nature portfolio

Peer Review File

Volatile-rich evolution of molten super-Earth L 98-59 d

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Comments for the Author)
In this manuscript, the authors investigate the low bulk density, intermediate atmospheric mean molecular weight, and recent spectral detections of SO_2 and H_2S on the super-Earth L 98-59 d. They propose that these observations can all be explained by the atmospheric evolution due to an underlying permanent magma ocean. Using a comprehensive atmosphere-interior modelling framework, they investigate the coupled evolution of magma ocean solidification, volatile degassing, and atmospheric escape. They find that the observations can be explained by a reducing mantle and high primordial sulfur and hydrogen content, while simultaneously matching the observed abundances of H2S and SO2.

This is a highly relevant and timely study, as it represents, to my knowledge, one of the first works to directly infer interior properties and formation conditions of a super-Earth from current observational constraints. In addition, the authors provide compelling evidence for the presence of a long-lived magma ocean on L 98-59 d. The methodology is rigorous. The authors are careful to ensure that their results are robust against a range of uncertain parameters. Many of the potential concerns that arose while reading were subsequently addressed within the manuscript, leaving little to criticize in terms of the modeling approach. I would recommend publication after the following points have been addressed or clarified:

1. As the authors are probably aware, shortly after submission of this manuscript, Cadieux et al. published updated parameters for L 98-59 d with a new planet radius of 1.627+-0.041 R_Earth and mass at 1.64+-0.07 M_E, which yields a significantly lower density of 2.2+-0.2 g/cm^3. As a result, none of the evolution models presented here fit the 1 sigma density constraints anymore. In addition, the eccentricity has been refined to below 0.02, which puts it close to the complete mantle solidfication case in the sensitivity analysis (Fig. 7). To establish the robustness of the conclusions, I would therefore advise the authors to revisit their calculations using the updated planetary parameters, and to assess whether their proposed scenario still holds under these new constraints.

2. The models fit the SO2 and H2S abundance constraints from retrieval models. In line 74, the authors argue that there are weak constraints on H2O, CO2, etc, citing Banerjee et al. 2024. However, the other retrieval paper cited two lines above (Gressier et al. 2024) actually puts fairly strong upper limits on the water mixing ratio of <10^-5, which as far as I am aware is not addressed anywhere in the text. This value is at odds with the water vmr shown in Fig 4 to be above 10^-2. This discrepancy should be addressed in the manuscript.

3. I'm a bit puzzled by the sensitivity analysis for the impact of core size, and specifically why even extremely large cores yield essentially the same or even larger planet radii than the cases with small cores. From a rough back-of-the-envelope calculation, a 2.14 Earth mass planet with an 90% iron core by radius (eqivalent to about the same core mass) by radius should have an overall R_int of about 1 Re, meaning that the atmosphere needs to have a thickness of about 3000-4000 km to make up a planet radius, which is about double the thickness of the nominal case in Fig 3. However, the mantle mass would only be 10% of the (solid) planet. From Fig. 3 we see that pretty much the entire H inventory of the mantle is exsolved, so this should also happen with a thin magma ocean. However, compared to the nominal case shown in the manuscript with a mantle mass of about 70%, the total mass of H available should therefore only be about 1/7 of the nominal case. I don't understand how such a thick atmosphere can be built up from . Wouldn't a thinner magma ocean also impact the atmospheric composition? How well do these atmospheres still fit the observed MMW?

Minor comments:
1. I'm missing a bit more details on the XUV evolution calculations, especially given that the results appear sensitive to atmospheric escape, as shown in Fig. 7. For instance, according to Johnstone et al. (2020), if L 98-59 is a slow or fast

rotator, the XUV saturation timescale could range from roughly 1 to 2 Gyr, which could affect some of the results.
2. Regarding the sensitivity analysis, the authors should state the parameters of the nominal simulation they used.
3. Line 433: I think the reference is wrong here. Rajpaul et al. only investigate L98-59 d.

(Remarks on code availability)

Reviewer #2

(Comments for the Author)
I recommend this paper for publication after addressing the concerns below, which principally focus on the framing of their results rather than the technical work itself, which I find to be satisfactory. In this paper, the authors propose that the atmospheres of larger-than-the-radii gap planets, exemplified by L 98-59 d, may have a secondary de-gassed origin from a reduced mantle composition. The authors don't make this contention, but in many ways what they propose is much closer to the secondary atmosphere formation mechanism that is the leading hypothesis for Earth, Venus, and Mars—it's unclear exactly what fraction of our atmosphere degassed as the final magma ocean crystallized and how much is from later volcanism (limited evidence is that Earth's atmosphere has not changed much in mass over time). This is an important result that is deserving of a high profile publication. I do find that the framing of the state-of-the-field to be somewhat misrepresentative since it appears to portray two proposed ideas as in competition when I think the field has recognized that there are likely more than one pathway to forming planets of these radii. Indeed, the authors are proposing a very reasonable third pathway.

This issue regarding how the results is framed begins from the second sentence of the abstract, "Two competing [1, 2] scenarios are that they formed either with rocky interiors and H2-He atmospheres ('gas-dwarfs') [3, 4], or instead with bulk compositions dominated by H2O phases ('water-worlds') [5, 6]." [5] describes how observationally the larger-than-the-radii-gap planets are actually two populations (H/He and H2O) of different density planets and [6] gives a formation theory explanation for why the population of larger-than-the-radii-gap planets are actually two populations (H/He and H2O), so this paper seemingly misstates the current state of the field—these ideas are not competitive but accretive. Relatedly, this paper seems to confuse the proposed origin of the smaller-than-the-radii-gap planets. This population is made of planets that formed below the gap and stayed there as well as planets were in the gap and couldn't maintain their atmospheres. There is evidence that the gap widens over time and the authors already cite the evidence for this [16] and [25]. All this is to say that all these ideas appear to work in conjunction with each other, yet this paper is written as if they exclude each other, which I find jarring and confusing.
The modeling of the mantle is done assuming that the atmosphere and the molten portion of the mantle are in chemical equilibrium? If so, this should be stated plainly. If not, then the relationship should be better explained. Is the solidified mantle considered chemically inert after crystallization?

For the successful models described in 106-108, is it contraction or atmospheric loss that drives the bulk density change of the planet? If contraction, what role does loss play at all? In these high H mass cases, does de-gassing of the mantle play a significant role at all in the radius? How does the mean molecular weight of the atmosphere change in these cases (a figure showing MMW as a function of time may help tell the story for each of the different evolutionary cases)?
It may be worth pointing out to the reader that 10,000 ppm H2O is considered a good nominal estimate of Earth's water content (e.g. Hirschman; and also Mars), so less than 10,000 ppm is a dry planet (like the Moon).

Regarding, "The super-Earth (radii $\lesssim 1.7R_\oplus$) and sub-Neptune (radii ~ 1.7– $4R_\oplus$) exoplanet populations have no Solar-System analogues …" is a frustrating tautology that gets repeated in the exoplanet literature like it's a major insight. The words super-Earth and sub-Neptune clearly already imply that there is no Solar System analog. 'If we consider the population of all those not living in the UK or the Netherlands, then the population contains no people from the UK or the Netherlands.'
"HST observations of the innermost planet (mass $0.47M_\oplus$, radius $0.85R_\oplus$) rule out a low mean-molecular-weight (MMW) primary atmosphere [27], but recent JWST transit spectroscopy has indicated an SO2-rich atmosphere [28]." It's unclear why this planet is described in this paper. Is the point that SO2 has been found elsewhere in the system? Perhaps the authors could add a clause explaining why discussing the other planet is relevant.

On line 374, what do the authors mean by nominally uses this other model. Is another model actually used?

What tidal parameters are used to describe the planet's Maxwell Rheology? How do these choices effect the outcome?

(Remarks on code availability)

Version 1:

Reviewer comments:

Reviewer #1

(Comments for the Author)
I thank the authors for revising the manuscript. All of my comments have been satisfactorily addressed, and I would therefore like to recommend this manuscript for publication.

I have a few minor comments from rereading the manuscript:

- I did not find a mention on how the initial atmosphere is set up. I imagine that it forms purely from the volatile oversaturation of the magma ocean, and that no accreted atmosphere remnant is considered. This should be stated somewhere (e.g. around l. 423).

l.69: Consider adding something akin to "... for the available lowest density estimate" to the parentheses. Otherwise, this is a bit at odds with the later statement of using a density of 3.45 g/cm^3.

l.84: State the actual density value and reference here.

Fig.11: "Core [%]" should be "Core radius [%]"

(Remarks on code availability)

Reviewer #2

(Comments for the Author)
I recommend this paper for publication. All of my concerns have been addressed.

(Remarks on code availability)

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Responses to Reviewer1

1) As the authors are probably aware, shortly after submission of this manuscript, Cadieux et al. published updated parameters for L 98-59 d with a new planet radius of 1.627+-0.041 R_Earth and mass at 1.64+-0.07 M_E, which yields a significantly lower density of 2.2+-0.2 g/cm^3. As a result, none of the evolution models presented here fit the 1 sigma density constraints anymore. In addition, the eccentricity has been refined to below 0.02, which puts it close to the complete mantle solidification case in the sensitivity analysis (Fig. 7). To establish the robustness of the conclusions, I would therefore advise the authors to revisit their calculations using the updated planetary parameters, and to assess whether their proposed scenario still holds under these new constraints.

Thank you for highlighting these new estimates of the orbital and planetary parameters for this system. Relevant updated parameters are listed in **Table 1**, referenced throughout the text, and in the introduction near lines **70** and **82**. We have run new evolutionary calculations accounting for these changes, which are presented in the SI as **Fig 11** and discussed from line **1038** onwards. These additional results strengthen our assessment that this planet formed volatile-rich with a reducing interior, and overall increase the relevance of our manuscript given that the new radius estimate places it within the radius valley of M-dwarf planets.

2) The models fit the SO2 and H2S abundance constraints from retrieval models. In line 74, the authors argue that there are weak constraints on H2O, CO2, etc, citing Banerjee et al. 2024. However, the other retrieval paper cited two lines above (Gressier et al. 2024) actually puts fairly strong upper limits on the water mixing ratio of <10^-5, which as far as I am aware is not addressed anywhere in the text. This value is at odds with the water vmr shown in Fig 4 to be above 10^-2. This discrepancy should be addressed in the manuscript.

Our percentage-level H2O is consistent with the posterior distribution shown by Fig3 in Banerjee+24. To address the reviewers comments we have clarified and further contextualised the modelled H2O abundance near line **326**.

3) I'm a bit puzzled by the sensitivity analysis for the impact of core size, and specifically why even extremely large cores yield essentially the same or even larger planet radii than the cases with small cores. From a rough back-of-the-envelope calculation, a 2.14 Earth mass planet with an 90% iron core by radius (equivalent to about the same core mass) by radius should have an overall R_int of about 1 Re, meaning that the atmosphere needs to have a thickness of about 3000-4000 km to make up a planet radius, which is about double the thickness of the nominal case in Fig 3. However, the mantle mass would only be 10% of the (solid) planet. From Fig. 3 we see that pretty much the entire H inventory of the mantle is exsolved, so this should also happen with a thin magma ocean. However, compared to the nominal case shown in the manuscript with a mantle mass of about 70%, the total mass of H available should therefore only be about 1/7 of the nominal case. I don't understand how such a thick atmosphere can be built up from . Wouldn't a thinner magma ocean also impact the atmospheric composition? How well do these atmospheres still fit the observed MMW?

Thank you for pointing this out. In this particular sensitivity test we probe the model sensitivity to the core fraction parameter *only*, keeping all else constant where possible. To do this, we absolutely specify the H inventory as H=914 Earth oceans for this test only, so the trend in Fig 7g is a result of preferential degassing at large r_c when the mantle mass is comparatively small and the (constant) H inventory is not accommodated by dissolution into the magma ocean. It is true that H is primarily outgassed, which explains the minor sensitivity shown in this test, except for the especially small mantles when r_c is large (which are not compatible with the observations anyway). We have updated **Fig 7** to include both interior and photospheric radii (dotted and solid lines) and expanded text description near line **839** to clarify this behaviour.

4) I'm missing a bit more details on the XUV evolution calculations, especially given that the results appear sensitive to atmospheric escape, as shown in Fig. 7. For instance, according to Johnstone et al. (2020), if L 98-59 is a slow or fast rotator, the XUV saturation timescale could range from roughly 1 to 2 Gyr, which could affect some of the results.

Our modelling of stellar XUV emission over time is calculated using the semi-emperical 'MORS' model of Johnstone+21 (A&A). We have updated the text from line **422** to clarify our calculations. The re-analyses provide tight constraints on the star's rotation rate, which are incorporated into our response to point (1) from line **1038** onwards. These models account for this uncertainty alongside our prior existing sensitivity tests to the escape efficiency.

5) Regarding the sensitivity analysis, the authors should state the parameters of the nominal simulation they used.

We have updated the text in this section, line **802** in the SI, to describe the nominal parameters used in these sensitivity tests. These are roughly 'median' parameters on each grid axis.

6) Line 433: I think the reference is wrong here. Rajpaul et al. only investigate L98-59 d.

In this part of the text, near line **491**, we describe which modelled scenarios are taken to be consistent with the observations. This was done using the density from Rajpaul+24 and the MMW from Gressier+24. We provide an additional comparison between our models and new estimates in response to comment (1) above, but retain our main analysis using the earlier Rajpaul+24 density estimate since our original findings are found to be robust.

Responses to Reviewer2

1) The authors don't make this contention, but in many ways what they propose is much closer to the secondary atmosphere formation mechanism that is the leading hypothesis for Earth, Venus, and Mars—it's unclear exactly what fraction of our atmosphere degassed as the final magma ocean crystallized and how much is from later volcanism (limited evidence is that Earth's atmosphere has not changed much in mass over time)

Thank you for highlighting this connection. We agree that there is important overlap with suggestions pertaining to the inner Solar System planets, so have now included a short discussion on this connection near line **264**.

2) I do find that the framing of the state-of-the-field to be somewhat misrepresentative since it appears to portray two proposed ideas as in competition when I think the field has recognized that there are likely more than one pathway to forming planets of these radii. so this paper seemingly misstates the current state of the field—these ideas are not competitive but accretive. Relatedly, this paper seems to confuse the proposed origin of the smaller-than-the-radii-gap planets. This population is made of planets that formed below the gap and stayed there as well as planets were in the gap and couldn't maintain their atmospheres. There is evidence that the gap widens over time and the authors already cite the evidence for this [16] and [25]. All this is to say that all these ideas appear to work in conjunction with each other, yet this paper is written as if they exclude each other, which I find jarring and confusing.

This is a fair assessment. While there are some works which argue in favour of one paradigm versus the other without permitting both as separate populations (e.g. Bean+20, Rogers+25, Luque&Palle+22, Otegi+20), some recent works support the view that both water-worlds and gas-dwarfs exist together, and that multiple pathways operate to shape the SN and SE populations. However, our hybrid planet evolution scenario does not fall into either of these categories, so we have re-framed part of our abstract (line **28**), literature review (near line **50**) and concluding remarks (line **385**) with a more representative description of the literature.

3) The modeling of the mantle is done assuming that the atmosphere and the molten portion of the mantle are in chemical equilibrium? If so, this should be stated plainly. If not, then the relationship should be better explained. Is the solidified mantle considered chemically inert after crystallization?

Yes, we assume that volatiles partition in equilibrium between the atmosphere and molten portion of the mantle, which does not degas anymore after solidification, and the mantle itself is treated as chemically inert. We have clarified this in the Methods section near line **422**.

4) For the successful models described in 106-108, is it contraction or atmospheric loss that drives the bulk density change of the planet? If contraction, what role does loss play at all?

Both processes drive a long-term decrease to planetary radius, increase in bulk density, and change in atmospheric MMW. We qualitatively illustrate when each process 'dominates' in **Fig 3** using the pink arrows with annotations. However, we have now expanded on this description in the main text near line **242** and included a new section in the SI from line **921** to quantitatively compare these factors using a new figure (**Fig 9**).

5) In these high H mass cases, does de-gassing of the mantle play a significant role at all in the radius? How does the mean molecular weight of the atmosphere change in these cases (a figure showing MMW as a function of time may help tell the story for each of the different evolutionary cases)?

Degassing does impact the planet's radius at different points in time. This question is linked to point (4) above, for which we included a new figure (**Fig 9**) and discussion in the SI to demonstrate this for our case study. However, this behaviour varies in different cases because of the physics, which we further discuss from line **947** and illustrate in **Fig 10** by plotting modelled isochrones.

6) It may be worth pointing out to the reader that 10,000 ppm H2O is considered a good nominal estimate of Earth's water content (e.g. Hirschman; and also Mars), so less than 10,000 ppm is a dry planet (like the Moon).

A comparison of the planet's H content to its total mass is a valuable point of discussion considering the significant potential for H storage into the core. We have now updated the text near line **369** to highlight this point.

7) Regarding, "The super-Earth (radii ≲ 1.7R⊕) and sub-Neptune (radii~ 1.7– 4R⊕) exoplanet populations have no Solar-System analogues …" is a frustrating tautology that gets repeated in the exoplanet literature like it's a major insight. The words super-Earth and sub-Neptune clearly already imply that there is no Solar System analog.

Thank you for highlighting this wider issue which is common across the literature; we agree that this phrasing is tautological. We aim to highlight for the wider audience of the journal that exoplanets with radii between ~1.5 and ~4 R⊕ are important because we have no such planets in the Solar System. The text has been adapted accordingly in this opening paragraph, near line **49**.

8) "HST observations of the innermost planet (mass 0.47M⊕, radius 0.85R⊕) rule out a low mean-molecular-weight (MMW) primary atmosphere [27], but recent JWST transit spectroscopy has indicated an SO2-rich atmosphere [28]." It's unclear why this planet is described in this paper. Is the point that SO2 has been found elsewhere in the system? Perhaps the authors could add a clause explaining why discussing the other planet is relevant.

Yes, here we provide additional motivation for our discussion on sulfur chemistry, with the suggestion that this system may be sulfur rich. We have clarified this motivation on line **67**.

9) On line 374, what do the authors mean by nominally uses this other model. Is another model actually used?

We use the SPIDER interior evolution model in all cases, which is part of the PROTEUS modelling framework. We have clarified the text on line **420** to reflect this.

10) What tidal parameters are used to describe the planet's Maxwell Rheology? How do these choices effect the outcome?

We have now clarified our tidal modelling in the Methods section near line **439**, which explains how the material parameters and Maxwell timescale are derived. Other parametrisations of viscoelastic response (e.g. Andrade, Sundberg–Cooper) are known to yield even larger tidal heating fluxes, which we now discuss in the same paragraph near line **442**.

Responses to final comments from Reviewer #1

1) I did not find a mention on how the initial atmosphere is set up. I imagine that it forms purely from the volatile oversaturation of the magma ocean, and that no accreted atmosphere remnant is considered. This should be stated somewhere (e.g. around l. 423).
This is correct. We have now clarified our initial conditions in this section of the text (subsection entitled “Modelling framework”).

2) l.69: Consider adding something akin to "... for the available lowest density estimate" to the parentheses. Otherwise, this is a bit at odds with the later statement of using a density of 3.45 g/cm^3.
We have clarified in the text that this value is an alternative recent estimate, in comparison to our main analysis which adopts a more conservative value. Both estimated densities necessitate substantial volatiles.

3) l.84: State the actual density value and reference here.
We have specified the density of 2.2 g/cm^3 here, citing the observational analysis by Cadieux+2025.

4) Fig.11: "Core [%]" should be "Core radius [%]"
We have updated the figure panel y-axis with this more-descriptive label.