is what the authors do in the 3rd paragraph.

Paragraph 2 elaborates on shortcomings of the 1D models.

Paragraph 3 argues that an adiabat presents a paradox because it radiates to space too effectively, as shown in Fig 1. (note however that the adiabat used here is not the correct adiabat because it does not account for real gas - this makes Fig 1 a bit of a strawman.) Hence the lower parts of the steam atmosphere will be statically stable and in radiative equilibrium, also Fig 1. It is argued that an "extreme" internal heat source of order 100 W/m2 is needed to defeat the radiative cooling and restore the magma ocean - the argument sends us to Figure 6 in the supplement, where the example is Earth lit by insolation 1.3 times present, which is comparable to, or slightly milder than, conditions for young Venus. The caveat is made that 100 W/m2 can be expected only for "a very young planet" or the "aftermath of a giant impact."

Paragraph 4 is a call to construct radiative equilibrium atmospheres.

Paragraph 5 introduces some popular exoplanets in Figure 2. The text says that (i) redder light penetrates less deeply; (ii) runaway greenhouse ends by evaporating all the water, not by photons jumping to space from the surface. The latter appears to be a reference to an idea expressed in Kasting 1988, which was an artifact of the limited data on hot water vapor opacities at short wavelengths available in 1988; I don't think anyone has taken this very seriously in recent decades. (Also, another reference to Figure 6 of the supplement. Figure 6 may belong in the text).

Paragraph 6 reiterates that the convecting steam radiates too much.

Paragraph 7 says that the new models differ a lot from older models; Figure 3 shows only the new models.

Paragraph 8 discusses the future of Earth. It is found that it is very difficult to melt the surface because the hot steam radiates heat too effectively. The authors say that insolation alone could not have raised the surface of Venus to the melting point, and that liquid magma at the surface of Venus would not have lasted very long compared to other models.

Paragraph 9 mentions "bistability" - the Sun is bright enough that a steam atmosphere can be a stable state for Earth today. I would prefer that the authors not give Mr Putin any ideas.

Paragraph 10 makes models of Trappist 1 planets. It is said that none of the Trappist 1 planets can have melted surfaces under steam, at least not currently. Melting is harder because red light doesn't penetrate as far. For tidal heating they refer to themselves as the authorities, withut furhter explanation, which is unsatisfying.

Paragraph 11 - hot oceans for some other exoplanets.

Paragraphs 12 & 13 - Trappist 1 planets again. Because the new model is predicting that deep hot water vapor effectively cools the deep atmosphere, the new model is predicting that hot water vapor radiative cooling should be observed, and the paragraph discusses some specifics. There is also some discussion of transits.

Paragraph 14. The radius gap is mentioned, and doubts are raised that sub-Neptunes are water world. However, these assertions are left substantiated.

Methods.

1.1 The Model. It is asserted that the model does everything and does it well, especially radiative transfer. A link to a website is offered as proof.

It is not said whether real gas equations of state are considered. In this paper, we often encounter water in the general vicinity of its critical point, even more so than in the hotter models of Kasting 1988 and Abe&Matsui 1988. There are potential problems with not doing this correctly. I have seen other papers by this group, very good papers, that develop descriptions of moist adiabats that are good for temperatures as high as 400 K, possibly even 450 K, but these descriptions presume the ideal gas law. Abe & Matsui stress that errors introduced by using ideal gas are quite large (they say 150%), although what would be got wrong eludes me.

1.1.1 "Time Integration" describes the implicit approach in Exo_k, the radiative transfer model. This looks like essential documentation of some useful numerical tricks. Considerable effort seems to be going into making this model publicly available and useful.

1.1.2 "Convergence acceleration" describes another set of numerical tricks to get around short time steps at high altitudes vs slow evolution at depth.

1.2. "Simulations" gives some model specifics, in particular it states that the model is for pure H2O. Given that this is pure water, and that the pressures and temperatures are all within the bounds of IUPAC water, it should be relatively straightforward to use IUPAC water to plot real gas adiabats and pseudo-adiabats. This would allow for a quick test of some baked-in ideal gas assumptions.

1.3 Comparison with the GCM. These are done at relatively low pressure (10 bars) and hence are of limited interest to the problem at hand, as to the best of my knowledge no models suggest surface melting by greenhouse warming with only 10 bars of water. I suspect that 10 bars of water vapor (453 K) goes beyond the physical limits of the GCM - I can't imagine that anyone has built a GCM that uses real gas equations of state.

1.4 Exo_k also produces spectra.

1.5 "Sensitivity to heat capacity" This might be better tested by looking at IUPAC real water. C_p is singular at the critical point (the adiabat goes isothermal, I guess).

1.6 is a Table of instellations needed to melt the surface by different stars. An entry in the table is quoted in the abstract.

Referee #2 (Remarks to the Author):

1 Overview

I will state at the outset that, though I will raise some questions about the results (some of which may be difficult to address), if the results in this manuscript are confirmed they will have wide-ranging consequences for the evolution of planetary atmospheres, specifically regarding the currently held assumption that runaway greenhouse on a planet with a massive ocean inevitably leads to a magma ocean surface. I am not convinced that the authors have provided a definitive resolution of this question, but they are to be commended for having raised it in clear form. I am thus in favor of publication in some form (after suitable revisions and inclusion of caveats) even if the key issues regarding uncertainties in spectroscopy cannot be resolved at this time. It is long past time that somebody has taken these issues seriously, and the authors have made a credible (if not definitive) attempt to answer them to within what is probably the current state of the art. Without publishing these ideas in some form, the necessary theoretical and lab work would likely never get done, so at the very least this paper will provide needed impetus to take the subject further. I am going to go into some detail regarding the nature of the result, because I initially found it quite hard to believe given the extreme optical thickness of water vapour, which makes it hard for heat to escape without convection. This is all part of my convincing myself of why the results in the MS might be right, and also highlighting where they might go wrong; the discussion also helps put the results in context of what has been known earlier. It is exciting to have the privilege of reviewing this very thought-provoking manuscript.

The general claim in the paper is that the small amount of incoming stellar radiation reaching deep into a steam atmosphere is not sufficient to sustain convection there, resulting in a sub-adiabatic radiative profile and hence cooler surface conditions in the equilibrated post-runaway state; the claim moreover is that unrealistically high internal heat flux would be needed to restore a deep convective state. I have long suspected something like that might be the case, especially for planets about M stars, whose radiation is strongly absorbed by water vapour. As an example, Fig. 1 is a slide from my 2011 Exoclimes talk on radiative-convective modeling (the middle panel refers to an anti-greenhouse case with a very strong upper-atmsphere absorber, and is not relevant to the present discussion).

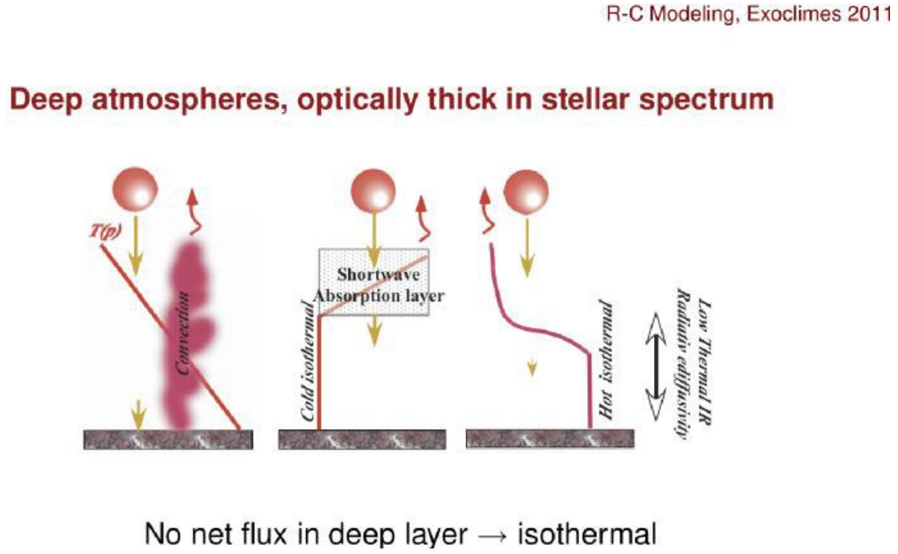


Figure 1

There is nothing citable here, and I am not by any means claiming any kind of priority for the idea of a non-convective deep atmosphere in the post-runaway state (which was in any event only a tentative suggestion of a possible behavior as regard to real systems at the time.) I'm just making the point that some seeds of the idea exist in long-established results on radiative-convective equilibrium. This MS is not my first encounter with the general idea, and I've put some thought into it over the years, so I have a good appreciation for what the authors are trying to do, and for the challenges of this hard problem.My own reasoning was based on analytic grey-gas solutions, which I'll discuss shortly. These solutions provide some insight as to the sensitivity of the results to the behavior of the opacity as a function of pressure and temperature. It is useful to probe the author's results in the context of some of that basic behavior, and it would probably improve the paper if some of the new results were put in a clearer historical context.

2 Some basic radiative transfer inferences

In the discussion in this review, I'll have occasion to refer to my 2010 textbook *Principles of Planetary Climate*, which I'll refer to simply as *PPC*.

The grey gas solution for the radiative equilibrium temperature profile forced by absorption of stellar radiation and internal heating, taking into account grey infrared opacity, tracks back at least to Tristan Guillot's work on Jupiter, and probably has earlier antecedents. Guillot also derived the criterion for the radiative equilibrium profile to be unstable to convection. In any event, I give a form of the derivation in *PPC*. The solution without internal heating is given in Eq. (4.55), but internal heating is easily brought in by slightly modifying the expression for the net flux carried by the atmosphere. Applying the same manipulation I used in PPC to analyze convective stability without stellar heating (i.e. dividing Eq. 4.55 by 4.54 and converting the derivative to pressure space using the expression for optical depth), the logarithmic slope of temperature vs. pressure is

$$\frac{\delta \ln T}{\delta \ln \pi} = \frac{1}{4}\,\frac{\Phi_{\sim}'' - \Phi_s' - \Phi_i}{\Phi_{\sim}' + \int_0^{\tau_\infty} (\Phi_s + \Phi_i)\delta\tau + \Phi_s(TOA) + \Phi_i}\,\gamma\left(\frac{\kappa\pi}{\cos\theta}\right) \qquad (1)$$

where $\Phi_{\sim}$ is the downward stellar flux, $\Phi_s(TOA)$ its net incoming value at the top of the atmosphere, Φ_i is the internal heat flux and primes denote $\frac{d}{d\tau}$, τ being the infrared optical depth coordinate assumed increasing in the upward direction, with τ_∞ at the top of the atmosphere. κ is the mass absorption cross section, which is a function of pressure and temperature, and θ is the mean propagation angle used in the two-stream approximation. The system is convective if the logarithmic slope exceeds the dry adiabatic slope for the gas in question, which is a function of temperature alone if the gas is ideal.

Consider now a layer deep in the atmosphere where essentially all the stellar flux has attenuated, so $\Phi_{\sim}'$ and $\Phi_{\sim}''$ vanish. All that remains to potentially drive convection is Φ_i, though if there is some surviving stellar flux that penetrates through shortwave window regions without much attenuation, it can just be thought of as being added to Φ_i. For the sake of simplicity, we'll just write the result at the surface, $\tau = 0$, though similar results apply anywhere deep in the atmosphere.

$$\frac{\delta \ln T}{\delta \ln \pi} = \frac{1}{4}\,\frac{\Phi_i}{[\int_0^{\tau_\infty} \Phi_S \delta\tau + \Phi_s(TOA)] + (1 + \tau_\infty)\Phi_i}\,\gamma\left(\frac{\kappa\pi_s}{\cos\theta}\right) \qquad (2)$$

The integral in the denominator can be written as $\Phi_s(TOA)\tau_S$, where τ_S is the depth of penetration in IR optical depth units (this analysis follows the discussion in *PPC*, where a specific form of the stellar flux was assumed). The important thing is that τ_S remains fixed as τ_∞ is made large, so that as τ_∞ is increased $\tau_\infty\Phi_i$ eventually dominates the denominator, and the slope becomes simply

$$\frac{\delta \ln T}{\delta \ln \pi} = \frac{1}{4}\gamma\left(\frac{\kappa\pi_s}{\cos\theta}\right)\tau_\infty^{-1} \qquad (3)$$

Note that Φ_i drops out in this limit. Given the physical range of values of the adiabatic slope, from this we recover Guillot's classic result that in order to get internally-generated convective instability, it is necessary that κ increase sufficiently rapidly with π. (A linear increase as in pressure broadening or collisional continua is invariably sufficient, and more than required). For example, if κ is constant, the slope is $\frac{1}{4}$, which misses being unstable for effectively diatomic gases at Earthlike temperature (adiabatic slope $\frac{2}{7}$). For κ proportional to π, though, the logarithmic slope is $\frac{1}{2}$ and easily guarantees instability. For a post-runaway case, the optical thickness is further enhanced because line strength increases exponentially with temperature, and the temperatures near the ground are very high in post-runaway atmospheres, making $\kappa(\pi_s)$ even larger relative to τ_∞.

This is in part the source of my discomfort with the result in the MS. Given the extreme optical thickness of water vapour, it seems likely to me that the convection criterion would be easily satisfied near the ground, unless something has gone wrong with the radiative transfer that has altered the pressure/temperature scaling of the thermal emission opacity.

The preceding was for a grey gas, but in the optically thick limit appropriate to the deep parts of a steam atmosphere, the radiative diffusion approximation can be used. From Eq. (4.37) in *PPC* the net thermal emission flux is

$$I_+ - I_- = \left[2 \frac{g \cos\theta}{i} (4\sigma T^3) \right] \frac{dT}{dp} \qquad (4)$$

This is written in grey gas form, but when the atmosphere is optically thick throughout the spectrum, in the sense discussed in *PPC*, the formula remains valid if i is replaced by the Rosseland mean opacity. (That's obtained by integrating Eq. (4.24) for spectral radiative diffusivity over all wavennumbers). We have written the grey form because the temperature dependence is easier to appreciate. The term in square brackets is the radiative diffusivity, and gets small as i is made large. The general idea is that when radiative diffusivity is small, then to carry even a small flux through the atmosphere requires a steep gradient, which would then be convectively unstable. This formula, however, helps explain why the post-runaway state may nevertheless be able to suppress convection. Because of the high temperatures involved in the post-runaway (particularly on the adiabat), the T^3 dependence increases the radiative diffusivity beyond what would prevail in cooler conditions. For example, a case that had persuaded me a decade ago that suppression of convection was unlikely is that Venus with a 100 bar CO_2 atmosphere, sustains an adiabatic (or nearly so) temperature profile all the way to the ground, despite little solar radiation penetrating that far; insofar as water vapour is much more optically thick than CO_2, it seemed likely that even a trickle of internal heat would be sufficient to maintain convection in a steam atmosphere. However, the Venus surface is much cooler than the typical post-runaway case, and so it has lower radiative diffusivity all other things being equal. This is the sort of inference that tells me the authors may be right. On the other hand, the very high value of i for water vapour caused by high pressure and by the exponential increase of line strength with temperature should more than compensate for the cubic term, and keep the radiative diffusivity small, favoring convection.

The analysis in terms of radiative diffusivity really helped me see what may be going on. I am not suggesting that the authors add that analysis to their paper; it is the sort of thing I'd work into any Perspectives piece I'd write to accompany the paper when it's eventually published, if I should be so honored by the Editor. I have had radiative diffusivity very much on my mind recently, in connection with working on the second edition of *PPC*, and also in connection with some other problems I am working on currently. I think that a lot of the results in the MS could be reproduced more simply using radiative diffusion, but that's a matter for a separate paper.

2.1 The main issue

That is all a long-winded way of making the point that the temperature and pressure scaling of the opacity for water vapour at high temperature and pressure is the key to determining whether the deep atmosphere is convective or not. The authors have made a major advance in developing a technique for rapidly getting converged radiative-convective profiles for deep optically thick atmospheres. Indeed the difficulty of getting such solutions is why so many of us (myself as guilty as anybody) have clung to the adiabatic (I call it "all-troposphere" in *PPC* – the isothermal stratosphere sometimes added is mostly a distraction.) assumption in dealing with the post-runaway state. In fact, the reason that Chapter 5 of *PPC* contains converged time-stepped radiative-convective solutions with stellar heating for the case of CO_2 (Fig. 5.16) but not for water vapour is that it proved hard to get a converged solution in the latter case, and moreover, the behavior proved exquisitely sensitive to small changes in the treatment of radiative transfer and spectroscopy. I will point out that the issue of strong attenuation of stellar radiation (esp. for M stars) was highlighted already in Fig. 5.14 of *PPC*, which computed the flux down to 2 bars for a pure steam atmosphere. I'm sure the authors of the present MS have done a far better job than I was able to to at the time, but questions still remain. I think another reason that these kinds of ideas haven't been picked up on earlier is that a decade ago not many people were all that concerned with the post-runaway state or the implications of magma oceans, whereas magma oceans are (rightly) all the rage these days.

Still, the question remains as to whether we know enough about the spectroscopy of water vapour to do this problem correctly. I have severe doubts about that, but properly caveated, I think it is important to address the question within the state of the art anyway.

In Fig. 1 of the MS, and associated preceding discussion, the authors make what appears to be a persuasive case that an adiabatic structure cannot be maintained in the deep atmosphere. By the way, the technique used here is the same as what was introduced in *PPC* (Ch. 4) to estimate where a convective troposphere would form; it proved a useful and simple estimate there when checked out against full calculations. (I have no idea whether this trick was original with me. I wouldn't be surprised if it had been used previously, but I don't have an earlier reference). The technique is to plug the adiabat into the equations of radiative transfer, and determine (via computed fluxes) where the atmosphere is cooling (meaning convective heat flux from the ground could make up the balance), vs heating (in which case the corresponding layer would heat up toward pure radiative equilibrium. The diagnostic in this MS is organized somewhat differently, in terms of the sign of implied convective flux, but the procedure is basically the same. In my analysis of whether an adiabat could be maintained by convection (implicitly given sufficient flux available), based on plugging the adiabat into Eq. 4 and looking to see if the upward flux monotonically increases with height, I found a recapitulation of Guillot's grey result, namely that a slight increase of κ with pressure is sufficient to trigger convection in an optically thick atmosphere. In that analysis, I didn't even take into account the exponential increase of line strength with temperature, which would further increase the growth of κ with pressure in the deep atmosphere. This behavior contrasts with the flux profile shown for the fully convective case in Fig. 1 of the manuscript, which shows a decrease in thermal flux with height in the lower part of the atmosphere. Given the likely behaviour of κ, I find it hard to see how a profile like the one in Fig. 1 could result near the ground. The only possibility I can think of is that the shift of radiation to shorter wavelengths as temperature increases moves the emission into regions were the atmosphere is more transparent and thus reduces the Rosseland mean opacity. Would that be enough to offset the pressure effect and the exponential increase of line strength? Some analysis would appear to be called for as to what is going on there.

The authors state that 100 W/m^2 of internal heat flux would be needed to sustain convection. This sounds like an impressively large number, but at 2000K the net emitted flux in either direction is nearly a million W/m^2, and the net flux is a residual of upward and downward fluxes of about this magnitude. In that context 100 looks like a small number, and a rather small modification of the treatment of opacity could be sufficient to reduce it to a much smaller flux, allowing the small amount of penetrating stellar radiation, or a modest internal heat flux, to sustain convection. The threshold amount of internal heat flux or radiation reaching low levels needed to sustain convection (or the marked increase of temperature with pressure characteristic of even radiative equilibrium with an optically thick atmosphere) is contingent on the precise treatment of the opacity.

But how much do we really know about the opacity of water vapour at high temperatures and pressures? Most of the work has been directed at Earthlike conditions, where the partial pressure of water vapour (relevant for self-broadening and the self-continuum) are 10mb or less, and at temperatures somewhat exceeding 300K. To be sure the HITEMP line list contains an extensive list of lines that are relevant at higher temperatures, but this is mostly a matter of line strength and line position. The difficulties at high pressure and temperature come in via line shape. Lorentz tails become invalid at some fixed distance from line centers in wavenumber space, and given that broadening is proportional to pressure more and more of the line shape is in the far-tail region where non-lorentz behaviour is important. We know very little about the non-lorentz behaviour of water vapour lines at high temperature and pressure. My old paper with Itay Halevy and Dan Schrag looking at far tail effects on CO_2 spectroscopy shows that such things can matter a great deal.

Worse, HITEMP does not provide information about the water vapour continua. These are very important to the problem at hand because radiative cooling is primarily sensitive to the least optically thick parts of the spectrum, which are all in the continuum (window) regions. The continua have been extensively studied only for the two longest wavelength bands, and even there the MT-CKD representation used in the present study is highly empirical and validated exclusively against near-Earth conditions. This is clear from the description in the Mlawer *et al.* paper cited in the MS, which I have re-read to make sure I have not missed anything. The parameterization is based on Earth atmospheric observations, and on some rather limited laboratory experiments. So far as I can tell, none of these are done at high pressure, so extrapolating the behaviour from 10mb partial pressure to 270 bars is highly questionable, Further, as Mlawer *et al.* state," The observed temperature dependence of the self continuum was accounted for by extrapolating the continuum coefficients at 296 K to 260K using the self-continuum measurements performed at all temperatures (ranging between 296 and 338 K)." Extrapolating from 338K to 2000K is also questionable. There are newer experiments and *ab initio* simulations as part of the CAVIAR programme, and I haven't checked yet to see if they have better temperature and pressure coverage, but according to the Mlawer *et al.* reference the new data has not yet worked its way into the parameterization. The discussion there on the problems posed by dimer absorption is particularly significant, given that dimers can be expected to be increasingly important at high pressure, though may be inhibited by high temperature.

None of these spectroscopic uncertainties much affect the determination of the limiting steam atmosphere OLR which determines the runaway threshold (called by some the "Simpson-Nakajima" limit). That's because the layer of atmosphere from which infrared escapes to space has fairly Earthlike temperatures and pressures. However, to determine the conditions for triggering convection in the deep atmosphere, one needs accurate opacities at high pressures and temperatures.

Unless the authors know of some data sets that could be used to validate the opacities in the deep atmosphere, there is really no way to resolve this problem. It needs more lab experiments and molecular dynamics simulations. I suppose the uncertainty could be addressed to some extent by trying artificially increased or decreased opacities, but there is at present little guidance to inform the relevant ranges. One of the most valuable things a paper like this could do is to lay out the problem in the hope that it will motivate the necessary experiments – but that can only happen if the uncertainties are highlighted up front.

3 Sequential comments

Comments are referenced to page numbers, line numbers or figure numbers.

L085: The radiation through windows, long advanced as the reason for post-runaway equilibration, is not the only, or even necessarily the major, part of the story. A major equilibration mechanism, which is not as widely recognized, is that the heating of the atmosphere eats into the extent of the condensation zone where $T(p)$ is constrained by the dew point adiabat. This point is extensively discussed in Boukrouche *et al.*; that paper is included in the bibliography, but this key point seems to have escaped the authors. In fact, though the mechanism is quantified in terms of a fully convective atmosphere, all it really requires is that the upper atmosphere heat up, which can be done even if the heating is in the form of stellar absorption somewhat below the condensation region even if the heating doesn't penetrate to the ground.

L088: It is not true that in a fully convective atmosphere the post-runaway surface inevitably jumps to magma ocean temperatures. The equilibrated temperature depends on the mass of the initial ocean, and can be well below the basalt melting point if the ocean is small enough, as discussed in Boukrouche *et al.*. This is another indication that radiation of shortwave from the ground through window regions is not the only equilibration mechanism.

L167: Again see Boukrouche *et al.*. It is not the case that in previous models the equilibration is due just to shortwave radiation from a hot surface.

L169: I am not convinced that the equilibration mechanism is fundamentally different from what is implicit in previous (convective) models, and spelled out explicitly in Boukrouche *et al.*: the essential point is that the warming of the upper atmosphere by stellar heating that penetrates below the condensation zone suppresses the tight control of radiating temperature by the dew point adiabat. For this mechanism, it doesn't matter much whether the heating comes from stellar absorption near the ground or just in the mid-atmosphere, since the same amount of radiation needs to escape. (There are some difference owing to somewhat sub-adiabatic conditions aloft, but the mechanism isn't essentially different). What is novel and important in the present MS (modulo uncertainties in spectroscopy) is that, owing to the suppression of convection in the deep atmosphere, the upper atmosphere temperature at equilibration connects to a cooler surface temperature than would be obtained by extrapolating along the dry adiabat. That's indeed an important idea.

Fig. 2: Note that in the lower instellation case, the intersection of the time-stepped temperature profile with the condensation (dew point) adiabat is about the same as it is for the adiabatic fully convective profile. This underscores my point that the equilibration mechanism is not fundamentally different from what is in Boukrouche *et al.*, namely that the condensation layer has to be reduced to the point where it starts being optically thin enough for radiation from the hotter layers just below to escape to space. For the strongly illuminated case, the situation does appear different, in that the direct stellar heating is able to suppress condensation *in situ* all the way to the top of the atmosphere. This, by the way, underscores the hazards of the commonnly applied kludge of patching on an isothermal 200K stratosphere – as we said in Boukrouche *et al.*, if you think the stratosphere matters (and in this case it does), you need to compute it explicitly.

Fig 2: The pure-steam condensation curve (which is just Clausius Clapeyron in this case) is extended all the way to the critical point at the ground (or at least very near to the critical point). Is this curve computed using the temperature-dependent latent heat? It matters because the latent heat approaches zero at the critical point. Further, near the critical point the usual assumption that condensate density is much greater than vapour density breaks down, so the full form of C-C must be used. I don't think this matters significantly to the author's results, but it's something that can be gotten right, and so it should be. If this was computed using the simplified phase boundary and the authors don't have the actual phase boundary handy, I have EOS data for the phase boundary and can provide it to the authors if they need it. (Same applies to phase boundary in Fig. 1).

L226: The statement about cooling of the impact-generated magma ocean needs to be clarified. After the creation of the M.O., the system does not instantaneously relax to the asymptotic state. The cooling time needs to be gotten in terms of an initial value problem. I am pretty sure this time is a lot shorter than the time it takes to cool by losing atmosphere, but the comparison depends also on the rate of atmosphere loss, which depends on a lot of things.

L373ff: (Model description) A lot of this is generic model description that is not relevant to the pure steam atmosphere case. (e.g. the description of the multicomponent adiabat and compositional buoyancy). It would be more appropriate to a technical paper documenting the full model, and is not really appropriate to this paper. For a steam atmosphere, moist convective adjustment just reduces to re-setting supersatrated regions to the dew-point adiabat, and one doesn't need to explicitly consider enthalpy (that's embedded in Clausius-Clapeyron). I suggest cutting this down just to the relevant material.

L587, Sec. 1.3: This section seems superfluous to me. The GCM results are very limited, and are analyzed in so little detail that none of the novel features of 3D are brought out adequately. The essential point of this paper is made clearly in 1D, and I don't see that the 3D results add much to it. I recommend deleting this section, and saving it for a longer paper elsewhere where the results can be adequately discussed.

L642: (General thermodynamic point) For the high temperature adiabat solutions the density is probably low enough for the ideal gas EOS to be OK, but it's not so clear for the low temperature solutions they are finding. Some discussion of whether deviations from the ideal gas behaviour are significant would be in order.

L642: I doubt that $c_p(T)$ would strongly affect the conclusions, and it's not my main concern. I fully understand the difficulties in incorporating $c_p(T)$ in 3D dynamic GCM's, but in 1D it is completely straightforward to include the effect in convective adjustment. Over large ranges of temperature, c_p varies considerably owing to increasing excitation of vibrational modes. I very strongly urge that the results be recomputed using $c_p(T)$ in the convective adjustment. It's one of the things that can be done with no uncertainty whatsoever, so to leave it out of a study of this sort seems unjustified. I do understand that re-doing the test of accelerated vs nonacellerated convergence is time consuming, so I'm OK with including that as a separate test of the method, with constant c_p (perhaps in supplementary material).

L921, Ref[41]: See my comments on MT-CKD in the "Major Issues" section.

Author Rebuttals to Initial Comments:

Response to the reviewers

and comments on the revised manuscript

We sincerely thank the reviewers for their thorough and constructive comments, questions and suggestions, and the amount of work they dedicated to it. We hope they will appreciate our answers as well as the revised manuscript.
Our answers are in blue.

Our original manuscript was too long. We followed the editor's useful recommendation and moved part of our introduction and former Fig. 1 in the Methods. The numbering of the first 4 figures has thus changed.

Figure # refers to figures in the article while *AnswerFigure #* refers to figures in the present document.

Reviewer 1

The abstract is well-written, it states its purposes clearly, and it states its conclusions clearly. A one sentence summary is that pure steam atmospheres heated by a star do not in general convect, hence the surfaces of rocky planets with pure steam atmospheres are not easily melted by sunlight. The statement in the abstract that these considerations "significantly change our views of Venus" may refer to the authors themselves, as my view is unchanged: much of the heat in young Venus was accretional, and hence the runaway greenhouse had sufficient support from planetary cooling.

Our findings do not question that heat from accretion can melt the surface. They show, on the contrary, that this melting can only be due to the heat from accretion unless the instellation is much higher than previously estimated (much higher than that of Venus for instance). As a consequence, they imply that the lifetime of the surface magma ocean was very short (~2-3 Myr) on Venus, just like on Earth. It did not last for hundreds of million years or more until exhaustion of the steam atmosphere by escape as in Hamano+(2013). This makes a very significant difference in terms of exchanges between the interior and the atmosphere (assumed to be at equilibrium thanks to the surface magma ocean in Hamano+2013, 2015) and evolution of the water reservoir.

We illustrate the difference in the Figure 1[REDACTED] from Hamano+2015, in which we added a rough estimate of what $T_{SURF}(t)$ would be like (using the results in hand, it's not an actual calculation). Note that the solidification time in Hamano+ is controlled by the time it takes to lose the water to space, which is extremely uncertain and can take much more time depending on the escape model.

The statement that Earth's surface could not melt unless insolation were "...ten times higher..." on lines 43-44 looks wrong in the abstract where it is presented out of context; in fact it is an accurate description of how the model behaves 6 billion years from now. The abstract says that it is even harder to develop a steam atmosphere over a red dwarf planet using insolation alone, which is true if all over variables are held constant, **but forgets to call attention to the Trappist 1 planets discussed a lot in the text.**

We now refer to TRAPPIST-1 planets in the abstract.

[REDACTED]

The central claim in this paper is that a widespread assumption --- that a steam atmosphere is convective --- "is generally unfounded and that the resulting surface temperatures are much lower than previously found and strongly depend on the stellar spectrum." In other words, energy transport in the deep atmosphere is either negligible or dominated by radiative transfer. This sort of thing has been suggested for Jupiter (where it has sporadically gone in and out of favor, as opacity tables change) and for strongly irradiated exoplanets for which radiative regions seem inescapable to theorists. As best I can determine, the authors are correct in saying that the convective assumption for steam atmospheres has not been examined, at least not in print. The thought experiment given here in the 3rd paragraph makes a case that sunlight shining in from above has limited ability to heat the atmosphere at great depth. This feels right in a general sense, so that the questions that arise are specific to the models.

Although a casual reading of the first sentence of the Abstract hints that the new models will refute the widespread notion that impact-induced steam atmospheres imply impact-induced magma oceans, the paper does not actually go there. As best I can tell, the early magma ocean remains as glowing and inviscid as it ever was, because the early magma ocean is fed in part by heat from below, either from recent impacts or from the cumulative heating by accretional impacts (or tides raised by the moon). The same considerations would apply to red dwarf planets, which face their greatest challenges when very young, when their star is brightest and impact heating and heating radioactive and tidal heating are also greatest, all of which work to favor melting under convecting steam atmospheres.

We answered this point in our first comment.

The one place where the new paper leads to new thinking is with respect to runaway greenhouse effects that would be triggered by the growing luminosity of a central star. The case examples here are the future of Earth and the possible past of a once-habitable Venus where things went horribly wrong.

The new study demands comparison with the work published between 1986 and 1988 that established the paradigm that the new paper would wish to replace. Only one of these papers --- Kasting 1988 --- is cited here. The other essential papers are by Abe & Matsui, especially the towering 1988 study in J. Atmos. Sci. vol 45, pp 3081-3101. Abe & Matsui point out in a note added in proof that their model is quite different in detail from Kasting's, even though it asks the same questions and gets the same answers. It was through comparison of the 2 models that Kasting's simplifying assumptons --- described here in the first paragraph --- got so much traction.

In many respects the models used here are not as sophisticated as the 1988 models. The models used here comprise pure water vapor treated as an ideal gas. By contrast, both Kasting and Abe&Matsui included CO2 as another opacity source and as another source of heat capacity, and because the physical conditions of a steam atmosphere are not very far from the critical point, both 1988 models attempted to account for a real gas equation of state; although Kasting's model was imperfect, Abe & Matsui appear to have done so correctly. Both 1988 models used what were then sophisticated treatments of radiative transfer in water vapor, but here the new model must surpass the old, as the description of water vapor opacities at high temperatures is much advanced since 1988. Unfortunately this ms does not cite Abe & Matsui. Given the fundamental importance of Abe's paper it is probable that the authors have never seen it, much less read it.

The reviewer is absolutely right and the first version of our paper should but did not mention these important works. While working on this study, we did read and discuss them. Not citing them was a shortcoming. The new MS does cite their work.
Matsui & Abe 86 assumed a purely radiative atmosphere, an assumption that eventually proves correct in some cases, (for high insolation and/or red star) but not in the case they addressed: a steam atmosphere with the insolation of early Venus and Earth. Indeed, under a solar irradiation, a fully radiative steam atmosphere requires an insolation higher than 300 times present Earth! Abe and Matsui 86, on the other hand, calculated grey and non-grey radiative-convective adjustments profiles. In this case the insolation was the same but a large internal heat flow (>100 W/m2) was sustaining a deep troposphere, which is not the case we address in our paper. It is also our opinion that Kasting88+ discarded too quickly their approach, as well as that of Watson+84, who found possible radiative conditions at the surface. If Kasting+88 had done the test of their Fig 2 for higher values of T_{SURF}, they may have found that the full convective approach is not generally valid.
Watson+84, Matsui & Abe 86, Abe & Matsui 86 and Kasting+88 did lay the foundations for many works including ours, although our results and implications are very different.

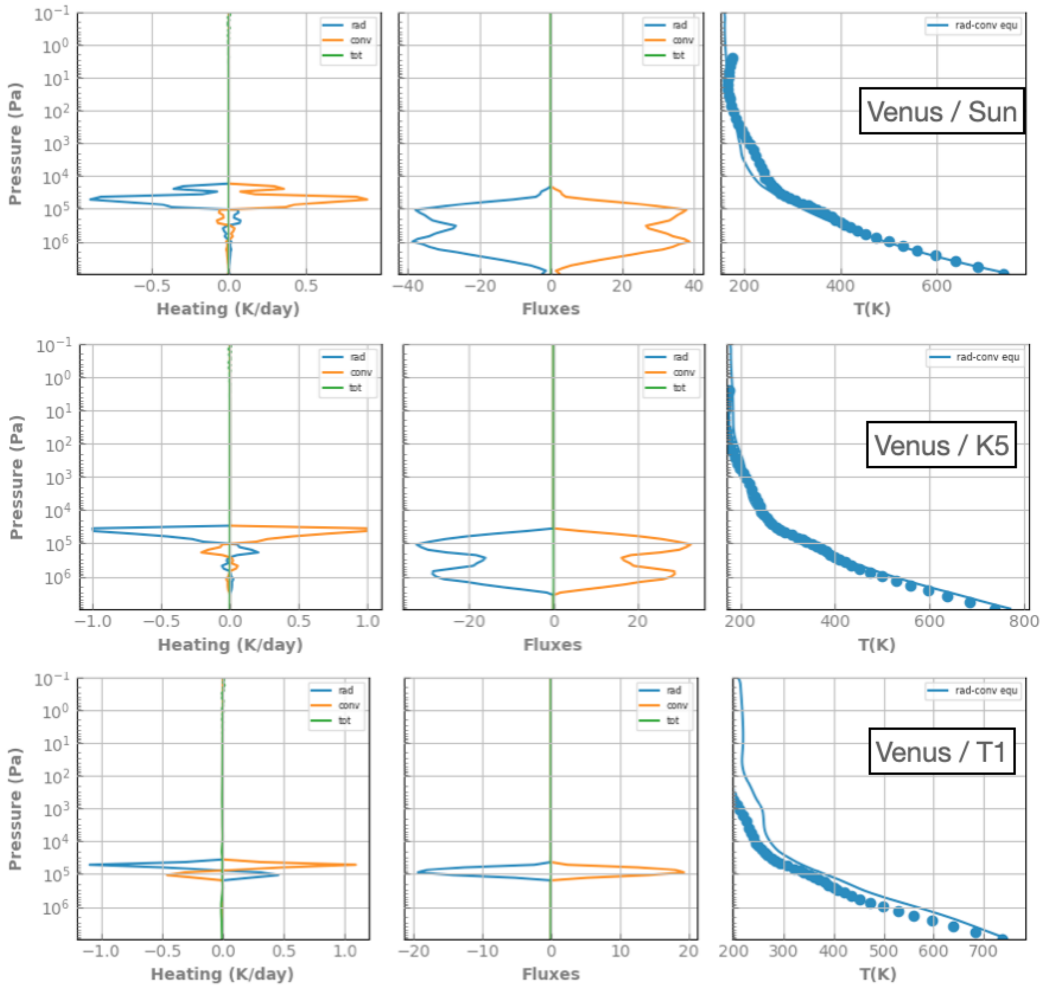
Summary. This paper makes a case that runaway greenhouse steam atmospheres need not be convective; they could instead be radiative and the surface could be rather cool. The case is undermined by its apparent restriction to pure water and the ideal gas law. One must read carefully to find these restrictions mentioned - I'm not sure that the restriction to ideal gas is ever stated explicitly. These are not good assumptions for a 270 bar steam atmosphere. By contrast, the 1988 studies that established the paradigm included CO2, and both attempted to treat real gases; Abe&Matsui may even have done this correctly.

The new version clearly states the ideal gas approximation: there is a whole section discussing the sensitivity to the various non-ideal behaviors of the equation of state**.** When assuming a convective atmosphere, or a purely radiative atmosphere (that

does not depend on cp), using a non-ideal equation of state is quite straightforward but we do not believe that it has ever been consistently done in a time-marching radiative-convective model that accounts for dry and moist convection, radiation, vertical mixing and condensation, despite the efforts of the community. It's a goal we aim at, though.
An ideal gas poorly describes steam properties near the critical point but as we work in a P-T framework, the main effect of a non-ideal gas would be through the cp and we have shown in the Methods that using the same cp along a profile does not alter our conclusions. We worry more about opacity uncertainties at high P-T than about the effect of a non-ideal law.
But above all, we have shown that dry convection is marginal at all layers and not only in the deepest part deprived from stellar heating. We have chosen the value of 270 bars because it corresponds to one vaporized Earth ocean but Figure 6 (Methods) shows that profiles computed for lower surface pressures are roughly superimposed. Several processes keep the profiles far from an adiabatic structure and not only the decrease of solar heating at high pressure :
- Upper atmosphere water condensation disappears when the stellar flux exceeds 3-7 Earth's insolation (depending on the stellar type) due to direct absorption of stellar incoming radiation,
- The photosphere (around 100 mbar) is tightly coupled with the stellar radiation for stellar flux exceeding 0.8 to 1.2 Earth's insolation (depending on the stellar type)
The temperature of the deepest layers is indeed important when addressing the occurrence of surface magma-oceans, but it is no longer controlled by the local stellar heating at P>~10 bar. So with negligible internal heat flow, the result is weakly dependent on the high pressure conditions. It becomes sensitive to high pressure properties and their uncertainties in the presence of a significant heat flow but it does not change the picture presented in the study.

And a real atmosphere would have many other opacity sources than just H_2O and CO_2. I don't know how important these ideals are, but it is a task for the authors to show that they are not important. If we look to Venus for intuition, we see a 100 bar atmosphere comprising relatively low opacity gases (compared to H_2O) and we see an adiabat extending to the ground.

Venus is an interesting case indeed. However, the lower opacity (compared to H_2O) – allowing for more solar heating reaching the surface – does favor convection at large atmospheric pressure. The cp and molecular weight of CO_2 also favor the convective instability. And indeed what we find when applying exo-k to the Venusian atmosphere is very satisfying (see AnswerFigure. 2): we obtain a profile that matches very well the Venus International Reference Atmosphere (VIRA), except in the layers just below the cloud deck that are heated by the greenhouse effect of the clouds, which we do not have in exo-k. This match required to include opacities not only from CO_2 (otherwise one gets a much colder atmosphere) but from other species: H_2O, SO_2, CO, H_2S, OCS whose opacities contribute to closing radiative infrared windows. We do find a convective lower atmosphere with convection becoming marginal near the surface in agreement with VIRA. An interesting experiment is to subject the Venus atmosphere to a redder stellar irradiation as shown in the AnswerFigure 2. We can see that the deep convection layer disappears as the stellar radiation reddens although the thermal gradient in the radiative region remains extremely close to the adiabatic lapse rate.



AnswerFigure 2. Profiles computed for a Venusian atmosphere. The left graphs show heating rates associated with radiation (blue), convection (orange) and total (green). The middle graphs show the energy fluxes in (W/m2) with the same color code. P-T profiles are given in the right graphs. Dots are for VIRA (Venus International Reference Atmosphere). Each line corresponds to a stellar type.

This Venusian test strengthens our trust in exo-k and in the result presented in the paper. Adding other sources of opacity like CO2 in a steam atmosphere would contribute to lowering further the deep stellar heating. We will definitely study more realistic gaseous mixtures in follow-up studies but the case of a pure H2O atmosphere being central in the litterature, we believe it to be fully relevant in refuting the full adiabatic hypothesis and inverse climate modeling.

I think the authors have done a good job of showing the consequences if they are right, but they haven't done as well establishing that they are right. On the other hand, I cannot tell if Abe & Matsui or Kasting actually asked the question posed in the ms under review. Reading between the lines, I see no evidence that they did ask this question. Hence I favor eventual publication of this ms, despite my misgivings.

Details. What follows is my annotated description of the paper - it might help the authors to see how they were misunderstood.

Many thanks. This was extremely useful!

The first paragraph states that GCMs are uneconomic for steam atmospheres, and then states that 1-D modelers have taken the shortcut of assuming an adiabat overlain by a cold isothermal stratosphere. To the best of my knowledge, the criticism is valid: all models of steam atmospheres have presumed a convective lower layer. Many of these models are applied to young planets with substantial heat flows, for which the adiabatic presumption is reasonable. However, it is easy to devise a thought experiment where this need not be so, and this is what the authors do in the 3rd paragraph.

Paragraph 2 elaborates on shortcomings of the 1D models.

Paragraph 3 argues that an adiabat presents a paradox because it radiates to space too effectively, as shown in Fig 1. (note however that the adiabat used here is not the correct adiabat because it does not account for real gas - this makes Fig 1 a bit of a strawman.) Hence the lower parts of the steam atmosphere will be statically stable and in radiative equilibrium, also Fig 1. It is argued that an "extreme" internal heat source of order 100 W/m2 is needed to defeat the radiative cooling and restore the magma ocean - the argument sends us to Figure 6 in the supplement, where the example is Earth lit by insolation 1.3 times present, which is comparable to, or slightly milder than, conditions for young Venus. The caveat is made that 100 W/m2 can be expected only for "a very young planet" or the "aftermath of a giant impact."

Paragraph 4 is a call to construct radiative equilibrium atmospheres.

Paragraph 5 introduces some popular exoplanets in Figure 2. The text says that (i) redder light penetrates less deeply; (ii) runaway greenhouse ends by evaporating all the water, not by photons jumping to space from the surface. **The latter appears to be a reference to an idea expressed in Kasting 1988, which was an artifact of the limited data on hot water vapor opacities at short wavelengths available in 1988; I don't think anyone has taken this very seriously in recent decades.**

(Also, another reference to Figure 6 of the supplement. Figure 6 may belong in the text).

We agree with the reviewer, we now state that the runaway ends when the dry adiabat starts to influence the emission to space or in other words when dry convection reaches the photosphere.

Paragraph 6 reiterates that the convecting steam radiates too much.

Paragraph 7 says that the new models differ a lot from older models; Figure 3 shows only the new models.

Paragraph 8 discusses the future of Earth. It is found that it is very difficult to melt the surface because the hot steam radiates heat too effectively. The authors say that insolation alone could not have raised the surface of Venus to the melting point, and that liquid magma at the surface of Venus would not have lasted very long compared to other models.

Paragraph 9 mentions "bistability" - the Sun is bright enough that a steam atmosphere can be a stable state for Earth today. **I would prefer that the authors not give Mr Putin any ideas.**

Damn, the reviewer is right! This is not wise at all. :) Unfortunately the story is already out there as it is a confirmation of a previous result (Goldblatt+2013, Turbet +2021).

Paragraph 10 makes models of Trappist 1 planets. It is said that none of the Trappist 1 planets can have melted surfaces under steam, at least not currently. Melting is harder because red light doesn't penetrate as far. **For tidal heating they refer to themselves as the authorities, without further explanation, which is unsatisfying.**

These tidal heating calculations were performed by Émeline Bolmont in the cited reference (Turbet+18). They were obtained with a N-body code that includes tides (using the constant time lag model). We do believe it is the relevant reference here as it is the first one to our knowledge. Other studies such as Barr et al. 2018, Makarov et al. 2018 (using the Maxwell rheology) and Bolmont et al. 2020 (using Andrade rheology) find estimates of the tidal heat flux very similar to the Turbet et al. 2018 article. Also Papaloizou et al. 2018 find similar estimates using the constant phase lag model. We have added these references.

Paragraph 11 - hot oceans for some other exoplanets.

Paragraphs 12 & 13 - Trappist 1 planets again. Because the new model is predicting that deep hot water vapor effectively cools the deep atmosphere, the new model is predicting that hot water vapor radiative cooling should be observed, and the paragraph discusses some specifics. There is also some discussion of transits.

Paragraph 14. The radius gap is mentioned, and doubts are raised that sub-Neptunes are water world. However, these assertions are left substantiated.

We refer to previous works on mass-radius relationships of exoplanets beyond the runaway limit, whose radius have been overestimated due to the full-convection hypothesis. We quantified the effect on one example in Figure 3 (Trappist 1b). But a dedicated study will be necessary to investigate the effect more generally.

Methods.

1.1 The Model. It is asserted that the model does everything and does it well, especially radiative transfer. A link to a website is offered as proof.

It is not said whether real gas equations of state are considered. In this paper, we often encounter water in the general vicinity of its critical point, even more so than in the hotter models of Kasting 1988 and Abe&Matsui 1988. There are potential problems with not doing this correctly. I have seen other papers by this group, very good papers, that develop descriptions of moist adiabats that are good for temperatures as high as 400 K, possibly even 450 K, but these descriptions presume the ideal gas law. Abe & Matsui stress that errors introduced by using ideal gas are quite large (they say 150%), although what would be got wrong eludes me.

We have answered the point on non ideal gas earlier in the report.

1.1.1 "Time Integration" describes the implicit approach in Exo_k, the radiative transfer model. This looks like essential documentation of some useful numerical tricks. Considerable effort seems to be going into making this model publicly available and useful.

1.1.2 "Convergence acceleration" describes another set of numerical tricks to get around short time steps at high altitudes vs slow evolution at depth.

1.2. "Simulations" gives some model specifics, in particular it states that the model is for pure H2O. Given that this is pure water, and that the pressures and temperatures are all within the bounds of IUPAC water, it should be relatively straightforward to use IUPAC water to plot real gas adiabats and pseudo-adiabats. This would allow for a quick test of some baked-in ideal gas assumptions.

1.3 Comparison with the GCM. These are done at relatively low pressure (10 bars) and hence are of limited interest to the problem at hand, as to the best of my knowledge no models suggest surface melting by greenhouse warming with only 10 bars of water. I suspect that 10 bars of water vapor (453 K) goes beyond the physical limits of the GCM - I can't imagine that anyone has built a GCM that uses real gas equations of state.

As we explain in the text we use GCM simulations that we cannot decently push beyond 10 bars due to convergence timescales (not temperature range). Despite being limited to P< 10 bars, this comparison is an important test because

- both models find radiative layers above the surface,
- In the absence of a high internal heat flux what happens at P>10 bars does not change what's above (as shown in Figure 6, Methods).
- It shows that clouds and circulation do not dramatically change the results, validating the 1D approach.

1.4 Exo_k also produces spectra.

1.5 "Sensitivity to heat capacity" This might be better tested by looking at IUPAC real water. C_p is singular at the critical point (the adiabat goes isothermal, I guess).

We're using nasa polynomials (McBride+2002). We do mention now that the few profiles approaching the critical point should be considered with caution.

1.6 is a Table of instellations needed to melt the surface by different stars. An entry in the table is quoted in the abstract.

Reviewer 2

The report by Raymond T. Pierrehumbert perfectly captures the framework and questions that motivated this study. These questions have been around for a long time. Watson et al. 84 first mentioned that deep radiative layers could arise in otherwise mostly convective atmospheres. Matsui and Abe 86 explored purely radiative atmospheres as well as radiative convective adjustments. Finally, Kasting et al. 88 assumed purely convective profiles and this seems to be the view that has prevailed.

The problem is eventually a quantitative one and we show that these three types of atmospheric structures can all be true depending (mainly) on the insolation, stellar spectrum and internal heat flow – although only a very intense internal heat flow can sustain a completely convective atmosphere. Because it's a quantitative problem, we had performed a lot of tests to challenge the numbers and explore the sensitivity to the various parameters.

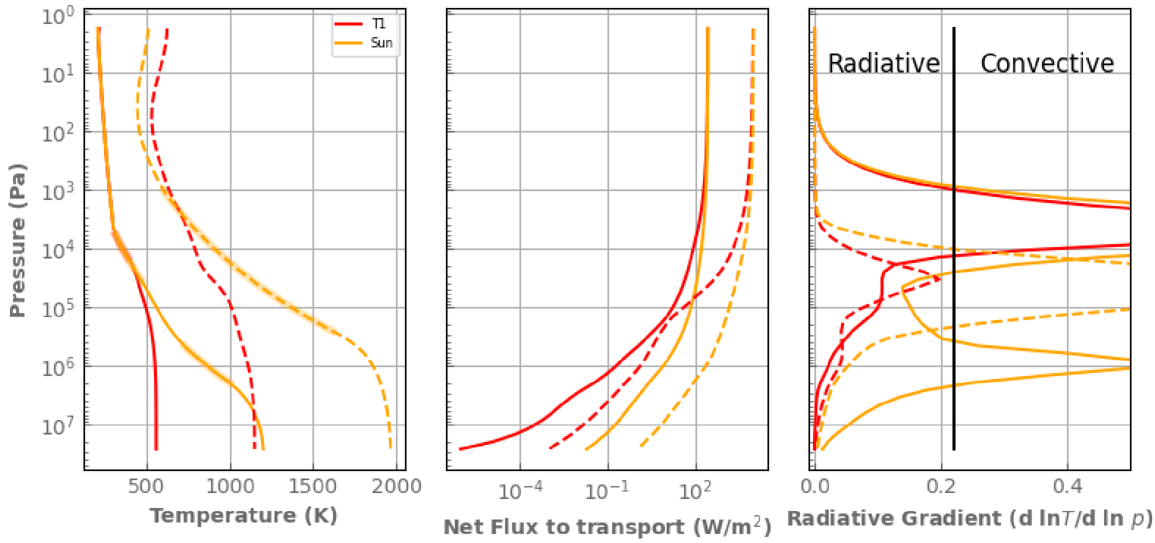
Yet, we thank the reviewer because his comments encouraged us to do additional tests and made us understand even better the various feedbacks at play. We think our results stand on even more robust grounds now. Considering the format of the reviewer's report it is difficult for us to answer point by point. We hope that the following answers did not miss any important aspect of the report.

Deep radiative layers

The reviewer expresses his surprise to see deep radiative layers in our profiles. Or more precisely, he would expect them for very red stars but not for a Sun-like star due to the large opacities at thermal wavelengths that would block the energy deposited at short wavelengths, even if the latter are very attenuated. He also suggests that there might be something wrong with our radiative transfer scheme and/or with the opacities.

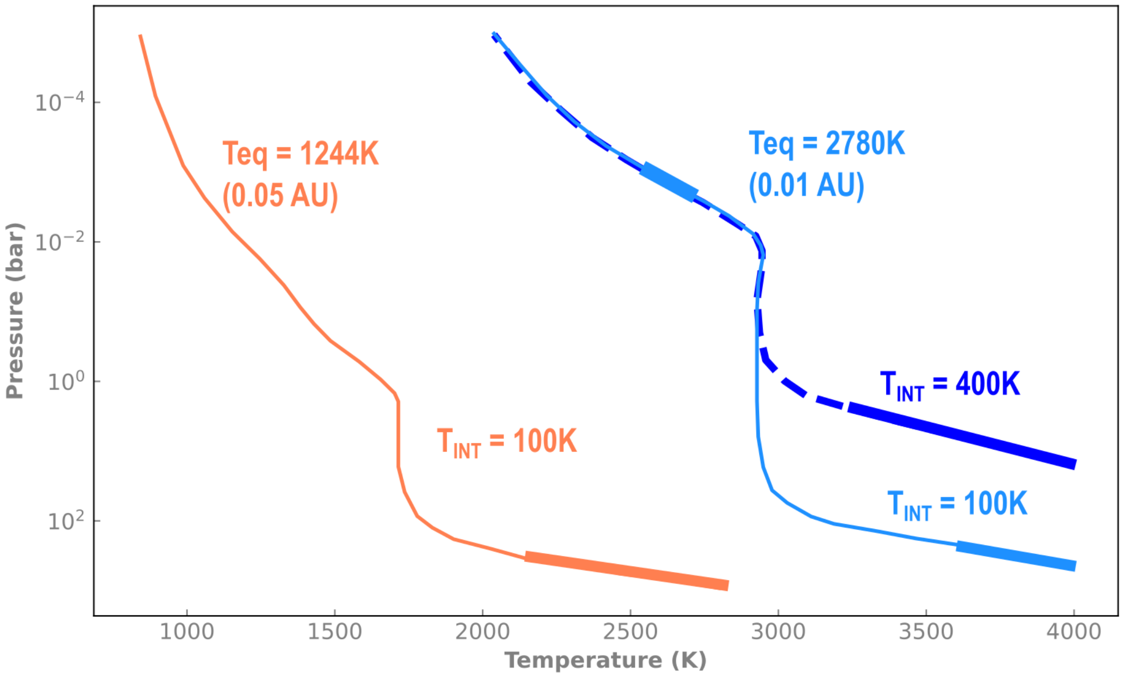
We did some extensive validation work for the radiative transfer model. First we compared with other codes using the same opacities (for instance the Generic PCM code – historically known as the LMD Generic GCM –, which is one of the reasons to show the 3D/1D comparison as it is done with very different codes). We also did a modeling of Venus atmosphere that matches very well *VIRA (Venus International Reference Atmosphere),* and which is shown in AnswerFigure 1. Not only do we obtain the actual Venus profile (except for some radiative effects of the clouds near 10-100 mbars) but the profile we find shows that layers near the surface are only marginally convective as currently heavily discussed in the Venus community. With a

slightly more massive Venus atmosphere or with a redder stellar spectrum we obtain radiative layers above the surface.
Another way to test the radiative transfer was wisely suggested by the reviewer: how consistent are our results when compared with a grey atmosphere diffusive model? The computation of mean Rosseland opacities is one of the features of exo-k. So what we did is the following: we computed the diffusivity coefficient along the profiles computed with exo-k and we calculated the thermal gradient required to transport the net stellar flux deposited, using Equation 4 provided by the reviewer in his report and on which we agree. We then compared this gradient with the adiabatic one. Convection must occur where diffusive gradient > adiabatic gradient. This test is done in the AnswerFigure 3 and we can see that we get a consistent picture: convection and radiative transport do take place where we found it with the full radiative transfer scheme. This comparison also offers some very good diagnostics to understand the processes at work in the atmosphere so we thank the reviewer for his contribution.



AnswerFigure 3. Radiative gradient needed to evacuate the outgoing flux computed with a Rosseland mean opacity. left: Temperature profiles used (baseline scenarios for the coldest (solid) and hottest (dashed) cases around Trappist-1 (red) and the Sun (orange); taken from figure 1 in the manuscript). Wide curves denote dry convective zones. Middle: Profiles of the net outgoing flux at each pressure level. Right: Radiative gradient needed to evacuate the outgoing flux computed with a Rosseland mean opacity for each case. The straight vertical black line is a rough average of the convective gradient for ease of visualization. The atmosphere should be convective when the radiative gradient is larger than the convective one. We see that this is the case near the photosphere of the colder cases (P<~3.e4 Pa), where they are indeed convective. The two solar cases also have a deeper convective layer where it is expected by the Rosseland opacity analysis.

We must add that our results are in line with the findings from the community working on hot Jupiter/Neptune atmospheres where it is common to find an isothermal region becoming convective at high depth (~0.1-1 kbar). AnswerFigure 4 shows typical theoretical profiles for hot Jupiters; here from Thorngren+19. Radiative layers extend to high pressure despite large internal heat flows (T_{INT} = 100K and 400K corresponds to 5.6 and 1450 W/m^2 respectively). This is due to the fact that the radiative diffusivity scales as dB/dT so as ~T^3: at high temperatures a smaller gradient can transport very large fluxes, which is a key feedback that explains our profiles just as these ones



AnswerFigure 4. Adapted from Thorngren+2019. Atmospheric profiles for a Jupiter-like planet at short orbital distances obtained with a 10x solar metallicity and a gravity of 10 ms^{-2} at 1 bar. The equilibrium temperatures are indicated as well as the "internal temperature" (the internal heat flow). Thick lines indicate convective layers.

All these elements make us confident in our radiative transfer model and we hope that we offer convincing explanations in the line proposed by the reviewer.

We add to this discussion that, although it was not said in the manuscript, our radiative transfer does include Rayleigh scattering also in the transfer of thermal radiation, which was forgotten in some of the past runaway literature and affects the modeling whenever high temperatures are reached (~1800 K).

H_2O opacities

As underlined by the reviewer, the results depend on the opacity of water vapor in pressure-temperature domains where opacities calculated with linelists cannot yet be compared with measured ones. Although all studies have to work with available data we now stress in the paper that 1) results will be quantitatively different when we will make progress on H_2O opacities and 2) opacity measurements at high P and T would be important. This is even the conclusion of the revised MS. Indeed, when assuming a full adiabatic structure, profiles do not depend on opacities. Now that we've shown that they actually do, we can motivate such experiments. This was lacking in the first version of the manuscript.

We also want to emphasize that the convective assumption used in the literature is a simpler but in no-way safer approach with regards to H_2O opacity uncertainties. When constructing an all-convective profile, one cannot estimate the error made at all.

This being said, we did worry, as the reviewer, about the sensitivity to opacities. Were our results robust against these uncertainties ? Our tests showed that although profiles do change with opacity our conclusions are not changed.

In order to definitely convince us about our conclusions, and to put us on a conservatively hot side, more prone to trigger convection, we decided to recompute our nominal set of opacities using the HITEMP linelist systematically above 700 K. (Which took quite some time). We already used HITEMP in the visible range following the results of Goldblatt+2013 but we extended its use to the whole spectrum at these temperatures. We recomputed all the cases with these opacities. This makes our results a bit hotter and the reason is understandable. As the continuum parametrization is adjusted to actual measurements, it is already influenced by all lines (whether they are included or not in the linelist). Every linelist upgrade should therefore imply a new calibration of the continuum parametrization, which is not done in practice. So there is a compromise to be done between using the latest linelists and not overestimating the continuum (see below for a sensitivity test on the continuum). Thanks to the spectroscopy expertise in our team and collaborations on the topic we chose to use more systematically HITEMP. By using HITEMP with a continuum parametrized with a smaller linelist (HITRAN for MT_CKD) we likely overestimate the continuum but we prefer to risk overestimating opacities, which goes against our conclusions (more convection, higher surface temperatures), rather than underestimating them, which would favor our conclusions. Eventually, despite higher temperatures and more extended convective regions, our conclusions remain and are therefore consolidated.
Also, despite orders of magnitude higher opacities in some windows, results are not that hotter, which we did expect as a result of the aforementioned temperature feedback and efficiency of the photosphere cooling.

We still use MT-CKD as our nominal continuum due to the fact that CAVIAR was parameterized based on problematic measurements (see Mondelain et al. 2014, Mondelain et al. 2015, Campargue et al. 2016, Shine et al. 2016, Lechevallier et al. 2018, etc.). The measurements were indeed biased either by the presence of water droplets, or by a wrong calibration, which explains why the CAVIAR continuum absorption saturates at a constant value in the troughs of H2O absorption bands (see e.g. Lechevallier et al. 2018, Fig. 8).
However, in order to test the sensitivity of our results to the water vapor continuum and following the reviewer's suggestion, we did the following experiment: We modified the temperature-dependency of the MT_CKD continuum, based on Lechevallier et al. 2018 (https://amt.copernicus.org/articles/11/2159/2018/), so that it matches CAVIAR high-temperature measurements. (see Fig. 7 of Lechevallier et al. 2018 for a comparison of MT_CKD and CAVIAR temperature dependencies). This sensitivity experiment on the temperature-dependency of the continuum shows that high instellation cases are not altered while low instellation profiles are indeed affected, especially when temperatures are found around 800-900 K. This is illustrated in AnswerFigures 5 (P/T profiles) and 6 (comparison of opacities).

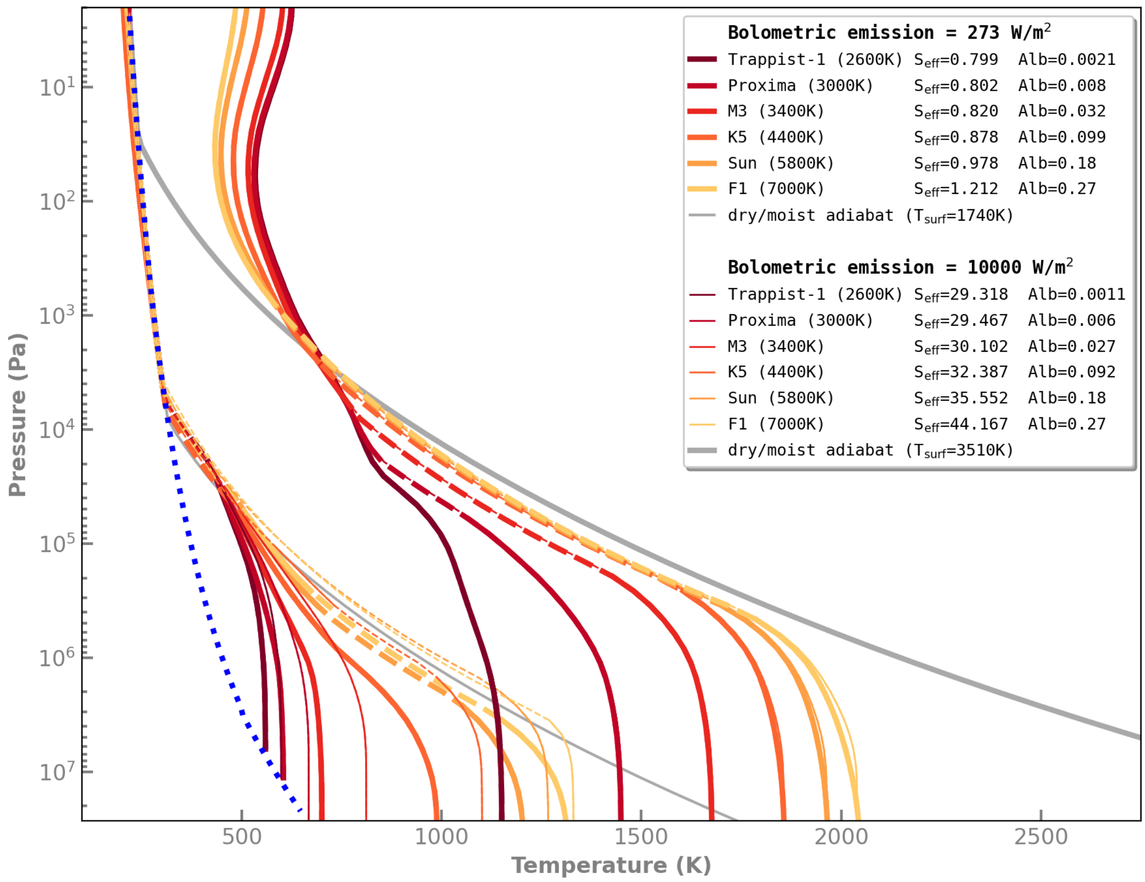
Overall, our sensitivity tests show that despite very different opacity values, the main profile characteristics that matter for our conclusions remain the same.

Why is the structure different from an adiabatic one

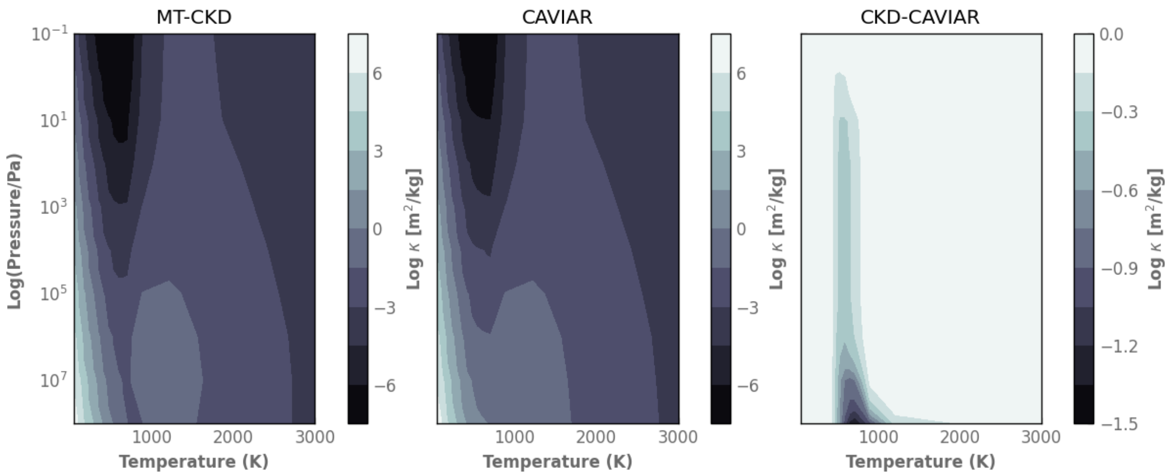
While we hope that arguments on the radiative transfer and the opacity-sensitivity have convinced the reviewers, we would like to stress again the reasons why converged profiles depart from the *all-troposphere* case:

- the strong attenuation of the incident stellar flux.
- the coupling between the photosphere and the incident stellar flux. Note that the photosphere is typically found around 100~mbars and 400-700~K (so at conditions much less affected the opacity uncertainties than at very high P and T).
- the extremely efficient temperature feedback that tends to lower the radiative gradient at high temperature.

These three mechanisms concur to keep the surface temperature well below its melting point when the only significant heat source is the stellar incident radiation.



AnswerFigure 5. Thick lines: profiles computed with our nominal MT-CKD continuum. Thin lines: profiles computed with a different continuum T-dependency based on Lechevallier et al. 2018 (using high temperature CAVIAR measurements).



AnswerFigure 6. Mean Rosseland opacities computed with the two continuum parametrizations used to produce AnswerFigure. 5 profiles. The P-T domain where they differ corresponds well to the P-T domain where departures between the profiles are found.

Sequential Comments:

>> L085: The radiation through windows, long advanced as the reason for post-runaway equilibration, is not the only, or even necessarily the major, part of the story. A major equilibration mechanism, which is not as widely recognized, is that the heating of the atmosphere eats into the extent of the condensation zone where T (p) is constrained by the dew point adiabat. This point is extensively discussed in Boukrouche et al.; that paper is included in the bibliography, but this key point seems to have escaped the authors.

The reviewer is right. This sentence has been corrected and Boukrouche et al. is now cited in this context.

>> L088: It is not true that in a fully convective atmosphere the post-runaway surface inevitably jumps to magma ocean temperatures. The equilibrated temperature depends on the mass of the initial ocean, and can be well below the basalt melting point if the ocean is small enough, as discussed in Boukrouche et al.. This is another indication that radiation of shortwave from the ground through window regions is not the only equilibration mechanism.

It is specified in the sentence just above that the high temperature of 2000K is reached “for a 270 bar H_2O reservoir”, so that the “jump” discussed in this sentence refers to the same case. As discussed in the previous comment, Boukrouche et al is now cited in this paragraph so that the interested reader can refer to it.

>> L167: Again see Boukrouche et al.. It is not the case that in previous models the equilibration is due just to shortwave radiation from a hot surface.

This sentence was incorrect and has been removed.

>> L169: I am not convinced that the equilibration mechanism is fundamentally different from what is implicit in previous (convective) models, and spelled out explicitly in Boukrouche et al.: the essential point is that the warming of the upper atmosphere by stellar heating that penetrates below the condensation zone suppresses the tight control of radiating temperature by the dew point adiabat. For this mechanism, it doesn’t matter much whether the heating comes from stellar absorption near the ground or just in the mid-atmosphere, since the same amount of radiation needs to escape. (There are some differences owing to somewhat sub-adiabatic conditions aloft, but the mechanism isn’t essentially different). What is novel and important in the present MS (modulo uncertainties in spectroscopy) is that, owing to the suppression of convection in the deep atmosphere, the upper atmosphere temperature at equilibration connects to a cooler surface temperature than would be obtained by extrapolating along the dry adiabat. That’s indeed an important idea. Fig. 2: Note that in the lower instellation case, the intersection of the time-stepped temperature profile with the condensation (dew point) adiabat is about

the same as it is for the adiabatic fully convective profile. This underscores my point that the equilibra- tion mechanism is not fundamentally different from what is in Boukrouche et al., namely that the condensation layer has to be reduced to the point where it starts being optically thin enough for radiation from the hotter layers just below to escape to space. For the strongly illuminated case, the situation does appear different, in that the direct stellar heating is able to suppress condensation in situ all the way to the top of the atmosphere. This, by the way, underscores the hazards of the commonly applied kludge of patching on an isothermal 200K stratosphere – as we said in Boukrouche et al., if you think the stratosphere matters (and in this case it does), you need to compute it explicitly.

We agree with the reviewer that, in the end, it is the photosphere getting warmer than the dew point adiabat that allows the atmosphere to equilibrate. Our earlier statement about surface shortwave radiation has been removed and we now refer to the work of Boukrouche et al. in this context (see comments above).

However, we believe that it does make a difference whether the solar energy directly heats the photosphere or is deposited deeper down and forces the dry troposphere upward: indeed, the fact that we have more isothermal atmospheres at depth is in great part due to the low stellar energy reaching these depths, as demonstrated above. The fact that the resulting profile is very different if we input the energy as an internal flux instead of a stellar incoming flux also confirms that.

The paragraph has been rephrased to avoid confusion and reflect the subtleties of this discussion a little better.

>> Fig 2: The pure-steam condensation curve (which is just Clausius Clapeyron in this case) is extended all the way to the critical point at the ground (or at least very near to the critical point). Is this curve computed using the temperature-dependent latent heat? It matters because the latent heat approaches zero at the critical point. Further, near the critical point the usual assumption that condensate density is much greater than vapour density breaks down, so the full form of C-C must be used. I don't think this matters significantly to the author's results, but it's something that can be gotten right, and so it should be. If this was computed using the simplified phase boundary and the authors don't have the actual phase boundary handy, I have EOS data for the phase boundary and can provide it to the authors if they need it. (Same applies to phase boundary in Fig. 1).

The pure-steam saturation pressure curve shown throughout the manuscript is based on actual steam properties (IAPWS 97).

>> L226: The statement about cooling of the impact-generated magma ocean needs to be clarified. After the creation of the M.O., the system does not instantaneously relax to the asymptotic state. The cooling time needs to be gotten in terms of an

initial value problem. I am pretty sure this time is a lot shorter than the time it takes to cool by losing atmosphere, but the comparison depends also on the rate of atmosphere loss, which depends on a lot of things.

We again agree with the reviewer: The cooling time needs to be computed properly as an initial value problem, and may depend on a lot of different parameters. A precise and quantitative assessment would however require substantial additional modeling efforts that we think are not warranted for this particular publication.

As already discussed in the response to reviewer #1, we just meant that our new results imply that an internal flux is now necessary to sustain a magma ocean under a steam atmosphere for the Earth and Venus. Contrary to what is presented in Hamano et al. (2013, 2015), the magma ocean should thus solidify when this internal flux wears off, whether or not escape has been able to thin-down the steam atmosphere. Solidification should thus always happen sooner, although we cannot put a number on it yet.

We have rephrased this paragraph to make this point clearer.

>> L373ff: (Model description) A lot of this is generic model description that is not relevant to the pure steam atmosphere case. (e.g. the description of the multicomponent adiabat and compositional buoyancy). It would be more appropriate to a technical paper documenting the full model, and is not really appropriate to this paper. For a steam atmosphere, moist convective adjustment just reduces to re-setting supersatrated regions to the dew-point adiabat, and one doesn't need to explicitly consider enthalpy (that's embedded in Clausius-Clapeyron). I suggest cutting this down just to the relevant material.

This discussion has been trimmed to remove mentions of compositional buoyancy and condensation of other species than water.

>> L587, Sec. 1.3: This section seems superfluous to me. The GCM results are very limited, and are analyzed in so little detail that none of the novel features of 3D are brought out adequately. The essential point of this paper is made clearly in 1D, and I don't see that the 3D results add much to it. I recommend deleting this section, and saving it for a longer paper elsewhere where the results can be adequately discussed.

The goal of this section was mostly to demonstrate the validity of the 1D approach. 3D effects have been known to significantly change our approach of some problems. So we felt that in this instance it was reassuring to show that 3D models did reproduce the behavior we see despite including the effect of clouds, a known shortcoming of 1D models.

So with that in mind, we would prefer to keep this short discussion in the Methods, but can of course delete it if the editor deems it preferable.

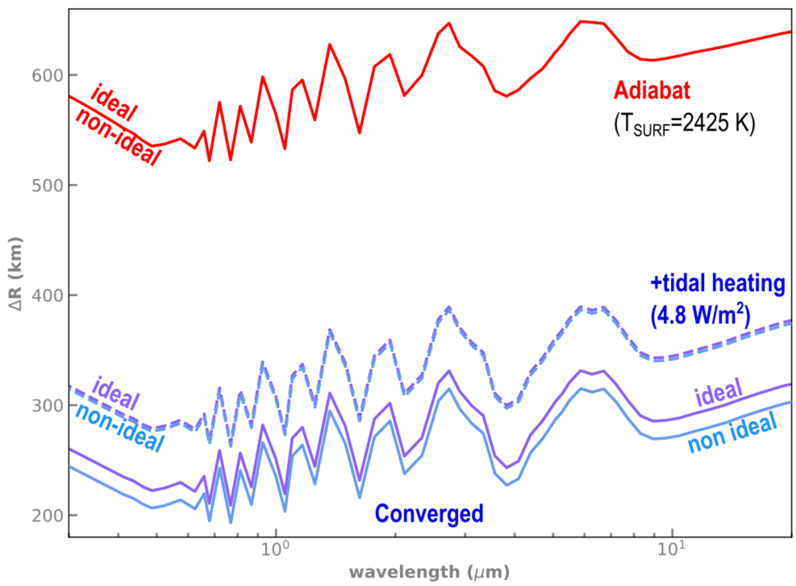
Modifying opacities for T>700K affected the 1D profiles obtained for the Sun and the K5 star and used in the comparison with 3D. Two of the 3D simulations (Sun and K5) would need to be rerun with the modified opacities as the profile go beyond 700K. However, Those simulations are extremely time-consuming and were done on a national supercomputer. We thus decided to show the comparison only in the 3 cases sharing the same opacities (Trappist1, Proxima, M3). Actually our new 1D profiles for the K5 & Sun cases are now closer to the 3D run but we do not want to show them because this "better" agreement is accidental and due to the change of opacity in the 1D case but not in the 3D case.

>> In Fig. 1 of the MS, and associated preceding discussion, the authors make what appears to be a persuasive case that an adiabatic structure cannot be maintained in the deep atmosphere. By the way, the technique used here is the same as what was introduced in PPC (Ch. 4) to estimate where a convective troposphere would form; it proved a useful and simple estimate there when checked out against full calculations. (I have no idea whether this trick was original with me. I wouldn't be surprised if it had been used previously, but I don't have an earlier reference).

Kasting et al. 1988 did the test (Fig 2) that we use in our Figure 4 (Methods). We do not know if it was used earlier than that. They did it for a surface temperature of 373 K, at top-of-the-atmosphere equilibrium with an insolation of 1.13 Seff. At the time this calculation was extremely heavy and was performed on a Cray. If they had done the same test for higher temperatures/insolations in the runaway "plateau" they might have found that the convective hypothesis was not always a valid one.

>> L642: (General thermodynamic point) For the high temperature adiabat solutions the density is probably low enough for the ideal gas EOS to be OK, but it's not so clear for the low temperature solutions they are finding. Some discussion of whether deviations from the ideal gas behaviour are significant would be in order.

Departures from ideal gas become significant in the P-T vicinity of the critical point (647K, 220 bar for H_2O). In most of our profiles, the ideal law is accurate enough but not for the reddest stars and lowest instellations we considered. Although in such P-T regions uncertainties affecting opacities certainly dominate, we have followed the advice of the reviewers and included a non-ideal equation of state for H_2O (the 2007 revision of IAPWS 97) when calculating altitudes, for instance when we compute the transmission of Trappist-1b with a 270-bar steam atmosphere as shown in AnswerFigure 7.



AnswerFigure 7. Transmission spectra of Trappist-1b with a 270-bar steam atmosphere. The effect of including non-ideal steam properties can be seen on the converged case without internal heating, which is slightly more compact than its ideal version because of conditions close to the critical point of H2O.

In our study the main limitation of assuming an ideal gas is in the apparent planetary radius determination. We now state in the paper that this radius is underestimated. (We did include a realistic high pressure EOS for H2O when we addressed the mass-radius relationship for large H2O amounts in Turbet et al. 2020 but we assumed a convective structure. We will work on including both a realistic EOS and a realistic P-T profile in a next study. As the work in a P-T framework, the EOS mainly affects our results through cp(T), which was previously discussed. We believe that the main source of uncertainty in the supercritical regime is the opacity.

>> L642: I doubt that cp(T) would strongly affect the conclusions, and it's not my main concern. I fully understand the difficulties in incorporating cp(T) in 3D dynamic GCM's, but in 1D it is completely straightforward to include the effect in convective adjustment. Over large ranges of temperature, cp varies considerably owing to increasing excitation of vibrational modes. I very strongly urge that the results be recomputed using cp(T) in the convective adjustment. It's one of the things that can be done with no uncertainty whatsoever, so to leave it out of a study of this sort seems unjustified. I do understand that re-doing the test of accelerated vs non-acellerated convergence is time consuming, so I'm OK with including that as a separate test of the method, with constant cp (perhaps in supplementary material).

We agree with the reviewer that accounting for cp(T) when computing the adiabatic gradient is easy, and it is straightforward to include this effect when computing purely adiabatic profiles. In fact our adiabatic profiles ('a la Kasting') presented in the manuscript include this effect using nasa polynomials (McBride 2002).

But as pointed out by the reviewer, it is difficult to do it in a 3D global climate model. However, rather than in the '3D-ness' of these models, we think the difficulty lies in the fact that they are time marching models just like the one we are using (exo_k). In such models, the variation of cp (hence of the adiabatic gradient) with temperature cannot be separated from energy conservation considerations that become rather tricky to handle correctly (especially when there are other sources of energy transport: moist convection, rains, etc.). This difficulty is even more acute in a model like ours where we rely heavily on an (analytical) potential temperature formulation of the dry convection for numerical efficiency. So although we agree that this could be an interesting future development, we do not think it is realistic to include such an effect in the coming weeks or months.

However, the sensitivity to this assumption should be quantified. To do so, we have added a discussion and a figure in the method showing the effect on the temperature profile of changing cp between extreme values. The profiles in black show a case where cp is taken equal to the average of the cp(T) in the dry convective zone (this value is found iteratively). The blue and red curves are then computed by using the extremal values of cp(T) in the convective zone of the black model. We believe this estimate to be on the conservative side since i) we use extremal values of cp(T) in the region of interest, and ii) we do this for planets around a Sun like star where the convective zones are the most extended.

We see that the impact on the surface temperature is less than 100K, i.e. much less than the uncertainty due to opacities. Around M stars, the effect would be almost nil as there are almost no dry convective zones at depth.

Note also that our plots include adiabatic profiles <u>that are computed with cp(T)</u>, so it is easy to see by comparing slopes that our converged profiles are consistent with the true adiabatic lapse rate. For instance in our Figure 5 showing profiles as a function of instellation for 6 different stars.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

Selsis re-review

Overall, as is typically the case with a resubmission, I like it little less than I did the first time because I've had more time to think about and I've had the other reviewer's thoughts to consider. Also, the new text (in blue) is not as well written, with some false cognates (e.g., "evacuate" line 103 is not a good synonym for "radiate").

Let us get what annoys me out of the way first. The rebuttal says that the revised ms now addresses work by Abe and Matsui, and they cite some papers from 1986. However, this is not what Rev 1 asked. I quote from the review: "The other essential papers are by Abe & Matsui, especially the towering 1988 study in J. Atmos. Sci. vol 45, pp 3081-3101." The 1988 paper is still not cited. It is the one that really matters. The authors obviously have no idea what is in it or how much more deeply Abe developed his ideas. THIS is the paper they need to read and understand and discuss and compare to, because this is the paper most responsible for establishing the paradigm. This is a field with only a handful of original papers. Abe and Matsui 1988 is one of them. I cannot recommend this manuscript for publication until this failure of scholarship is rectified.

Ok, now back to what the authors have been working on.
This reviewer had not realized that the Venus standard model features a magma ocean that lasts for hundreds of millions of years. That is certainly not what I had thought - escape alone is fast enough to limit the magma ocean phase to be contemporaneous with and responsive to "giant" impacts during the last phase of accretion. In any event, these different ideas illustrate my point: when the authors say "these findings significantly change our views of Venus’ past..." [emphasis added] they are literally referring to themselves and people they talk to. I.e., they are trying to negate a social construct. Rather than vaguely negating a nebulous idea, a better approach would be positive and specific: state explicitly what the new work is claiming - e.g., magma ocean episodes on Venus were shorter than 2 or 3 million years, if that is the point to be made. I think in general it is always better in an abstract to say what you did find, rather than what someone else didn't.

I think the authors say what they want to say much more clearly in the main text in lines 135-136: "For Venus, in particular, the surface does not require the steam atmosphere to escape to solidify, as was envisioned before[4, 5]." A more direct phrasing: "For Venus, in particular, the surface can solidify before the steam atmosphere escapes, which is opposite to previous work [4, 5]." I am not sure that the claim is true, but it could be true, and in any event this is the sort of positively stated result that would fit well in the abstract.

I don't understand what a "moist adiabat" means when there is only H2O. It would seem to me the air is either dry or saturated.
I must remind myself that the saturation vapor pressure curve is an isentrope only when all the

condensate remains in the parcel. In an atmosphere the condensate falls out and lifting is not adiabatic. In this model all the condensate falls out instantly. In these atmospheres the falling condensate evaporates again before it falls very far. How the evaporating rain adjustment works is described only vaguely. Would it be possible to describe exactly what is done? There is considerable energy involved in evaporating rain. This would seem likely to have a considerable effect on the convective adjustment. I suspect that how rainfall is parameterized would have a visible effect on the p-T profiles.

In a 1-D model these things must be parameterized arbitrarily. In principle a perfect GCM would correctly compute precipitation, evaporation, etc. In practice I suspect that GCMs use similar arbitary assumptions. If the GCM is in fact handling the cloud base significantly better than the 1-D model, it would be good to know. Otherwise the comparison with GCM still seems rather pointless. (the authors like it and it is their paper.)

I should point out that IUPAC H2O tabulates c_p for vapor along the saturation vapor pressure curve, should there be any desire to do c_p self-consistently.

note in passing: Ray mentions in re to Venus in his review that maybe radiative diffusion is more effective with a much hotter (magma ocean) surface, but one must point out that the surface in this paper is not much hotter - Selsis et al are obtaining radiative solutions with rather low surface temperatures. I am still open to the idea that Venus provides a real world example of what actually happens.

Summary of review: The main point still stands: no one seems to have asked the question before, hence this is novel work, and technically the work looks good enough. I'd prefer another revision. I would like more clarity in the treatment of condensation, rainfall, evaporation, and "moist adiabat." I would also require that the authors read Abe & Matsui 1988 JAS, so that they can accurately discuss how their new work differs from the paper that established the paradigm.

Thirty years ago, JAS would have been the right place for this paper, where the technical issues could have taken precedence over big picture consequences, but the JAS communuty seems no longer interested in general questions. Nature seems to me the best place for opening the discussion, and it would help (and make Ray happy) if you accepted his offer to raise the profile with a N&V piece.

Referee #2 (Remarks to the Author):

The authors have done a diligent job of considering my comments. In addition Reviewer 1 brought up a number of important points I didn't treat, notably with regard to the consequences for magma oceans, and the importance of the work of Matsui and Abe, as well as the important role of other opacity sources. It is true that the original draft over-reached with regard to the magma ocean issue, and could have been read to imply the conclusions in Matsui and Abe (and follow-ons) with regard to magma oceans were invalid. In reality, the new results only apply to the case of instellation-created magma oceans such as discussed by Hamano. Reviewer 1 correctly points out that the picture Selsis et al is overturning is not everybody's picture -- it is a picture that I think primarily stems from Hamano. (Certainly it was a picture I had of magma oceans, but I mainly picked that up from Hamano.) The revision goes pretty far towards rectifying the confusion regarding magma oceans, though I still think there is room to make the language more cautious. In the response to referees, the authors claim that their calculations imply a shorter lifetime of primordial magma oceans, but they haven't done the calculation and so at this point that conclusion is unwarranted. Conceivably, with the high flux of a magma ocean maintaining convection, the hot state could be self-maintaining for a long time. The calculation of time evolution (as the authors acknowledge) needs to be done using the author's new accelerated time stepping for the atmosphere, though it's unclear if that would be worthwhile until better opacity data is available.

The biggest strength of this paper is the development and use of a novel accelerated convergence scheme that makes 1D full radiative-convective calculations feasible for optically thick atmospheres. This will be of widespread use, though progress will still be limited until there are better measurements of opacity at high temperatures and pressures. The weakness is that the conclusions still rely on water vapour continuum opacity extrapolated for many orders of magnitude from the Earthlike range for which they have been verified. Despite their checks, I don't think the authors fully appreciate the limitations of MTCKD when pushed past Earthlike conditions. MTCKD is based on summing up empirical far tails of water vapour lines, and there is no theoretical basis for extending these empirical tails beyond the verification range. Further, as noted by MTCKD authors, at higher pressures/temperatures new physics starts to take hold, such as dimer formation, that is not at all captured by the MTCKD approach. Add to that the issues raised by Reviewer 1 about additional opacity sources (especially CO2), and the fact that we know nothing about the CO2-H2O continuum at high temperature/pressure, and the uncertainties mount.

I am still in favour of publication, but while the authors have added some more caveats and highlighted the opacity uncertainties, the language they use throughout still conveys far more certainty about their conclusions than is warranted given the current state of affairs. The wording of the abstract is somewhat improved, but, for example, still states "we show that this widely-adopted hypothesis is generally unfounded..." That goes way beyond what is justified. First of all, there are still circumstances where the convective assumption is justified (though it should be checked more often than is typically the case), and secondly the most that can be stated is that the assumption is not supported for thick pure steam atmospheres based on limited water vapour opacity data commonly in use. I'd like to see a prominent acknowledgement in the abstract regarding the lack of suitable opacity data (This qualification applies to all of the conclusions stated in the Abstract.) There are other places in the text where more cautious language would be warranted.

Regarding a few lesser points:

*Around line 293 the authors point out the limited temperature range for experimental study of the water vapour continuum, but a bigger point is the low (Earthlike) partial pressures of water for which the continuum has been studied.

* Reviewer 1 raised the issue of the nonideal equation of state somewhat more emphatically than I did, based in part on the results in Matsui and Abe. The authors in their response in essence did an end-run around the issue, and I don't think they tackled it head-on. I don't find their discussion of this issue especially convincing. I am certainly not suggesting they should re-do their calculations with a non-ideal EOS. That would be hard and time consuming. Still, more caveats regarding possible shortcomings of their calculation would be in order. I think the authors are right to be more worried about opacity than non-idea EOS issues.

*I still don't see why the authors can't do a proper job of incorporating cp(T) in the 1D calculation. If one is not doing a non-ideal EOS, it's just a matter of the enthalpy being a function h(T) easily tabulated instead of cp*T. I know these authors are not by any means lazy -- they are some of the most industrious people I know -- but their avoidance of a proper cp(T) comes off as lazy to the uninformed reader, and requires a lot of pointless messing around checking different values of cp, etc. I don't think it will change the results, but the paper would be a lot cleaner if they just did this properly.

*I still feel very strongly that the 3D results do not belong in this paper, and so would support an editorial decision to leave them out. I do not generally like complex 3D results just laid out in the short Nature format without a proper discussion of what is going on. The 1D calculation amply makes the author's point, and I just don't see any value added by the very limited discussion of the 3D case. that can fit into this format. The Turbet et al Nature paper on Venus ocean condensation was an exemplary model of deriving physical insight from a 3D GCM within the short Nature format, but the 3D discussion here is just lines 567 to 625, and is far too short to make the results compelling.

Referee #3 (Remarks to the Author):

I’ve carefully read the latest paper version and the first round of referee responses. The key issue the paper addresses is the question: how hot can the surface of a rocky planet with a steam atmosphere get due to stellar heating? The conclusion is that it is much less than has been estimated previously, and in general not enough to allow magma oceans for a wide range of cases.

This is a very interesting paper with major implications if the result turns out to be robust. The previous referees did a good job of summarizing most of the main issues and the authors have already spent time responding, so I’ll aim to keep my comments relatively brief here.

My most major comment is that the authors are not the first to find that shortwave absorption in the lower atmosphere can alter convection on planets with H2O-rich atmospheres. We did some 1D modeling back in 2013 where we found temperature inversions appearing in the lower atmospheres of planets with CO2-H2O dominated composition due to this effect (Wordsworth & Pierrehumbert, 2013). We were unclear about the significance of this result because our 1D model (like all 1D models) couldn’t do a good job of convection and precipitation, so we were cautious about how we discussed it in the text. Later, Wolf and Toon 2015 found the same effect in a 3D GCM, which was particularly interesting to us, because it suggested the phenomenon might be robust. However, just last year, we published a follow-up to this (Seeley & Wordsworth, 2021) that changed our view of this problem again. Using a 3D convection-resolving model coupled to line-by-line radiative transfer, we found that when shortwave absorption due to H_2O becomes high (starting at about 320 K), convection starts to become highly episodic, but on average the temperature profile still ends up following a moist adiabat (see e.g. Extended Data Figure 4 of Seeley & Wordsworth 2021). The evaporative cooling of falling condensate was particularly important to this result.

I think the authors need to discuss their work in the context of these previous results, and in particular the results from the convection-resolving model. Does their 1D model also predict lower atmosphere radiative zones in the T_{surf} ~ 330 K regime? If it does, are they confident that using a convection-resolving model in the high temperature regime that they are studying wouldn't also change their main conclusions there?

Other comments:

I agree with the authors that the difference between real gas and ideal gas adiabats is likely not critical here.

I didn't understand why pure H_2O was used in the main simulations. If there's one thing we can be confident about regarding steam atmospheres, it's that they won't just contain steam. Lupu et al. 2014 contains a good discussion of this. I don't think the authors need to add 10+ minor species to their simulations, but they should at least discuss how their results might change if they were to add some of the obvious ones, such as CO_2 and SO_2.

I think the main text should contain some discussion of the similarities and differences between the atmospheres modeled here and convection in hot Jupiters and gas giants. There doesn't need to be paragraphs of text devoted to it, but a sentence or two would place this work in much better context.

Some of the albedos in Figure 1 are tiny. This makes sense, because the authors are running clear sky simulations, but real atmospheres are not clear sky. Clouds and hazes also have important IR effects. Some discussion of how the main results will change when clouds are present is needed.

L182 'decouples the water reservoirs' This really depends on the tectonic regime of the planet. 'Weakens the coupling' would be more appropriate here I think.

L599: Minor gripe: I'm not a fan of the phrase 'excellent agreement' in data intercomparisons and suggest the authors use the more neutral 'close agreement' instead.

Robin Wordsworth

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Wolf, E.T. and Toon, O.B., 2015. The evolution of habitable climates under the brightening Sun. Journal of Geophysical Research: Atmospheres, 120(12), pp.5775-5794.

Wordsworth, R.D. and Pierrehumbert, R.T., 2013. Water loss from terrestrial planets with CO2-rich atmospheres. The Astrophysical Journal, 778(2), p.154.

Referee #4 (Remarks to the Author):

The focus of this review of “A cool runaway greenhouse without surface magma ocean” is on the study’s treatment of H2O opacities. Much of the detailed relevant information about the authors’ methodology and thoughts about H2O opacities appears in their rebuttal to a previous review rather than in the manuscript itself. I believe that constitutes a problem for the manuscript, but first comments on the text in the rebuttal are warranted.

Overall, the approach to H2O opacities detailed in the rebuttal show a good understanding of the issues involved and the deficiencies in the HITRAN line list and MT_CKD continuum at high temperatures. The sensitivity tests the authors present in the rebuttal indicate that the qualitative perspective in this study remains unchanged despite the large changes in H2O opacities in these tests. Fundamentally, the authors have shown that the headline conclusions in their paper are not affected by any flaws in their H2O opacities.

There are some issues with what has been written in the rebuttal. The main issue is that I do not agree with the authors’ thought that there is a significant consistency issue using the MT_CKD continuum with HITEMP lines. Most likely the key spectral regions that are most relevant to this analysis are the windows between H2O absorption bands (rather than the bands themselves). In IR and almost all near-IR windows, MT_CKD absorption coefficients come from observations at or near room temperature. The additional lines in HITEMP would be irrelevant in analyzing those observations, so those coefficients should be fine. The temperature dependence in MT_CKD in these spectral regions is wrong, but that is not related to which line list was used in the analysis. Therefore, it would be better for the authors to use HITEMP rather than HITRAN for the infrared in the analysis they present in the manuscript. Second, a recent change to MT_CKD (v3.6) addressed the main reason for its issue related to temperature dependence, which was the mathematical expression used in previous versions (leads to the curvature of the MT_CKD curves shown in the figure in

Lechevallier et al.). The authors might want to consider using this released version of MT_CKD rather than creating their own hybrid version.

To sum up, the optimal H2O spectroscopic parameters for this study are HITEMP and MT_CKD_3.6. This combination would be far from perfect given the lack of high T and P measurements that might improve both data sets, but it should be more accurate than what is presented in the manuscript.

As for what is presented in the manuscript, I do see a significant issue with the current approach. The targeted audience is likely to think along the same lines as the previous reviewer who raised the H2O opacity issue and suspect that the main conclusions of this study might evaporate with a reasonable change to what is specified in lines 552-557. Readers with informed knowledge of H2O spectroscopy might also justifiably question the use of MT_CKD_3.5 and of HITRAN in the infrared (as I do above), both of which are known to have significant issues at temperatures of relevance to this study. The text as currently written could put a cloud over how this manuscript's result is viewed, and it would be an unnecessary cloud given the sensitivity analysis the authors present in the rebuttal. I do not believe the manuscript should be published with the current text.

The best choice would be for the authors to redo their analysis using HITEMP and MT_CKD_3.6, use these runs throughout the paper. The authors should then also add a sentence in the Simulations section that describes the sensitivity test that was performed (i.e. change to HITRAN in the infrared and MT_CKD_3.5) and how it didn't change the qualitative conclusions of the study. A second choice would be to present as the main results the runs using the revised opacity choices described in the rebuttal (HITEMP and the MT_CKD_3.5/CAVIAR hybrid), and after that information is the Simulations section there should be a sentence that describes the sensitivity test that was performed (i.e. change to HITRAN in the infrared and MT_CKD_3.5) and how it didn't change the qualitative conclusions of the study. The last choice that should be considered, and one that is clearly worse than the other two, is to not change the results presented in the paper (i.e. continue to use HITRAN in the infrared and MT_CKD_3.5), but then add a sentence after line 557 that describes the sensitivity test that was performed (i.e. HITEMP and the MT_CKD_3.5/CAVIAR hybrid) and how it didn't change the qualitative conclusions of the study. This last choice would be acceptable, just not as good as the others.

Specific issue:
Line 554 – "CKD" is listed instead of "MT_CKD"

Author Rebuttals to First Revision:

Response to the reviewers

and comments on the revised manuscript

We apologize for this late resubmission. The first author, Franck Selsis, has been in hospital since October 27th for multiple fractures after being hit by a car while crossing a street. He has been unable to work until recently and has then been busy with his reeducation.

The main change in the present version is that we re-computed all profiles using the latest version of MT_CKD as suggested by the 4th reviewer. The profiles are slightly different without changing our previous conclusions. We also took into account the reviewers' comments on the writing. We now emphasize more on the opacity uncertainties, including in the abstract and the conclusion. We are also more prudent in our conclusions regarding early post-accretion atmospheres that must have consisted in more complex mixtures than pure steam atmospheres and will require dedicated studies.

Reviewer 1

Overall, as is typically the case with a resubmission, I like it little less than I did the first time because I've had more time to think about and I've had the other reviewer's thoughts to consider. Also, the new text (in blue) is not as well written, with some false cognates (e.g., "evacuate" line 103 is not a good synonym for "radiate").

Let us get what annoys me out of the way first. The rebuttal says that the revised ms now addresses work by Abe and Matsui, and they cite some papers from 1986. However, this is not what Rev 1 asked. I quote from the review: "The other essential papers are by Abe & Matsui, especially the towering 1988 study in J. Atmos. Sci. vol 45, pp 3081-3101." The 1988 paper is still not cited. It is the one that really matters. The authors obviously have no idea what is in it or how much more deeply Abe developed his ideas. THIS is the paper they need to read and understand and discuss and compare to, because this is the paper most responsible for establishing the paradigm. This is a field with only a handful of original papers. Abe and Matsui 1988 is one of them. I cannot recommend this manuscript for publication until this failure of scholarship is rectified.

We apologize for this fault. We now cite this specific paper, which we should have cited in the first place. We want to reassure the reviewer by saying that the work of Abe and his group has been important for us in both our training as students and our research. Our mistake was to pick papers that were not the most relevant for the present study. This is because his work is developed in many key papers of that period while we somehow built a synthetic view of his contributions. It's indeed in Abe and Matsui 1988 that, to our knowledge, was developed the first consistent radiative-convective model for such dense atmospheres and it was the relevant step to cite. The manuscript now reads: "It is therefore necessary to self-consistently calculate the atmospheric thermal profile, without imposing convection, following the tracks of pioneering works on runaway atmospheres^{Watson et al. 1984} and formation/condensation of post-accretion atmospheres^{Abe & Matsui, 1988}."

Ok, now back to what the authors have been working on.

This reviewer had not realized that the Venus standard model features a magma ocean that lasts for hundreds of millions of years. That is certainly not what I had thought - escape alone is fast enough to limit the magma ocean phase to be contemporaneous with and responsive to "giant" impacts during the last phase of accretion. In any event, these different ideas illustrate my point: when the authors say "these findings significantly change our views of Venus' past..." [emphasis added] they are literally referring to themselves and people they talk to. I.e., they are trying to negate a social construct. Rather than vaguely negating a nebulous idea, a better approach would be positive and specific: state explicitly what the new work is claiming - e.g., magma ocean episodes on Venus were shorter than 2 or 3 million years, if that is the point to be made. I think in general it is always better in an abstract to say what you did find, rather than what someone else didn't.

In the current version we give quantitative values for the internal heat flux required to sustain magma ocean temperatures on early Venus for a steam atmosphere and as a function of surface pressure. The fact that a significant internal heat flux is needed to keep the surface melted differs, we believe, from the standard scenario.

I think the authors say what they want to say much more clearly in the main text in lines 135-136: "For Venus, in particular, the surface does not require the steam atmosphere to escape to solidify, as was envisioned before[4, 5]." A more direct phrasing: "For Venus, in particular, the surface can solidify before the steam atmosphere escapes, which is opposite to previous work [4, 5]." I am not sure that the claim is true, but it could be true, and in any event this is the sort of positively stated result that would fit well in the abstract.

Thanks, we have used the suggested sentence.

I don't understand what a "moist adiabat" means when there is only H2O. It would seem to me the air is either dry or saturated.

It's either dry or saturated whether H_2O is the only constituent or not. We used "moist adiabat" to specify when the atmosphere is saturated, in contrast to dry regions. But as "moist adiabat" and "saturated adiabat" are equivalent, we now avoid the use of "moist adiabat" whenever a pure H_2O atmosphere is considered.

I must remind myself that the saturation vapor pressure curve is an isentrope only when all the condensate remains in the parcel. In an atmosphere the condensate falls out and lifting is not adiabatic. In this model all the condensate falls out instantly. In these atmospheres the falling condensate evaporates again before it falls very far. How the evaporating rain adjustment works is described only vaguely. Would it be possible to describe exactly what is done? There is considerable energy involved in evaporating rain. This would seem likely to have a considerable effect on the convective adjustment. I suspect that how rainfall is parameterized would have a visible effect on the p-T profiles.

The procedure is explained in the Methods and we describe the effect of rain re-evaporation on the p-T profiles: it creates a small convection region below the saturated layers. The exact treatment can indeed change slightly the profiles, but does not change qualitatively our results. The reviewer can all also read our answer to reviewer 3, that specifically deals with this point.

In a 1-D model these things must be parameterized arbitrarily. In principle a perfect GCM would correctly compute precipitation, evaporation, etc. In practice I suspect that GCMs use similar arbitrary assumptions. If the GCM is in fact handling the cloud base significantly better than the 1-D model, it would be good to know. Otherwise the comparison with GCM still seems rather pointless. (the authors like it and it is their paper.)

The GCM includes a microphysical model for condensation as well as a radiative model for clouds and resolves the large-scale circulation that controls the cloud distribution in such steam atmospheres (see Turbet et al. 2021). This is not comparable to the simple treatment in the 1D model. As we explain in the paper, the 3D/1D comparison is important because 3D shows that clouds do not form on the dayside, which was already shown for a Sun-like spectrum but not for red dwarfs. It also shows that, nevertheless, the p-T profile on the dayside sticks to the moist convective gradient, due to homogenization by circulation with the nightside, where condensation does occur. These two points make the use of the 1D model legitimate enough. It was clearly not granted at the start.

I should point out that IUPAC H2O tabulates c_p for vapor along the saturation vapor pressure curve, should there be any desire to do c_p self-consistently.

Thanks, we use NASA polynomials that are consistent with IUPAC tabulated data.

note in passing: Ray mentions in re to Venus in his review that maybe radiative diffusion is more effective with a much hotter (magma ocean) surface, but one must point out that the surface in this paper is not much hotter - Selsis et al are obtaining radiative solutions with rather low surface temperatures. I am still open to the idea that Venus provides a real world example of what actually happens.

It's a quantitative question: opacity and temperature determine the gradient necessary to radiate a given flux. We calculated (as shown in our previous exchanges) that on Venus, convection becomes marginal at the surface. It would be replaced by radiative layers for a slightly redder spectrum or a slightly higher surface pressure.

Summary of review: The main point still stands: no one seems to have asked the question before, hence this is novel work, and technically the work looks good enough. I'd prefer another revision. I would like more clarity in the treatment of condensation, rainfall, evaporation, and "moist adiabat." I would also require that the authors read Abe & Matsui 1988 JAS, so that they can accurately discuss how their new work differs from the paper that established the paradigm.

Thirty years ago, JAS would have been the right place for this paper, where the technical issues could have taken precedence over big picture consequences, but the JAS community seems no longer interested in general questions. Nature seems to me the best place for opening the discussion, and it would help (and make Ray happy) if you accepted his offer to raise the profile with a N&V piece.

Reviewer 2

The authors have done a diligent job of considering my comments. In addition Reviewer 1 brought up a number of important points I didn't treat, notably with regard to the consequences for magma oceans, and the importance of the work of Matsui and Abe, as well as the important role of other opacity sources. It is true that the original draft over-reached with regard to the magma ocean issue, and could have been read to imply the conclusions in Matsui and Abe (and follow-ons) with regard to magma oceans were invalid. In reality, the new results only apply to the case of instellation-created magma oceans such as discussed by Hamano. Reviewer 1 correctly points out that the picture Selsis et al is overturning is not everybody's picture -- it is a picture that I think primarily stems from Hamano. (Certainly it was a picture I had of magma oceans, but I mainly picked that up from Hamano.) The revision goes pretty far towards rectifying the confusion regarding magma oceans, though I still think there is room to make the language more cautious. In the response to referees, the authors claim that their calculations imply a shorter lifetime of primordial magma oceans, but they haven't done the calculation and so at this point that conclusion is unwarranted. Conceivably, with the high flux of a magma ocean maintaining convection, the hot state could be self-maintaining for a long time. The calculation of time evolution (as the authors acknowledge) needs to be done using the author's new accelerated time stepping for the atmosphere, though it's unclear if that would be worthwhile until better opacity data is available.

The biggest strength of this paper is the development and use of a novel accelerated convergence scheme that makes 1D full radiative-convective calculations feasible for optically thick atmospheres. This will be of widespread use, though progress will still be limited until there are better measurements of opacity at high temperatures and pressures. The weakness is that the conclusions still rely on water vapour continuum opacity extrapolated for many orders of magnitude from the Earthlike range for which they have been verified. Despite their checks, I don't think the authors fully appreciate the limitations of MTCKD when pushed past Earthlike conditions. MTCKD is based on summing up empirical far tails of water vapour lines, and there is no theoretical basis for extending these empirical tails beyond the verification range. Further, as noted by MTCKD authors, at higher pressures/temperatures new physics starts to take hold, such as dimer formation, that is not at all captured by the MTCKD approach. Add to that the issues raised by Reviewer 1 about additional opacity sources (especially CO2), and the fact that we know nothing about the CO2-H2O continuum at high temperature/pressure, and the uncertainties mount.

I am still in favour of publication, but while the authors have added some more caveats and highlighted the opacity uncertainties, the language they use throughout still conveys far more certainty about their conclusions than is warranted given the current state of affairs. The wording of the abstract is somewhat improved, but, for example, still states "we show that this widely-adopted hypothesis is generally unfounded..." That goes way beyond what is justified. First of all, there are still circumstances where the convective assumption is justified (though it should be checked more often than is typically the case), and secondly the most that can be stated is that the assumption is not supported for thick pure steam atmospheres based on limited water vapour opacity data commonly in use. I'd like to see a prominent acknowledgement in the abstract regarding the lack of suitable opacity data (This qualification applies to all of the conclusions stated in the Abstract.) There are other places in the text where more cautious language would be warranted.

We do agree that an important conclusion of the work is that thermal profiles do depend on opacities and internal heat fluxes when they are not fully convective. Which means that they depend on poorly constrained data (opacities) and on a value that we do not possess in most cases (the internal heat flow). This indeed deserved to be included in the abstract which now reads: ”While assumed adiabatic structures do not depend on high-pressure opacities, radiative-convective profiles are sensitive to their poorly-constrained values, making new measurements of these data necessary for a broad field of (exo)planetary science.”

About "we show that this widely-adopted hypothesis is generally unfounded..." The hypothesis here is that the convective region starts from the surface, includes the photosphere and reaches saturated levels. This is what allows the *inverse climate modeling* that is used in a really wide literature. We insist that we do not find such full convection happening in general. We find large convective regions, for instance with high internal heat fluxes but we did not find a full convection from surface to photosphere for ranges of instellation and internal heat flows spanning several orders of magnitude. And the (poorly known) high-P opacities are not the only reason for that (which is why our results stand while changing them by orders of magnitude) : it is also due to opacities at the photosphere, so around 10 mbar, and to a strong temperature feedback that makes radiative diffusion very efficient to transport heat upward.

Also, what we say about the duration of the magma ocean is simply that once the internal heat flux drops below a certain value (that depends on the surface pressure), the magma ocean solidifies even if the instellation is above the runaway greenhouse. It is not prolonged by the sole greenhouse effect. In the Hamano model, it persists as long as the H_2O greenhouse effect remains (until it eventually disappears through the photolysis of H_2O and escape of H to space). Further calculations coupling the interior and the atmosphere won’t change that but are indeed needed to determine how long it takes for the heat flow to drop below this solidification value and to quantify the effect of other atmospheric constituents.

Regarding a few lesser points:

*Around line 293 the authors point out the limited temperature range for experimental study of the water vapour continuum, but a bigger point is the low (Earthlike) partial pressures of water for which the continuum has been studied.

Indeed. Corrected.

* Reviewer 1 raised the issue of the non-ideal equation of state somewhat more emphatically than I did, based in part on the results in Matsui and Abe. The authors in their response in essence did an end-run around the issue, and I don't think they tackled it head-on. I don't find their discussion of this issue especially convincing. I am certainly not suggesting they should re-do their calculations with a non-ideal EOS.

That would be hard and time consuming. Still, more caveats regarding possible shortcomings of their calculation would be in order. I think the authors are right to be more worried about opacity than non-idea EOS issues.

*I still don't see why the authors can't do a proper job of incorporating cp(T) in the 1D calculation. If one is not doing a non-ideal EOS, it's just a matter of the enthalpy being a function h(T) easily tabulated instead of cp*T. I know these authors are not by any means lazy -- they are some of the most industrious people I know -- but their avoidance of a proper cp(T) comes off as lazy to the uninformed reader, and requires a lot of pointless messing around checking different values of cp, etc. I don't think it will change the results, but the paper would be a lot cleaner if they just did this properly.

We would love to have found a way to include cp(T) in a time marching model! This would be useful to a large community working with a constant cp. As the reviewer says, that would have saved us messing around with cp to check that we were not off the tracks. But, to the best of our knowledge, the problem remains to be solved for time marching models. Using a constant cp is done in many publications, despite temperatures spanning large domains. These publications, dedicated to either hot exoplanets or Venus, rarely discuss the implication of this approximation, as we do. We hope to find a way to tackle this issue properly in future works.

*I still feel very strongly that the 3D results do not belong in this paper, and so would support an editorial decision to leave them out. I do not generally like complex 3D results just laid out in the short Nature format without a proper discussion of what is going on. The 1D calculation amply makes the author's point, and I just don't see any value added by the very limited discussion of the 3D case. that can fit into this format. The Turbet et al Nature paper on Venus ocean condensation was an exemplary model of deriving physical insight from a 3D GCM within the short Nature format, but the 3D discussion here is just lines 567 to 625, and is far too short to make the results compelling.

We strongly believe that these 3D results are necessary to justify the use of a 1D model. Questions by Reviewer 3 about the role of clouds and circulation demonstrate it. Beyond the fact that this 1D/3D comparison is a kind of validation for the evolution module of exo-k, it is important to show that circulation keeps the upper atmosphere to a moist adiabat despite the absence of condensation on the dayside. Clouds on the dayside, dry convection where we assume saturation, too high a nightside greenhouse warming by clouds could have made a 1D model unfit for this study. This 1D/3D comparison is actually at the roots of this work. It made us confident that we could use a 1D model for steam atmospheres. We agree that 3D modeling is so complex that presenting GCM results in such a short format could be problematic. However, there is already a Nature paper describing the 3D simulations we use here: the only difference from Turbet et al. 2021, cited by the reviewer as exemplary, is the stellar spectrum and the synchronous rotation. So all the technical details can be found there. In addition, all the effects that are important for us (no clouds on the dayside, a moist adiabat maintained by circulation, a moderate nightside greenhouse due to clouds) were explained in Turbet at al., but here we need to show that they remain towards the reddest stars.

Reviewer 3

I've carefully read the latest paper version and the first round of referee responses. The key issue the paper addresses is the question: how hot can the surface of a rocky planet with a steam atmosphere get due to stellar heating? The conclusion is that it is much less than has been estimated previously, and in general not enough to allow magma oceans for a wide range of cases.

This is a very interesting paper with major implications if the result turns out to be robust. The previous referees did a good job of summarizing most of the main issues and the authors have already spent time responding, so I'll aim to keep my comments relatively brief here.

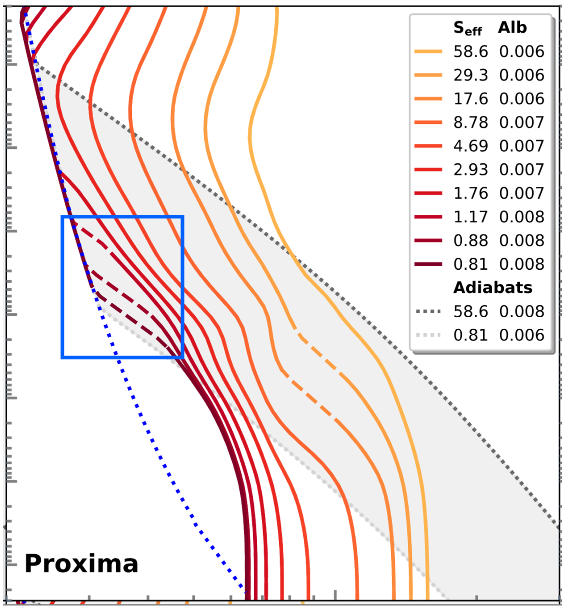
My most major comment is that the authors are not the first to find that shortwave absorption in the lower atmosphere can alter convection on planets with H2O-rich atmospheres. We did some 1D modeling back in 2013 where we found temperature inversions appearing in the lower atmospheres of planets with CO2-H2O dominated composition due to this effect d. We were unclear about the significance of this result because our 1D model (like all 1D models) couldn't do a good job of convection and precipitation, so we were cautious about how we discussed it in the text. Later, Wolf and Toon 2015 found the same effect in a 3D GCM, which was particularly interesting to us, because it suggested the phenomenon might be robust. However, just last year, we published a follow-up to this (Seeley & Wordsworth, 2021) that changed our view of this problem again. Using a 3D convection-resolving model coupled to line-by-line radiative transfer, we found that when shortwave absorption due to H2O becomes high (starting at about 320 K), convection starts to become highly episodic, but on average the temperature profile still ends up following a moist adiabat (see e.g. Extended Data Figure 4 of Seeley & Wordsworth 2021). The evaporative cooling of falling condensate was particularly important to this result.

I think the authors need to discuss their work in the context of these previous results, and in particular the results from the convection-resolving model. Does their 1D model also predict lower atmosphere radiative zones in the Tsurf ~ 330 K regime? If it does, are they confident that using a convection-resolving model in the high temperature regime that they are studying wouldn't also change their main conclusions there?

If all condensates are transported instantaneously to the surface, the cooling associated with their evaporation can indeed produce such near-surface thermal inversion. We obtained the kind of thermal inversion described by the reviewer in early tests of the code where rains were all released at the surface.

In the current version, condensates are released instantaneously but in several levels through the following process (described in Methods/Rains): from top to bottom, H_2O (or any condensing species) found in excess relative to saturation is collected. In this downward process, condensates are released as vapor when an unsaturated layer is met. If the temperature in the unsaturated layer is below the boiling temperature, the layer receives as much vapor as possible from the falling condensates, the maximum being set to a fraction ε (a parameter of the model) of the amount required to bring the layer to saturation.

When rainfalls enter a layer that is hotter than the boiling temperature, all the remaining precipitations are evaporated. We believe this is very physical as boiling raindrops evaporate very rapidly in this regime. In the cases studied here, this level is found well above the surface and the evaporation region is restricted to a small region below the saturated layers. The resulting evaporation cooling triggers convection by steepening the thermal gradient (as seen in the following figure in the blue rectangle). Our conclusions are not affected by these effects as the results show little sensitivity to the arbitrary ε evaporation efficiency. Surely, a high resolution model resolving convection would find temporal and spatial variations in these layers, with dry and rainy regions. We added a discussion about this subject where we now cite Seeley and Wordsworth (2021) in the paragraph related to condensation.



Extract from Sup. Fig. 5 in the manuscript. The blue rectangle shows where dry convection is triggered by rain re-evaporation.

Other comments:

I agree with the authors that the difference between real gas and ideal gas adiabats is likely not critical here.

Thanks. We will work on improving the code on this aspect, which is not trivial due to the way the code is written to allow quick convergence.

I didn't understand why pure H2O was used in the main simulations. If there's one thing we can be confident about regarding steam atmospheres, it's that they won't just contain steam. Lupu et al. 2014 contains a good discussion of this. I don't think the authors need to add 10+ minor species to their simulations, but they should at least discuss how their results might change if they were to add some of the obvious ones, such as CO2 and SO2.

We do agree that real atmospheres are unlikely to only consist of water. This issue is now extensively discussed in section 1.9 of the manuscript. We chose to focus this paper on the pure steam case for several reasons:

- It is the reference case of Kasting et al. 1988 and 1993 and is the basis of many works. Revisiting it is an important step.
- The atmosphere resulting from the vaporization of an ocean by the increase of stellar luminosity is naturally treated as an atmosphere dominated by H_2O. And what happens next to its composition depends,

among other things, on whether the surface is melted or not. As it represents the future of our planet, it's a significant case.

- Mass-radius relationships for rocky planets surrounded by a pure water vapor atmosphere are commonly used to bracket the water content of exoplanets. These relationships are drastically affected by our results.
- The post-accretion composition of atmosphere is not at all well constrained for the Earth or Venus and is expected to vary from one planet to another. See for instance Sossi et al., 2023 for the range of H_2O, CO_2, CO and H_2 abundances. And that's only for the major constituents. Absorbing minor species can affect the thermal structure: we showed in the previous answers that reasonably reproducing Venus profile, for instance, requires at least 5 absorbants. We decided to show, in one archetypal case, that thermal profiles must be computed without assuming full convection, before exploring a compositional parameter space with several dimensions.
- In addition, adding species makes the question of opacity uncertainties much more difficult to treat with sensitivity studies. Extrapolating the comments we had for a pure H_2O atmosphere to a mixture of H_2O, CO_2, CO, SO_2, etc (+ foreign broadening) would make the study impossible to fit in a single article and within a reasonable time frame. It's a necessary work but that will require time and a collective effort from the community.

I think the main text should contain some discussion of the similarities and differences between the atmospheres modeled here and convection in hot Jupiters and gas giants. There doesn't need to be paragraphs of text devoted to it, but a sentence or two would place this work in much better context.

The comparison is indeed very relevant. We added a sentence on the subject and a reference when describing the processes controlling the thermal structure.

Some of the albedos in Figure 1 are tiny. This makes sense, because the authors are running clear sky simulations, but real atmospheres are not clear sky. Clouds and hazes also have important IR effects. Some discussion of how the main results will change when clouds are present is needed.

We do discuss this point. 3D simulations show a cloudless dayside. Turbet et al. (2021) showed this for a Sun-like star and we did new simulations for red dwarfs to compare with the 1D runs. They confirm the behavior seen previously. The effect of clouds is therefore restricted to the greenhouse warming on the night side, making 3D runs a bit hotter.

L182 'decouples the water reservoirs' This really depends on the tectonic regime of the planet. 'Weakens the coupling' would be more appropriate here I think.

Agreed.

L599: Minor gripe: I'm not a fan of the phrase 'excellent agreement' in data intercomparisons and suggest the authors use the more neutral 'close agreement' instead.

We are in excellent agreement with the reviewer here and have changed to "close".

Robin Wordsworth

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Reviewer 4

The focus of this review of "A cool runaway greenhouse without surface magma ocean" is on the study's treatment of H2O opacities. Much of the detailed relevant information about the authors' methodology and thoughts about H2O opacities appears in their rebuttal to a previous review rather than in the manuscript itself. I believe that constitutes a problem for the manuscript, but first comments on the text in the rebuttal are warranted.

Overall, the approach to H2O opacities detailed in the rebuttal show a good understanding of the issues involved and the deficiencies in the HITRAN line list and MT_CKD continuum at high temperatures. The sensitivity tests the authors present in the rebuttal indicate that the qualitative perspective in this study remains unchanged despite the large changes in H2O opacities in these tests. Fundamentally, the authors have shown that the headline conclusions in their paper are not affected by any flaws in their H2O opacities.

There are some issues with what has been written in the rebuttal. The main issue is that I do not agree with the authors' thought that there is a significant consistency issue using the MT_CKD continuum with HITEMP lines. Most likely the key spectral regions that are most relevant to this analysis are the windows between H2O absorption bands (rather than the bands themselves). In IR and almost all near-IR windows, MT_CKD absorption coefficients come from observations at or near room temperature. The additional lines in HITEMP would be irrelevant in analyzing those observations, so those coefficients should be fine. The temperature dependence in MT_CKD in these spectral regions is wrong, but that is not related to which line list was used in the analysis. Therefore, it would be better for the authors to use HITEMP rather than HITRAN for the infrared in the analysis they present in the manuscript.

Second, a recent change to MT_CKD (v3.6) addressed the main reason for its issue related to temperature dependence, which was the mathematical expression used in previous versions (leads to the curvature of the MT_CKD curves shown in the figure in Lechevallier et al.). The authors might want to consider using this released version of MT_CKD rather than creating their own hybrid version.

To sum up, the optimal H2O spectroscopic parameters for this study are HITEMP and MT_CKD_3.6. This combination would be far from perfect given the lack of high T and P measurements that might improve both data sets, but it should be more accurate than what is presented in the manuscript.

As for what is presented in the manuscript, I do see a significant issue with the current approach. The targeted audience is likely to think along the same lines as the previous reviewer who raised the H2O opacity issue and suspect that the main conclusions of this study might evaporate with a reasonable change to what is specified in lines 552-557. Readers with informed knowledge of H2O spectroscopy might also justifiably question the use of MT_CKD_3.5 and of HITRAN in the infrared (as I do above), both of which are known to have significant issues at temperatures of relevance to this study. The text as currently written could put a cloud over how this manuscript's result is viewed, and it would be an unnecessary cloud given the sensitivity analysis the authors present in the rebuttal. I do not believe the manuscript should be published with the current text.

The best choice would be for the authors to redo their analysis using HITEMP and MT_CKD_3.6, use these runs throughout the paper. The authors should then also add a sentence in the Simulations section that describes the sensitivity test that was performed (i.e. change to HITRAN in the infrared and MT_CKD_3.5) and how it didn't change the qualitative conclusions of the study. A second choice would be to present as the main results the runs using the revised opacity choices described in the rebuttal (HITEMP and the MT_CKD_3.5/CAVIAR hybrid), and after that information is the Simulations section there should be a sentence that describes the sensitivity test that was performed (i.e. change to HITRAN in the infrared and MT_CKD_3.5) and how it didn't change the qualitative conclusions of the study. The last choice that should be considered, and one that is clearly worse than the other two, is to not change the results presented in the paper (i.e. continue to use HITRAN in the infrared and MT_CKD_3.5), but then add a sentence after line 557 that describes the sensitivity test that was performed (i.e. HITEMP and the MT_CKD_3.5/CAVIAR hybrid) and how it didn't change the qualitative conclusions of the study. This last choice would be acceptable, just not as good as the others.

We thank the reviewer for pointing out the new version of MT_CKD that matches better available data, solving the discrepancy found in Lechevallier et al. (2018). The results shown and discussed in the paper have all been recomputed with MT_CKD 4.0.1. (There was even another version released very recently but, fortunately, the changes concern only foreign broadening). This update yields slightly increased surface temperatures while not changing qualitatively our results. This new continuum provides temperatures that are a bit lower than the ones we had with our home-made CAVIAR-like continuum, showing that this test was indeed conservative. We took advantage of this update to add a short paragraph and a figure in the Methods to illustrate how an update in opacities impacts the results.

Sorry if we were not clear enough in our manuscript and answers but at high temperatures (>= 700K), we do use HITEMP at all wavelengths. It's only at lower temperatures AND in the infrared that we switch to HITRAN. Computing high-res spectra with HITEMP requires considerable CPU time so we use HITEMP only in the T-domain where differences with HITRAN have been shown to be significant, guided by comparisons performed for instance in *Goldblatt et al. 2013 Nature Geoscience Volume 6, Issue 8* (see Supplementary Materials). In addition, although differences are small with HITRAN at low temperatures, we lack some benchmark supporting the use of HITEMP vs HITRAN at low temperatures in the infrared, for instance simulations of present Earth climate, which probably implies a continuum parametrization that has been consistently determined for HITEMP.

Specific issue:

Line 554 – “CKD” is listed instead of “MT_CKD”

Corrected.

Reviewer Reports on the Second Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

I am satisfied by the authors' responses to my comments, and impressed by the authors' responses to the other reviewers.

minor quibble: I note in one response the authors state that they use pure H2O because Kasting used pure H2O in 1988, 1993 papers. I don't know what 1993 paper is being referred to, but the 1988 paper used a description of moist adiabats that required a nonzero amount of a noncondensible species (he usually used 3e-4 CO2 mixing ratio). So Kasting's atmospheres were only very nearly pure H2O.

Some suggestions for the Abstract: (i) Make clear that these are pure H2O models that neglect other sources of opacity. (ii) A style point: Don't interject "in fact" unless it is a fact. Here it just calls attention to the fact that what you are describing is your theory, and not (yet) a fact.

"...we find that steam atmospheres are in fact commonly..."
would be better written
"...we find that pure steam atmospheres are commonly..."

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"Venus surface..."
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For my part I will make no further suggestions. It's an inventive paper that asks important questions, using tools that seem pretty much state-of-the-art. Be it right or wrong, it shakes things up in a good way.

Referee #2 (Remarks to the Author):

First, let me say that I am very sorry to hear of Franck's accident, and send my best wishes for a swift and complete recovery.

I have no such excuse myself for being so delinquent in my re-review of this MS, and tempted though I am I won't attempt to hide my tardiness behind anonymity. I try to do my best on completing reviews, but things quickly became very busy upon my return from my US trip. I would have ordinarily expected the Editor to have given up on me long before this, but evidently my comments are wanted, and I have finally been able to make time to look over the revision and the response to the previous referee reports.

I'll reiterate that publication of this MS would be helpful to the field because of the possibilities that are raised, even if I continue to have questions about whether the conclusions will survive better models of the opacity data, including effects of additional sources of opacity (such as CO2, which could be very abundant once silicate weathering ceases). In terms of the general framing, I find one of the remarks by Reviewer 1 quite apt, namely that the authors are making an unwarranted assumption in presuming that it is "generally thought" (a paraphrase) that the runaway greenhouse would inevitably terminate in a magma ocean. Words to this effect still appear in the Abstract First of all, the abstract still doesn't include the qualification that insofar as there were claims of a magma ocean end-state, they only applied to cases with oceans comparable to Earth's ocean mass, not to significantly smaller water inventories. But that is a minor quibble. The main point regarding framing is that the authors are in no position to say what is generally thought in the community; they haven't taken a poll, and it would be pointless to do so. A lot of the planetary science community hasn't thought much about the end-state of runaway greenhouse atmospheres at all. A lot of the current focus on magma-ocean end states really only dates to the Hamano paper classifying magma oceans into Type I and Type II corresponding to the energy source creating them; I myself was in the magma ocean camp largely stimulated by Hamano, so that's at least two of us, but even I put in a caveat in the Boukrouche paper that the result was contingent on the atmosphere being convective all the way to the bottom (for all that I thought high opacity would be likely to assure that, and I still think that may turn out to be the case once the opacity estimates are clarified in future work.) This is not a big ask, but I do wish the authors would go through their manuscript and scrub out any presumptions about what has been generally thought regarding this issue. The paper will get plenty of attention anyway on the basis of the demonstration of circumstances in which runaway may not lead to a magma ocean, without the implication that this is overturning some established dogma. A more neutral way to rephrase the first two sentences of the abstract would be something like "Models of the end-state of a runway water vapour greenhouse which assume that the atmosphere has a convective layer reaching to the ground predict that a magma ocean will form, provided the water vapor inventory is massive enough." That tells the story in a matter-of-fact way without making unwarranted presumptions.

I still find some deficiencies in the general context provided for the problem. The situation is that deep radiative (and often isothermal layers) in atmospheres are expected on the basis of analytic grey-gas solutions dating back decades, when there is insufficient deep-atmosphere absorption of stellar shortwave, or insufficient internal heat flux. These have been studied quantitatively for real-gas radiative transfer for gas planet atmospheres (primarily hydrogen dominated). For a recent example, see Figs 3 and 9 in Piette & Madhu's paper on K2-18b; these are H2-dominated atmospheres, but Fig. 9 does include water vapor effects. For that matter, there have been prior solutions for pure steam atmospheres using time-stepped radiative convective models for post-runaway atmosphere, though these have been for subNeptunes not terrestrial planets (though for deep atmospheres the nature of the surface doesn't much affect the T(p) profile). The clearest example of this is the subNeptune paper by Aguichine et al, Ref. [42] in the MS. My reading of that paper is that the radiative-convective solver in the atmosphere (though not the deep interior structure) permits radiative layers. I'm thus puzzled that in the MS [42] is used as an argument that the new results in the MS imply radii more puffy than in [42]. Does [42] not already incorporate these effects? Emanuel Marcq is a co-author of both [42] and the present MS, so should be in a position to clarify this; I am only going from the rather condensed model description in [42].

In any event, the correct context, I'd say, is that the implications of such radiative layers for formation of magma oceans in the post-runaway state of rocky planets have, up to this paper, not been quantified. (This applies just to the magma oceans formed by a runaway; the young-planet or post-impact case, I would argue, was thoroughly explored already, as noted by Reviewer 1; The authors have done a good job of taking Rev. 1's comments on this issue onboard in the revision.)

But we still come down to the issue of opacities at high temperature and pressure, upon which the whole calculation rests. I am happy to see this uncertainty reflected in the abstract, now, though the authors could be a bit clearer that the opacity issue affects not just the "broad field" of (exo)planetary research but the authors own conclusions about lack of a magma ocean in particular. I do welcome the additional sensitivity checks on changing opacities, and am happy they've incorporated my remark that high temperature increases the efficiency of radiative heat transfer, but the concern about opacities is still just tacked on at the end, in the paragraph starting line 294 just before the methods section. I would have been happier to see the issue introduced also earlier in the discussion of the equilibrium results, together with some detail on the opacity sensitivity checks that are currently buried in Methods. I was rather surprised that Reviewer 4, who is evidently very familiar with water vapor spectroscopy, so ardently defended the use of CKD (or MT-CKD) at these temperatures and pressures, making just some casual remark about some concern over lack of laboratory data at high T and p. The fact is that MT-CKD is based on an entirely empirical assumption about line wings, calibrated against very limited laboratory data. For example, taking one of the most recent data papers I have found, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2008JD011355 , the limited spectral and p-T coverage of the lab data is highlighted. The lab results in Table 2 extend only to a maximum of 351K and 156.5 mb. There is absolutely no basis in having confidence in applying the empirical fits from this range at 100 bars and 1000K, especially because of the likelihood of additional physics coming in for such regimes. The uncertainty surrounding opacities is compounded when one brings in additional opacity sources, with even more sparse data on foreign continua. So, I continue to think that the authors are over-confident in their results. We have absolutely no basis currently to say what range of sensitivity checks on opacity would be adequate to cover the range of possibilities, since we have no idea what the upper bound of that range might be.

I still do not think the authors have sufficiently justified the inclusion of the rather sketchily analyzed 3D GCM results in this MS. Their response in the rebuttal does not add much information to the resolution of this issue. I have no more to say about it. It's an editorial decision, and I have to leave that in the hands of the Editor.

I really do not want to hold up the publication of this MS further, since I think will do a lot to redirect thinking on the subject of runaway-induced magma oceans and atmospheric evolution. I don't think it would detract significantly from the well-earned glory the authors will get from this paper if they tweak the exposition in the direction I have suggested. I have said as much as I can on this matter, and at this point will leave it in the hands of the Editor as to how much to encourage the authors to make final revisions in the direction I have suggested. It certainly would not be productive for me to have another look at any further revision that might result from that.

--Ray Pierrehumbert

Referee #3 (Remarks to the Author):

First, as I've already conveyed to the authors separately, I'm really sorry to hear of Franck's news, and I hope his recovery is going well.

Regarding the manuscript, I've carefully read the revised version and the authors' responses to the latest round of reviews. Overall, as before, I think this result is important enough and for the most part carefully argued enough for this paper to be publishable in Nature. I do have some final comments that I'd like the authors to incorporate, the implementation of which I'm going to leave at the editor's discretion.

First, overall I'm happy with the changes in the main text. The additions on lines 173 to 180 in particular help place the work in context. That said, parts of the abstract still seem a bit misleading to me. The wider claims made are all dependent on things like the opacity of realistic (as opposed to pure) steam atmospheres, which is explicitly not tackled here. In addition, the sentence "Around the reddest dwarfs, surface magma oceans cannot form by stellar forcing alone." is going to confuse, because the results reported here don't change anything for USP planets with high received dayside flux and thin atmospheres.

Regarding precipitation, the authors have made a pretty cursory alteration to the Methods text at the very end of the paper in response to my comment. Their claim that all rain will be evaporated rapidly may or may not be true in reality. It's not just a question of temperature, because falling raindrops enhance RH locally in regions where they are evaporated, so cold pool formation can give rise to large differences in the transport rate of drops to the deeper atmosphere vs. what a homogeneous 1D picture would suggest. Doing mesoscale runs is out of the scope of the study, but the authors at least need to acknowledge in the main text that mesoscale convection and raindrop microphysics are physical phenomena that exist, that they are not properly captured either by their 1D or GCM runs, and that they may or may not affect their results to an appreciable degree.

Referee #4 (Remarks to the Author):

My previous review of “A cool runaway greenhouse without surface magma ocean” focused on the study’s treatment of H2O opacities. The changes the authors have made in this latest draft have resolved the issues in my previous review, and I am now satisfied with the manuscript.

Specific issue:
Figure 8/Line ~694 – the version number in the legend should be MT_CKD_4.0.1 and not MT_CKD_4.0.

Author Rebuttals to Second Revision:

Response to the reviewers

and comments on the revised manuscript #3

We thank again the reviewers and the editor for their valuable contribution to the article.
Our answers are given in blue.

Referee #1 (Remarks to the Author)

I am satisfied by the authors' responses to my comments, and impressed by the authors' responses to the other reviewers.

minor quibble: I note in one response the authors state that they use pure H_2O because Kasting used pure H_2O in 1988, 1993 papers. I don't know what 1993 paper is being referred to, but the 1988 paper used a description of moist adiabats that required a nonzero amount of a noncondensible species (he usually used 3e-4 CO_2 mixing ratio). So Kasting's atmospheres were only very nearly pure H_2O.

We now mention that in the section discussing the atmospheric composition : “While the vaporization of the Earth ocean by the increase of solar luminosity would produce, at least at first, a nearly pure steam atmosphere (with 0.3% of N_2 and 0.0001% of CO_2 as in Kasting et al., 1988), the atmosphere released by the accretion of a terrestrial planet or by a giant impact would be a mixture of outgassed volatiles.

Some suggestions for the Abstract: (i) Make clear that these are pure H_2O models that neglect other sources of opacity. (ii) A style point: Don't interject "in fact" unless it is a fact. Here it just calls attention to the fact that what you are describing is your theory, and not (yet) a fact.

"...we find that steam atmospheres are in fact commonly..."
would be better written
"...we find that pure steam atmospheres are commonly..."

We have adopted this suggestion.

"Associated surface temperatures are much lower than previously thought: melting Earth surface would for instance require an insolation several times higher than today..."
would be better written
"Associated surface temperatures can be much lower than previously thought: melting Earth's surface with a pure steam atmosphere would for instance require an insolation several times higher than today..."

We wrote this sentence differently but in the same spirit : “*Not imposing a convective profile often results in a much cooler surface: melting Earth's crust would for instance require an insolation several times higher.”*

"Venus surface..."
is better written
"Venus's surface..."

This is now corrected.

For my part I will make no further suggestions. It's an inventive paper that asks important questions, using tools that seem pretty much state-of-the-art. Be it right or wrong, it shakes things up in a good way.

Referee #2 (Remarks to the Author)

First, let me say that I am very sorry to hear of Franck's accident, and send my best wishes for a swift and complete recovery.

I have no such excuse myself for being so delinquent in my re-review of this MS, and tempted though I am I won't attempt to hide my tardiness behind anonymity. I try to do my best on completing reviews, but things quickly became very busy upon my return from my US trip. I would have ordinarily expected the Editor to have given up on me long before this, but evidently my comments are wanted, and I have finally been able to make time to look over the revision and the response to the previous referee reports.

I'll reiterate that publication of this MS would be helpful to the field because of the possibilities that are raised, even if I continue to have questions about whether the conclusions will survive better models of the opacity data, including effects of additional sources of opacity (such as CO2, which could be very abundant once silicate weathering ceases). In terms of the general framing, I find one of the remarks by Reviewer 1 quite apt, namely that the authors are making an unwarranted assumption in presuming that it is "generally thought" (a paraphrase) that the runaway greenhouse would inevitably terminate in a magma ocean. Words to this effect still appear in the Abstract First of all, the abstract still doesn't include the qualification that insofar as there were claims of a magma ocean end-state, they only applied to cases with oceans comparable to Earth's ocean mass, not to significantly smaller water inventories. But that is a minor quibble. The main point regarding framing is that the authors are in no position to say what is generally thought in the community; they haven't taken a poll, and it would be pointless to do so. A lot of the planetary science community hasn't thought much about the end-state of runaway greenhouse atmospheres at all. A lot of the current focus on magma-ocean end states really only dates to the Hamano paper classifying magma oceans into Type I and Type II corresponding to the energy source creating them; I myself was in the magma ocean camp largely stimulated by Hamano, so that's at least two of us, but even I put in a caveat in the Boukrouche paper that the result was contingent on the atmosphere being convective all the way to the bottom (for all that I thought high opacity would be likely to assure that, and I still think that may turn out to be the case once the opacity estimates are clarified in future work.) This is not a big ask, but I do wish the authors would go through their manuscript and scrub out any presumptions about what has been generally thought regarding this issue. The paper will get plenty of attention anyway on the basis of the demonstration of circumstances in which runaway may not lead to a magma ocean, without the implication that this is overturning some established dogma. A more neutral way to rephrase the first two sentences of the abstract would be something like "Models of the end-state of a runway water vapour greenhouse which assume that the atmosphere has a convective layer reaching to the ground predict that a magma ocean will form, provided the water vapor inventory is massive enough." That tells the story in a matter-of-fact way without making unwarranted presumptions.

We have removed all the sentences that could presume what the scientific community would think or believe in general. Whenever we mention a given hypothesis or practice we now refer to the specific papers.

As for the comment that: ”the abstract still doesn't [mentions that] claims of a magma ocean end-state, [...] only applied to cases with oceans comparable to Earth's ocean mass”, The first sentence of the abstract has been modified as follows : “*A water vapour atmosphere with content equivalent to the Earth's oceans, resulting from impacts or a high insolation, had been found to yield a surface magma ocean.”*

I still find some deficiencies in the general context provided for the problem. The situation is that deep radiative (and often isothermal layers) in atmospheres are expected on the basis of analytic grey-gas solutions dating back decades, when there is insufficient deep-atmosphere absorption of stellar shortwave, or insufficient internal heat flux. These have been studied quantitatively for real-gas radiative transfer for gas planet atmospheres (primarily hydrogen dominated). For a recent example, see Figs 3 and 9 in Piette & Madhu's paper on K2-18b; these are H2-dominated atmospheres, but Fig. 9 does include water vapor effects. For that matter, there have been prior solutions for pure steam atmospheres using time-stepped radiative convective models for post-runaway atmosphere, though these have been for subNeptunes not terrestrial planets (though for deep atmospheres the nature of the surface doesn't much affect the T(p) profile). The clearest example of this is the subNeptune paper by Aguichine et al, Ref. [42] in the MS. My reading of that paper is that the radiative-convective solver in the atmosphere (though not the deep interior structure) permits radiative layers. I'm thus puzzled that in the MS [42] is used as an argument that the new results in the MS imply radii more puffy than in [42]. Does [42] not already incorporate these effects? Emanuel Marcq is a co-author of both [42] and the present MS, so should be in a position to clarify this; I am only going from the rather condensed model description in [42].

Emmanuel Marcq is not a co-author of the present paper. But we do know well the study [42]: it's classic *inverse climate modeling* assuming an adiabatic troposphere underneath an isothermal stratosphere.
Our results produce cooler and therefore less puffy radii than [42].

Concerning studies with H2O as a minor species in a H2-dominated atmospheres, yes there have been previous studies and we did cite one as a typical example : Thorngren et al, 2019. We chose this one because it shows the extension of the radiative layer as a function of various parameters including the internal heat flux.

In any event, the correct context, I'd say, is that the implications of such radiative layers for formation of magma oceans in the post-runaway state of rocky planets have, up to this paper, not been quantified. (This applies just to the magma oceans formed by a runaway; the young-planet or post-impact case, I would argue, was thoroughly explored already, as noted by Reviewer 1; The authors have done a good job of taking Rev. 1's comments on this issue onboard in the revision.)

But we still come down to the issue of opacities at high temperature and pressure, upon which the whole calculation rests. I am happy to see this uncertainty reflected in the abstract, now, though the authors could be a bit clearer that the opacity issue affects not just the "broad field" of (exo)planetary research but the authors own conclusions about lack of a magma ocean in particular. I do welcome the additional sensitivity checks on changing opacities, and am happy they've incorporated my remark that high temperature increases the efficiency of radiative heat transfer, but the concern about opacities is still just tacked on at the end, in the paragraph starting line 294 just before the methods section. I would have been happier to see the issue introduced also earlier in the discussion of the equilibrium results, together with some detail on the opacity sensitivity checks that are currently buried in Methods. I was rather surprised that Reviewer 4, who is evidently very familiar with water vapor spectroscopy, so ardently defended the use of CKD (or MT-CKD) at these temperatures and pressures, making just some casual remark about some concern over lack of laboratory data at high T and p. The fact is that MT-CKD is based on an entirely empirical assumption about line wings, calibrated against very limited laboratory data. For example, taking one of the most recent data papers I have found, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2008JD011355 , the limited spectral and p-T coverage of the lab data is highlighted. The lab results in Table 2 extend only to a maximum of 351K and 156.5 mb. There is absolutely no basis in having confidence in applying the empirical fits from this range at 100 bars and 1000K, especially because of the likelihood of additional physics coming in for such regimes. The uncertainty surrounding opacities is compounded when one brings in additional opacity sources, with even more sparse data on foreign continua. So, I continue to think that the authors are over-confident in their results. We have absolutely no basis currently to say what range of sensitivity checks on opacity would be adequate to cover the range of possibilities, since we have no idea what the upper bound of that range might be.

We clearly stated that the sensitivity checks are not an estimate of the uncertainty, which we cannot quantitatively address in the absence of uncertainties on high pressure opacities. These sensitivity checks are here to show, however, that the temperature feedback is very efficient to keep the structure radiative when modifying the continuum opacity, which is a very important aspect of the problem.

I still do not think the authors have sufficiently justified the inclusion of the rather sketchily analyzed 3D GCM results in this MS. Their response in the rebuttal does not add much information to the resolution of this issue. I have no more to say about it. It's an editorial decision, and I have to leave that in the hands of the Editor.

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--Ray Pierrehumbert

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First, as I've already conveyed to the authors separately, I'm really sorry to hear of Franck's news, and I hope his recovery is going well.

Regarding the manuscript, I've carefully read the revised version and the authors' responses to the latest round of reviews. Overall, as before, I think this result is important enough and for the most part carefully argued enough for this paper to be publishable in Nature. I do have some final comments that I'd like the authors to incorporate, the implementation of which I'm going to leave at the editor's discretion.

First, overall I'm happy with the changes in the main text. The additions on lines 173 to 180 in particular help place the work in context. That said, parts of the abstract still seem a bit misleading to me. The wider claims made are all dependent on things like the opacity of realistic (as opposed to pure) steam atmospheres, which is explicitly not tackled here.

>> We now write "pure steam atmospheres" in the abstract.

In addition, the sentence "Around the reddest dwarfs, surface magma oceans cannot form by stellar forcing alone." is going to confuse, because the results reported here don't change anything for USP planets with high received dayside flux and thin atmospheres.

Ultra Short Period (USP) planets can still have a surface magma ocean if they have a tenuous or no atmosphere for early F, G, K and early M stars but the case of the reddest dwarfs is different: they are simply not bright enough to melt the closest planets. For instance, putting a planet at the

Roche limit of Trappist-1 (5 star radii), would give it a substellar temperature Tss=1130 K with a null Bond albedo, far below rock melting. For Proxima, at the Roche limit (4.4 star radii), Tss=1390 K, still below melting. However, “reddest dwarfs” was indeed vague so we completed it by “reddest stars (Teff<3000~K)”.

Regarding precipitation, the authors have made a pretty cursory alteration to the Methods text at the very end of the paper in response to my comment. Their claim that all rain will be evaporated rapidly may or may not be true in reality. It’s not just a question of temperature, because falling raindrops enhance RH locally in regions where they are evaporated, so cold pool formation can give rise to large differences in the transport rate of drops to the deeper atmosphere vs. what a homogeneous 1D picture would suggest. Doing mesoscale runs is out of the scope of the study, but the authors at least need to acknowledge in the main text that mesoscale convection and raindrop microphysics are physical phenomena that exist, that they are not properly captured either by their 1D or GCM runs, and that they may or may not affect their results to an appreciable degree.

We agree with the reviewer but we do not make claims about precipitations as it is only in the extreme and unrealistic case where all the condensed water evaporates immediately at the surface that the profile is significantly altered due to the associated cooling. Otherwise, the profile shows little sensitivity to transport/evaporation parameters. And we do account for the change of RH where evaporation occurs. Of course it remains a 1D model and simulations like those performed by Seeley & Wordsworth (2021) would be very interesting to run for the profiles we find where moist convection occurs. It would be a relevant follow-up study.

Referee #4 (Remarks to the Author)

My previous review of “A cool runaway greenhouse without surface magma ocean” focused on the study’s treatment of H2O opacities. The changes the authors have made in this latest draft have resolved the issues in my previous review, and I am now satisfied with the manuscript.

Specific issue:
Figure 8/Line ~694 – the version number in the legend should be MT_CKD_4.0.1 and not MT_CKD_4.0.

>> We have modified Figure 8 using the legend “MT_CKD_4.0.1”.