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Peer Review File

Indirect stratospheric moisture increase after a Pinatubo-magnitude eruption can be comparable to direct increase after 2022 Hunga

Corresponding Author: Dr Clarissa Kroll

Version 0:

Decision Letter:

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Dear Dr Kroll,

Your manuscript for a Comment entitled "Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga" 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 before we can consider the manuscript further. Moreover, in light of the substantial technical aspects, we feel that a revised article would not be suitable for our Comment format, but would need to be submitted as a primary research article, with appropriately documented methodology and background literature. We 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 the criticisms and rewrite the article as a primary research article, 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.

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,

Heike Langenberg, PhD
Chief Editor

Communications Earth & Environment

On Twitter: @CommsEarth

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist: (https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

I cannot recommend publication of this manuscript. This manuscript is written as if there were no measurements of stratospheric water vapor or temperature in the 1990's. Apparently, this conclusion is arrived upon based upon statements made in reference [8].
There were measurements of stratospheric water vapor and tropopause temperature made during the years after the Pinatubo eruption by the HALOE instrument, and these were confirmed by ground-based measurements. After considering the effect of changes in CH_4 oxidation on H_2O, Evans et al. [1998] and Nedoluha et al. [1998] estimated increasing trends for H_2O entering the stratosphere of ~0.06 to 0.07 ppmv/yr for the years 1992-1996 or 1997 respectively. Nedoluha et al. [1998] did suggest that the increasing trend in H_2O was consistent with an increasing trend at the tropical tropopause of ~0.1K/yr, however there was no evidence that this increase was associated with the Pinatubo eruption. Randel et al. [2004], while finding warm anomalies in the lower stratosphere associated with the Pinatubo eruption, found relatively small effects near the tropopause. Thus, while there was an increase in H_2O entering the stratosphere in the years following the Pinatubo eruption, it is not clear that this increase was in any way associated to the eruption itself.
In the absence of available measurements, the study engages in speculation about what the effects a 1K increase in tropical cold-point temperature on stratospheric H_2O would be. The calculation of the increase in stratospheric H_2O that would accompany a volcano that produced a 1K increase in the cold-point temperature seems reasonable, but there is no new science here, and there is no evidence that a volcano has ever produced such an increase. The title itself is therefore incorrect.
References:
Evans, S. J., et al., Trends in stratospheric humidity and the sensitivity of ozone to these trends, J. Geophys. Res., 103, 8715– 8725, 1998.
Nedoluha, G. E., et al., 1998: Increases in middle atmospheric water vapor as observed by the Halogen Occultation Experiment and the ground-based Water Vapor Millimeter-Wave Spectrometer from 1991 to 1997. J. Geophys. Res., 103, 3531–3543.
Randel, W. J., et al., Interannual changes of stratospheric water vapor and correlations with tropical tropopause temperatures, J. Atmos. Sci., 61, 2133-2148, 2004.

Reviewer #2 (Remarks to the Author):

Review of: "Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga" by C. A. Kroll and A. Schmidt

This paper explores the impact of the 2022 Hunga Tonga eruption on stratospheric moisture, comparing it to the effects of a Pinatubo-magnitude eruption. While the Hunga Tonga eruption directly injected an unprecedented amount of moisture into the stratosphere, the study suggests that volcanic sulfate aerosol heating, as seen in a Pinatubo-magnitude eruption, can lead to indirect moisture increases of comparable or even larger magnitude. The authors present a conceptual framework for quantitatively comparing direct and indirect moisture pathways. The indirect pathway involves volcanic sulfate aerosol heating, which leads to increased tropical cold-point temperatures and subsequent moisture flux into the stratosphere. The study emphasizes that the uniqueness of the 2022 Hunga Tonga eruption lies in factors such as injection altitude, observational coverage, and the radiatively-driven plume descent, rather than the magnitude of the moisture increase alone.

Overall this paper is clear and well-written but some of its conclusions may be misleading. While the temperature at the cold point tropopause is a fundamental factor influencing the entry of water vapor into the stratosphere, other finer-scale processes can also play a significant role in modulating this entry.
For instance, the impact of sulfate aerosols on cirrus cloud formation in the Tropical Tropopause Layer (TTL) is a crucial aspect that warrants further discussion. The paper could be strengthened by addressing the potential effects of sulfate

aerosols on cirrus clouds, considering their role in ice nucleation, radiative effects, and moisture distribution. Furthermore, an examination of how these effects could challenge the assumption of a fixed partitioning of moisture fluxes into frozen and non-frozen contributions would enhance the paper's depth and accuracy. Incorporating these considerations would contribute to a more nuanced and comprehensive analysis of the complex interactions between volcanic aerosols and atmospheric processes in the TTL.

The paper's conclusion regarding the influence of sulfate aerosols on the moisture budget of the stratosphere is primarily based on a simple first-order thermodynamic argument, specifically the impact of aerosols on the equivalent frost point temperature. However, this approach oversimplifies a complex problem, and the paper lacks robust support from observations or detailed modeling work. Notably, the absence of exploration into other key parameters, such as changes in large-scale circulation (e.g., Brewer-Dobson circulation) due to aerosol heating and the potential interactions between aerosols and microphysical processes influencing cirrus clouds, weakens the paper's overall analysis. While recognizing the limitations of satellite coverage in 1991 for observing stratospheric water vapor at the scale of the Hunga Tonga eruption, the lack of evidence supporting the substantial indirect pathway claimed by the authors raises concerns. A more comprehensive investigation involving a multi-faceted approach, including modeling various atmospheric processes, and assessing their impact on the moisture budget, would provide a more robust foundation for the paper's conclusions.

Hence in its present form, I do not think the paper is suitable for publication in Communications Earth & Environment.

Reviewer #3 (Remarks to the Author):

Review of comment: “Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga”, Kroll & Schmidt

Dear Editor, dear Authors,

In this comment, the Authors discuss on the possible indirect injections of water in the stratosphere from past eruptions, i.e. through radiatively-driven modifications of tropopause temperatures, in particular in the case of tropical eruptions. The Authors compare the possible indirect injections for selected past eruptions (not well documented with observations for Pinatubo and not documented at all for other test-cases) with the extensively observed direct injection for the recent Hunga Tonga-Hunga Ha’apai eruption in 2022. The reflections in this comment are quite necessary at this point of the debate on HTHH eruption – a rare case of a phreato-Plinian eruption – and the manuscript is well written and clear. I encourage publication of this short comment after the following minor comments are addressed.

My best regards
Pasquale Sellitto

1) L29-30 : Please note that there is a new estimation with IASI observations, as large as 1.0 Tg (https://essopenarchive.org/doi/full/10.22541/essoar.169091894.48592907).

2) L30-32: “Even when considering… [14]”, it would be maybe more appropriate to Zhu et al., 2022 here (your ref. 32)

3) L36: “…water vapour, sulphur dioxide and aerosol…”, this was basically the effect of water vapour. Please remove "sulfur dioxide and aerosol".

4) L37: Please correct: "moisture"-> "plume"

5) L38: Please correct "Heating caused by sulfate aerosol" to "diabatic heating due to radiation absorption within the sulfate aerosol plume” or similar phrasing

6) L40 and more in general: is all discussed in this commentary only applicable to tropical volcanoes eruptions? In case, please state it clearly.

7) L45-46: “The increase…not immediate”, this sentence is not clear to me.

8) L47: You might mean “*sulfate aerosol plume* build-up phase”

9) L67: Please use the wording "temperature increase" instead of "warming"

10) L75: “a 1K increase” --> “a 1K temperature increase”

11) L80: “stratopsheric”: typo

12) L96-107: More on details on typical timescales of the indirect pathways is needed here.

13) L113: “localalised”: typo

14) L114-117: What really drives the indirect pathway is not the sulphur mass estimated using SO2 observations but the particulate sulphur mass (i.e. in the sulphate aerosol). The perturbations of the stratospheric aerosol layer from HTHH are still relatively large - larger than what expected basing on sulphur mass present estimations with SO2 observations. Please correct and develop.

15) L118-119: Not clear to me why it is theoretically impossible to have a positive forcing due to indirect injection of water vapour. Stratospheric water vapour remains a greenhouse gas, no matter how it is injected. Please rephrase this section to account for that.

16) Something is lacking in the title? I would suggest to modifiy “Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga” to “Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga Tonga-Hunga Ha’apai eruption”

Author Rebuttal letter:

Indirect stratospheric moisture increase after
Pinatubo-magnitude eruption at least as large as
direct increase after 2022 Hunga
1. Reviewer #1 (Remarks to the Author):

We want to thank reviewer #1 for their detailed comments. While reviewer #1 agrees with the reasoning of our calculations, their main concern is that the existing observational evidence of the indirect pathway is not covered satisfactory and that there is no observational evidence of the existence of the indirect pathway after the 1991 Mt. Pinatubo eruption. Below, we outline our detailed response (shown in blue font) to the reviewer´s remarks (shown in black font) together with the revisions we made to the text of our commentary piece.

I cannot recommend publication of this manuscript. This manuscript is written as if there were no measurements of stratospheric water vapor or temperature in the 1990’s. Apparently, this conclusion is arrived upon based upon statements made in reference [8].
We do not deny the existence of observations anywhere in the manuscript but discuss the inadequacy of the existing measurements for the respective analysis.
Reference [8] refers to magmatic degassing measurements of the 1991 Mt. Pinatubo eruption and the direct injection. The observational coverage of stratospheric water vapour is discussed later on, perhaps reference [23] of the unrevised manuscript is meant, where we state: “Aside from the difference in timescales, a quantitative comparison between the two moisture transport pathways is further complicated, as the 1991 Mt. Pinatubo eruption – the most-relevant volcanic eruption for the indirect pathway in the satellite era to date – occurred during a time with observational data coverage inferior to today’s standards [23].” This sentence does not claim that there were no observations in the 1990s. Measurements that could be used to evaluate the effect of the 1991 Mt. Pinatubo eruption on stratospheric water vapour concentrations however do neither offer the required large-scale spatial coverage nor a continuous surveillance of the induced

changes as the instruments were launched after the eruption and suffered from several outages. Measurements were acquired with SAGE II (Thomason et al., 2004 [1]), UARS HALOE (Russell III et al., 1993 [2]) and UARS MLS (Livesey et al., 2003 [3]). Both SAGE II and HALOE retrievals failed or are biased due to the large amount of volcanic aerosol in the TTL region and can thus not be used without precautions (e.g. Hegglin et al., 2014 [4], Fueglistaler, 2012 [5], Fueglistaler et al., 2013 [6]). Data from MLS is not flagged as unreliable, but the record starts during the outage of SAGE II and HALOE,

after the eruption of Mt. Pinatubo, and only lasts until 1993 (Davis et al., 2016 [7]).
In consequence the usage of the corresponding data is often depreciated and discarded from analysis. Additionally, the Boulder balloon data (e.g. V¨omel et al., 1995 [8]) and measurements from three short term ATMOS missions (Michelsen et al., 2000 [9]) exist. However, they are spatially and/or temporally limited, which makes them unsuitable for a budget analysis. Importantly, the three references the reviewer pointed out are no exception and also refer to inferior retrieval quality:

- Randel et al., 2004 [10] state:

(i) ”The water vapor retrievals are more uncertain in the enhanced stratospheric aerosol environment following the 1991 Mt. Pinatubo volcanic eruption (Hervig et al. 1995). We screen these data by using only profiles where the retrieval uncertainty is less than 10%, and this effectively causes a loss of data in the lowest stratosphere until late 1992. The HALOE retrievals also utilize temperature profiles from National Centers for Environmental Prediction (NCEP) Climate Prediction Centers (CPC) data (see section 3b) below 32 km for pressure registration and other effects, so that erroneous temperature changes could bias retrievals.”

(ii) ”After omitting the first 6 months of 1992 (due to questions of HALOE data quality), the time series covers July 1992–December 2002 (N = 126 months).”

Note that the eruption of Mt. Pinatubo occurred in June 1991

- Evans et al., 1998 [11] state: ”Because of the eruption of the volcano Mount Pinatubo in the middle of 1991, prior to the launch of UARS, the lower stratosphere was polluted with high concentrations of sulfate aerosol [Bluth et al., 1992; McCormick and Veiga, 1992]. Over time these aerosols have been removed as clean air is circulated through the stratosphere. Corrections for the enhanced scattering caused by these aerosols have been made in the retrieval of HALOE data [Hervig et al., 1995]. Nevertheless, data from below 30 km have been excluded from this study owing to the increased uncertainty arising as a result of this.”
- Nedoluha et al., 1998 [12] state: ”In the lower stratosphere the observed HALOE water vapor trends may be affected by the large change in the stratospheric aerosol loading which has occurred since the eruption of Pinatubo (June 15, 1991). Therefore, although we do note that the HALOE trends in the lower stratosphere seem physically reasonable, we shall not make a direct comparison between the HALOE water vapor measurements and the lower stratospheric measurements of Oltmans and Hofmann [1995] in this study.”

As can be seen in the file with tracked difference (line 55-80) we further elaborate on the measurements in the respective paragraph and also include the references pointed out by the reviewer.

There were measurements of stratospheric water vapor and tropopause temperature made during the years after the Pinatubo eruption by the HALOE instrument, and

these were confirmed by ground-based measurements.
In fact the cited source [23] discusses exactly these measurements by HALOE. It is true that not the entire HALOE data set is affected by the Mt. Pinatubo aerosol, but unfortunately biases are co-located with the aerosol cloud and corresponding heating. As stated by Randel et al. 2004, the loss of data occurs in the lower stratosphere, the atmospheric layers essential to estimate the water vapour entry (e.g. before reaction of water vapour with aerosol or/ozone).

After considering the effect of changes in CH4 oxidation on H2O, Evans et al. [1998] and Nedoluha et al. [1998] estimated increasing trends for H2O entering the stratosphere of 0.06 to 0.07 ppmv/yr for the years 1992-1996 or 1997 respectively. Nedoluha et al. [1998] did suggest that the increasing trend in H2O was consistent with an increasing trend at the tropical tropopause of 0.1K/yr, however there was no evidence that this increase was associated with the Pinatubo eruption.
The authors would in fact be surprised if an increased trend in water vapour entry values from 1992 - 1996/7 could be attributed to the Mt. Pinatubo eruption as the eruption of Mt. Pinatubo occurred in June 1991. Peak temperature increases were

observed 90 days after the eruption [13], e.g. in September 1991. Simulations indicate that the additional water vapour entry into the stratosphere may peak between January 1992 to May 1993 and then start decreasing to base levels for a Pinatubo-magnitude eruption [14, 15]. To investigate the change in water vapour entry values due to the indirect pathway an analysis of the anomalies from 1991-1993 would be needed. In fact, there should not be an increase in water vapour entry due to the indirect pathway in the time frame 1992-1996/7, on the contrary with the reference of 1992 the indirect pathway would lead to a decline of the water vapour values entering the stratosphere, at the latest after May 1993, as the water vapour entry values are returning back to the baseline in this time period. The time frame for the trend analysis by [11] and [12] is therefore unfitting to investigate the indirect pathway, which also was not the aim of these studies. We therefore disagree with the reviewer that the studies they pointed out disprove the existence of the indirect pathway. For a rigorous investigation of the volcanic effect on stratospheric moisture in observational data a regression analysis, including volcanic, ENSO, QBO etc. terms and a suitable time frame would be needed. We would like to refer the reviewer to the study by Tao et al, 2019 [16] who provide the corresponding analysis for three reanalysis data products, showing increased water vapour entry both for 1982 El Chich´on and 1991 Mt. Pinatubo. For Mt. Pinatubo peak values in stratospheric water vapour entry values are reached in early 1992 and return to baseline be 1995. Peak increases of 0.3 - 0.8 ppmv are in agreement with modeling studies on the 1991 Mt. Pinatubo eruption (e.g. [14, 17]). Further, similar analysis can be found in the study by [18], who use satellite and reanalysis data.

Randel et al. [2004], while finding warm anomalies in the lower stratosphere associated with the Pinatubo eruption, found relatively small effects near the tropopause. Thus,

while there was an increase in H2O entering the stratosphere in the years following the Pinatubo eruption, it is not clear that this increase was in any way associated to the eruption itself.
The reviewer refers to the following sentence in Randel et al. [2004]: ”A second source of stratospheric temperature variability in Fig. 16 is the Mt. Pinatubo volcanic eruption in June 1991, which resulted in warm temperature anomalies for approximately 2 yr, but with relatively small effects near the tropopause.” In this sentence the word ”relatively” is crucial. By nature of the aerosol distribution the warming in the TTL will naturally be smaller than in the lower stratosphere, which severed as reference for the temperature increase. This however does not mean, that there is no warming. The small effects mentioned by Randel, 2004 refer to Fig. 16 which lists temperature increases in the TTL ranging between 0.5 - 1.5 K in the TTL. This is in fact in perfect agreement with our assumption of 1 K (0.65 to 2 K). We therefore disagree with the conclusion of reviewer #1 that the increase in H2O cannot be linked to the 1991 Mt. Pinatubo eruption. On the contrary, the model simulations we use as a basis for our estimate seem to be validated by the data in Randel et al., 2004.

In the absence of available measurements, the study engages in speculation about what the effects a 1K increase in tropical cold-point temperature on stratospheric H2O would be.
In our comment we estimate increases in water vapour entry. We feel that the word ”speculation” is quite harsh given that our argumentation is supported by a number of published studies and also not rebutted by the studies pointed out by the reviewer as outlined above. By nature of a comment paper, we have a limited number of references we can cite, but here we list peer-reviewed and published studies on/including the indirect pathway e.g. by Angell et al., 1997 [19], Considine et al., 2001 [20], Rosenlof et al., 2001 [21], Joshi et al., 2003 [22], Krueger et al., 2008 [23], Dessler et al., 2014 [18], Loeffler et al., 2016 [17], Tao et al., 2019 [16], Kroll et al., 2021 [14], Kroll et al., 2023 [24], Killian et al., 2022 [25].

The calculation of the increase in stratospheric H2O that would accompany a volcano that produced a 1K increase in the cold-point temperature seems reasonable, but there is no new science here, and there is no evidence that a volcano has ever produced such an increase.
From this statement we conclude that the reviewer would follow our argumentation chain, if we discussed the observations in more detail. The statement that no new primary research is included in the manuscript is actually obligatory for the comment format: ”Comments do not normally contain primary research data [...]” (compare: https://www.nature.com/commsenv/submit/content-types). We, however, think that a comparison of the water vapour amount injected directly by the Hunga eruptions and the amount entering the stratosphere indirectly would place the Hunga eruption into context to other volcanically-induced stratospheric water vapour perturbations, which is

of scientific interest. The reviewer's comments also demonstrate that the indirect pathway seems to merit more attention from the scientific community as they previously questioned its existence.

The title itself is therefore incorrect.

We disagree with the reviewer. First, we write "Pinatubo-magnitude eruption" and not specifically "1991 Mt. Pinatubo". Second, as outlined above, there is extensive literature on model and reanalysis studies about stratospheric water vapour increases after large-magnitude volcanic eruptions (of which 1991 Mt. Pinatubo is one).

References:

Evans, S. J., et al., Trends in stratospheric humidity and the sensitivity of ozone to these trends, J. Geophys. Res., 103, 8715– 8725, 1998.
Nedoluha, G. E., et al., 1998: Increases in middle atmospheric water vapor as observed by the Halogen Occultation Experiment and the ground-based Water Vapor Millimeter-Wave Spectrometer from 1991 to 1997. J. Geophys. Res., 103, 3531–3543.
Randel, W. J., et al., Interannual changes of stratospheric water vapor and correlations with tropical tropopause temperatures, J. Atmos. Sci., 61, 2133-2148, 2004.

2. Reviewer #2 (Remarks to the Author):

We want to thank reviewer #2 for their comments on our commentary. The main concern of reviewer #2 is that the conclusions drawn in our commentary are not supported by enough studies focusing on individual processes, which can influence the stratospheric moisture budget. They therefore suggest to provide a new study in form a research paper (instead of a comment). We agree that a full research paper might be useful, but our aim was to write a commentary piece. In the following we respond to the review in more detail and explain why our current conclusions are backed up by existing studies without the need of further analysis.

Review of: "Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga" by C. A. Kroll and A. Schmidt

This paper explores the impact of the 2022 Hunga Tonga eruption on stratospheric moisture, comparing it to the effects of a Pinatubo-magnitude eruption. While the Hunga Tonga eruption directly injected an unprecedented amount of moisture into the stratosphere, the study suggests that volcanic sulfate aerosol heating, as seen in a Pinatubo-magnitude eruption, can lead to indirect moisture increases of comparable or even larger magnitude. The authors present a conceptual framework for quantitatively comparing direct and indirect moisture pathways. The indirect pathway involves volcanic sulfate aerosol heating, which leads to increased tropical cold-point temperatures and subsequent moisture flux into the stratosphere. The study emphasizes that the uniqueness of the 2022 Hunga Tonga eruption lies in factors such as injection altitude, observational coverage, and the radiatively-driven plume descent, rather than the magnitude of the moisture increase alone.

Overall this paper is clear and well-written but some of its conclusions may be misleading. While the temperature at the cold point tropopause is a fundamental factor influencing the entry of water vapor into the stratosphere, other finer-scale processes can also play a significant role in modulating this entry.

For instance, the impact of sulfate aerosols on cirrus cloud formation in the Tropical Tropopause Layer (TTL) is a crucial aspect that warrants further discussion. The paper could be strengthened by addressing the potential effects of sulfate aerosols on cirrus clouds, considering their role in ice nucleation, radiative effects, and moisture distribution. Furthermore, an examination of how these effects could challenge the assumption of a fixed partitioning of moisture fluxes into frozen and non-frozen contributions would enhance the paper's depth and accuracy. Incorporating these considerations would contribute to a more nuanced and comprehensive analysis of the complex interactions between volcanic aerosols and atmospheric processes in the TTL.

The authors agree that volcanic sulfate aerosol may in principle also affect cirrus cloud formation, although results from both measurements and modelling studies are ambiguous (e.g. [26–31]). The existing literature on the stratospheric water vapour budget can already explain the water vapour trends based on cold-point temperature deviations caused by the seasonal variations, ENSO, QBO, the BDC and volcanic aerosol heating (e.g. [16, 18, 32, 33]).

One study by Corti et al. [34], suggested cirrus clouds as a key ”transporter” of frozen moisture into the stratosphere, further studies could not verify this hypothesis, which might be related to the fact that sedimentation - which was not considered in the study - counterbalances a huge amount of the radiatively-induced upward motion within cirrus clouds [35]. An observational study has identified deep convection as the main transport mechanism of frozen moisture to enter the stratosphere [36]. High-resolution model based studies have verified this transport mechanism [37,38] in limited area models. Deep convection as main transporter of frozen moisture has also been identified in global storm-resolving simulations [39, 40]. This situation does not change under volcanic perturbations and geoengineering scenarios [24, 35].

As suggested by the reviewer we now also include a short discussion of the aerosol-microphysics interaction in the comment: ”Apart from diabatic heating, the presence of volcanic sulfate aerosols may also influence cloud formation processes, thus impacting the upper tropospheric moisture budget by introducing additional cloud condensation nuclei. The overall impact on upper tropospheric clouds remains a subject of ongoing research, given the only recently emerging model capability to simulate corresponding aerosol-cloud-microphysical processes in global convection-resolving models with the required temporal and vertical resolution. Based on observational data, 80% of the stratospheric moisture budget can be explained by water vapour with the remaining portion being transported into the stratosphere by deep convection with no significant contribution from transport in cirrus clouds [26]. Observations also suggest that accounting for changes in cloud condensation nuclei under volcanically-perturbed conditions may not be explicitly necessary to sufficiently explain the stratospheric moisture budget [27,28].”

The paper’s conclusion regarding the influence of sulfate aerosols on the moisture budget of the stratosphere is primarily based on a simple first-order thermodynamic argument, specifically the impact of aerosols on the equivalent frost point temperature. However, this approach oversimplifies a complex problem, and the paper lacks robust support from observations or detailed modeling work.

Yes, the estimation of the moisture budget changes are based on first-order thermodynamic arguments, this however does not mean that they are incorrect. With our comment we want to highlight the use of a simple conceptual model to estimate moisture increases due to the indirect pathway. We think that, in form of a commentary paper, it is perfectly fine to provide a set if first estimates of the moisture budget changes based on main thermodynamical principles, which we then put in context with the 2022 Hunga eruptions. We absolutely agree with the reviewer that no claims should be made without corresponding supporting studies. Indeed there is an extensive number of published studies supporting the stated thermodynamic description and ”constant partitioning” referred to in the comment. They comprise model, reanalysis and observation based studies. Due to the brevity of the comment format we could not discuss all of them in detail. We now expanded upon the supporting literature and give a short overview on the existing studies here:

Observational studies:

The studies by Liu et al., 2010 [41] and Fueglistaler et al., 2013 [6] find a constant partitioning of the moisture flux in frozen and non-frozen moisture at the cold-point tropopause, equivalent to a constant ΔT offset to the equivalent frost point temperature governing the water vapour at the cold-point tropopause.

Model studies:

Constant partitioning is also found between the seasons when comparing the partitioning in different seasons in the sudies by Ueyama et al. [42, 43]. The thermodynamic control and constant partitioning is evident in the study by Dessler et al., 2016 [32], which uses trajectory models. The study by Smith et al., 2022 [44] demonstrates constant partitioning for a climate change scenario, whereas the studies by Kroll et al., 2023 & 2024 [24, 35] find a constant partitioning for a volcanic eruption/geoengineering scenario.

Finally, when considering the energetics involved in the setting of the tropopause height (e.g. [45]) a constant partitioning would also be expected based on physical principles.

Notably, the absence of exploration into other key parameters, such as changes in large-scale circulation (e.g., Brewer-Dobson circulation) due to aerosol heating and the potential interactions between aerosols and microphysical processes influencing cirrus clouds, weakens the paper’s overall analysis.

We already addressed the interaction between aerosol and microphysical processes influencing cirrus clouds in the corresponding remark by the reviewer above. With respect to the BDC we want to point out that changes in the Brewer Dobson Circulation (BDC) are simulated and thus taken into account in the cited studies to the moisture flux partitioning under sulfate aerosol perturbations [16,24]. As the results from these studies do not change the conclusion of the comment, we have chosen to focus on the main driving mechanisms in order to enhance the clarity of the manuscript rather than to include too many details, for which we would refer the reader to the referenced studies.

While recognizing the limitations of satellite coverage in 1991 for observing stratospheric water vapor at the scale of the Hunga Tonga eruption, the lack of evidence supporting the substantial indirect pathway claimed by the authors raises concerns. A more comprehensive investigation involving a multi-faceted approach, including modeling various atmospheric processes, and assessing their impact on the moisture budget, would provide a more robust foundation for the paper's conclusions.

In our response to reviewer #1, we discuss the observational coverage of the 1991 Mt. Pinatubo eruption in more detail. The limited number of observed eruptions large enough to open the indirect pathway is unfortunate. However, this only means that investigating this physical process is not as straightforward as investigating the direct injection of 2022 Hunga. In the case of the indirect pathway, as discussed in the response to reviewer #1, there are some observations and reanalysis data products, which can be used for verification. Furthermore, all existing climate model studies on the indirect pathway have shown an increase in stratospheric water vapour, whereas no effect - e.g. the non-existence of the indirect pathway - has not been documented. Additionally, the indirect pathway can be explained based on physical principles.

We would not write this commentary paper if there was no literature on the (existence of the) indirect pathway. Examples include peer-reviewed studies on/including the indirect pathway e.g. by Angell et al., 1997 [19], Considine et al., 2001 [20], Rosenlof et al., 2001 [21], Krueger et al., 2008 [23], Joshi et al., 2003 [22], Dessler et al., 2014 [18], Loeffler et al., 2016 [17], Tao et al., 2019 [16], Kroll et al., 2021 [14], Kroll et al., 2023 [24], Kroll et al., 2024 [35], Killian et al., 2022 [25].

The word count in comments is limited, however to address the reviewers comments we now incorporated more supporting studies in the manuscript: "Nevertheless, some radiosonde measurements suggest moisture increases in the TTL and lower stratosphere of 1-2 ppmv following the 1991 Mt. Pinatubo eruption [46]. Importantly, temperature measurements in the TTL at the time of the 1991 Mt. Pinatubo eruption show increases in the lower stratosphere and TTL temperatures of 0.75 - 2 K [19, 47]. Based on these temperature changes, inferences of the stratospheric moisture increase can be made in reanalysis studies integrating corresponding temperature measurements and the reliable portion of stratospheric water vapour measurements [16, 18] or in model studies prescribing the measured sulfur emission or aerosol distribution [15, 17, 20, 22]."

Hence in its present form, I do not think the paper is suitable for publication in Communications Earth Environment.

3. Reviewer #3 (Remarks to the Author):

Review of comment: "Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga", Kroll Schmidt

Dear Editor, dear Authors,

In this comment, the Authors discuss on the possible indirect injections of water in the stratosphere from past eruptions, i.e. through radiatively-driven modifications of tropopause temperatures, in particular in the case of tropical eruptions. The Authors compare the possible indirect injections for selected past eruptions (not well documented with observations for Pinatubo and not documented at all for other test-cases) with the extensively observed direct injection for the recent Hunga Tonga-Hunga Ha'apai eruption in 2022. The reflections in this comment are quite necessary at this point of the debate on HTHH eruption – a rare case of a phreato-Plinian eruption – and the manuscript is well written and clear. I encourage publication of this short comment after the following minor comments are addressed.

My best regards
Pasquale Sellitto

We want to thank P. Sellitto for his comments and suggestions on how to improve our comment. In the following we respond in full to the individual remarks.

1) L29-30 : Please note that there is a new estimation with IASI observations, as large

as 1.0 Tg (https://essopenarchive.org/doi/full/10.22541/essoar.169091894.48592907). We now also include the estimate of the mentioned preprint: ”In contrast, the observed emitted sulfur dioxide (SO2) amounts to only 0.6-1.0 Tg SO2 [48, 49].”

2) L30-32: “Even when considering. . . [14]”, it would be maybe more appropriate to Zhu et al., 2022 here (your ref. 32)
We now cite both studies, LeGrande et al. and Zhou et al.: ”Even when considering potential low-biases in the observational based SO2 estimates, caused by the speedup of aerosol formation due to the increased availability of moisture within the plume [50,51], moisture values are still at least one order of magnitude larger than SO2 values.”

3) L36: “. . . water vapour, sulphur dioxide and aerosol. . . ”, this was basically the effect of water vapour. Please remove ”sulfur dioxide and aerosol”.
The text was adapted correspondingly.

4) L37: Please correct: ”moisture” to ”plume”
Corrected.

5) L38: Please correct ”Heating caused by sulfate aerosol” to ”diabatic heating due to radiation absorption within the sulfate aerosol plume” or similar phrasing
We adapted the text as follows: ”In contrast, the indirect pathway is a consequence of the diabatic heating caused by the volcanic sulfate aerosol layer and relies on the control of moisture fluxes entering the stratosphere by the lowest temperatures between troposphere and stratosphere in the tropical tropopause layer (TTL) – the tropical cold-point temperatures [52, 53].”

6) L40 and more in general: is all discussed in this commentary only applicable to tropical volcanoes eruptions? In case, please state it clearly.
The indirect pathway depends on the warming in the tropical tropopause layer. We therefore focus on tropical eruptions in the commentary or even more general stratospheric water vapour increases due to heating in the tropical tropopause layer. To make this even clearer we added further references to the investigated region (all references are italics), the adapted text now reads: ”In contrast, the indirect pathway is a consequence of the diabatic heating caused by the volcanic sulfate aerosol layer and relies on the control of moisture fluxes entering the stratosphere by the lowest temperatures between troposphere and stratosphere in the tropical tropopause layer (TTL) – the tropical cold-point temperatures [52,53]. In detail, volcanic sulfate aerosol not only scatters incoming solar shortwave radiation back to space, leading to surface cooling, but also absorbs near-infrared and terrestrial longwave radiation. If aerosol is present in the TTL and lowermost tropical stratosphere, the associated tropical temperature increase will then allow for higher saturation specific humidity values and enhanced moisture fluxes into the stratosphere [21, 22]. The increase in water vapour attributable to the indirect pathway depends on the magnitude and duration of volcanic sulfate aerosol heating in the TTL and therefore does not occur immediately after the climactic phase of an eruption but over a longer timescale in the aftermath of the eruption. For a tropical eruption of Pinatubo-magnitude it takes around three to six months after the eruption for the TTL temperature increase to reach peak values and elevated TTL temperatures can then persist for around two years [13, 17].”

7) L45-46: “The increase. . . not immediate”, this sentence is not clear to me.
We rephrased the sentence, adding ”indirect pathway” and expanded upon the second part of the sentence: ”The increase in water vapour attributable to the indirect pathway depends on the magnitude and duration of volcanic sulfate aerosol heating in the TTL and therefore does not occur immediately after the climactic phase of an eruption but over a longer timescale in the aftermath of an eruption.”

8) L47: You might mean “*sulfate aerosol plume* build-up phase”
We wanted to refer to the build-up phase of the temperature increase. We now make this clearer: ”For a tropical eruption of Pinatubo-magnitude it takes around three to

six months after the eruption for the TTL temperature increase to reach peak values and elevated TTL temperatures can then persist for around two years [13, 17].”

9) L67: Please use the wording ”temperature increase” instead of ”warming”.
We rephrased to “rise per degree increase of the EFT” as EFT already includes the term “temperature”.

10) L75: “a 1K increase” to “a 1K temperature increase”
See response to comment above.

11) L80: “stratopsheric”: typo
Is corrected.

12) L96-107: More on details on typical timescales of the indirect pathways is needed here.
We added more details on the timescales for observed temperature increases in the respective paragraph: ”The respective EFT warming is based on the average EFT warming for the year after reaching the highest EFT, whereas maximum temperature increases occur around 90 days after the eruption itself [13].”

13) L113: “localalised”: typo
Is corrected.

14) L114-117: What really drives the indirect pathway is not the sulphur mass estimated using SO2 observations but the particulate sulphur mass (i.e. in the sulphate aerosol). The perturbations of the stratospheric aerosol layer from HTHH are still relatively large - larger than what expected basing on sulphur mass present estimations with SO2 observations. Please correct and develop.
We corrected the respective formulation to: ”Of additional interest is the comparatively low observed amount of sulfur emitted along with the moisture – a similar ratio would never be obtainable for the indirect pathway as the moisture increase is always a function of the heating in the TTL induced by the sulfate aerosol formed from the emitted sulfur.” Unfortunately, the brevity of the format does not allow for an in depth discussion of the sulfate aerosol formation.

15) L118-119: Not clear to me why it is theoretically impossible to have a positive forcing due to indirect injection of water vapour. Stratospheric water vapour remains a greenhouse gas, no matter how it is injected. Please rephrase this section to account for that.
We refer to the total net forcing of water vapour and volcanic aerosol. To make this clearer we rephrased to: “This decoupling led to the possibility of a net positive total forcing after the Hunga eruptions with the positive forcing of the emitted water vapour counterbalancing the negative forcing by the volcanic sulfate aerosol, although the sign of the long-term net radiative forcing is still being discussed.”

16) Something is lacking in the title? I would suggest to modify “Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga” to “Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga Tonga-Hunga Ha’apai eruption”
With the title we are following the volcanologists’ naming convention for the respective volcano which is “Hunga”. In the climate community an increasing number of papers is referring to the “Hunga Tonga-Hunga Ha’apai”, which is the name for the island formation formed due to the eruptions of Hunga.

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Version 1:

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 Kroll,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "Indirect stratospheric moisture increase after Pinatubo-magnitude eruption at least as large as direct increase after 2022 Hunga". It has now been seen by our reviewers, 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, provided you modify the title to ensure it is in line with the level of certainty of your quantitative analysis, and clarify your calculations, as requested by the reviewers. If appropriate, we will publish your manuscript as a Perspective, under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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Best regards,

Heike Langenberg, PhD
Chief Editor
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

What is most concerning regarding this manuscript is that the phrase “at least as large as” in the title. This presents as settled the very important question of the extent to which stratospheric volcanic aerosols change the temperature of the cold point tropopause and allow additional H2O to enter the stratosphere. With a more modest title (e.g. “may be comparable to”) is required.
In order to justify the current title the authors would need to do one of two things, and they do neither.
1) The authors could show from tropopause temperature measurements that the cold point tropopause temperatures after Pinatubo actually did change by the magnitude suggested. This question was studied in the 1990’s following the Pinatubo eruption and, despite a general understanding that such a change in the cold point temperature was important to H2O entering the stratosphere. There are certainly numerous papers (including [34] and [35]) that showed an increase in the stratospheric temperatures, but [34] and [35] do not mention the CPT temperatures, the only relevant temperature for this study.
or
2) The authors could show from measurements of H2O that more H2O entered the stratosphere after Pinatubo than after Hunga. As they point out, stratospheric measurements made after Pinatubo were difficult because of aerosol contamination. The only evidence given for increased stratospheric H2O is two balloon measurements from Boulder taken off of a plot in [33] which, necessarily, shows significant scatter, and which the authors of that manuscript (despite obviously being aware of the Pinatubo eruption) do not themselves highlight. As noted in my first review, there was an increase in upper stratospheric observed in H2O in the years following the eruption, but this increase was much smaller than that which has been observed following Hunga (see DOI: 10.22541/essoar.170688802.25763873/v1).
The final point in the rebuttal, that because the title is “Pinatubo-magnitude eruption” and not “1991 Pinatubo” it is okay to say “at least as large as” makes no logical sense. If the single element “1991 Pinatubo” in the set of “Pinatubo-magnitude eruptions” has not been shown to inject a greater than or equal to amount of H2O into the stratosphere than the Hunga eruption, then the statement is certainly not true for the entire set.
Some other points:
Line 66 – Yes, there are problems with the HALOE measurements in the lower stratosphere in the first few months (and perhaps as long as a year) after the eruption. However, if moist air entered the stratosphere following Pinatubo, that air would take ~1 year to rise to the mid-stratosphere, and another year to rise to the stratopause. The HALOE measurements in the upper stratosphere 1 year after the eruption were not adversely affected by aerosols. The H2O from this period can be compared with the H2O observed by MLS following Hunga. There is no justification for ignoring all of these measurements and relying solely on two frost point hygrometer (not, “radiosonde” as in the current text) measurements.
Line 71 or thereabouts – One study that does actually provide information on CPT temperatures (and not just general TTL temperatures) is Tegtmeier et al. (doi.org/10.1029/2020GL089533). They report QBO variations of +/-0.7K in the cold point tropopause variations in the tropics. Perhaps this should be mentioned to provide context. Distinguishing these QBO-variations from the effect of volcanic aerosols is difficult, as was noted in [35].

Line 127: “1991 Mt. Pinatubo eruption indicated a stratospheric water vapor increase of up 0.8 ppmv between [-20,20]°N at = 400 K, based on trajectory calculations using MERRA-2 and JRA-129 55 reanalysis data with a one-year-average increase of transport values by 0.6 ppmv over a one year-average background transport value of 4.2 ppmv [27].
In [27] they also mention the CLaMS-ERA (for which the temperatures do not include an explicit temperature perturbation based on stratospheric aerosols) gives only a 0.4 ppmv increase for Pinatubo. But, even if this calculation is correct, the increase is still smaller than that observed after Hunga.
Line 130: “Taking into account the long-term moisture transport after 1991 Mt. Pinatubo, this suggests a total increase of around 130Tg over the course of 12 months compared to 50-150Tg for the 2022 Hunga eruptions.”
The 50 Tg estimate of Voemel et al. is a very conservative early estimate based on 9 local soundings taken immediately after the Hunga eruption when the plume was still very inhomogeneous. All subsequent studies suggest values of ~140-160 Tg.

Reviewer #2 (Remarks to the Author):

Overall, this comment paper is compelling and makes an interesting point that considering the indirect effect might be as crucial as the direct effect when assessing the impact of a volcanic eruption on the stratospheric water vapor budget. The study provides two different estimates for the increase in water vapor flux to the stratosphere following volcanic eruptions: one derived from Clausius-Clapeyron scaling (135 Tg) and another from trajectory calculations (130 Tg). These two estimates align closely, suggesting robustness in the overall assessment of the stratospheric water vapor increase following volcanic eruptions.

However, some calculations would benefit from further clarification, especially for the trajectory calculation estimate of 130 Tg. The estimate derived from trajectory calculations is less clear. The paper mentions a 0.6 ppmv increase in water vapor over a one-year period, but it is not explicitly stated over which region this increase applies. Given the context, it is reasonable to assume this is for the 20°S-20°N region at the 400 K isentropic level.

Further details on how the 0.6 ppmv increase translates to the 130 Tg mass increase would be beneficial. Specifically:
- Regional and Altitudinal Assumptions: Is the 0.6 ppmv increase assumed to be over the 20°S-20°N region at 400 K?
- Conversion to Mass Flux: How is the mass flux calculated from the trajectory analysis used to infer the total mass increase of 130 Tg? Detailed steps or an example calculation would help clarify this process.
- Temporal Integration: Is the 0.6 ppmv increase integrated over the entire year, and how does this integration account for atmospheric dynamics and transport processes?

Addressing these points would strengthen the paper by providing readers with a clearer understanding of the trajectory analysis and ensuring transparency in the calculation methods. I would recommend the paper for publication provided the authors address these points.

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Indirect stratospheric moisture increase after a Pinatubo-magnitude eruption can be comparable to direct increase after 2022 Hunga

Below, we outline our detailed response (shown in blue font) to the reviewers' remarks (shown in black font) together with the revisions we made to the text of our perspective piece.

1. Reviewer #1 (Remarks to the Author):

We want to thank reviewer #1 for their comments. Reviewer #1's main point relates to the phrasing of the manuscript title. They suggest to either make it less definite or to add some more information on observational evidence of increases in cold-point temperature or increases in stratospheric water vapour after the 1991 Mt. Pinatubo eruption. We follow both suggetions.

What is most concerning regarding this manuscript is that the phrase "at least as large as" in the title. This presents as settled the very important question of the extent to which stratospheric volcanic aerosols change the temperature of the cold point tropopause and allow additional H2O to enter the stratosphere. With a more modest title (e.g. "may be comparable to") is required.
We have revised the title as follows: "Indirect stratospheric moisture increase after a Pinatubo-magnitude eruption can be comparable to direct increase after 2022 Hunga".
In order to justify the current title the authors would need to do one of two things, and they do neither.
1) The authors could show from tropopause temperature measurements that the cold point tropopause temperatures after Pinatubo actually did change by the magnitude suggested. This question was studied in the 1990's following the Pinatubo eruption and, despite a general understanding that such a change in the cold point temperature was important to H2O entering the stratosphere. There are certainly numerous papers (including [34] and [35]) that showed an increase in the stratospheric temperatures, but [34] and [35] do not mention the CPT temperatures, the only relevant temperature for this study.
or
Most models and reanalysis products show a cold-point warming of around 1 K following the 1991 Mt. Pinatubo eruption [1]. This is also in agreement with observational products. MSU data shows increases of 0.5 to 1 K following the Mt. Pinatubo eruption, in the 150 -50 hPa range with maximum weighting at tropopause level, El Chicón,

an eruption of smaller magnitude even shows values increases of 2 K [2], this might be related to a very unusual meteorological background condition after the Mt. Pinatubo eruption [3]. As the reviewer requested higher resolution data to resolve the cold-point we would like to refer to radiosonde data as well. Corresponding measurements documented cold-point temperature warming of 0.5 to 1.5 K in the inner tropics which were found to be in good agreement with changes in the water vapour entry values of up to 0.5 ppmv [4] . Another study finds an increase of cold-point temperatures by around 1 K in the inner tropics for the Mt. Pinatubo period [5]. Regional studies over China confirm a cold-point temperature anomaly larger than 1 K [6]. We add a corresponding statement in the perspective: "Importantly, Microwave Sounding Units (MSU) temperature measurements in the TTL at the time of the 1991 Mt. Pinatubo eruption also show increases in the lower stratosphere and TTL temperatures of 0.5 - 1.5 K [2, 7], although the heating is not as pronounced at the tropopause level as for the 1982 El Chichón eruption, potentially due to the aforementioned extremely unusual meteorological background conditions in 1991. Radiosonde measurements with a higher vertical resolution however documented cold-point temperature warming of 0.5 to 1.5 K which were found to be in good agreement with changes in the water vapour entry values of up to 0.5 ppmv [4–6, 8]."

2) The authors could show from measurements of H2O that more H2O entered the stratosphere after Pinatubo than after Hunga. As they point out, stratospheric measurements made after Pinatubo were difficult because of aerosol contamination. The only evidence given for increased stratospheric H2O is two balloon measurements from Boulder taken off of a plot in [33] which, necessarily, shows significant scatter, and which the authors of that manuscript (despite obviously being aware of the Pinatubo eruption) do not themselves highlight. As noted in my first review, there was an increase in upper stratospheric observed in H2O in the years following the eruption, but this increase was much smaller than that which has been observed following Hunga (see DOI: 10.22541/essoar.170688802.25763873/v1).

The SPARC authors also confirm that the corresponding signal is characteristic for an episodic event such as the 1991 eruption of Mt. Pinatubo. Some frost point hygrometer measurements even report temporal and local moisture increases in the TTL and lower stratosphere of 1-2 ppmv following the 1991 Mt. Pinatubo eruption [9]. These measurements are in agreement with results from modelling studies of volcanic eruptions of different magnitudes [10].".

The final point in the rebuttal, that because the title is "Pinatubo-magnitude eruption" and not "1991 Pinatubo" it is okay to say "at least as large as" makes no logical sense. If the single element "1991 Pinatubo" in the set of "Pinatubo-magnitude eruptions" has not been shown to inject a greater than or equal to amount of H2O into the stratosphere than the Hunga eruption, then the statement is certainly not true for the entire set.

We are sorry if this statement was misleading. Fortunately it was only included in the response to the review and not the actual perspective work. We actually wanted to stress that our analysis was not performed solely for the 1991 Mt. Pinatubo eruption, as the available observational datasets are unsuited for the analysis without the use of additional reanalysis data and output from model simulations of Pinatubo-magnitude eruptions. In response to the reviewer´s point 1, we cite corresponding observational studies which came to the conclusion that the cold-point temperature after the 1991 Mt. Pinatubo eruption can be as large as 1 K. In the manuscript we also give an example for the water vapour increase of the 1991 Mt. Pinatubo eruption based on reanalysis data estimates which supports our conclusion. As requested by the reviewer we also changed the title, which now takes into account that not the entire set of Pinatubo-magnitude eruptions have to lead to corresponding water vapour increases, e.g. when internal variability is taken into account.

Some other points:
Line 66 – Yes, there are problems with the HALOE measurements in the lower stratosphere in the first few months (and perhaps as long as a year) after the eruption. However, if moist air entered the stratosphere following Pinatubo, that air would take 1 year to rise to the mid-stratosphere, and another year to rise to the stratopause. The HALOE measurements in the upper stratosphere 1 year after the eruption were not adversely affected by aerosols. The H2O from this period can be compared with the H2O observed by MLS following Hunga. There is no justification for ignoring all of these measurements and relying solely on two frost point hygrometer (not, “radiosonde” as in the current text) measurements.
No, the Mt.Pintabuo effect on water vapour concentrations can not directly be deduced one year after the eruption in higher atmospheric layers as they will not provide an accurate measure for the total water vapour entering the stratosphere. Especially in the case of a larger volcanic eruption, water vapour concentrations will become depleted before they reach higher altitudes as a result of oxidation of volcanic sulfur species. For 2022 Hunga the ratio of sulfur to water vapour is far lower than it was after 1991 Mt. Pinatubo, which is one of the aspects that makes 2022 Hunga so exceptional. Additional the point-by-point comparison is complicated by the fact that the amount of water vapour entering the stratosphere after 1991 Mt. Pinatubo is a time-integrated value. For 2022 Hunga, however, the water vapour was injected more or less instantaneously at the time of the eruption. This is why a point-wise comparison (e.g. one location and time) does not provide a fair comparison of the absolute additional amount of moisture entering the stratosphere after these two eruptions. The total water vapour in Tg, however, does. As also suggested in the reviewer’s comment above, we therefore concentrate on cold-point temperatures and now include more references related to the changes in cold-point temperatures in the perspective as outlined above.
We replaced ”radiosonde” with ”frost point hygrometers”.

Line 71 or thereabouts – One study that does actually provide information on CPT temperatures (and not just general TTL temperatures) is Tegtmeier et al. (doi.org/10.1029/2020GL089533). They report QBO variations of +/-0.7K in the cold point tropopause variations in the tropics. Perhaps this should be mentioned to provide context. Distinguishing these QBO-variations from the effect of volcanic aerosols is difficult, as was noted in [35].

Our context is not the indirect effect in relation to other perturbations of the cold-point temperature but the indirect effect in relation to the Hunga eruption. The reference pointed out by the reviewer also does not cover the entire time period in question (e.g. 1991-1993) as it focuses on the GNSS-RO data set (2002-2018) and not on spatially incomplete IGRA data set, which is only shown in Figure 1 and unfortunately not further analyzed.

It is true that the QBO has an impact on cold-point temperatures. QBO variations can be triggered by volcanic or geoengineering perturbations in the TTL and lower stratosphere. Franke et al., 2021 [11] analyse this mechanism and find that westerly shear is favoured. This westerly shear leads to an increase in the cold-point temperatures [12]. Westerly shear and QBO variation can thus be caused by the additional aerosol heating in the lower stratosphere. Both the direct aerosol effect and the indirect aerosol effect on the QBO lead to a warming. Since we focus on the total aerosol effect, the separation of direct, aerosol-induced cold-point warming and secondary QBO-caused warming, which is triggered by the aerosol, would not give further insight into the question we are trying to address, i.e. the total effect of aerosol perturbations on the stratospheric water vapour budget in comparison to the Hunga eruption.

The short format of the perspective makes it necessary to focus on one specific question, which we chose to be the direct v. indirect pathway for stratospheric moisture increases after volcanic eruptions. For clarity and because the magnitude of the QBO signal does not contradict our statement, we would like to stick to our primary question. Following the reviewer's request we have, however, added more discussion on cold-point temperature anomalies after volcanic eruptions as detailed above and in the revised manuscript.

Line 127: "1991 Mt. Pinatubo eruption indicated a stratospheric water vapor increase of up 0.8 ppmv between [-20,20]°N at = 400 K, based on trajectory calculations using MERRA-2 and JRA-129 55 reanalysis data with a one-year-average increase of transport values by 0.6 ppmv over a one year-average background transport value of 4.2 ppmv [27].

In [27] they also mention the CLaMS-ERA (for which the temperatures do not include an explicit temperature perturbation based on stratospheric aerosols) gives only a 0.4 ppmv increase for Pinatubo. But, even if this calculation is correct, the increase is still smaller than that observed after Hunga.

Yes, as pointed out by the reviewer, CLaMS-ERA does not explicitly account for the temperature perturbation based on stratospheric aerosol. This is the reason why their value is lower than the MERRA and JRA values. As this study focuses on the effect of

the aerosol perturbation, there is little point in including studies which only indirectly take the aerosol into account, we therefore omit it due to its lack of relevance.
The main difference between the stratospheric water vapour increase after 2022 Hunga and 1991 Mt. Pinatubo is that due to the nature of the direct injection Hunga shows very high *local* anomalies, whereas the water vapour increase due to the indirect injection shows smaller local concentration increases, which, however, cover a far larger volume and have to be integrated over the period of enhanced cold-point temperatures, e.g. 1-3 years. In the perspective we take a conservative measure of 1 year. Therefore, the relative magnitude of the additional water vapour mass can not be estimated by comparing local changes. The fact that the 0.4 ppmv are smaller than some of the *local* measurements shortly after the Hunga eruption is consequently not contradicting our work. It arises due to the difference in the nature of the direct and indirect pathway, i.e. the fact that for the indirect pathway one needs to integrate over time. When taking into account the time dimension, the total mass of additional water vapour after an eruption of Pinatubo-magnitude can reach values equal or larger than the total mass of additional water vapour injected by 2022 Hunga. We stress this point at the beginning and the end of the perspective: ”In the direct pathway, moisture is directly injected into the stratosphere from within the volcanic plume leading to anomalies in water vapour concentrations that are initially very high and highly localised.” and ”What makes 2022 Hunga unprecedented, however, is the injection altitude, the observational coverage of a direct injection of this magnitude, and the radiatively-driven plume descent shortly after the eruption due to the highly localised moist volcanic cloud.”.

Line 130: “Taking into account the long-term moisture transport after 1991 Mt. Pinatubo, this suggests a total increase of around 130Tg over the course of 12 months compared to 50-150Tg for the 2022 Hunga eruptions.” The 50 Tg estimate of Voemel et al. is a very conservative early estimate based on 9 local soundings taken immediately after the Hunga eruption when the plume was still very inhomogeneous. All subsequent studies suggest values of 140-160 Tg.
We now state the individual estimates explicitly in the introduction rather than providing a range of estimates. This makes it clear that the estimate stated in Voemel et al., 2022 is an outlier: ”Estimates for the directly injected water vapour include 139 ± 8 Tg [13], 146 ± 5 Tg [14], 50 Tg [15] and 70-150 Tg [16], which is approximately equal to 5-10 % of the stratospheric background water vapour burden.”.

2. Reviewer #2 (Remarks to the Author):

We want to thank reviewer #2 for their comments. Their main wish is to add some more information on the calculation of the stratospheric water vapour increase. We have addressed this comment in full as outlined below.

Overall, this comment paper is compelling and makes an interesting point that considering the indirect effect might be as crucial as the direct effect when assessing the impact of a volcanic eruption on the stratospheric water vapor budget. The study provides two different estimates for the increase in water vapor flux to the stratosphere following volcanic eruptions: one derived from Clausius-Clapeyron scaling (135 Tg) and another from trajectory calculations (130 Tg). These two estimates align closely, suggesting robustness in the overall assessment of the stratospheric water vapor increase following volcanic eruptions.

However, some calculations would benefit from further clarification, especially for the trajectory calculation estimate of 130 Tg. The estimate derived from trajectory calculations is less clear. The paper mentions a 0.6 ppmv increase in water vapor over a one-year period, but it is not explicitly stated over which region this increase applies. Given the context, it is reasonable to assume this is for the 20°S-20°N region at the 400 K isentropic level.
Yes, this is correct. The region between 20°S-20°N covers the area where most of the water vapour enters the stratosphere. We reformulated the corresponding paragraph to clarify this point: "In contrast, analysis of the 1991 Mt. Pinatubo eruption indicated a stratospheric water vapor increase of up 0.8 ppmv, based on trajectory calculations using MERRA-2 and JRA-55 reanalysis data between [-20,20]°N at $\theta = 400$ K. The corresponding one-year-average increase of transport values amounted to 0.6 ppmv over a one-year-average background transport value of 4.2 ppmv [17].".

Further details on how the 0.6 ppmv increase translates to the 130 Tg mass increase would be beneficial. Specifically:
- Regional and Altitudinal Assumptions: Is the 0.6 ppmv increase assumed to be over the 20°S-20°N region at 400 K?
Yes, as stated above the formulation was adapted to make this clearer.
- Conversion to Mass Flux: How is the mass flux calculated from the trajectory analysis used to infer the total mass increase of 130 Tg? Detailed steps or an example calculation would help clarify this process.
The calculation is based on the 900 Tg water vapour flux into the stratosphere [18] and the 0.6 ppmv increase over a 4.2 ppmv background. The corresponding calculation is:

$$\delta q_{entry} = \frac{900\,Tg}{year} x \frac{0.6ppmv + 4.2ppmv}{4.2ppmv} = \frac{128.57\,Tg}{year} \tag{1}$$

We added the corresponding calculation in the perspective.

- Temporal Integration: Is the 0.6 ppmv increase integrated over the entire year, and how does this integration account for atmospheric dynamics and transport processes?

As the calculation is based on values from the literature the specific atmospheric dynamics and transport processes in this setup could not be taken into account.

Atmospheric dynamics: We assume an unaltered upwelling term in the TTL. In the aftermath of volcanic eruption upwelling has been found to increase due to the additional heating tendencies in the stratosphere [19, 20]. However, studies focusing on observations have found little effect in the TTL [21], the atmospheric layer critical for water vapour entry into the stratosphere.

Transport processes: Not having performed the cited studies ourselves we do not have the information of potential changes in the transport processes. However, it is well documented in the literature (e.g. [22–24]) that the stratospheric water vapour budget is to first order described by changes in the cold point temperature which directly translates in the quantity used for our calculations, the water vapour values. The importance of different moisture transport pathways into the stratosphere have been investigated in [25] and it was found that their fractional importance did not change even under very strong perturbations (e.g. larger than 9 K in the tropical cold-point temperatures.).

Addressing these points would strengthen the paper by providing readers with a clearer understanding of the trajectory analysis and ensuring transparency in the calculation methods. I would recommend the paper for publication provided the authors address these points.

We now clarify the calculation by adding a longer explanation: " Considering these boundary conditions, a net influx of 900 Tg into the stratosphere [18] and unchanged relative importance of moisture transport pathways, which were demonstrated even under cold-point temperature perturbation of more than 9 K [25], the stratospheric water vapour increase after 1991 Mt. Pinatubo, amounts up to around 130 Tg over the course of 12 months compared to 50-150 Tg for the 2022 Hunga eruptions. The value of 130 Tg is based on

$$\delta \mathrm{q}_{entry} = 900\mathrm{Tg} \quad \frac{0.6\mathrm{ppmv} \dot{+} 4.2\mathrm{ppmv}}{4.2\mathrm{ppmv}} + 900\mathrm{Tg} = 128.57\mathrm{Tg}. \tag{2}$$

This estimate assumes a constant upwelling term. Some modelling studies have shown aerosol heating induced increases in upwelling after volcanic eruptions, especially near the atmospheric levels with highest aerosol concentrations [19,20], however observational based studies have found little effect in the TTL region [21].".

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