

Terrestrial atmospheric ion implantation occurred in the nearside lunar regolith during the history of Earth's dynamo

Corresponding Author: Shubhonkar Paramanick

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Dr. Paramanick,

Your manuscript titled "Terrestrial atmospheric ion implantation occurred in the nearside lunar regolith during the history of Earth's dynamo" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers. You may want to mainly consider the following three issues in revision:

- 1) Demonstrate that your treatment of the upper atmosphere in your models is robust and consistent and that your conclusions are therefore reliable.
- 2) Fully explain your simulations and justify or amend any inherent simplifications.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Major comments:

The authors tried to explain how Earth's intrinsic magnetic field affects the transportation of solar wind and Earth wind to the Moon, and then study how both "winds" affect the composition of the lunar soil via MHD simulations. This topic is very challenging because one has to address the coupling of the atmosphere, ionosphere, magnetosphere and solar wind in numerical simulations. There are a huge bunch of breakthroughs that should be made in numerical simulations, or the studied problem should be much simplified to be suitable for a multi-fluid MHD, PIC or other models in which multiple ion species are included. Here, the authors tried to solve this problem with a single-fluid MHD. It's an interesting trial, but may be not suitable to address the topic. Besides, the organization and writing of this paper are not friendly to readers. Those tables are really hard to understand.

1. There's a cutoff boundary for the intrinsic magnetic field, which is not a common treatment in simulation works of magnetospheres. This could introduce unphysical effects, why don't you just use a dipole field?
2. Please verify your magnetized simulation cases. Please compare your results with other mainstream simulations of Earth's magnetosphere and prove the validation of your model.
3. The laws of charged and neutral particles are different, please check the equations in your method section.

Minor Comments:

1. Line 57-58: Where do you get this conclusion? Seki et al., 2001 only discussed the possibility but has no evidence. Thus, it's not appropriate to use such an affirmative tone.
2. Line 96: What do the two "Case-I" mean? Are they the same one? Please reconsider the names of your cases. It's really confusing.
3. Line 105-107: If the upstream solar wind conditions are different, how can you compare them?
4. Line 110: Why the total atmosphere mass is the same for the two cases?
5. Line 268: Why do you set a cutoff boundary at this value?
6. Line 275: Does Earth rotate during the simulation?

7. Line 296-298: How does a plasmasphere form without an intrinsic dipolar magnetic field? Is there a corotation electric field? What's the definition of plasmopause boundary in the unmagnetized case? Why do you suppose the mass within the magnetopause in the magnetized case and the mass within the plasmopause in the unmagnetized case are the same?

8. Line 305-307: Where do you get this? If the thermal pressure of the Earth's atmosphere means the sum of the thermal pressures of the neutral and ionized particles, the equation you got here is not correct, please verify it.

9. Line 330-332: What is the physical meaning of this cutoff boundary?

10. Line 334-337: Does this explanation also work for unmagnetized cases? The macroscopic scales you used here are too large; It's not the appropriate scale for this study.

11. Line 410: What's the value of the simulation time? And how about the timestep?

12. Table 2: Why do you use different upstream solar wind conditions in the same comparison case of magnetized Earth and unmagnetized Earth?

13. What about the inner boundary around the planet? How do you treat the plasma around this boundary?

14. Figure 5: In panel a) Why there is a disk with high density and a sharp boundary around Earth? Where's the ring current? In panel b) Why is there a dipole-like field in the XY plane? Please check the coordinate system you used here. In panel c) Why does solar wind plasma flow out of Earth? In panel d) Why Earth wind plasma flows into the magnetosphere through the magnetopause? These results are strange, please verify your model.

Reviewer #2 (Remarks to the Author):

This manuscript uses a 3-D MHD model to investigate terrestrial ions implanted into lunar surface regolith during the different periods in Earth's dynamo history. They construct different models of ion transport in the Earth wind based on previous research on the evolution of magnetic fields and atmospheres in the Earth-Moon system. And they find that atmospheric transfer is efficient only when the Moon is within Earth's magnetotail. This research also suggests the Earth wind can be implanted into lunar regolith during the long history of the geodynamo, so the history of the terrestrial atmosphere could be preserved in buried lunar soils. This study will be beneficial to analyze the evolution of the Earth's atmosphere and magnetic field through lunar surface materials, and thus gain a deeper understanding of the evolution of the Earth-Moon system. This topic is very important and the results look very interesting. I highly recommend this article be published in this journal.

However, I have a major concern about some of the main results and statements presented in the manuscript. In my mind the following issues should be addressed before publication.

Line 45-46: The authors have mentioned Li et al. [16] attributed their observations of lunar surface water to the solar wind as the singular source during the Moon's passage through the magnetotail. However, another paper suggests that terrestrial ions in the magnetotail will be a possible source of lunar surface hydration which is lack of quotation.

Wang, H. Z., et al. (2021). Earth wind as a possible exogenous source of lunar surface hydration. *The Astrophysical Journal*, 907(2), L32. <https://doi.org/10.3847/2041-8213/abd559>

Line 55-58: In fact, sometimes the Moon is out of protection against solar wind at midnight region. Therefore, we believe that the author ought to incorporate the following quotation to render the article more rigorous.

Shang, W. S., et al. (2020). Unusual location of the geotail magnetopause near lunar orbit: A case study. *Journal of Geophysical Research: Space Physics*, 125, e2019JA027401. <https://doi.org/10.1029/2019JA027401>

line 124 : Do the authors consider the distance change of Earth and lunar in your model? How does it affect their model result?

line 197: Do the authors consider the atmosphere evolution in their model? Ancient Earth may have different composition of atmosphere, does it affect much of the inaccuracy of your model?

Figure 2: Could you explain when phase angle eq 0, why the Earth wind and solar wind flux are both little?

Line 231-262: Earth wind not only could implant particles into the lunar nearside surface, it also has earthward flux produced by magnetic reconnection in the distant magnetotail that could implant particles into the lunar farside surface. Could the author discuss the possible influence of these earthward flux and the asymmetry of atmospheric transfer on the lunar nearside and farside?

Zong, Q. G., et al. (1998). Energetic oxygen ion bursts in the distant magnetotail as a product of intense substorms: Three case studies. *Journal of Geophysical Research: Space Physics*, 103(A9), 20339–20363. <https://doi.org/10.1029/97JA01146>

Kiehas, S. A., et al. (2018). Magnetotail Fast Flow Occurrence Rate and Dawn-Dusk Asymmetry at XGSM ~ -60 RE. *Journal of Geophysical Research: Space Physics*, 123(3), 1767–1778. <https://doi.org/10.1002/2017JA024776>

Ma, Y., Wong, H.-C., & Xu, X. (2015). Subsonic and sunward-orientated lunar wake observed by ARTEMIS in the geomagnetotail. *Astrophysics and Space Science*, 358(2). <https://doi.org/10.1007/s10509-015-2430-4>

Line 231-262: Now that Chang'e-6 has successfully returned samples from the farside of the Moon, this research has further unveiled the evolution of the lunar magnetic field. I wonder if the author could elaborate in the discussion section on how these new findings impact the author's own results.

Cai, S. et al. A reinforced lunar dynamo recorded by Chang'e-6 farside basalt. *Nature* (2024). <https://doi.org/10.1038/s41586-024-08526-2>

Reviewer #3 (Remarks to the Author):

The paper by Paramanick et al. for the first time investigates the transport of ions from both the Earth wind and the solar wind to the lunar near side by modeling the Earth-Moon environment with the 3D MHD model AstroBear, their own photoionization model, and with atmospheric profiles for the present-day and Archean Earth. These simulations are performed for magnetized and unmagnetized cases to evaluate the effect of Earth's magnetosphere on the transport of ions to the Moon. In addition, the authors also vary solar wind conditions, atmospheric composition, Earth's surface atmospheric mass density, and the incident XUV flux to evaluate how the Earth wind contribution to the lunar volatile implantation may have changed from the Eoarchean to the present-day and to calculate possible scenarios for which the lunar isotopic signatures of N, H, He, and Ar can be explained by a mixture of a non-solar component (that is, the Earth wind) and a solar component (the solar wind) based on isotope mixing diagrams.

As their main outcome, they find that the isotopic signatures in the lunar regolith can indeed be explained by such a mixture, but only for cases where the Moon crossed the Earth's magnetotail, i.e., when a geodynamo existed. In contrast, an unmagnetized epoch during the Eoarchean cannot lead to the observed lunar isotopic signatures. In addition, their study suggests that the contribution of the Earth Wind strongly depends on the Earth's exobase altitude, which according to their present simulations cannot have been below 190 km at times of ion implantation onto the Moon. As a third outcome, that I find very much worth highlighting, their simulations also indicate that an intrinsic magnetic field provides a net shielding effect for the atmosphere, at least for their specific atmospheric, magnetic field and solar (solar wind ram pressure and XUV flux) parameters.

I find the study very interesting and timely. It is also novel; although it is not the first study that suggests that volatile implantation from Earth's atmosphere to the Moon is happening through Earth's magnetosphere instead of from an unmagnetized early Earth, it is indeed the first to actually model both Earth Wind (for unmagnetized and magnetized cases) and solar wind implantation onto the Moon for trying to resolve the puzzle of the isotopic signatures of volatiles in the lunar regolith. It was first suggested in 2005 by Ozima et al. that terrestrial volatiles may contribute to this signature, and the current study can add important results to (i) this still ongoing discussion, and (ii) our understanding of the Archean Earth's atmosphere. This is also timely as new in-situ data from the Moon (as from Chang'e 6) is finally becoming available to give further insights into this topic. The study further adds to another hotly debated topic, i.e., whether an intrinsic magnetic field protects an atmosphere.

However, I am unsure whether their current results are reasonable since there are inconsistencies in how the authors treat the upper atmosphere in their study (and/or how they describe the treatment of the upper atmosphere in the present version of the manuscript). I therefore suggest major revisions since a better implementation of atmospheric profiles may alter their results and conclusions substantially.

I have three major comments/suggestions and some additional minor ones that are mostly related to the treatment of the atmosphere in their model and the manuscript's readability.

Major comments:

1) The authors implement the barometric formula for calculating the atmospheric mass density/pressure from the surface of the Earth up to either (i) the altitude where the magnetic pressure of Earth's magnetosphere equals the atmospheric pressure (that's for the magnetized cases), or (ii) the altitude where the solar wind ram pressure equals the atmospheric pressure (for the unmagnetized cases). For both scenarios, the respective pressure equilibria are well beyond the exobase level. This is problematic. The barometric pressure equation loses its validity long before the magnetic field determines the global plasma structure and/or before a pressure equilibrium with the solar wind ram pressure is reached. The exobase is defined as the boundary in the atmosphere where the gas becomes collisionless (as correctly defined in the manuscript!). As soon as this becomes the case the barometric pressure equation cannot be applied any longer as the particles will start to move on ballistic orbits and the temperature between particles cannot be appropriately equilibrated within the gas as no (or very little) collisions take place. At this point, Equation (3) will fail to give accurate results (it will fail much earlier if no variation in temperature, and less so, in molecular weight will be considered). In reality, Chamberlain profiles should be used above the exobase (e.g., Chamberlain 1963). This may lead to quite different atmospheric profiles to the ones displayed in Fig. 1 and could further lead to very different results. I therefore suggest implementing this properly.

2) First, I am not sure whether I correctly understand how the exobase levels are varied. Either it is not explained properly in the manuscript, or it is performed incorrectly. At least as I read the manuscript, you simply assume the exobase to be at

various heights and further assume that all ions above the exobase escape. But based on Equations (5) and (6) you do not change the atmospheric pressure profile together with the exobase, correct? This would be wrong as the exobase is always where the atmosphere becomes collisionless. However, if you always use the same pressure profile but arbitrarily vary the exobase level, this is not the case. So, this should either be explained more properly (if you do it correctly but just didn't write it) or the change in exobase height should be coupled to appropriate pressure profiles to be consistent, i.e., to a pressure profile for which the atmosphere indeed becomes collisionless at the exobase altitude. From Fig. 4, one can see that the total ion escape rates from the atmosphere decrease for increasing exobase altitudes, which is a very strange behavior, as it should be the other way around. Such a behavior can already be easily seen by looking at the escape parameter λ , which decreases for increasing exobase radii and temperatures and decreasing gravity. And lower λ values mean more escape (for the Jeans escape parameter λ , see, e.g., Volkov et al. 2011, or, more recently Guo 2024, among many others). In addition, if your atmosphere becomes collisionless at the exobase, the exobase densities should be roughly similar (well, not entirely as the scale heights can be different). But if you always use the same pressure profile and arbitrarily choose your exobase, the density at such an "exobase" will become larger the more you decrease its altitude. Obviously, more and more atmosphere will be above the exobase and more will escape by definition, contrary to what one would expect. So, if you have not treated this properly, it could have strongly affected your results and you should rerun your simulations at least with proper atmospheric profiles (with the barometric equation up to the exobase, and with Chamberlain profiles above it). If you treated it properly, please describe it in more detail in the manuscript.

Second, if you want to be consistent, you cannot vary the exobase arbitrarily. The thermospheric structure (and hence the altitude of the exobase; but also its temperature) depends on atmospheric composition and the incident XUV flux (and on other things of course, like the planet's gravity). This means that you cannot assume arbitrary exobase altitudes for the Eoarchean and for the present. Earth's present N₂-dominated atmosphere, for instance, does not have an exobase altitude of only 250 km (the altitude that matches best the isotope data in your simulations) as N₂, the low abundances of CO₂ and other IR-coolers do not sufficiently cool the thermosphere. The incident XUV surface flux should be very low for this, lower than at present (see, e.g., Tian et al. 2008, and Johnstone et al. 2018, for how the XUV flux and CO₂ cooling affect the thermosphere). In contrast, the exobase altitude will increase if you increase the XUV and decrease if you increase the CO₂ abundance. The XUV flux in the Eoarchean was much higher than at present, likely more than an order of magnitude compared to today (e.g., Tu et al. 2015; Claire et al. 2012). So, from an aeronomical point of view, it is quite unlikely that Earth's volatiles were implanted onto the Moon for an exobase altitude of 250 km. You already utilize the atmospheric composition for the Eoarchean from Johnstone et al. (2021); in this paper you can also see how the exobase altitude varies as a function of CO₂ mixing ratio and the X-ray surface flux during the entire Archean eon (their Fig. 4). You can simply use exobase altitudes from this paper, and it would be much more consistent than the arbitrary exobase altitudes you implemented for the current manuscript. I highly recommend this, as this could significantly alter your results. And you may even be able to find atmospheric cases that can be excluded, which would be a great additional outcome!

3) I find the main body of the manuscript quite hard to understand and follow from time to time. It gets clearer after reading the methods, but I find it almost impossible to understand without reading the methods sections. For instance, you suddenly speak from 'Case 1' but it is neither explained what this is, nor that it is explained in the methods and in Table 2 (well, it is linked later in the manuscript). A brief explanation of what this means and a link to Table 2 would already be quite helpful. You only mention that you made mixing curves for different exobase heights in the main body but you do not describe how these different exobase heights are linked to your model (and as written above, it still isn't clear to me after reading the methods).

Another example is the combination of your photoionization model (and NRLMSISE-00) with your MHD model. In the main body, you only write that you combined these models; although you explain it in the methods in more detail (Section 4.2.3), I am still not sure whether I understood it properly. You obtain an escape flux that impinges onto the lunar surface from your MHD model. Then you utilize NRLMSISE-00 (at least for the present-day) and your photoionization model to calculate ionization and the relative fractions of the various species (since the MHD does not distinguish between the different species) above the exobase and assume that everything above the exobase escapes in your ionization model. Do I understand correctly that you obtain (different) escape rates from the MHD and the ionization model (together with the NRLMSISE model)? And then you scale the escape from the ionization model to match the MHD results, is that correct? Do the arbitrarily chosen exobase levels also feed into the MHD model? Related to this, I am also not sure where the numbers in Table 3 and ED3 (and ED4) come from. Let's take N: In Table ED3 the EW flux impinging onto the Moon for N comes from multiplying the total EW flux from the MHD in Table 2 with the relative fraction of N from ED3 (for an exobase altitude of 401 km). This gives 3.19×10^{-1} for magnetized Case 1. That's clear. The relative fraction N comes from all N⁺ that are above the exobase in your NRLMSISE-00/ionization model, correct? But where is the Earth wind flux for N in Table 3 (i.e., 2.41×10^5) coming from? And does Table 3 also relate to a specific exobase altitude? That still confuses me, and I am not sure whether I understand it correctly. It would be good to make this clearer. I also do not understand which profiles you use for the Archean Earth cases, as you cannot do this with NRLMSISE-00. They have to come from somewhere else.

So, all in all, I think that too much might have been moved to the methods for the main text to be smoothly understandable. I give minor comments below, some of them are also related to, at least for me, unclear understanding.

Minor comments:

L6: Reference for "the latter sourced in the absence of geomagnetic field"? I assume this refers to Ozima et al. (2005). However, Terada et al. (2017) already suggested that ions were transported to the Moon via the magnetotail. Wei et al. (2020) even suggest that ions could have been implanted into the Lunar far side. Also, Lammer et al. (2022) give arguments on why ion implantation through the magnetotail could have been significant in the past. Cao et al. (2024) review key

questions of the Earth Wind-Moon system mostly based on implantation through Earth's magnetotail. This manuscript is hence not the first to suggest this idea (but probably the first to test it!).

L14-16: It would be very interesting to see if this statement is still true if one also considers highly extended atmospheres (as, e.g., in Johnstone et al. 2021) on an unmagnetized early Earth for which escape rates would also be higher.

L19: The statement that the exobase was never smaller than 190 km is not surprising. The CO₂-dominated atmospheres of Venus and Mars have exobase levels around 200 km. Pure CO₂ atmospheres on Earth may have somehow lower exobase levels. As low as 190 km, or even lower, is only possible for CO₂-dominated atmospheres and low XUV fluxes. As the XUV flux, however, was higher in the Eoarchean, even the exobase level of a CO₂-dominated atmosphere would have been >200 km. You can nicely see this in Johnstone et al. (2021), the paper from which you took the Eoarchean's atmospheric composition. Exobase levels in the early Archean eon were always (much) higher than 190 km (see again their Figure 4). The exobase altitude for the specific case you took for your simulations is ~800 km!

L44: Is reference 14 (Poppe et al. 2021) the correct reference here? It is about lunar paleomagnetosphere simulations. Instead, Poppe et al. 2016 (<https://ui.adsabs.harvard.edu/abs/2016GeoRL...43.6749P/abstract>) talk about the detection of terrestrial magnetotail ions and may be the correct reference here.

L45-46: Wang et al. (2021), on the other hand, suggest the Earth Wind from the magnetotail to be a source for the lunar water!

L58-60: It is misleading to compare atmospheric loss rates from Earth, Venus, and Mars at present to argue that magnetic fields are not shielding the atmosphere. One crucial aspect that is often completely overseen, when discussing this aspect is atmospheric composition. Mars and Venus have a CO₂-dominated atmosphere whereas Earth's is N₂-dominated. Loss rates from Earth, Venus, and Mars could look entirely different if they interchanged atmospheric composition. It is not only the difference in molecular weight between CO₂ and N₂ but, more importantly, their role in shaping the thermal structure of the upper atmosphere since CO₂ is an IR-coolant whereas N₂ is not. Earth's upper atmosphere is hot and quite expanded (with an exobase temperature, Texo, around 1000 K and an exobase radius, Rexo, around 500 to 1000 km), whereas the upper atmospheres of Venus and Mars are compact and cold (with Texo and Rexo being around 200 K and 200 km in both cases). Give Venus or Mars an N₂-dominated atmosphere and escape rates increase. An N₂-dominated atmosphere at Venus could have values of Texo above 2000 K and Rexo around 1000 km due to the increase in the incident XUV flux. this would certainly affect atmospheric escape. The similarity of escape rates from Venus, Earth, and Mars might hence be pure coincidence, and the role of the magnetic field remains very poorly understood as long as no simulations are performed that study the effect of intrinsic magnetic fields for various atmospheric compositions, incident XUV fluxes, and solar wind ram pressures. For higher XUV fluxes and, therefore, more extended N₂-dominated atmospheres, an intrinsic magnetic field could have been an important shield against non-thermal escape. So even if the intrinsic magnetic field might not be important for atmospheric shielding at present, it could have been in the distant past. Way et al. (2023) discuss atmospheric composition in this context and it is worth highlighting in the manuscript.

L96: It would be good to link to the table where the cases are explained.

L102-103: The figure with the phase angle is Fig. 2. Should actually Fig. 2 be referenced here? If not, you may include the phase angle in Fig. 1a.

L105: The validity of this very strong statement ("Only at such early times could the Earth have possibly been unmagnetized") is disputed. It should hence be softened, and/or a caveat should be added and discussed. See, e.g., Fu et al. (2024) for a criticism of the cited paper and the paleomagnetospheric measurements.

L110: What is the atmosphere mass? The mass of Earth's present atmosphere (i.e., a pressure of 1 bar)?

L131. Where are the ancient solar wind data from?

L133-138: I am wondering whether this is still true if the atmospheric density profile is treated properly (i.e., with Chamberlain profiles above the exobase and with the exobase as the region where the atmosphere becomes collisionless).

L138-141: The total escape rates (in 1/s) for the different cases would also be interesting to know so that they can be compared with other studies and potentially also be used in future studies. I am, e.g., curious how the escape rates for the Eoarchean (specifically without a magnetic field) compare to Grasser et al. (2023) who studied polar outflow from Earth's atmosphere during the Archean. Note, however, that the ancient solar parameters in both studies are quite different (the solar wind parameters are from Johnstone et al. 2015).

L140-11: If there is indeed a net protection effect of the intrinsic magnetic field that's worth highlighting more prominently (and also writing it in the abstract)! I am curious whether there is indeed a protective effect for more extended atmospheres in the Eoarchean.

L142-144: This also means that the surface pressure is reduced significantly, right? I am not sure whether this behavior remains if the atmospheric profile is treated properly. Since the exobase always has comparable densities, and the upper atmosphere profiles usually behave similarly for different surface pressures, I would be surprised about such strong differences in escape rates. For instance, Tian et al. (2009) simulated CO₂-atmospheres at Mars for different atmospheric

surface pressures (6 mbar, 1 bar, 3 bar) and found a very similar behavior of the upper atmosphere including similar escape rates. Also, Mars and Venus show a difference in surface pressure of more than 3 orders of magnitude but escape rates are similar (well, that may again be a coincidence).

L177: The non-solar component is compared with the EW from the magnetized case and with the EW from the unmagnetized case, correct? If so, I assume that an "and" is missing in L177, and it should hence be "magnetized Earth and with those".

L189: "in the present-day atmosphere" is slightly confusing. I suggest writing "in the present-day atmosphere case".

L195-196: This conclusion could be different if the present and Archean atmospheres are treated properly. Usually, atmospheric escape tends to increase for more expanded atmospheres, i.e., for higher exobase levels, as should have been the case for the Eoarchean. In this manuscript, however, escape increases with decreasing exobase levels.

L198-199: As already written earlier, I am slightly confused and not sure whether I correctly understand how the exobase levels are varied.

L202-205: During Earth's history, its exobase altitude has likely been above 300 km most of the time - if not always; specifically for N-dominated atmospheres (see major comment 2). So, I am worried that this is not correct because of treating the atmosphere and the position of the exobase altitude erroneously. The same goes for the other exobase altitude estimates on the following page.

L224-226: As written above, it would be good to use more realistic exobase altitudes for 3.8 Ga, e.g., from Johnstone et al. (2021). In addition, I am also curious how this result would change for a different solar wind ram pressure (e.g., for the ones from Johnstone et al. 2015).

L238: Again, this statement is very strong and disputed.

L239-240: This conclusion may change, if a variation of realistic exobase levels are implemented.

L242: How does a flatter evolution of the solar mass loss rate compare to the solar wind values used for the Eoarchean? It would be interesting to make simulations with lower solar wind ram pressures.

L246: The classic late-heavy bombardment did likely not take place, e.g., Zellner et al. (2017), Hartmann (2019), Worsham & Kleine (2021), Cartwright et al. (2022). The LHB, however, is still often used as a constraining event, e.g., in biology to determine the earliest possible origin of life. It is therefore important to highlight its very uncertain existence.

L251-255: This should be stated in the abstract, as it is an important result!

L261-262: Yes, because in such a case atmospheric escape would always be insignificant.

L279-280: This is certainly true for the present. For the Eoarchean or even Hadean eon, it may not be. At least for certain atmospheric compositions and/or assumed XUV fluxes the atmosphere may turn hydrodynamic or can be in a transition regime between hydrostatic and hydrodynamic with a shifted Maxwellian. The Johnstone et al. 2021 article nicely shows that atmospheric composition and the XUV surface flux matter a lot during the Archean for whether your atmosphere will be hydrostatic or not. So, this assumption must not necessarily be valid for the Eoarchean. However, the 50% CO₂/50% N₂ case taken from Johnstone et al. (2021) is hydrostatic.

L287: dominates -> dominate

L283-284: The scale height H depends on atmospheric temperature. Was this set to be constant over the entire altitude or was a specific temperature profile used? If so, which one? The temperature of the thermosphere can vary substantially in dependence of the incident XUV flux and atmospheric composition. In addition, the scale height varies for each species above the homopause as it depends on the mass of the respective species, which can also lead to fractionated atmospheric escape of, e.g., ¹⁴N and ¹⁵N. But I see that you anyway took care about calculating different scale heights for the main species in the upper atmosphere (L450 onward)!

L288: The barometric pressure equation loses its validity long before the magnetic field determines the global plasma structure and/or before a pressure equilibrium with the solar wind ram pressure is reached, see major comment 1.

L435 form of the -> forms of

L433-436: But an increase in the incident XUV flux will also change the structure of the thermosphere significantly. An increase in XUV will heat up and expand the atmosphere. And these effects will be different for different atmospheric compositions. So, you cannot change the ionization fractionation without changing the profile of the atmosphere consistently!

L468-471: Exactly! But is this also implemented correctly in your simulations? From reading the manuscript, I get the feeling that it is not.

L488-490: It would be good to mention this in the main text. From Fig. 4, this was not clear to me.

L508: With the model of the early Archean homosphere you simply mean the atmospheric composition of the atmosphere (i.e., homosphere), right? As already written, it would consequently be great to also implement the exobase levels from this study for the various XUV fluxes and atmospheric compositions instead of arbitrarily varying the exobase altitude! It would then be very insightful if some of their scenarios would be favored/disfavoured by your model.

L510: To be consistent with Johnstone et al. 2021, you can then also cite Tu et al. (2015) and Claire et al. (2012) for the early solar XUV flux.

L517-519: I assume that ^{36}Ar is meant here? Then it should be also written explicitly as Ar in Earth's present atmosphere is dominated by ^{40}Ar . ^{40}Ar should have been different in the Eoarchean as it comes from the radioactive decay of ^{40}K (with a half-life of ~ 1 Gyr).

Fig. 1: The axis descriptions are very small. " $\times 10^7$ " in Fig. 1b is relatively hard to read because of its smallness and the line that goes directly through it. What is the actual mass of the atmosphere and hence its pressure? 1 bar? The pressure axis stops at 10^4 Pascal, that is 0.1 bar.

Fig. 2 and ED2: Same problem with axis descriptions ($\times 10^n$ is very small). What should the unit $1/(\text{m}^2\text{s})$ be, a typo I assume? It should be $1/(\text{m}^2\text{s})$ for a particle flux. However, in the figure caption it states "mass flux". Then it should be $\text{kg}/(\text{m}^2\text{s})$. The same strange unit appears in Table 2. I further assume Figure 2 shows the magnetized Case-1? Please state that explicitly in the caption.

Fig. 3b and 4b: Where are the underlying profiles for the Archean Earth coming from? And do these profiles take account of the proper atmospheric composition and expansion due to the XUV flux? What is the exobase altitude of the implemented profiles (because it would look different for different exobase altitudes, same for the present-day!)?

Fig. 4: The escape flux unit should again be corrected, i.e., to $1/(\text{cm}^2\text{s})$.

Do these figures directly give the escape flux from the atmosphere for the exobase altitudes, or do I have to integrate the lines above the exobase to obtain the escape flux? That is, if I look at the figure at, e.g., 400 km, is the total escape flux from the atmosphere displayed on the x-axis, or does the x-axis only show the escape flux from this certain altitude and I need to integrate over the whole line above this exobase altitude to obtain the total escape flux? This is not clear to me.

Then, the ion escape rates seem to be extremely low, for the Archean Earth case (compare, e.g., with Grassler et al. 2023) but also for present-day. For O^+ , escape rates are observed to be in the order of $1\text{e}24$ to $1\text{e}26$, depending on wind conditions (e.g., Schillings et al. 2019; Slapak et al. 2017). For N^+ , it is comparable or slightly lower (e.g., Yau et al. 1993). Your ion escape rates seem to be clearly below this, at least in Fig. 4. It would generally be good to compare your rates with present-day observations. If they are far off the observed values, it may have to do with the potentially wrong treatment of the exobase?

And does this plot really imply that the escape becomes lower for increasing the exobase altitude? I'd really be surprised if this is correct.

Fig. 5 and ED3: There is a typo. upto $\rightarrow$ up to

Fig. 6 and ED4: What are the references for the Ne, He, and Ar solar ratios? And for Earth's Ar? What is the reference for the Archean Earth data? In general, I wouldn't be surprised if these plots were to change significantly with an upgraded treatment of the atmosphere.

Table 2: It would be nice to also include the total escape flux (as $1/\text{s}$). There is again the wrong unit for the flux. And it would be good to give references for the ancient solar wind density. The solar wind velocity likely also increased toward the Archean (e.g., Johnstone et al. 2015).

Table 3: Again the wrong unit for the fluxes.

Fig. ED5: Also this figure may change with updated simulations.

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Version 1:

Decision Letter:

Dear Mr Paramanick,

Your revised manuscript titled "Terrestrial atmospheric ion implantation occurred in the nearside lunar regolith during the history of Earth's dynamo" has now been seen by the original 3 reviewers, whose comments are appended below. In light of the comments raised we are unable to publish your article.

You will see that although the reviewers appreciate the effort you put into the revisions, and reviewer 2 now recommends publication, reviewers 1 and 3 continue to raise concerns about the robustness of your analysis and modelling approach, and therefore the robustness of your conclusions. Given that these issues have been raised before and not fully addressed, we are unable to conclude that your findings are sufficiently compelling for publication in Communications Earth & Environment to be appropriate.

We are sorry that we cannot be more positive on this occasion and thank you for the opportunity to consider your work.

Best regards,

Prof Ke Zhu PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0003-3613-7239

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

Major Comment:

The authors attempted to investigate ion implantation in the lunar regolith by integrating atmospheric models with a single-fluid MHD model. However, several issues need to be clarified by the authors: First, are both the solar wind and the ionosphere plasma sources for the MHD model? If that is the case, where is the inner boundary of the MHD model located near the planet, and how are the initial parameters at the inner boundary determined? Since it is quite challenging for the solar wind to penetrate the inner magnetosphere in MHD simulations, ring current models are typically employed in such simulations. Is there any analogous treatment of the ring current in this study?

Although the atmospheric models and the MHD model are both valid, if the coupling or combination of them is not suitable, the results will not reflect the real situation. Since the single-fluid MHD cannot distinguish ion species, the dynamics of Earth's wind ions cannot be captured. So why can the flux of Earth wind ions be extracted? If those atmospheric models provide the inner boundary for the MHD simulations, then a multi-fluid MHD model may be more suitable for studying this topic. If those atmospheric models do not provide the inner boundary, and the single-fluid MHD model does not contain the physics of Earth ions, then extracting Earth wind from the MHD model via atmospheric models is not reliable.

To study the material transportation in Earth's space environment via the coupling of atmospheric models and MHD models is a really promising way. But there are a lot of challenges. This paper has raised an interesting topic and has made a lot of efforts, but the implication of the combination of an ionization model and a single-fluid MHD has been called into question.

Minor Comments:

1. Line 98, n_{sw} n_{ew} ?
2. Line 317: There is no need to set an outer cut-off for the dipole magnetic field in MHD simulations. Such a cut-off hasn't been treated as a conventional method.
3. Line 347-348: The term "plasmopause" has been clearly defined in Space Physics. Please revise this sentence. And the plasmopause is not the interface between Earth's plasma and the solar wind plasma.
4. Line 362-363 (Equation 12): The boundary between the solar wind and Earth's plasma environment should be the magnetopause. The subsolar point of this boundary is conventionally considered the position where the solar wind ram

pressure and the magnetospheric magnetic pressure are balanced. Additionally, please verify the relationship between thermal velocity and thermal pressure.

Magnetic pressure does not influence neutral particles. If you take neutral particles into account, the collisionless regime may not be appropriate, as collisions between neutral particles and ions must be considered. However, if the regime is assumed to be collisionless, the pressure of neutral particles should not be included in the equations. In reality, the magnetosphere near the subsolar point of the magnetopause is almost empty.

5. Line 386: Equation 2 does not describe the magnetic field. Please verify it.

6. Line 456-463: The description of the grid is overly complicated and could be better explained with a schematic diagram, such as Figure 1 in Jia et al., 2015 (<https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2015JA021143>).

7. Section 4.2: A brief and clear explanation of how the density of Earth ions is determined or calculated is needed. Additionally, the corresponding equations should be provided.

8. Figure 5: These four panels are similar; please use the same coordinate system. Panel (a): The disk-like region must be carefully checked. As the authors mentioned in their response, the total density provided by the simulations is $10^4\text{--}10^5\text{ cm}^{-3}$, which is several orders of magnitude higher than the density of the plasmasphere ($\sim 10^2\text{ cm}^{-3}$). Therefore, the authors must carefully examine their simulations and test their model against space observations.

9. Table 1: Different orientations of the interplanetary magnetic field (IMF) should be considered. It should be noted that the orientation of the IMF has a significant effect on the magnetosphere. When the IMF is southward (northward), the magnetosphere is most active (quiet). Based on Figure 5, it appears that the authors only considered the northward IMF, which results in a quiet magnetosphere. Since the authors are studying a phenomenon that spans a long period, the effects of IMF orientations should be taken into account.

10. The figure of Extended Data Figure ED1 is somewhat similar to the Figure 1 of Wei et al. (2019). Maybe you have to read their paper and cite it properly.

Wei, Y., Zhong, J., Hui, H., Shi, Q., Cui, J., He, H., et al. (2020). Implantation of Earth's atmospheric ions into the nearside and farside lunar soil: Implications to geodynamo evolution. *Geophysical Research Letters*, 47, e2019GL086208. <https://doi.org/10.1029/2019GL086208>

Reviewer #2 (Remarks to the Author):

I have reviewed the revised manuscript titled "Terrestrial atmospheric ion implantation occurred in the nearside lunar regolith during the history of Earth's dynamo" by Paramanick et al. in response to the initial reviewers' comments. The authors have provided detailed and thoughtful responses to all concerns. I therefore recommend accepting this manuscript for publication.

Reviewer #3 (Remarks to the Author):

I thank the authors for thoroughly addressing my comments. I am still surprised about the low modeled escape rates for the Eoarchean era, but they seem to make sense in the author's theoretical framework. Since the model is, to a certain extent, empirical due to relying mostly on lunar soil measurements, which cannot be directly translated into atmospheric data, for reconstructing the ancient "exobase" level, the results for the Eoarchean may further be taken with caution, even though their model provides reasonable outcomes for the present day. Besides this, I am fine with the article, and I only have a few minor comments.

Minor Comments:

L19 and onwards in the manuscript: I don't think "hydrodynamic exobase" is the best name for the given definition. It is very confusing to use "exobase" when it is not an exobase in the conventional sense. So, I suggest again renaming it and not using the term exobase at all.

L63: due to sharp deflection -> due to a sharp deflection

L162: Archean era -> Eoarchean era (the Archean is an eon, the Eoarchean is an era)

L184: integrating dayside -> integrating the dayside

L227 and L227: OHE and PHE are not defined in the text (it is in the methods, but not in the main text).

L306: Archean era -> Eoarchean era

L514: I guess Johnstone et al. is ref [91].

Concerning the escape flux unit in the figures and tables: I know that the unit $1/(\text{cm}^2\text{ s})$ is correct, but I find the "-" in the denominator (i.e., $1/(\text{cm}^2\text{-s})$) very confusing, as it reads as " cm^2 minus s". But I guess the editing will take care of this anyway.

Version 2:

Decision Letter:

**** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors ****

Dear Dr. Paramanick,

Your revised manuscript titled "Terrestrial atmospheric ion implantation occurred in the nearside lunar regolith during the history of Earth's dynamo" has now been seen by our original reviewers 1 and 3 and a new reviewer 4, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Prof Ke Zhu PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0003-3613-7239

Alireza Bahadori, PhD
Senior Editor
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

it seems authors have not made the revision as I suggested, and rebut the each comment I raised.

The MHD scheme this paper adopted is not suitable to address the transfer of atmospheric particles. Authors may need significant revision or need to rewrite this article.

I have no interest to continue the review of this paper, and I don't want to hinder the publication of this paper (I have no conflict of interest with authors), thus, I suggest the editor could seek the other reviewers who are familiar with the MHD or hybrid simulation to review this paper.

Reviewer #3 (Remarks to the Author):

The authors incorporated my remaining minor comments. As already stated in my last review, I am fine with the article and still recommend it for publication.

Reviewer #4 (Remarks to the Author):

After reviewing the entire manuscript and the author's rebuttal from the second round of review, I find the logic and methodology of this work to be reasonable. It offers valuable insights into solar wind-Earth wind-Moon interactions.

The primary concern raised by Reviewer #1 is that this study employs a single-fluid MHD model, which cannot account for the dynamics of different ion types in Earth wind. While a multi-fluid MHD approach would be more suitable for such investigations, this study introduces a "hydrodynamic escape boundary (HEB)" assumption. Above this boundary, solar wind picks up ions from Earth's atmosphere and transports them to the Moon. To my understanding, the MHD simulation in this work focuses on solar wind-Earth wind interactions above the HEB, thereby circumventing explicit modeling of ion-specific dynamics. Within this framework, the derived total Earth wind flux remains acceptable, and the analysis provides meaningful comparisons of lunar surface flux of Earth wind under magnetized vs. unmagnetized Earth conditions.

I only have a few minor comments.

[1] It seems the term "exobase" in the text should still be replaced with "hydrodynamic escape boundary" in certain contexts. For example, in the key words.

[2] Line 46-47: The citation to Li et al. [2017] here is inaccurate, they did not present solar wind as the singular source of lunar surface water, but rather as the primary source, while also highlighting the potential significant role of plasma sheet in the formation of lunar surface water.

[3] Since the escape rates of different ions vary with the HEB height (as shown in Figure 4), the corresponding Earth wind flux should also differ. Please specify the HEB height used to calculate the Earth wind flux values in Table 3.

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Version 3:

Decision Letter:

Dear Mr Paramanick,

We are delighted to accept your manuscript titled "Terrestrial atmospheric ion implantation occurred in the nearside lunar regolith during the history of Earth's dynamo" for publication in Communications Earth & Environment. Thank you for choosing to publish your interesting work with us.

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Joe Aslin

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Response to Comments

May 15, 2025

We thank the editor and referees for their constructive comments and have addressed them in the blue responses below. In the revised annotated manuscript, deleted text is shown with yellow strikethrough, and added text is highlighted in sky blue. Line numbers are from the unannotated revised version of the manuscript.

1 Editor comments

Comment: Demonstrate that your treatment of the upper atmosphere in your models is robust and consistent and that your conclusions are therefore reliable.

Response: We have performed additional simulations incorporating the Chamberlain exosphere model to demonstrate that our earlier approach, which uses the isothermal model, is robust and yields results consistent with the outflow flux obtained using the Chamberlain upper atmosphere model. These new results are consistent with those reported previously. Additional comments are provided below.

Comment: Fully explain your simulations and justify or amend any inherent simplifications.

Response: The simulations are explained in Section 4 (pp. 12–28), with further justifications now provided as described below in our response to the reviewers. Additionally, we have now provided a theoretical justification for the use of the barometric profile in the Archean Earth case in Subsection 4.1.7 and Figure 7 in the manuscript, demonstrating its resemblance to the Chamberlain profiles. Furthermore, we have conducted an additional global 3D-MHD simulation using the Chamberlain model (**Illustration 1**), which shows that the outflow flux is the same as that obtained earlier using the isothermal model, thereby supporting the validity of this simplification.

2 Reviewer – 1

Comment: The authors tried to explain how Earth’s intrinsic magnetic field affects the transportation of solar wind and Earth wind to the Moon, and then study how both “winds” affect the composition of the lunar soil via MHD simulations. This topic is very challenging because one has to address the coupling of the atmosphere, ionosphere, magnetosphere and solar wind in numerical simulations. There are a huge bunch of breakthroughs that should be made in numerical simulations, or the studied problem should be much simplified to be suitable for a multi-fluid MHD, PIC or other models in which multiple ion species are included. Here, the authors tried to solve this problem with a single-fluid MHD. It’s an interesting trial, but may be not suitable to address the topic. Besides, the organization and writing of this paper are not friendly to readers. Those tables are really hard to understand.

Response: Below and in the text, we have further justified our use of the MHD approximation combined with an ionization model that tracks individual ions a posteriori. There are inherent trade-offs between MHD and kinetic modeling approaches. Kinetic codes capture small-scale physics more accurately, but PIC models are not practical for such global simulations where the length scale of the system is large ($\gtrsim 60 R_{\oplus}$). Future work could incorporate kinetic codes to complement the results presented here, but that is beyond the current scope. Importantly, the reason MHD can still offer crucial insight into global dynamics, even for collisionless magnetized plasmas, is that the solar wind is magnetized and the gyro-radii of its particles are much smaller than system scales. As a result, the solar wind captures particles with the gyro-radius effectively replacing the mean free path. This holds true whether or not the Earth’s wind is magnetized. Note that we also combine our MHD model with an ionization model that tracks individual species.

We have now clarified the table captions.

Comment: There’s a cutoff boundary for the intrinsic magnetic field, which is not a common treatment in simulation works of magnetospheres. This could introduce unphysical effects, why don’t you just use a dipole field?

Response: We used the typical Earth’s dipole field from its interior ($\frac{R_{\oplus}}{2}$) to the outside, up to $\sim 20 R_{\oplus}$, which is well beyond the magnetopause boundary, maintaining a value of $31.2 \mu\text{T}$ at the surface (Lines 309-330). A cutoff boundary is introduced near $\sim 20 R_{\oplus}$, beyond which the ambient plasma pressure dominates over the pressure exerted by the dipole field. This transition is sharp, but it is a physically reasonable compromise to ensure that plasma does not rapidly escape along open dipole field lines at the beginning of the simulation (near $t = 0$). Shortly thereafter, the field evolves into an open dipole. As shown in Fig. 5(b), a snapshot of the quasi-steady state, this approach does not introduce any unphysical numerical effects.

Comment: Please verify your magnetized simulation cases. Please compare your results with other main-stream simulations of Earth’s magnetosphere and prove the validation of your model.

Response: We performed comparisons with several widely used simulation codes for star–planet interaction modeling to validate our results. In magnetized cases, our outputs closely match those generated by the BATS-R-US MHD code (Samsonov et al., 2007) and the PLUTO code (Varela et al., 2022). The magnetic field values in our simulations are in good agreement with those reported by Ogino (<https://cidas.isee.nagoya-u.ac.jp/ogino/simulation/simulation2.html>). Moreover, our code, AstroBEAR, has previously been applied to a range of astrophysical problems (see Anand et al., 2022; Carroll-Nellenback et al., 2017). We now summarize this in the manuscript in Subsection 4.1.5.

Comment: The laws of charged and neutral particles are different, please check the equations in your method section.

Response: Although the laws governing charged and neutral particles differ, MHD does not track individual particle species directly but instead approximates the evolution of the average bulk flow. Our Equations 17–20 are valid within the MHD regime (Lines 384-450), which we further justify in Subsection 4.1.5. Below

the exobase, as traditionally defined, collisions justify the use of the MHD approximation, while above the exobase, where particles are largely ionized, the small gyroradii relative to global scales further support the application of MHD, as discussed in the initial response above.

Comment: Line 57-58: Where do you get this conclusion? Seki et al., 2001 only discussed the possibility but has no evidence. Thus, it's not appropriate to use such an affirmative tone.

Response: We have modified the sentence now (Line 60-62).

Comment: Line 96: What do the two "Case – I" mean? Are they the same one? Please reconsider the names of your cases. It's really confusing.

Response: The two subcases correspond to the present day magnetized and early Archean Earth respectively. This distinction of cases also includes different solar wind fluxes. We have now clarified this in lines 98-106.

Comment: Line 105-107: If the upstream solar wind conditions are different, how can you compare them?

Response: The different upstream solar wind conditions in the subcases of Case – I represent the contemporary Sun-Earth-Moon system and the early Archean Earth. Therefore, we used the corresponding parameters for the two subcases as clarified in the previous response point above.

Comment: Line 110: Why the total atmosphere mass is the same for the two cases?

Response: We assumed the same total atmospheric mass for both present-day and early Earth due to the lack of studies constraining their difference and this allows us to compare the resultant planetary outflow in the two subcases with that variable removed. Additionally, we have now highlighted the atmospheric mass value used in Case – I (Line 127-128).

Comment: Line 268: Why do you set a cutoff boundary at this value?

Response: As stated above, beyond this radius, the solar wind ram pressure—or even the ambient plasma and magnetic energy densities—surpasses the dipole field energy density.

Comment: Line 275: Does Earth rotate during the simulation?

Response: Earth's rotation is not included dynamically. For further justification, please refer to Subsection S2.1 in the Supplementary Text (Lines 1163–1167).

Comment: Line 296-298: How does a plasmasphere form without an intrinsic dipolar magnetic field? Is there a corotation electric field? What's the definition of plasmopause boundary in the unmagnetized case? Why do you suppose the mass within the magnetopause in the magnetized case and the mass within the plasmopause in the unmagnetized case are the same?

Response: Plasma is initially trapped on field lines in the magnetized simulations. In the unmagnetized case, the plasmopause refers to the boundary between the plasma cloud and the solar wind ram pressure. Since the solar wind is magnetized, the interface thickness is on the order of the gyro-radius of thermal ions. As mentioned above, we conserve the total atmospheric mass to compare the planetary outflow flux between the two subcases.

Comment: Line 305-307: Where do you get this? If the thermal pressure of the Earth's atmosphere means the sum of the thermal pressures of the neutral and ionized particles, the equation you got here is not correct, please verify it.

Response: In the MHD approximation, the equation is reasonable. As explained in Subsection 4.1, we used

the MHD regime and verified the equations and simulation results with those from Varela et al. (2022) and Samsonov et al. (2007). We then combined our MHD model with our photoionization model (see Subsection 4.2).

Comment: Line 330-332: What is the physical meaning of this cutoff boundary?

Response: Explained above.

Comment: Line 334-337: Does this explanation also work for unmagnetized cases? The macroscopic scales you used here are too large; It's not the appropriate scale for this study.

Response: Yes, the argument holds true in both cases. In this study, we investigate the implantation of terrestrial ions on the lunar nearside. So, the macroscopic scale of the system is defined by the Earth–Moon separation, and we simulate the full planetary flux implantation accordingly. To accurately capture the interaction region, we employed adaptive mesh refinement (AMR), resolving atmospheric scales down to ~ 160 km. These are reasonable scales for our global MHD study.

Comment: Line 410: What's the value of the simulation time? And how about the timestep?

Response: Added to the text now (Lines 466-469).

Comment: Table 2: Why do you use different upstream solar wind conditions in the same comparison case of magnetized Earth and unmagnetized Earth?

Response: This is explained in the table caption.

Comment: What about the inner boundary around the planet? How do you treat the plasma around this boundary?

Response: This has been explained in detail in Subsection 4.1.4.

Comment: Figure 5: In panel a) Why there is a disk with high density and a sharp boundary around Earth? Where's the ring current? In panel b) Why is there a dipole-like field in the XY plane? Please check the coordinate system you used here. In panel c) Why does solar wind plasma flow out of Earth? In panel d) Why Earth wind plasma flows into the magnetosphere through the magnetopause? These results are strange, please verify your model.

Response: The results within the MHD approximation do not seem unusual and are consistent with similar models mentioned above (e.g. Samsonov et al., 2007; Varela et al., 2022).

a) As described above, the number density is relatively low compared to the atmosphere. It is $10^4/\text{cc}$ in the outer magnetosphere and about $10^5/\text{cc}$ in the inner magnetosphere. The structure may appear disk-like due to the large range in the colorbar scale. The ring current is not present because the plot shows number density, not current density. b) The coordinate system has been thoroughly validated and is already explained in detail in Subsections 2.1 and 4.1.5 (Lines 107–116 and 440–458). The dipole-like field represents Earth's magnetic field. The reference systems are also depicted in Figure 1(a). c) The solar wind plasma does not emanate from Earth; rather, it flows around the planet and mixes with the Earth wind. The color bar denotes the flux of the solar wind in the magnetotail, which is already five orders of magnitude lower than the upstream solar wind flux. d) The Earth wind plasma does not flow into the magnetosphere through the magnetopause. Instead, the quivers represent the plasma flow at the boundary due to mixing with the solar wind.

3 Reviewer – 2

Comment: This manuscript uses a 3-D MHD model to investigate terrestrial ions implanted into lunar surface regolith during the different periods in Earth's dynamo history. They construct different models of ion transport in the Earth wind based on previous research on the evolution of magnetic fields and atmospheres in the Earth-Moon system. And they find that atmospheric transfer is efficient only when the Moon is within Earth's magnetotail. This research also suggests the Earth wind can be implanted into lunar regolith during the long history of the geodynamo, so the history of the terrestrial atmosphere could be preserved in buried lunar soils. This study will be beneficial to analyze the evolution of the Earth's atmosphere and magnetic field through lunar surface materials, and thus gain a deeper understanding of the evolution of the Earth-Moon system. This topic is very important and the results look very interesting. I highly recommend this article be published in this journal. However, I have a major concern about some of the main results and statements presented in the manuscript. In my mind the following issues should be addressed before publication.

Response: We thank the reviewer for this positive comment.

Comment: Line 45-46: The authors have mentioned Li et al. [16] attributed their observations of lunar surface water to the solar wind as the singular source during the Moon's passage through the magnetotail. However, another paper suggests that terrestrial ions in the magnetotail will be a possible source of lunar surface hydration which is lack of quotation.

Wang, H. Z., et al. (2021). Earth wind as a possible exogenous source of lunar surface hydration. The Astrophysical Journal, 907(2), L32. <https://doi.org/10.3847/2041-8213/abd559>

Response: The Wang et al. (2021) reference has been added.

Comment: Line 55-58: In fact, sometimes the Moon is out of protection against solar wind at midnight region. Therefore, we believe that the author ought to incorporate the following quotation to render the article more rigorous.

Shang, W. S., et al. (2020). Unusual location of the geotail magnetopause near lunar orbit: A case study. Journal of Geophysical Research: Space Physics, 125, e2019JA027401. <https://doi.org/10.1029/2019JA027401>

Response: We have now included the reference to Shang et al. (2020).

Comment: Line 124: Do the authors consider the distance change of Earth and lunar in your model? How does it affect their model result?

Response: Yes, we account for the changing Earth-Moon separation in our model. We used a separation of $60 R_{\oplus}$ for the present-day magnetized Earth and $40 R_{\oplus}$ for the Archean Earth case (Lines 139-142). When all the parameters are kept constant (i.e., within a particular simulation run—for example, the contemporary Earth case), the terrestrial flux increases as the separation decreases. This is because the same amount of material gets distributed over a smaller surface area, resulting in a higher flux.

Comment: Figure 2: Could you explain when phase angle = 0, why the Earth wind and solar wind flux are both little?

Response: The flux of both Earth wind and solar wind is significantly lower (by at least an order of magnitude) at a phase angle of 0 compared to that at the magnetopause boundary, primarily due to the abrupt increase in densities within the shocked region near the boundary. We have also explained this further in the figure caption.

Comment: Line 231-262: Earth wind not only could implant particles into the lunar nearside surface, it also has earthward flux produced by magnetic reconnection in the distant magnetotail that could implant particles

into the lunar farside surface. Could the author discuss the possible influence of these earthward flux and the asymmetry of atmospheric transfer on the lunar nearside and farside?

Zong, Q. G., et al. (1998). Energetic oxygen ion bursts in the distant magnetotail as a product of intense substorms: Three case studies. *Journal of Geophysical Research: Space Physics*, 103(A9), 20339–20363. <https://doi.org/10.1029/97JA01146>

Kiehas, S. A., et al. (2018). Magnetotail Fast Flow Occurrence Rate and Dawn-Dusk Asymmetry at XGSM ~ -60 R_E. *Journal of Geophysical Research: Space Physics*, 123(3), 1767-1778. <https://doi.org/10.1002/2017JA024776>

Ma, Y., Wong, H.-C., & Xu, X. (2015). Subsonic and sunward-orientated lunar wake observed by ARTEMIS in the geomagnetotail. *Astrophysics and Space Science*, 358(2). <https://doi.org/10.1007/s10509-015-24>

Response: We appreciate the reviewer's suggestion. We have now added a discussion on the influence of Earthward flux on the nightside, highlighting the asymmetry of deposition for both the present-day magnetized and early unmagnetized Earth (Lines 285-300).

Comment: Line 231-262: Now that Chang'e-6 has successfully returned samples from the farside of the Moon, this research has further unveiled the evolution of the lunar magnetic field. I wonder if the author could elaborate in the discussion section on how these new findings impact the author's own results.

Cai, S. et al. A reinforced lunar dynamo recorded by Chang'e-6 farside basalt. *Nature* (2024). <https://doi.org/10.1038/s41586-024-08526-2>

Response: Our calculations focus on the lunar nearside. Therefore, the recent findings by Cai et al., based on Chang'e-6 farside samples, do not affect our results. However, we have now included a paragraph in the Discussion section that addresses the nearside–farside asymmetry. The lunar magnetic field remains a highly debated topic. Following Tarduno et al. (2021), we did not consider any magnetic anomalies in the Archean case, as the Moon likely lacked a significant global magnetosphere during that period. In general, a lunar field, if it existed, would funnel incoming charged particles toward its magnetic poles.

4 Reviewer – 3

Comment: The paper by Paramanick et al. for the first time investigates the transport of ions from both the Earth wind and the solar wind to the lunar near side by modeling the Earth-Moon environment with the 3D MHD model AstroBear, their own photoionization model, and with atmospheric profiles for the present-day and Archean Earth. These simulations are performed for magnetized and unmagnetized cases to evaluate the effect of Earth's magnetosphere on the transport of ions to the Moon. In addition, the authors also vary solar wind conditions, atmospheric composition, Earth's surface atmospheric mass density, and the incident XUV flux to evaluate how the Earth wind contribution to the lunar volatile implantation may have changed from the Eoarchean to the present-day and to calculate possible scenarios for which the lunar isotopic signatures of N, H, He, and Ar can be explained by a mixture of a non-solar component (that is, the Earth wind) and a solar component (the solar wind) based on isotope mixing diagrams.

As their main outcome, they find that the isotopic signatures in the lunar regolith can indeed be explained by such a mixture, but only for cases where the Moon crossed the Earth's magnetotail, i.e., when a geodynamo existed. In contrast, an unmagnetized epoch during the Eoarchean cannot lead to the observed lunar isotopic signatures. In addition, their study suggests that the contribution of the Earth Wind strongly depends on the Earth's exobase altitude, which according to their present simulations cannot have been below 190 km at times of ion implantation onto the Moon. As a third outcome, that I find very much worth highlighting, their simulations also indicate that an intrinsic magnetic field provides a net shielding effect for the atmosphere, at least for their specific atmospheric, magnetic field and solar (solar wind ram pressure and XUV flux) parameters.

I find the study very interesting and timely. It is also novel; although it is not the first study that suggests that volatile implantation from Earth's atmosphere to the Moon is happening through Earth's magnetosphere instead of from an unmagnetized early Earth, it is indeed the first to actually model both Earth Wind (for unmagnetized and magnetized cases) and solar wind implantation onto the Moon for trying to resolve the puzzle of the isotopic signatures of volatiles in the lunar regolith. It was first suggested in 2005 by Ozima et al. that terrestrial volatiles may contribute to this signature, and the current study can add important results to (i) this still ongoing discussion, and (ii) our understanding of the Archean Earth's atmosphere. This is also timely as new in-situ data from the Moon (as from Chang'e 6) is finally becoming available to give further insights into this topic. The study further adds to another hotly debated topic, i.e., whether an intrinsic magnetic field protects an atmosphere.

However, I am unsure whether their current results are reasonable since there are inconsistencies in how the authors treat the upper atmosphere in their study (and/or how they describe the treatment of the upper atmosphere in the present version of the manuscript). I therefore suggest major revisions since a better implementation of atmospheric profiles may alter their results and conclusions substantially.

I have three major comments/suggestions and some additional minor ones that are mostly related to the treatment of the atmosphere in their model and the manuscript's readability.

Response: We thank the reviewer for this assessment. We find that our results remain consistent even when the Chamberlain profile is used, as demonstrated in Subsection 4.1.7 and explained further below. **Illustration 1** compares the additional simulation using the Chamberlain profile with our previous results based on the barometric profile.

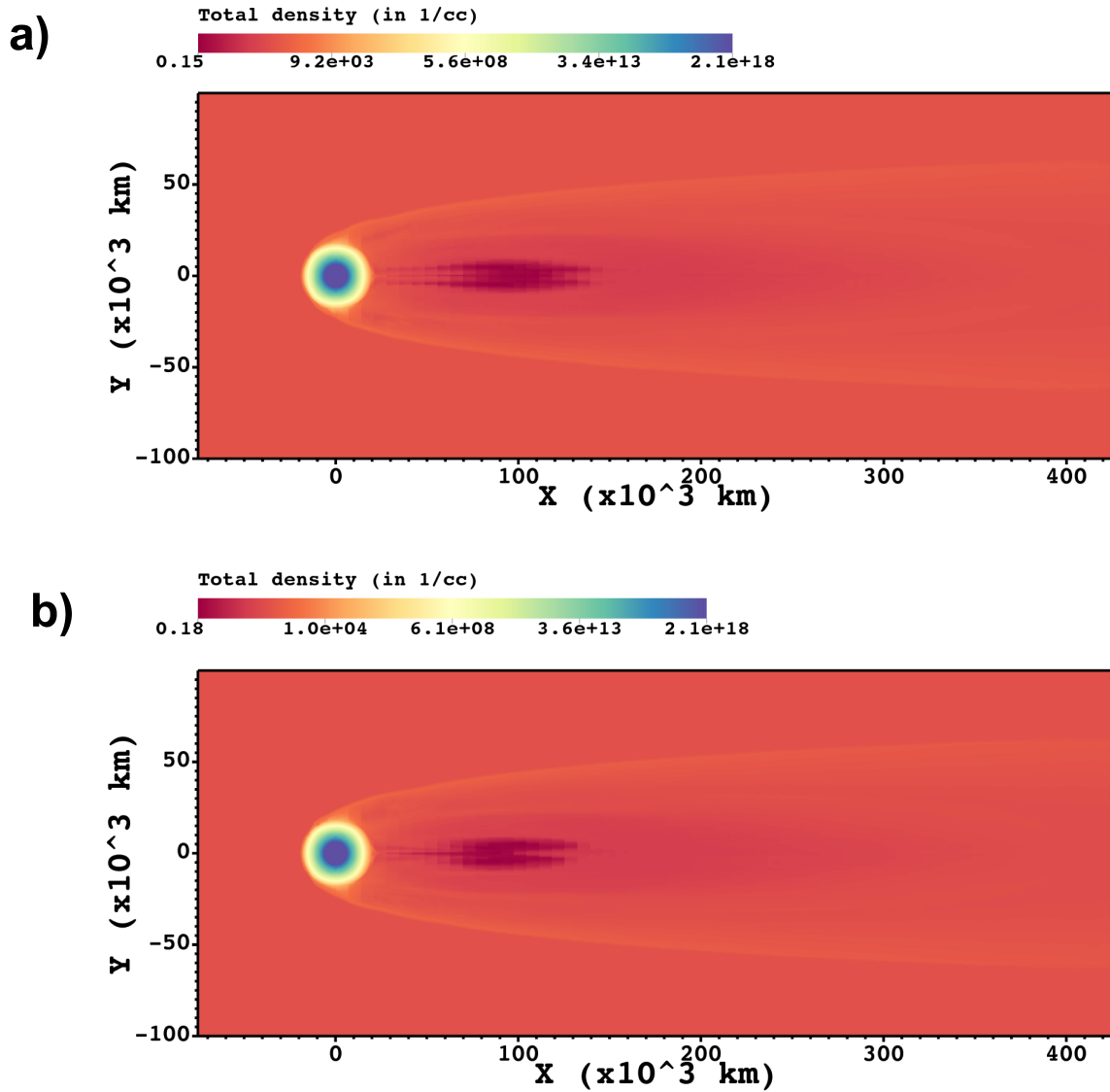


Illustration 1: **Comparison of the 3D MHD simulation results for different atmospheric profiles.** *Top panel (a):* Total number density plot of the wind-atmosphere interaction in the quasi-steady state for Archean Earth with the Chamberlain atmosphere model. *Bottom panel (b):* Same as *panel (a)*, but with isothermal atmosphere profile.

Major Comment I: The authors implement the barometric formula for calculating the atmospheric mass density/pressure from the surface of the Earth up to either (i) the altitude where the magnetic pressure of Earth's magnetosphere equals the atmospheric pressure (that's for the magnetized cases), or (ii) the altitude where the solar wind ram pressure equals the atmospheric pressure (for the unmagnetized cases). For both scenarios, the respective pressure equilibria are well beyond the exobase level. This is problematic. The barometric pressure equation loses its validity long before the magnetic field determines the global plasma structure and/or before a pressure equilibrium with the solar wind ram pressure is reached. The exobase is defined as the boundary in the atmosphere where the gas becomes collisionless (as correctly defined in the manuscript!). As soon as this becomes the case the barometric pressure equation cannot be applied any longer as the particles will start to move on ballistic orbits and the temperature between particles cannot be appropriately equilibrated within the gas as no (or very little) collisions take place. At this point, Equation (3) will fail to give accurate results (it will fail much earlier if no variation in temperature, and less so, in molecular weight will be considered). In reality, Chamberlain profiles should be used above the exobase (e.g., Chamberlain 1963). This may lead to quite different atmospheric profiles to the ones displayed in Fig. 1 and could further lead to very different results. I therefore suggest implementing this properly.

Response: The Chamberlain atmosphere model, which describes the collisionless exosphere of a planet, classifies particles by their energy into ballistic, satellite, and escaping types. However, the Chamberlain approach does not include solar wind interactions with terrestrial ions. When the solar wind interacts with the exosphere,

it sweeps all three populations, picking up ions from the upper atmosphere. These ions then gyrate around the solar wind's magnetic field lines and are advected. When all three partition functions are summed, the resulting profile nicely overlaps the barometric law. Please refer to Figure 7 in the manuscript.

Since the dominant contribution to the Chamberlain profiles closely resemble the exponential profile we use for the unmagnetized cases, the results that it implies are not substantively distinguishable from our results in our framework. We further verified this through an additional simulation, which demonstrated that the escape fluxes calculated with barometric and with the Chamberlain profiles are comparable. However, a key limitation of the Chamberlain approach is that it does not account for magnetic field effects, making it incomplete, if not unsuitable, for our magnetized cases.

Note also that the collisionless property does feature in the photoionization model that we use to predict fluxes for individual species (P.G. Richards et al., 1994; P.G. Richards et al., 2006).

Major Comment II: First, I am not sure whether I correctly understand how the exobase levels are varied. Either it is not explained properly in the manuscript, or it is performed incorrectly. At least as I read the manuscript, you simply assume the exobase to be at various heights and further assume that all ions above the exobase escape. But based on Equations (5) and (6) you do not change the atmospheric pressure profile together with the exobase, correct? This would be wrong as the exobase is always where the atmosphere becomes collisionless. However, if you always use the same pressure profile but arbitrarily vary the exobase level, this is not the case. So, this should either be explained more properly (if you do it correctly but just didn't write it) or the change in exobase height should be coupled to appropriate pressure profiles to be consistent, i.e., to a pressure profile for which the atmosphere indeed becomes collisionless at the exobase altitude. From Fig. 4, one can see that the total ion escape rates from the atmosphere decrease for increasing exobase altitudes, which is a very strange behavior, as it should be the other way around. Such a behavior can already be easily seen by looking at the escape parameter λ , which decreases for increasing exobase radii and temperatures and decreasing gravity. And lower λ values mean more escape (for the Jeans escape parameter λ , see, e.g., Volkov et al. 2011, or, more recently Guo 2024, among many others). In addition, if your atmosphere becomes collisionless at the exobase, the exobase densities should be roughly similar (well, not entirely as the scale heights can be different). But if you always use the same pressure profile and arbitrarily choose your exobase, the density at such an "exobase" will become larger the more you decrease its altitude. Obviously, more and more atmosphere will be above the exobase and more will escape by definition, contrary to what one would expect. So, if you have not treated this properly, it could have strongly affected your results and you should rerun your simulations at least with proper atmospheric profiles (with the barometric equation up to the exobase, and with Chamberlain profiles above it). If you treated it properly, please describe it in more detail in the manuscript.

Response: First, note that in the revision we have made a very important clarification regarding what we mean by the term "exobase" as extracted from our analyses of the mixing plots. Specifically, we are constraining a "hydrodynamic exobase"—that is, the lowest altitude at which the solar wind ram pressure would be capable of stripping the Earth's atmosphere in a way that best matches the isotope ratio data for each combination of isotopes. These are not measures of where the traditional, collisionless exobase arises for each species. The fact that the solar wind is magnetized allows the interaction between the wind and Earth's atmosphere to be approximated using MHD. This is now discussed in Lines 562-573.

For each mixing diagram, we constrain this hydrodynamic exobase that represents an average for the two species, and each mixing diagram provides a best-fit case for that pairing. The element pairing that gives the lowest exobase is then quoted as the overall hydrodynamic exobase. This value, being much lower than the magnetopause, indicates that the solar wind must gain access to Earth's atmosphere through the poles, where it can penetrate deeply enough.

Please refer to our response to Major Comment III for details on how the exobase levels are varied. Photoionization depends on the number of particles available to absorb photons. So, even if the solar XUV flux received is essentially the same at different altitudes in the Earth's atmosphere, the number of ionizations de-

depends on how many molecules are present to absorb it. This is directly related to density, and thus to pressure. Therefore, pressure is already accounted for in the semi-empirical photoionization model.

For a fixed ion population, Jeans escape becomes more efficient as one approaches the exobase from below, increasing with altitude due to lower gravity, fewer collisions, and higher upper-atmosphere temperatures. However, the escape rate for a given species, as shown in our results, is also influenced by the specific production rate, which decreases steeply with height. Our photoionization model accounts for the correct composition and average thermospheric temperature. Additionally, we validated our calculated production rates against values reported by Grebowsky and Aikin (2008) and Y. I. Stozhkov (2003).

Please see our response to Major Comment I regarding the Chamberlain discussion.

Major Comment III: Second, if you want to be consistent, you cannot vary the exobase arbitrarily. The thermospheric structure (and hence the altitude of the exobase; but also its temperature) depends on atmospheric composition and the incident XUV flux (and on other things of course, like the planet's gravity). This means that you cannot assume arbitrary exobase altitudes for the Eoarchean and for the present. Earth's present N₂-dominated atmosphere, for instance, does not have an exobase altitude of only 250 km (the altitude that matches best the isotope data in your simulations) as N₂, the low abundances of CO₂ and other IR-coolers do not sufficiently cool the thermosphere. The incident XUV surface flux should be very low for this, lower than at present (see, e.g., Tian et al. 2008, and Johnstone et al. 2018, for how the XUV flux and CO₂ cooling affect the thermosphere). In contrast, the exobase altitude will increase if you increase the XUV and decrease if you increase the CO₂ abundance. The XUV flux in the Eoarchean was much higher than at present, likely more than an order of magnitude compared to today (e.g., Tu et al. 2015; Claire et al. 2012). So, from an aeronomical point of view, it is quite unlikely that Earth's volatiles were implanted onto the Moon for an exobase altitude of 250 km. You already utilize the atmospheric composition for the Eoarchean from Johnstone et al. (2021); in this paper you can also see how the exobase altitude varies as a function of CO₂ mixing ratio and the X-ray surface flux during the entire Archean eon (their Fig. 4). You can simply use exobase altitudes from this paper, and it would be much more consistent than the arbitrary exobase altitudes you implemented for the current manuscript. I highly recommend this, as this could significantly alter your results. And you may even be able to find atmospheric cases that can be excluded, which would be a great additional outcome!

Response: First, note that we have clarified what we mean by the exobase in the response to the previous comment. Although we plot curves in the mixing diagrams for a range of exobase choices, our ultimate determination of the best-fit values from this set is not truly "arbitrary", as we use the soil data to then back out the optimal value for a given atmospheric model. The atmospheric models use altitude-averaged temperatures. There are ingredients therein that may not be complete, but that is a layer of complexity and uncertainty beyond the present scope and can be left for future work.

Also, the "exobase" heights in Figure 6 do not exclusively represent this value for any one species. Instead, each figure depicts a binary mixing plot involving two elements and two end-members. The best-matching curve reflects a mixture between the Earth composition originating from that specific altitude and the solar wind.

We used homopause composition values and average thermospheric temperatures from Johnstone et al. (2021), and incorporated Archean XUV flux values from Johnstone et al. (2021), Tu et al. (2015), and Claire et al. (2012). All of these parameters were integrated into our photoionization model, as discussed in Section 4.2.2.

As shown in Figure 4(b), only H⁺ dominates above ~ 800 km when using the results of Johnstone et al. (2021). This is consistent with our finding in Table 3 that the Archean unmagnetized Earth cannot account for lunar soil volatiles.

Comment: I find the main body of the manuscript quite hard to understand and follow from time to time. It gets clearer after reading the methods, but I find it almost impossible to understand without reading the methods sections. For instance, you suddenly speak from 'Case – I' but it is neither explained what this is, nor that it is explained in the methods and in Table 2 (well, it is linked later in the manuscript). A brief

explanation of what this means and a link to Table 2 would already be quite helpful. You only mention that you made mixing curves for different exobase heights in the main body but you do not describe how these different exobase heights are linked to your model (and as written above, it still isn't clear to me after reading the methods). Another example is the combination of your photoionization model (and NRLMSISE-00) with your MHD model. In the main body, you only write that you combined these models; although you explain it in the methods in more detail (Section 4.2.3), I am still not sure whether I understood it properly. You obtain an escape flux that impinges onto the lunar surface from your MHD model. Then you utilize NRLMSISE-00 (at least for the present-day) and your photoionization model to calculate ionization and the relative fractions of the various species (since the MHD does not distinguish between the different species) above the exobase and assume that everything above the exobase escapes in your ionization model. Do I understand correctly that you obtain (different) escape rates from the MHD and the ionization model (together with the NRLMSISE model)? And then you scale the escape from the ionization model to match the MHD results, is that correct? Do the arbitrarily chosen exobase levels also feed into the MHD model? Related to this, I am also not sure where the numbers in Table 3 and ED3 (and ED4) come from. Let's take N: In Table ED3 the EW flux impinging onto the Moon for N comes from multiplying the total EW flux from the MHD in Table 2 with the relative fraction of N from ED3 (for an exobase altitude of 401 km). This gives 3.19×10^{-1} for magnetized Case – I. That's clear. The relative fraction N comes from all N+ that are above the exobase in your NRLMSISE-00/ionization model, correct? But where is the Earth wind flux for N in Table 3 (i.e., 2.41×10^5) coming from? And does Table 3 also relate to a specific exobase altitude? That still confuses me, and I am not sure whether I understand it correctly. It would be good to make this clearer. I also do not understand which profiles you use for the Archean Earth cases, as you cannot do this with NRLMSISE-00. They have to come from somewhere else.

So, all in all, I think that too much might have been moved to the methods for the main text to be smoothly understandable. I give minor comments below, some of them are also related to, at least for me, unclear understanding.

Response: We have now clarified some essential aspects of the overall model approach in the Introduction and Results sections.

Additionally, we have now clarified the different cases in the "Main" section as well as in the caption of Table 2. Regarding the combination of our MHD model with the ionization model, you are correct and have summarized it well. It is also worth noting that the escape rates obtained from the MHD model and the photoionization model are nearly comparable, as seen by comparing Table 2 and Figure 4.

As explained in the response to Major Comment III, we do not arbitrarily set the exobase altitude; instead, we determine it based on the mean molecular mass of each species and the temperature, providing a theoretical estimate. These estimates are then used alongside other altitudes specific to different simulations and matched with lunar soil data.

The Earth wind flux value in Table 3 comes from scaling the escape rate from the ionization model to match the MHD results, as described in the caption of Figure 3. Therefore, this represents our theoretical estimate. Different species have different exobase altitudes, which also highlights the need for species-dependent exobase determination. For each species, we report the best-matched height in Table 3. The lowest height determines our overall lower radial integration bound and represents our constraint on overall hydrodynamic exobase.

Comment: Line 6: Reference for "the latter sourced in the absence of geomagnetic field"? I assume this refers to Ozima et al. (2005). However, Terada et al. (2017) already suggested that ions were transported to the Moon via the magnetotail. Wei et al. (2020) even suggest that ions could have been implanted into the Lunar far side. Also, Lammer et al. (2022) give arguments on why ion implantation through the magnetotail could have been significant in the past. Cao et al. (2024) review key questions of the Earth Wind-Moon system mostly based on implantation through Earth's magnetotail. This manuscript is hence not the first to suggest this idea (but probably the first to test it!).

Response: We have now explained the key differences between our paper and those papers in Subsection 1.1 (Lines 42-52). These studies do not consider how the solar wind and escaping Earth's atmospheric ion end-

members interact and mix during their transport. They also lack a dynamic model explaining how nitrogen or oxygen ions escape from the atmosphere. Additionally, we compare our results with lunar data, which these studies do not include—except for Ozima et al. (2005). However, Ozima did not study the difference between a magnetized and an unmagnetized Earth with a dynamical model. Importantly, our results demonstrate that the geomagnetic field is essential for explaining the Apollo data, whereas Ozima et al. (2005) argued that it should be absent.

Comment: Line 14-16: It would be very interesting to see if this statement is still true if one also considers highly extended atmospheres (as, e.g., in Johnstone et al. 2021) on an unmagnetized early Earth for which escape rates would also be higher.

Response: Yes, this statement still holds true. It, in fact, strengthens our results that implantation occurred during Earth's magnetized phase. The very high exobase height for the Archean case from Johnstone et al. (2021) does not match well with the lunar observations.

Comment: Line 19: The statement that the exobase was never smaller than 190 km is not surprising. The CO₂-dominated atmospheres of Venus and Mars have exobase levels around 200 km. Pure CO₂ atmospheres on Earth may have somehow lower exobase levels. As low as 190 km, or even lower, is only possible for CO₂-dominated atmospheres and low XUV fluxes. As the XUV flux, however, was higher in the Eoarchean, even the exobase level of a CO₂-dominated atmosphere would have been >200 km. You can nicely see this in Johnstone et al. (2021), the paper from which you took the Eoarchean's atmospheric composition. Exobase levels in the early Archean eon were always (much) higher than 190 km (see again their Figure 4). The exobase altitude for the specific case you took for your simulations is ~ 800 km!

Response: We would like to clarify that the lower bound of the exobase at 190 km was constrained and determined using lunar Apollo sample data (see Figure 6). We did not assume an exobase altitude of ~ 800 km. Instead, we used homosphere data for various species and the solar XUV flux in the Eoarchean Earth from Johnstone et al. (2021) and incorporated these values into our photoionization model. This point has now been addressed in Section 4.2.2.

Comment: Line 44: Is reference 14 (Poppe et al. 2021) the correct reference here? It is about lunar paleomagnetosphere simulations. Instead, Poppe et al. 2016 (<https://ui.adsabs.harvard.edu/abs/2016GeoRL..43.6749P/abstract>) talk about the detection of terrestrial magnetotail ions and may be the correct reference here.

Response: Corrected.

Comment: Line 45-46: Wang et al. (2021), on the other hand, suggest the Earth Wind from the magnetotail to be a source for the lunar water!

Response: Yes, the reference to Wang et al. (2021) has been included (Line 45–48).

Comment: Line 58-60: It is misleading to compare atmospheric loss rates from Earth, Venus, and Mars at present to argue that magnetic fields are not shielding the atmosphere. One crucial aspect that is often completely overseen, when discussing this aspect is atmospheric composition. Mars and Venus have a CO₂-dominated atmosphere whereas Earth's is N₂-dominated. Loss rates from Earth, Venus, and Mars could look entirely different if they interchanged atmospheric composition. It is not only the difference in molecular weight between CO₂ and N₂ but, more importantly, their role in shaping the thermal structure of the upper atmosphere since CO₂ is an IR-coolant whereas N₂ is not. Earth's upper atmosphere is hot and quite expanded (with an exobase temperature, Texo, around 1000 K and an exobase radius, Rexo, around 500 to 1000 km), whereas the upper atmospheres of Venus and Mars are compact and cold (with Texo and Rexo being around 200 K and 200 km in both cases). Give Venus or Mars an N₂-dominated atmosphere and escape rates increase.

An N₂-dominated atmosphere at Venus could have values of Texo above 2000 K and Rexo around 1000 km due to the increase in the incident XUV flux. this would certainly affect atmospheric escape. The similarity of escape rates from Venus, Earth, and Mars might hence be pure coincidence, and the role of the magnetic field remains very poorly understood as long as no simulations are performed that study the effect of intrinsic magnetic fields for various atmospheric compositions, incident XUV fluxes, and solar wind ram pressures. For higher XUV fluxes and, therefore, more extended N₂-dominated atmospheres, an intrinsic magnetic field could have been an important shield against non-thermal escape. So even if the intrinsic magnetic field might not be important for atmospheric shielding at present, it could have been in the distant past. Way et al. (2023) discuss atmospheric composition in this context and it is worth highlighting in the manuscript.

Response: All good points. We have now emphasized the significance of the thermal structure and its influence on the loss rate. Additionally, we have included a citation to Way et al. (2023). Also contributing to the difficulty of interpreting these similarities is the fact that the measurements are taken at specific times and places, not global time averages around each planet.

Comment: Line 96: It would be good to link to the table where the cases are explained.

Response: The table is cross-referenced now.

Comment: Line 102-103: The figure with the phase angle is Fig. 2. Should actually Fig. 2 be referenced here? If not, you may include the phase angle in Fig. 1a.

Response: The phase angles in Figure 1 are now included.

Comment: Line 105: The validity of this very strong statement ("Only at such early times could the Earth have possibly been unmagnetized") is disputed. It should hence be softened, and/or a caveat should be added and discussed. See, e.g., Fu et al. (2024) for a criticism of the cited paper and the paleomagnetospheric measurements.

Response: Paleomagnetosphere measurements for the Eoarchean-Hadean are among the most challenging of paleomagnetic measurements, and given their great antiquity, some uncertainties may always remain. We have added this caveat and a short description to the Supplementary Information, along with the relevant paper (Tang et al., 2018), which criticized the original discovery of zircon magnetizations based on secondary Fe oxides observed in altered zircons they studied (this issue was subsequently addressed in Tarduno et al., 2020). The criticism by Fu et al. (2024), which challenges the stagnant lid interpretation of the early Earth and is not germane to the present manuscript, is problematic and will be addressed in a separate work.

Comment: Line 110: What is the atmosphere mass? The mass of Earth's present atmosphere (i.e., a pressure of 1 bar)?

Response: We used an atmospheric mass of $\sim 6.0 \times 10^{18}$ kg (Line 127-128).

Comment: Line 131. Where are the ancient solar wind data from?

Response: We used the data from Sterenborg et al. (2012) and have now added the reference.

Comment: Line 133-138: I am wondering whether this is still true if the atmospheric density profile is treated properly (i.e., with Chamberlain profiles above the exobase and with the exobase as the region where the atmosphere becomes collisionless).

Response: Yes, this holds true. As explained above, the Chamberlain profiles closely resemble our exponential profile used for the unmagnetized cases. We confirmed this through an additional simulation, which showed that the escape fluxes calculated with and without the Chamberlain profiles are comparable. However, it is important to note a key limitation of the Chamberlain approach: it does not account for magnetic field effects

and, as such, is not applicable to our magnetized cases. Nevertheless, the collisionless circumstance has already been incorporated into our atmospheric model.

Comment: Line 138-141: The total escape rates (in $1/s$) for the different cases would also be interesting to know so that they can be compared with other studies and potentially also be used in future studies. I am, e.g., curious how the escape rates for the Eoarchean (specifically without a magnetic field) compare to Grasser et al. (2023) who studied polar outflow from Earth's atmosphere during the Archean. Note, however, that the ancient solar parameters in both studies are quite different (the solar wind parameters are from Johnstone et al. 2015).

Response: We have now included the total escape rates (in $1/s$) in the last column of Table 1. Note that the polar outflow rate (Grasser et al., 2023; Schillings et al., 2019; Slapak et al., 2017) may differ from the total loss rate for each species, since some of this apparent loss can actually return to the atmosphere. We compared our total escape rate values for O^+ in the present-day terrestrial atmosphere ($\sim 5 \times 10^{23} s^{-1}$) with those estimated by Seki et al. (2001) ($\sim 10^{24} s^{-1}$), and they are within the same range—falling within the same order of magnitude.

Comment: Line 140-141: If there is indeed a net protection effect of the intrinsic magnetic field that's worth highlighting more prominently (and also writing it in the abstract)! I am curious whether there is indeed a protective effect for more extended atmospheres in the Eoarchean.

Response: We have now highlighted this in that section.

Comment: Line 142-144: This also means that the surface pressure is reduced significantly, right? I am not sure whether this behavior remains if the atmospheric profile is treated properly. Since the exobase always has comparable densities, and the upper atmosphere profiles usually behave similarly for different surface pressures, I would be surprised about such strong differences in escape rates. For instance, Tian et al. (2009) simulated CO_2 -atmospheres at Mars for different atmospheric surface pressures (6 mbar, 1 bar, 3 bar) and found a very similar behavior of the upper atmosphere including similar escape rates. Also, Mars and Venus show a difference in surface pressure of more than 3 orders of magnitude but escape rates are similar (well, that may again be a coincidence).

Response: Yes, the corresponding surface atmospheric pressure also decreases significantly. This behavior remains consistent when the Chamberlain profile is used. As outlined in Subsection 2.1 (Lines 98–106), we used this case to estimate the analytical scaling relations for mass loss.

Comment: Line 177: The non-solar component is compared with the EW from the magnetized case and with the EW from the unmagnetized case, correct? If so, I assume that an "and" is missing in Line 177, and it should hence be "magnetized Earth and with those".

Response: Added.

Comment: Line 189: "in the present-day atmosphere" is slightly confusing. I suggest writing "in the present-day atmosphere case".

Response: Changed.

Comment: Line 195-196: This conclusion could be different if the present and Archean atmospheres are treated properly. Usually, atmospheric escape tends to increase for more expanded atmospheres, i.e., for higher exobase levels, as should have been the case for the Eoarchean. In this manuscript, however, escape increases with decreasing exobase levels.

Response: As already explained in Major Comment I, the Archean atmosphere is properly treated in our study,

and the use of the suggested Chamberlain profile does not alter our conclusions whatsoever (**Illustration 1**).

The referee appears to be mentioning Jeans escape, which becomes more efficient the closer one gets to the exobase and increases with altitude due to lower gravity, fewer collisions, and higher upper-atmosphere temperatures. This holds true if the populations of ions do not change significantly with altitude. However, the escape rate shown in our results is also representative of the ion production rate, which decreases steeply with height and accounts for the correct composition and average thermospheric temperature. We also verified our computed production rates with those reported by Grebowsky and Aikin (2008) and Y. I. Stozhkov (2003).

Comment: Line 198-199: As already written earlier, I am slightly confused and not sure whether I correctly understand how the exobase levels are varied.

Response: This is explained in detail in Major Comment II.

Comment: Line 202-205: During Earth's history, its exobase altitude has likely been above 300 km most of the time - if not always; specifically for N-dominated atmospheres (see Major Comment II). So, I am worried that this is not correct because of treating the atmosphere and the position of the exobase altitude erroneously. The same goes for the other exobase altitude estimates on the following page.

Response: In this study, we do not estimate any specific exobase altitude. Rather, we constrain the minimum possible exobase height during the ion implantation phase by comparing our models with lunar soil data. Please see our response to Major Comment III.

Comment: Line 224-226: As written above, it would be good to use more realistic exobase altitudes for 3.8 Ga, e.g., from Johnstone et al. (2021). In addition, I am also curious how this result would change for a different solar wind ram pressure (e.g., for the ones from Johnstone et al. 2015).

Response: As explained above, we conducted several simulation runs for the Archean case, considering different exobase heights, including those theoretically estimated. The use of exobase altitudes from Johnstone et al. (2021) further supports our conclusion, as H^+ is the dominant species over other volatiles. As a result, the Archean case does not account for the lunar soil composition. We used the SW data from Sterenborg et al. (2012). Besides, Johnstone et al. (2015) do not provide any estimates for the solar mass loss rate or solar wind ram pressure during the Eoarchean eon. Their models focus on the 2 Ga and 5 Ga epochs, which are not relevant to our study.

Comment: Line 238: Again, this statement is very strong and disputed.

Response: We have modified the statement now.

Comment: Line 239-240: This conclusion may change, if a variation of realistic exobase levels are implemented.

Response: No, the conclusions do not change. Please refer to the response to Major Comment II.

Comment: Line 242: How does a flatter evolution of the solar mass loss rate compare to the solar wind values used for the Eoarchean? It would be interesting to make simulations with lower solar wind ram pressures.

Response: We used the solar wind parameters for the Eoarchean based on the solar mass loss rate during that era. For a spherically symmetric wind at 1 AU, the loss rate is proportional to the mass flux

$$\dot{M}_{sw} = 4\pi r^2 \rho_{sw} v_{sw} \implies \frac{\dot{M}_{sw,Eo}}{\dot{M}_{sw,P}} = \frac{\rho_{sw,Eo} v_{sw,Eo}}{\rho_{sw,P} v_{sw,P}} \quad (1)$$

Plugging in the Archean SW values, we obtain $\dot{M}_{sw,Eo} \sim 2.24 \times 10^{-13} \frac{M_{\odot}}{yr}$, which is one order greater than the present-day loss rate $\left(\frac{\dot{M}_{sw,Eo}}{\dot{M}_{sw,P}} = 15.8\right)$. This estimated loss rate aligns well with previous results reported by

Tarduno et al. (2010), Holzwarth and Jardine (2007), and Sterenborg et al. (2012). While studying the effect of terrestrial atmospheric escape under hypothetical lower solar wind ram pressures would indeed be interesting, it falls outside the purview of this study. Moreover, performing global MHD simulations that capture the necessary atmospheric scales is extremely computationally demanding, requiring nearly three months and 220 CPUs to complete.

Comment: Line 246: The classic late-heavy bombardment did likely not take place, e.g., Zellner et al. (2017), Hartmann (2019), Worsham & Kleine (2021), Cartwright et al. (2022). The LHB, however, is still often used as a constraining event, e.g., in biology to determine the earliest possible origin of life. It is therefore important to highlight its very uncertain existence.

Response: It has been highlighted now.

Comment: Line 251-255: This should be stated in the abstract, as it is an important result!

Response: We thank the reviewer for this suggestion. It has now been added.

Comment: Line 261-262: Yes, because in such a case atmospheric escape would always be insignificant.

Response: This is not immediately obvious. While Ozima et al. (2005) proposed that terrestrial implantation occurred during Earth's unmagnetized phase, our findings indicate that it took place during the current magnetized phase.

Comment: Line 279-280: This is certainly true for the present. For the Eoarchean or even Hadean eon, it may not be. At least for certain atmospheric compositions and/or assumed XUV fluxes the atmosphere may turn hydrodynamic or can be in a transition regime between hydrostatic and hydrodynamic with a shifted Maxwellian. The Johnstone et al. (2021) article nicely shows that atmospheric composition and the XUV surface flux matter a lot during the Archean for whether your atmosphere will be hydrostatic or not. So, this assumption must not necessarily be valid for the Eoarchean. However, the 50% CO₂/50% N₂ case taken from Johnstone et al. (2021) is hydrostatic.

Response: The Chamberlain profile matches the hydrostatic profile we used. Please see Major Comment I above and Figure 7 in the manuscript for further details. The Archean atmospheric composition and the XUV flux in our photoionization model are already from Johnstone et al. (2021).

Comment: Line 287: dominates -> dominate

Response: Corrected.

Comment: Line 283-284: The scale height H depends on atmospheric temperature. Was this set to be constant over the entire altitude or was a specific temperature profile used? If so, which one? The temperature of the thermosphere can vary substantially in dependence of the incident XUV flux and atmospheric composition. In addition, the scale height varies for each species above the homopause as it depends on the mass of the respective species, which can also lead to fractionated atmospheric escape of, e.g., ¹⁴N and ¹⁵N. But I see that you anyway took care about calculating different scale heights for the main species in the upper atmosphere (Line 450 onward)!

Response: Yes, we calculated the altitude-dependent scale height for each species in the upper atmosphere, using two distinct photoionization models that incorporate the respective average thermospheric temperatures of the present-day and early Archean atmospheres.

Comment: Line 288: The barometric pressure equation loses its validity long before the magnetic field determines the global plasma structure and/or before a pressure equilibrium with the solar wind ram pressure is

reached, see Major Comment I.

Response: We find that in fact, the Chamberlain profile closely resembles the barometric profile (Figure 7 in the manuscript). Please see Major Comment I and Subsection 4.1.7.

Comment: Line 435 forsm of the -> forms of

Response: Corrected.

Comment: Line 433-436: But an increase in the incident XUV flux will also change the structure of the thermosphere significantly. An increase in XUV will heat up and expand the atmosphere. And these effects will be different for different atmospheric compositions. So, you cannot change the ionization fractionation without changing the profile of the atmosphere consistently!

Response: True, and we did not arbitrarily change the ionization fraction. We used different atmospheric compositions, temperatures, and XUV flux values for both the present-day atmosphere and the Archean case. For our Archean photoionization model, we adopted the homopause composition, temperature, and XUV flux from Johnstone et al. (2021) and calculated the corresponding photoionization rate (Subsection 4.2.2).

Comment: Line 468-471: Exactly! But is this also implemented correctly in your simulations? From reading the manuscript, I get the feeling that it is not.

Response: Yes, the implementation is correct. We conducted simulations using the theoretically estimated exobase values for each species, along with variations in exobase height, in order to compare the outflow flux with the lunar data. This is now explained in more detail in that paragraph.

Comment: Line 488-490: It would be good to mention this in the main text. From Fig. 4, this was not clear to me.

Response: We have now mentioned this in Subsection 2.2 and in the caption of Figure 4.

Comment: Line 508: With the model of the early Archean homosphere you simply mean the atmospheric composition of the atmosphere (i.e., homosphere), right? As already written, it would consequently be great to also implement the exobase levels from this study for the various XUV fluxes and atmospheric compositions instead of arbitrarily varying the exobase altitude! It would then be very insightful if some of their scenarios would be favored/disfavoured by your model.

Response: We adopted the homopause composition values from Johnstone et al. (2021) and used Archean XUV flux values from Johnstone et al. (2021), Tu et al. (2015), and Claire et al. (2012). These parameters were then incorporated into our photoionization model. Rather than arbitrarily setting the exobase altitude, we determine it based on the mean molecular mass of each species and the temperature, providing a theoretical estimate. These estimates are used alongside other altitudes on the Y-axis in Figure 4. The exobase altitude we aim to constrain in this study—shown in the binary mixing plot in Figure 6—corresponds to the height of the two-element mixture. Accordingly, we consider a range of values near the theoretical exobase altitude for each species and constrain them using observed lunar data.

Even if a single exobase altitude is assumed for all species—as done by Johnstone et al. (2021), which oversimplifies the system and is incorrect—it still supports our conclusion. As shown in Figure 4(b), only H^+ dominates above ~ 800 km, aligning with the conclusions in Table 3. This indicates that the Archean case does not account for the lunar soil volatiles derived from Earth.

Comment: Line 510: To be consistent with Johnstone et al. 2021, you can then also cite Tu et al. (2015) and Claire et al. (2012) for the early solar XUV flux.

Response: The citations have been included now.

Comment: Line 517-519: I assume that ^{36}Ar is meant here? Then it should be also written explicitly as Ar in Earth's present atmosphere is dominated by ^{40}Ar . ^{40}Ar should have been different in the Eoarchean as it comes from the radioactive decay of ^{40}K (with a half-life of ~ 1 Gyr).

Response: Indeed, it is ^{36}Ar . Corrected.

Comment: Fig. 1: The axis descriptions are very small. " $\times 10^7$ " in Fig. 1(b) is relatively hard to read because of its smallness and the line that goes directly through it. What is the actual mass of the atmosphere and hence its pressure? 1 bar? The pressure axis stops at 10^4 Pascal, that is 0.1 bar.

Response: Enlarged. The atmospheric mass is $\sim 6.0 \times 10^{18}$ kg, and the pressure is ~ 0.1 bar.

Comment: Fig. 2 and ED2: Same problem with axis descriptions ($\times 10^n$ is very small). What should the unit $1/(m^2\text{-s})$ be, a typo I assume? It should be $1/(m^2\text{ s})$ for a particle flux. However, in the figure caption it states "mass flux". Then it should be $\text{kg}/(m^2\text{-s})$. The same strange unit appears in Table 2. I further assume Figure 2 shows the magnetized Case – I? Please state that explicitly in the caption.

Response: Corrected. It should be particle flux.

Comment: Fig. 3(b) and 4(b): Where are the underlying profiles for the Archean Earth coming from? And do these profiles take account of the proper atmospheric composition and expansion due to the XUV flux? What is the exobase altitude of the implemented profiles (because it would look different for different exobase altitudes, same for the present-day!)?

Response: The profiles for Archean Earth are based on the work of Johnstone et al. (2021). The exobase altitude for each species depends on its mean molecular mass, assuming the atmosphere is well-mixed. This is the theoretical estimate of the exobase altitude, which we use along with the other altitudes on the Y-axis in Figure 4. It's important to note that the exobase altitude we aim to constrain in this study (as shown in the binary mixing plot in Figure 6) using lunar data corresponds to the height of the two-element mixture. Thus, we consider a range of exobase altitudes near the theoretically estimated values for each species and constrain it using the observed data.

Comment: Fig. 4: The escape flux unit should again be corrected, i.e., to $1/(cm^2\text{-s})$. Do these figures directly give the escape flux from the atmosphere for the exobase altitudes, or do I have to integrate the lines above the exobase to obtain the escape flux? That is, if I look at the figure at, e.g., 400 km, is the total escape flux from the atmosphere displayed on the X-axis, or does the x-axis only show the escape flux from this certain altitude and I need to integrate over the whole line above this exobase altitude to obtain the total escape flux? This is not clear to me. Then, the ion escape rates seem to be extremely low, for the Archean Earth case (compare, e.g., with Grasser et al. 2023) but also for present-day. For O^+ , escape rates are observed to be in the order of $1e24$ to $1e26$, depending on wind conditions (e.g., Schillings et al. 2019; Slapak et al. 2017). For N^+ , it is comparable or slightly lower (e.g., Yau et al. 1993). Your ion escape rates seem to be clearly below this, at least in Fig. 4. It would generally be good to compare your rates with present-day observations. If they are far off the observed values, it may have to do with the potentially wrong treatment of the exobase? And does this plot really imply that the escape becomes lower for increasing the exobase altitude? I'd really be surprised if this is correct.

Response: The escape flux unit is correct. Yes, these figures directly provide the escape flux from the atmosphere at exobase altitudes, eliminating the need for any integration. However, it is misleading to compare the polar outflow rate (Grasser et al., 2023; Schillings et al., 2019; Slapak et al., 2017) with the total loss rate for each species, as part of this apparent loss can actually return to the atmosphere. We compared our total escape rate values for O^+ in the present-day terrestrial atmosphere ($\sim 5 \times 10^{23} \text{ s}^{-1}$) with those estimated by Seki et

al., 2001 ($\sim 10^{24} \text{ s}^{-1}$), and they are within the same range, falling within the same order of magnitude. With more detailed modeling, the results from Seki et al. could very well match even more closely with ours.

Comment: Fig. 5 and ED3: There is a typo. upto -> up to

Response: Corrected.

Comment: Fig. 6 and ED4: What are the references for the Ne, He, and Ar solar ratios? And for Earth's Ar? What is the reference for the Archean Earth data? In general, I wouldn't be surprised if these plots were to change significantly with an upgraded treatment of the atmosphere.

Response: The references have been added now. As explained above, the plots do not change with the upgraded treatment of the atmosphere.

Comment: Table 2: It would be nice to also include the total escape flux (as $1/\text{s}$). There is again the wrong unit for the flux. And it would be good to give references for the ancient solar wind density. The solar wind velocity likely also increased toward the Archean (e.g., Johnstone et al. 2015).

Response: We have now included the total planetary escape flux (in $1/\text{s}$). The solar wind and planetary fluxes are number fluxes and hence have the correct units already ($\text{m}^{-2}\text{s}^{-1}$). The reference for the ancient solar wind density and velocity has been added.

Comment: Table 3: Again the wrong unit for the fluxes.

Response: We meant number flux, not mass flux, and have corrected this throughout the manuscript.

Comment: Fig. ED5: Also this figure may change with updated simulations.

Response: As previously explained and illustrated in the Chamberlain profile plot (Figure 7 in the paper), Figure ED5 remains unchanged because the Chamberlain profile closely resembles the barometric profile.

Brief Response to Editor and Reviewer 1 (Case Report)

July 3, 2025

Below, we provide brief responses to the comments from the editor and Reviewer 1 regarding our manuscript “**Terrestrial atmospheric ion implantation occurred in the nearside lunar regolith during the history of Earth’s dynamo**” submitted to *Communications Earth & Environment*. We are appealing the decision made by the handling editor, Ke Zhu, who emphasized Reviewer 1’s comments as central to the outcome. As documented below, Reviewer 1’s comments did not adequately consider our responses or, more broadly, the contributions and advances of our work. In fact, many of their comments are duplicates of those made during the round 1 review, which we have noted in this report.

Editorial summary

Editor comment: Given that these issues have been raised before and not fully addressed...

Response: We provided a detailed, point-by-point response to all comments, including those from the editor, in our rebuttal letter and supported our replies with citations. (We reiterate some of these responses to Reviewer 1 below.) Therefore, the statement from the editor is not accurate and is not backed by any evidence. Moreover, the editor did not explain how this conclusion was reached, suggesting that no comparison between the review reports and our responses was made. This seems to reflect a cursory evaluation, in which the editor looked at the reviews without digesting our responses.

In the first round of review, Reviewer 1 acknowledged that significant breakthroughs are needed in numerical simulations for studies similar to the one presented in our paper. The reviewer's suggestion to use multi-fluid MHD or PIC simulations is outside the scope of the present study. Moreover, multi-fluid MHD and PIC are not suitable for global simulations involving the very large system length scales considered in this study. Therefore, as the same reviewer pointed out in the second round of review, studying material transport in Earth's space environment through the coupling of atmospheric models and MHD models is a particularly promising approach.

Reviewer 1 comments (Round 1 review)

Comment 1: The laws of charged and neutral particles are different, please check the equations in your method section.

Response: This comment by the referee is not correct, as MHD does not track individual particle species directly but models the evolution of the average bulk flow. Therefore, all our equations are correct. We have also verified them against other MHD simulations (Varela et al., 2022). It appears that the referee is mistaking it for multi-fluid MHD, which, as previously explained, is not suitable for global simulations.

Comment 2: Line 105-107: If the upstream solar wind conditions are different, how can you compare them?

Response: This was already explained in the original manuscript; however, the reviewer seems to have missed it. In fact, it is mentioned in the previous sentence (Lines 104–105).

Comment 3: Line 110: Why the total atmosphere mass is the same for the two cases?

Response: This was already present in the paper, but the reviewer may have overlooked it.

Comment 4: Line 275: Does Earth rotate during the simulation?

Response: This was already described in the supplementary section of the original draft of the paper.

Comment 5: Table 2: Why do you use different upstream solar wind conditions in the same comparison case of magnetized Earth and unmagnetized Earth?

Response: This was already explained in the caption of the table in the original manuscript.

Reviewer 1 comments (Round 2 review)

Comment 1: The authors attempted to investigate ion implantation in the lunar regolith by integrating atmospheric models with a single-fluid MHD model. However, several issues need to be clarified by the authors: First, are both the solar wind and the ionosphere plasma sources for the MHD model? If that is the case, where is the inner boundary of the MHD model located near the planet, and how are the initial parameters at the inner boundary determined? Is there any analogous treatment of the ring current in this study?

Response: In our original manuscript (Subsection 4.1.5) and in our previous rebuttal response, we justified our use of the MHD approximation combined with an ionization model that tracks individual ions a posteriori (see the comment: "The authors tried to explain how Earth's intrinsic magnetic field affects the transportation of solar wind and Earth wind to the Moon..."). We did not assume separate ionospheric plasma sources for the MHD model; therefore, a separate ring current model is not needed. Besides, the question regarding the location of the inner boundary is being repeated. This was already addressed in our original manuscript (see Lines 322–325) and was also highlighted in our response. As for the initial parameters, they are already provided in Tables 1 & 2.

Comment 2: Since it is quite challenging for the solar wind to penetrate the inner magnetosphere in MHD simulations, ring current models are typically employed in such simulations.

Response: Again, our inner boundary is not at the magnetopause, and we do not need a separate ring current model.

Comment 3: Although the atmospheric models and the MHD model are both valid, if the coupling or combination of them is not suitable, the results will not reflect the real situation.

Response: The coupling was already explained in both our original and revised manuscripts, specifically in Subsection 4.2.3. The reviewer's assertion regarding its unsuitability is unsubstantiated, lacking supporting evidence or technical justification, and appears to be merely an assumption.

Comment 4: Since the single-fluid MHD cannot distinguish ion species, the dynamics of Earth's wind ions cannot be captured. So why can the flux of Earth wind ions be extracted? If those atmospheric models provide the inner boundary for the MHD simulations, then a multi-fluid MHD model may be more suitable for studying this topic. If those atmospheric models do not provide the inner boundary, and the single-fluid MHD model does not contain the physics of Earth ions, then extracting Earth wind from the MHD model via atmospheric models is not reliable.

Response: The reviewer is incorrect in stating that our computation of Earth wind (EW) flux is unreliable. It also appears that the reviewer is somewhat confusing the total EW flux with the fluxes of individual ions. Our methodology for calculating the total EW flux is explicitly described in Subsection 2.1. We then combine this with an ionospheric model to determine the specific ion contributions. The reviewer seems to conflate our atmospheric model with the ionospheric model, which are two distinct models employed in our study. The ionospheric model, specifically the EUVAC model (P.G. Richards et al., 1994; P.G. Richards et al., 2006), is semi-empirical, meaning that the solar wind (SW) effects and its variations are inherently accounted for. The coupling between the MHD and ionospheric models is described above, and the relevant physics is accurately captured, as demonstrated by both the total flux values (Table 2) and the species-specific flux values (Figure 4). Contrary to the reviewer's suggestion, a multi-fluid MHD model would not significantly improve the fidelity of this approach, but it would impose impractical computational costs. Future investigators may be encouraged to explore alternative approaches.

Comment 5: To study the material transportation in Earth's space environment via the coupling of atmospheric models and MHD models is a really promising way. But there are a lot of challenges. This paper has raised an interesting topic and has made a lot of efforts, but the implication of the combination of an ionization model

and a single-fluid MHD has been called into question.

Response: Explained above.

Comment 6: Line 98, n_{sw} to n_{ew} ?

Response: Corrected.

Comment 7: Line 317: There is no need to set an outer cut-off for the dipole magnetic field in MHD simulations. Such a cut-off hasn't been treated as a conventional method.

Response: This repeats a comment made by the reviewer in their previous review report (see the comments: "There's a cutoff boundary for the intrinsic magnetic field...", "Why do you set a cutoff boundary at this value?", and "What is the physical meaning of this cutoff boundary?" in our rebuttal response). We had already addressed it in our original manuscript, and we also highlighted this explanation in our earlier responses.

Comment 8: Line 347-348: The term "plasmopause" has been clearly defined in Space Physics. Please revise this sentence. And the plasmopause is not the interface between Earth's plasma and the solar wind plasma.

Response: This is a repetition of an earlier comment by the reviewer, which we have addressed in our previous rebuttal (see the comment: "How does a plasmasphere form without an intrinsic dipolar magnetic field..."). This is a minor semantic issue. For the magnetized case the definition of "plasmopause" and "magnetopause" are different but for the unmagnetized case, we use the term "plasmopause" for the boundary of the SW and Earth atmosphere as that is a natural location where the Earth plasma drops to essentially zero. This is also consistent with other literature. One can refer to the papers by the Mariner Stanford Group (1967) [www.science.org/doi/10.1126/science.158.3809.1678] and McElroy et al. (1982) [<https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/GL009i006p00649>].

Comment 9: Line 362-363 (Equation 12): The boundary between the solar wind and Earth's plasma environment should be the magnetopause. The subsolar point of this boundary is conventionally considered the position where the solar wind ram pressure and the magnetospheric magnetic pressure are balanced. Additionally, please verify the relationship between thermal velocity and thermal pressure.

Response: This is a rephrased version of their comment, which we already responded to earlier (see the comment: "The laws of charged and neutral particles are different, please check the equations in your method section." in our rebuttal). The equations in our manuscript are correct. A similar version of the equation is used in Varela et al. (2022) for the PLUTO code-based simulations.

Comment 10: Magnetic pressure does not influence neutral particles. If you take neutral particles into account, the collisionless regime may not be appropriate, as collisions between neutral particles and ions must be considered. However, if the regime is assumed to be collisionless, the pressure of neutral particles should not be included in the equations. In reality, the magnetosphere near the subsolar point of the magnetopause is almost empty.

Response: This is a modified version of their earlier comment: "The laws of charged and neutral particles are different, please check the equations in your method section." We have already addressed this in our rebuttal letter, which the editor appears to have overlooked.

Comment 11: Line 386: Equation 2 does not describe the magnetic field. Please verify it.

Response: It is Equation 1. Corrected.

Comment 12: Line 456-463: The description of the grid is overly complicated and could be better explained

with a schematic diagram, such as Figure 1 in Jia et al., 2015 (<https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2015JA021143>).

Response: On the contrary, it is quite straightforward; however, we can include a schematic diagram in the supplementary materials, similar to the one presented in that paper.

Comment 13: Section 4.2: A brief and clear explanation of how the density of Earth ions is determined or calculated is needed. Additionally, the corresponding equations should be provided.

Response: It appears that the reviewer did not review Subsection 4.2 and the references cited therein.

Comment 14: Figure 5: These four panels are similar; please use the same coordinate system. Panel (a): The disk-like region must be carefully checked. As the authors mentioned in their response, the total density provided by the simulations is $10^4 - 10^5 \text{ cm}^{-3}$, which is several orders of magnitude higher than the density of the plasmasphere ($\sim 10^2 \text{ cm}^{-3}$). Therefore, the authors must carefully examine their simulations and test their model against space observations.

Response: This is again a repetition of their last comment in the previous report, which we have already addressed (see the comment: "In panel a) Why there is a disk with high density and a sharp boundary around Earth..."). Our models have been tested and benchmarked against both observational data and other simulations. We also highlighted this in our rebuttal letter in response to another comment raised by the same reviewer. The reviewer misinterpreted the densities. The values $10^4 - 10^5 \text{ cm}^{-3}$ represent the density at the outer and inner edges of the inner magnetosphere, respectively.

Comment 15: Table 1: Different orientations of the interplanetary magnetic field (IMF) should be considered. It should be noted that the orientation of the IMF has a significant effect on the magnetosphere. When the IMF is southward (northward), the magnetosphere is most active (quiet). Based on Figure 5, it appears that the authors only considered the northward IMF, which results in a quiet magnetosphere. Since the authors are studying a phenomenon that spans a long period, the effects of IMF orientations should be taken into account.

Response: A detailed study of the effect of solar wind field reversals is beyond the scope of the present work. However, our results are most sensitive to the standoff distance, which scales with the cube root of Earth's magnetic field strength. Consequently, even if Earth's field were halved during reconnection events, the resulting change in standoff distance and therefore in atmospheric mass loss would be relatively small.

Comment 16: The figure of Extended Data Figure ED1 is somewhat similar to the Figure 1 of Wei et al.(2019). Maybe you have to read their paper and cite it properly. Wei, Y., Zhong, J., Hui, H., Shi, Q., Cui, J., He, H., et al. (2020). Implantation of Earth's atmospheric ions into the nearside and farside lunar soil: Implications to geodynamo evolution. Geophysical Research Letters, 47, e2019GL086208. <https://doi.org/10.1029/2019GL086208>

Response: The Extended Data Figure ED1 is cited appropriately. The above-mentioned paper does not consider the ions in the full tailside magnetosphere. The escaping materials are not confined to a specific location in the magnetotail, as incorrectly depicted in the gray region in the figure of that paper.

Response to Comments (Revision round: 2)

July 3, 2025

We thank the editor and referees for their constructive comments and have addressed them in the blue responses below. In the revised annotated manuscript, deleted text is shown with ~~maroon strikethrough~~, and added text is highlighted in **green**. Line numbers are from the unannotated revised version of the manuscript.

Editor comments

Comment I: In this specific case we felt that there remained significant concerns regarding the robustness of the analysis and modeling approach.

Response: We have carefully addressed all specific comments on this theme in our rebuttal in each round of our responses to the referees and in the case report. There is an opportunity for further work beyond the present scope, using different methods to focus, for example, on smaller physical scales of the processes involved. However, that approach comes with its own set of complementary approximations and limitations. In the long run, combining different approaches across physical scales is how the community's understanding of phenomenology, particularly that which depends on multi-scale processes, ultimately converges.

It would also be very helpful for us, the authors, if the editorial board could clearly specify any particular aspects of the 'robustness of the analysis and modeling approach' that may still be of concern to them. We fully explained our modeling approach in detail in the manuscript, from pages 12 to 28. The atmosphere and ionization models we used are semi-empirical, meaning they are based on observational data. Furthermore, we have validated our results through comparisons with other MHD models, space-based instruments, and lunar data. All of these details are explained in the Methods section of the paper and in our previous rebuttal.

Reviewer 1 comments

Our case report includes responses to the comments provided by Reviewer 1.

Reviewer 2 comments

Comment I: I have reviewed the revised manuscript titled "Terrestrial atmospheric ion implantation occurred in the nearside lunar regolith during the history of Earth's dynamo" by Paramanick et al. in response to the initial reviewers' comments. The authors have provided detailed and thoughtful responses to all concerns. I therefore recommend accepting this manuscript for publication.

Response: We appreciate the reviewer's positive comment and their recommendation for publication

Reviewer 3 comments

Comment I: I thank the authors for thoroughly addressing my comments. I am still surprised about the low modeled escape rates for the Eoarchean era, but they seem to make sense in the author's theoretical framework. Since the model is, to a certain extent, empirical due to relying mostly on lunar soil measurements, which cannot be directly translated into atmospheric data, for reconstructing the ancient "exobase" level, the results for the Eoarchean may further be taken with caution, even though their model provides reasonable outcomes for the present day. Besides this, I am fine with the article, and I only have a few minor comments.

Response: We thank the reviewer for their positive feedback, insightful evaluation, and support for publication.

Comment II: Line 19 and onwards in the manuscript: I don't think "hydrodynamic exobase" is the best name for the given definition. It is very confusing to use "exobase" when it is not an exobase in the conventional sense. So, I suggest again renaming it and not using the term exobase at all.

Response: We have now removed the term "exobase" and replaced it with "hydrodynamic escape boundary".

Comment III: Line 63: due to sharp deflection -> due to a sharp deflection

Response: Corrected.

Comment IV: Line 162: Archean era -> Eoarchean era (the Archean is an eon, the Eoarchean is an era)

Response: This has now been corrected throughout the manuscript.

Comment V: Line 184: integrating dayside -> integrating the dayside

Response: Corrected.

Comment VI: Line 227: OHE and PHE are not defined in the text (it is in the methods, but not in the main text).

Response: We have now moved the definition to the main body of the text (Lines 208-212).

Comment VII: Line 306: Archean era -> Eoarchean era

Response: Corrected.

Comment VIII: Line 514: I guess Johnstone et al. is ref [91].

Response: The citation for Johnstone et al. has now been included.

Comment IX: Concerning the escape flux unit in the figures and tables: I know that the unit $1/\text{cm}^2 \text{ s}$ is correct, but I find the "-" in the denominator (i.e., $1/\text{cm}^2 - \text{s}$) very confusing, as it reads as " cm^2 minus s". But I guess the editing will take care of this anyway.

Response: We thank the reviewer for pointing this out. We have now removed the hyphen from the escape flux unit.

Response to Comments (Revision round: Final)

October 24, 2025

We thank the editor and referees for their constructive comments and have addressed them in the blue responses below. In the revised annotated manuscript, deleted text is shown with ~~brown-strikethrough~~, and added text is highlighted in navy blue. Line numbers are from the unannotated revised version of the manuscript.

Editor comments

Comment I: Your revised manuscript titled "Terrestrial atmospheric ion implantation occurred in the nearside lunar regolith during the history of Earth's dynamo" has now been seen by our original reviewers 1 and 3 and a new reviewer 4, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

Response: We thank the editors for this update and for their valuable feedback.

Reviewer 1 comments

Comment I: It seems authors have not made the revision as I suggested, and rebut the each comment I raised.

Response: We provided a detailed, point-by-point response to all comments in our previous rebuttal letter, supported by relevant citations. Therefore, this comment by the referee is unjustified and inaccurate.

Comment II: The MHD scheme this paper adopted is not suitable to address the transfer of atmospheric particles. Authors may need significant revision or need to rewrite this article. I have no interest to continue the review of this paper, and I don't want to hinder the publication of this paper (I have no conflict of interest with authors), thus, I suggest editor could seek the other reviewers who are familiar with the MHD or hybrid simulation to review this paper.

Response: We prefer not to provide further comments at this stage, as the manuscript has already been reviewed by the fourth reviewer. Nevertheless, as the reviewer notes, given that the paper focuses on MHD simulations and ion implantation, a reviewer with broader expertise in MHD or hybrid simulations might have been more appropriate than this one.

Reviewer 3 comments

Comment I: The authors incorporated my remaining minor comments. As already stated in my last review, I am fine with the article and still recommend it for publication.

Response: We appreciate the reviewer's positive comment and their recommendation for publication.

Reviewer 4 comments

Comment I: After reviewing the entire manuscript and the author's rebuttal from the second round of review, I find the logic and methodology of this work to be reasonable. It offers valuable insights into solar wind–Earth wind–Moon interactions. The primary concern raised by Reviewer #1 is that this study employs a single-fluid MHD model, which cannot account for the dynamics of different ion types in Earth wind. While a multi-fluid MHD approach would be more suitable for such investigations, this study introduces a “hydrodynamic escape boundary (HEB)” assumption. Above this boundary, solar wind picks up ions from Earth's atmosphere and transports them to the Moon. To my understanding, the MHD simulation in this work focuses on solar wind–Earth wind interactions above the HEB, thereby circumventing explicit modeling of ion-specific dynamics. Within this framework, the derived total Earth wind flux remains acceptable, and the analysis provides meaningful comparisons of lunar surface flux of Earth wind under magnetized vs. unmagnetized Earth conditions.

Response: We thank the reviewer for their positive feedback, insightful evaluation, and support for publication.

Comment II: It seems the term “exobase” in the text should still be replaced with “hydrodynamic escape boundary” in certain contexts. For example, in the key words.

Response: We have now removed the keyword "exobase" and replaced it with "hydrodynamic escape boundary" (Lines 21-22).

Comment III: Line 46-47: The citation to Li et al. [2017] here is inaccurate, they did not present solar wind as the singular source of lunar surface water, but rather as the primary source, while also highlighting the potential significant role of plasma sheet in the formation of lunar surface water.

Response: This has been corrected now (Lines 44-48).

Comment IV: Since the escape rates of different ions vary with the HEB height (as shown in Figure 4), the corresponding Earth wind flux should also differ. Please specify the HEB height used to calculate the Earth wind flux values in Table 3.

Response: We thank the reviewer for pointing this out. We have now added the columns for HEB heights in Table 3.