Peer Review File

Manuscript Title: Nightside clouds inhibit early ocean formation on Venus but not on Earth

Editorial Notes:

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1 (Remarks to the Author):

This is an exciting new paper that revolutionizes theoretical modeling of early post-accretionary atmospheres on Venus and Earth. As the authors correctly point out, all 3-D climate modeling simulations of early terrestrial planet atmosphere (Venus, Earth, and Mars) have assumed that liquid water was present initially on the surface. One-dimensional modeling of Venus and Earth by Hamano et al. (2013) (ref. 3) did not make this assumption. Like the present authors, Hamano et al. assumed (or calculated, actually) the presence of a steam atmosphere during the final stages of accretion. That atmosphere condensed to form oceans on Earth, due to its lower insolation, but on Venus the steam atmosphere remained in place and was eventually lost by photodissociation and hydrogen escape. The results here are similar, but with an important twist: The steam atmosphere on Earth condenses, but only if solar luminosity was less than 92% of its present value. In other words, a faint young Sun was required for Earth to have become habitable. That's a stunning prediction, well worthy of appearing in Nature.

These calculations are timely, as well. The planetary science decadal survey, of which this reviewer is a member, is currently in the process of preparing its report. I sit on the Venus panel and can attest that the papers by Michael Way and colleagues (refs. 4 and 11 in the present manuscript) have received lots of attention. Way's 3-D model (the ROCKE-3D model from NASA GISS) predicts that Venus could have had liquid water for up to several billion years if it began its life as a slow rotator. Some Venus panelists like that because it allows for the possibility that life evolved there and later migrated to the clouds when surface water disappeared. That rosy scenario is in accord with the Greaves et al. paper on Venus phosphine, published last year in Nature Astronomy. (Nature itself appears to have had the good sense to pass it by.) Those same panelists will be disappointed by this paper because it goes against that optimistic scenario. But that is all the more reason to publish it. Science progresses when researchers propose hypotheses, then see them tested either observationally, experimentally, or theoretically.

All this does not mean that the present paper will end discussion on this topic. 3-D climate models are complicated, and other modeling teams like the group at NASA GISS will want to test these conclusions. But they will have to work hard to do so because the LMD model used here is almost certainly the best model in the world for simulating hot, H2O-dominated, atmospheres. In particular, the absorption coefficients used in their radiative transfer algorithms take advantage of the HITEMP database, and that is critical for calculating the absorption of solar radiation that keeps the daysides of these model planets free of clouds. The authors point out that the 1-D radiative-convective version of their model agrees well with other state-of-the-art 1-D models, e.g., ref. 22 (Kopparapu et al., 2013). But that agreement is actually to be expected, as Kopparapu et al. (our research group) received help from the LMD group in deriving our own HITEMP absorption coefficients.

A few detailed comments follow, but they are minor and can be easily addressed. This paper should also be reviewed by a competent 3-D climate modeler, preferably someone from a group not directly involved in the current arguments about early Venus. You may reveal my name to the authors.

Jim Kasting
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1. (l. 13) 'The conditions required for water to have ever condensed on the surface of solar system terrestrial planets are highly uncertain, as they have so far only been studied with one-dimensional numerical climate models..
--I think I know what the authors are saying here, i.e., that 3-D models have not been used to study terrestrial planetary atmospheres during or directly following accretion. But the way it is said implies that 3-D models have not been used at all to study early climate evolution, and of course this is incorrect (see, e.g., refs 4 and 11 in the present manuscript). Reword! (I think it would suffice to replace the word 'ever' with 'initially'.)

2. (l. 33) 'Deuterium-hydrogen isotopic ratio measurements (12, 13), ~10^2 times higher than on Earth) suggest indeed that the early Venus superficial water reservoir ranged from roughly 4 to 500 m GEL (4)'..
--Well, Way et al. may have claimed this – they quote Donahue and Russell (1997) – but in reality no upper bound can be placed on Venus' initial water inventory from D/H ratios, as the fractionation factor during (hydrodynamic) escape of hydrogen could be close to unity (Kasting and Pollack, Icarus, 1983). What fractionation factors did Donahue and Russell assume, and why?

3. (Fig. 2) What rotation rates are assumed for Earth and Venus in these simulations? The figure caption should say. The text says that the cloud locations are independent of rotation rate but one would still like to know this parameter.

4. (l. 102) 'The preferential formation of stratospheric nightside water clouds in both the Earth (Prot ~24 h) and early Venus simulations (Prot ~ 5833 h, i.e. ~244 x that of Earth)..'
--Ah, here are the numbers I was looking for in the previous comment. They should appear in the Fig. 2 caption, as well.

5. (l. 14) '(Turbet et al., work in preparation)'
--This self-reference is gratuitous unless the work will have appeared (and can be properly referenced) before the current manuscript is published. I suggest removing it.

6. (l. 157) 'This suggests that the Faint Young Sun, which has long been problematic in explaining the longevity of the Earth's liquid water oceans (17), may in fact have been a necessary opportunity for life to appear on Earth.'
--This is a really cool result! If it holds up, it will have implications for the habitability of exoplanets that form within the conventional habitable zone of their star but at higher instellations than that of early Earth. There is no need to say this here, as the last sentence is already ideal, but that is where the real significance of this work lies.
--One other thought: The results shown here confirm and extend the 1-D predictions of Hamano et al. (2013). It might be worth adding a sentence somewhere in the last few paragraphs pointing that out. Hamano et al. had the right idea – they just didn't have as good of a climate model with which to explore it.

Referee #2 (Remarks to the Author):

The goal of this paper is to understand early ocean formation on Venus and Earth. The authors use a 3D global climate model developed at LMD to argue that oceans would not have formed on early Venus because cloud formation occurred preferentially on the nightside of the planet and provided significant greenhouse warming. This result is different from previously published studies, and the reason for that difference is related to the initial conditions of the simulations—the results of this paper are based on simulations that start hot with all of the water in the atmosphere (not on the surface), which leads to a different equilibrated state than previous studies that started with the water on the surface. This current result is very interesting and will likely be a significant contribution to our understanding of early Venus and Earth.

The modeling work was convincing, well-motivated, and well-described. The GCM used is an established model that includes tested and referenced physics parameterizations that are appropriate for this study. Modeling issues that could decrease the viability of the study—for example, uncertainties in the number of seed nuclei available and issues related to equilibration and convergence—are convincingly handled with carefully designed sensitivity studies. The conclusions drawn from the modeling work were thus convincing and sound.

Referee #3 (Remarks to the Author):

This paper presents detailed 3D climate simulations for both early Venus and Earth environments, taking into account rotation rates and insolation fluxes from the "faint young sun". The authors deduce that, rather than forming at the subsolar point, clouds would have preferentially formed on the nightside. The lack of clouds on the dayside (which would lower albedo) and clouds on the nightside (providing greenhouse warming) would result in water being unable to condense on the surface. This is a new and interesting result that provides a fresh insight into a possible explanation for the present condition of the Venusian surface. The paper is well written and provides a detailed description of the methodology used. I would like to see the paper published but have two potentially major comments and several minor comments to be addressed by the authors.

Major comments.

1. The Deuterium/Hydrogen isotopic ratio is mentioned on line 33, but is not mentioned again thereafter. This seems to be a major point that requires explanation from the model presented. Specifically, it is not clear that substantial water vapor remaining in the atmosphere, and then subsequently desiccated by UV/XUV flux, would result in the currently observed D/H ratio. If would be useful if the authors returned to that issue to describe the self-consistency of their model with the present observations.

2. The GCM models are quite sensitive to initial conditions, and these initial conditions for Venus are poorly constrained relative to those chosen for the simulations. This includes the atmospheric composition and the water content threshold that would determine nightside cloud formation and subsequent warming. For example, the authors state in lines 102-105 that the formation of stratospheric nightside water clouds for Earth and Venus "suggests that the mechanism is also robust to a wide range of rotation periods". This is not necessarily true, since the combination of factors combined with rotation can produce partially stochastic climate feedback loops. See for example Jansen et a. (2018), where the equator-to-pole heat transport does not monotonically increase or decrease over the range of rotation rates explored. Moreover, the evolution of the rotational angular momentum vector for Venus, including both rotation and obliquity (which is never mentioned) are expected to have changed substantially since the faint young Sun period discussed. As such, although the results presented are certainly important, they may not be as definitive as implied in the paper at explaining the Venus/Earth dichotomy due to the gaps in our knowledge for Venus initial conditions and the limited parameter space explored in the work.

Minor comments.

Line 25: I suggest removing the word "miraculously".

Line 63: It would be helpful to specify that the insolations described here (and throughout) are average top of the atmosphere fluxes.

Figure 2: The numbers on the axes are difficult to see, so making those larger would be appreciated. Also, is it feasible to use the same scaling for the water cloud column and thermal flux rows so that the figures in each row can be more easily visually compared?

Figure 3: I don't see the d label.

Author Rebuttals to Initial Comments:

<u>Responses to referee #1</u>

Dear reviewer, dear Jim,

Please find below our responses to your comments.

1. (l. 13) 'The conditions required for water to have ever condensed on the surface of solar system terrestrial planets are highly uncertain, as they have so far only been studied with one-dimensional numerical climate models..
--I think I know what the authors are saying here, i.e., that 3-D models have not been used to study terrestrial planetary atmospheres during or directly following accretion. But the way it is said implies that 3-D models have not been used at all to study early climate evolution, and of course this is incorrect (see, e.g., refs 4 and 11 in the present manuscript). Reword! (I think it would suffice to replace the word 'ever' with 'initially'.)

>> We have now replaced the word "ever" with "initially" as suggested.

2. (l. 33) 'Deuterium-hydrogen isotopic ratio measurements (12, 13), ~10^2 times higher than on Earth) suggest indeed that the early Venus superficial water reservoir ranged from roughly 4 to 500 m GEL (4)'..
--Well, Way et al. may have claimed this – they quote Donahue and Russell (1997) – but in reality no upper bound can be placed on Venus' initial water inventory from D/H ratios, as the fractionation factor during (hydrodynamic) escape of hydrogen could be close to unity (Kasting and Pollack, Icarus, 1983). What fractionation factors did Donahue and Russell assume, and why?

>> We agree with the reviewer. We have rephrased the sentence as follows:

"Deuterium-hydrogen isotopic ratio measurements (12, 13), ~10^2 times higher than on Earth) suggest indeed that the early Venus superficial water reservoir ranged from roughly 4 to 500 m GEL (4), **but could have been even higher as the D/H fractionation factor during (hydrodynamic) escape of hydrogen can be close to unity (Kasting & Pollack 1983).**"

3. (Fig. 2) What rotation rates are assumed for Earth and Venus in these simulations? The figure caption should say. The text says that the cloud locations are independent of rotation rate but one would still like to know this parameter.

>> We have now added the assumed rotation rates in the Fig. 2 caption.

4. (l. 102) 'The preferential formation of stratospheric nightside water clouds in both the Earth (Prot ~24 h) and early Venus simulations (Prot ~ 5833 h, i.e. ~244 x that of Earth)..'
--Ah, here are the numbers I was looking for in the previous comment. They should appear in the Fig. 2 caption, as well.

>> As stated above, they are now included in the Fig. 2 caption.

5. (l. 14) '(Turbet et al., work in preparation)'
--This self-reference is gratuitous unless the work will have appeared (and can be properly referenced) before the current manuscript is published. I suggest removing it.

>> We have now removed the self-reference because the work is unlikely to appear before the current manuscript is published.

6. (l. 157) 'This suggests that the Faint Young Sun, which has long been problematic in explaining the longevity of the Earth's liquid water oceans (17), may in fact have been a necessary opportunity for life to appear on Earth.'
--This is a really cool result! If it holds up, it will have implications for the habitability of exoplanets that form within the conventional habitable zone of their star but at higher instellations than that of early Earth. There is no need to say this here, as the last sentence is already ideal, but that is where the real significance of this work lies.

>> We totally agree with the reviewer. As stated in the initial version of the manuscript, we currently have a second manuscript in preparation that does explore the impacts of this nightside cloud feedback in the context of exoplanets.

--One other thought: The results shown here confirm and extend the 1-D predictions of Hamano et al. (2013). It might be worth adding a sentence somewhere in the last few paragraphs pointing that out. Hamano et al. had the right idea – they just didn't have as good of a climate model with which to explore it.

>> We have now modified the following sentence in the manuscript to account for this suggestion:

"As this minimal insolation is much higher than the water condensation insolation threshold (~325W/m2), oceans should never have formed on the surface of Venus, **which confirms and extends previous 1-D cloud-free climate predictions (Hamano et al. 2013)."**

<u>Responses to referee #3</u>

Dear reviewer,

Please find below our responses to your comments.

1. The Deuterium/Hydrogen isotopic ratio is mentioned on line 33, but is not mentioned again thereafter. This seems to be a major point that requires explanation from the model presented. Specifically, it is not clear that substantial water vapor remaining in the atmosphere, and then subsequently desiccated by UV/XUV flux, would result in the currently observed D/H ratio. If would be useful if the authors returned to that issue to describe the self-consistency of their model with the present observations.

>> To the best of our knowledge of the literature on the D/H interpretation of the atmosphere of Venus (see the reasoning in Gurwell 1995, Nature and references e.g. Grinspoon 1987, 1993 therein), it is not possible to use the D/H ratio measurement of Venus to favor one or the other scenario (i.e. that Venus had a primitive liquid water ocean or not).

However, the current measurements of the water vapor mixing ratio in the atmosphere of Venus seem to favor (see Gillmann et al. 2020, Nature Geoscience ; or Persson et al. 2020, JGR Planets) the scenario we propose in our manuscript (i.e. that Venus was unable to form primitive oceans) based on the fact that the water loss on Venus should have occurred very early (Lichtenegger et al. 2016 JGR Space Physics, Lammer et al. 2018, A&A Review) and is therefore less compatible with the scenario in which a relatively late ocean (as proposed e.g. in Way & Del Genio 2020, JGR Planets) would have been stable, then suffered a runaway greenhouse episode, and eventually escaped to space.

We have modified the manuscript to reflect the reviewer's comment as follows:

"The water above the surface of Venus -- always in the form of vapor -- most likely gradually escaped from the atmosphere. **It is not possible to tell from D/H measurements alone (Grinspoon 1987, 1993, Gurwell 1995) whether this scenario is more likely than the one in which a late ocean was present (Way & Del Genio 2020). However, recent estimates of water delivery (Gillmann et al. 2020) and atmospheric escape calculations (Persson et al. 2020, Gillmann et al. 2020) suggest together that water escaped very early from the atmosphere of Venus when the Sun's extreme UV radiation was stronger (Lichtenegger et al. 2016, Lammer et al. 2018), in better agreement with our proposed scenario.**"

2. The GCM models are quite sensitive to initial conditions, and these initial conditions for Venus are poorly constrained relative to those chosen for the simulations. This includes the atmospheric composition and the water content threshold that would determine nightside cloud formation and subsequent warming. For example, the authors state in lines 102-105 that the formation of stratospheric nightside water clouds for Earth and Venus "suggests that the mechanism is also robust to a wide range of rotation periods". This is not necessarily true, since the combination of factors combined with rotation can produce partially stochastic climate feedback loops. See for example Jansen et a. (2018), where the equator-to-pole heat transport does not monotonically increase or decrease over the range of rotation rates explored. Moreover, the evolution of the rotational angular momentum vector for Venus, including both rotation and obliquity (which is never mentioned) are expected to have changed substantially since the faint young Sun period discussed. As such, although the results presented are certainly important, they may not be as definitive as implied in the paper at explaining the Venus/Earth dichotomy due to the gaps in our knowledge for Venus initial conditions and the limited parameter space explored in the work.

>> We agree with the reviewer that a larger sensitivity study would be desirable to check the robustness of this result. We believe though that the fact that the dichotomous distribution of clouds appears in all simulations we performed suggest that the result (although it may change quantitatively) is unlikely to change qualitatively.

We have modified the manuscript to reflect the reviewer's comment as follows:

"… suggests that the mechanism is also robust to a wide range of rotation periods. **A comprehensive sensitivity study is needed to quantitatively confirm this result, as it has been shown that cloud and atmospheric circulation feedbacks can vary non-linearly and non-monotonically with rotation period (Jansen et al. 2019).**"

3. Line 25: I suggest removing the word "miraculously".

>> We have now removed the word 'miraculously'.

4. Line 63: It would be helpful to specify that the insolations described here (and throughout) are average top of the atmosphere fluxes.

>> We have now rephrased line 63 as follows: "atmospheres of early Venus and Earth (forced to insolations between 300 and 675 W m^{-2}, **defined here as average top of the atmosphere fluxes**)"

Figure 2: The numbers on the axes are difficult to see, so making those larger would be appreciated. Also, is it feasible to use the same scaling for the water cloud column and thermal flux rows so that the figures in each row can be more easily visually compared?

>> We have now updated the Fig. 2 with (1) a larger font for the numbers on the axes and with (2) the same colorbar scaling for the water cloud column maps and with (3) the same colorbar scaling for the thermal flux maps (for a fixed top of the atmosphere flux). nb: using the same colorbar for the two different top of the atmosphere fluxes makes the thermal flux maps too difficult to read.

6. Figure 3: I don't see the d label.

>> The 'd' label has now been added on Fig. 3.

Reviewer Reports on the First Revision:

Referee #1 (Remarks to the Author):

The authors have done a good job in responding to my comments and to those of the other reviewers. All three of us were basically positive about this paper initially. I recommend that the paper be accepted and published in close to its present form. I do have one minor editorial comment, listed below.

Jim Kasting

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1. (l. 105) ‘This very peculiar distribution of clouds produces a very strong net warming effect.’
-- Editorial suggestion: Eliminate the two ‘very’s’. The statement is cleaner without either one of them.

Referee #3 (Remarks to the Author):

Thank you for your clarifications and responses to my questions. Congratulations on this wonderful result!

Stephen Kane