nature portfolio

Peer Review File

Pristine helium from the Karoo mantle plume within the shallow asthenosphere beneath Patagonia

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

Thank you for giving me the opportunity to review this manuscript. This study continues the effort to identify subducted oceanic crust and hotspot signatures in mafic lavas. The concept of mantle re-fertilization from subducted hotspots has been shown in Central American basalts, but not in Central-North Patagonia during the Cenozoic. The authors put forth a novel sequence of events that integrates radiogenic isotopes, trace elements, noble gas isotopes, geochronology, and seismic tomography to trace the source of magmatism underneath Somuncura and Canquel to a fossil hotspot trail. These methods are appropriate for assessing a range of processes. Constraining the transfer and cycling of elements and volatiles via metasomatism is important for understanding the origin of enriched mafic lavas and their fate during subduction.

The results draw upon previous work from Central Patagonia and several SE Pacific hotspots. The authors take care to choose representative geochemical data for their regional setting and relevant localities. Overall, the analytical procedures and methods are suitable and consistent with previous studies of the region. I am unable to comment on the specifics of Patagonian regional tectonics or seismic tomography, but I have some concerns about the underlying assumptions used to interpret Helium isotopes and clarity of the geologic model. I believe this work is publishable, and would be highly impactful if the following points are addressed:

1. The fossil(ized) hotspot trail is a major focus of the title and abstract. It would be an extremely novel discovery– however, the existence is mostly supported by Helium isotopes, extracted from crushing. Is the Helium from the same source as the SE Pacific hotspots? Is Helium decoupled from Sr-Nd-Pb? Is it possible to quantify the relative contributions of mantle, radiogenic, and atmosphere? Can there be more explicit characterization of the 3He/4He of the hotspot and preservation of this signature? Such estimates or discussion of the limitations should be included.

2. The sequence of events, beginning with fossil hotspot volcanism and leading up to Andean back-arc volcanism, was introduced in the abstract and further illustrated in the discussion. While highly detailed, it is unclear which findings were supported directly by the new data, and/or previous studies. A numbered list with approximate ages and separation of events would be useful. Considering the multidisciplinary breadth of the journal, the manuscript would benefit significantly with the addition of a figure showing the proposed sequence. This is a very dynamic setting with distinct periods of activity over millions of years. A clear diagram would further communicate the impact of the findings.

In summary, this work is publishable, and the information will be more impactful and accessible with minor revisions. Specific comments are attached, and these include suggestions for figures.

Best Regards,

Thi Truong

Reviewer #2 (Remarks to the Author):

The article entitled Recycling of a fossil hotspot trail beneath Central-North Patagonia during Cenozoic presents new geochemical, isotopic and geochronological data on the Cenozoic magmatism of Patagonia. Although these data are valuable, the interpretations made from them in this article must be fully revised and corrected. The article has numerous problems, which have been explained in detail in the attached file. First, the authors indicate that the low-velocity seismic anomaly beneath Patagonia is produced by the hot-spot track subducted material, in order to explain the peculiarities of the Patagonian Cenozoic magmatism. The back-arc magmatism of Patagonia began ~50 Ma ago, so the hot spot track must have been subducted during the Late Cretaceous-Early Paleocene, and today is still exactly below the Cenozoic intraplate basalts. There was not westward continental drift?

There are multiple papers showing the opening a slab window by the Chile Ridge subduction at ~46° S. Excactly the same location. It is widely known that a slab window induces partial melting of the sub-slab portion of the upper mantle at ~100 km depth (e.g., Breitsprecher and Thorkelson, 2009; Guillaume et al., 2010; Georgieva et al., 2019), so the low seismic velocity anomaly under a sector of Patagonia is, and has been attributed to the subduction of the Chilean ridge. The authors have mentioned this slab window in this work, so it is striking that they do not consider it to explain this seismic anomaly. The chile ridge began to be subducted beneath Patagonia at 16-14 Ma (e.g., Muller et al., 2016 between many others) and with it, the beggining of the most modern intraplate magmatism of Patagonia.

Likewise, the seismic tomography of Patagonian region has also shown an N-S slab tearing at western Patagonia (e.g., Aragón et al., 2011; Navarrete et al., 2020), so, the northward elongation of the seismic anomaly can also be explained by this gap.

The authors should take into account these previous data from Patagonia.

Also, it is obvious that both the Chile ridge and the Juan Fernández hot spot show similar low velocity seismic anomalies at ~100 km depth, since both the ridge and the "plume" generate decompression-induced magmatism at those depths. The seismic resemblance cannot be used as evidence of a subducted hot spot.

Moreover, it is widely known that the subduction of a hot spot track induces modifications on the subducted slab such as its shallowing (i.e., Juan Fernández hot spot track). It was not considered by the authors.

Finally, the authors do not consider any other possibility to explain MORB mantle metasomatism, such as the mantle transition zone that is currently being studied worldwide since much of the intraplate magmatism has recently been attributed to recycled subducted materials in this mantle layer.

Reviewer #3 (Remarks to the Author):

First, I want to acknowledge that the data itself is outstanding and that the discovery merits a high-impact publication. That said, the interpretation is overcomplicating the results. The authors discovered a clear connection to a primordial, deep Earth source (3/4He up to 27 RA!) for these lavas. There is no need to invoke recycling of hotspot tracks where the signal is so unambiguous. This interpretation is also not supported by modeling of physicochemical mechanisms that will allow 3/4 He to be recycled from subducted material that conceptually will be degassed. I think the authors need to carefully evaluate the alternative, and more logical explanation these these Patagonia lavas sampled a mantle plume.

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

It appears that the authors have addressed the reviewers' suggestions and clarified the interpretation in the context of the data and previous understanding of regional tectonic events for the given time period. The geodynamic interpretation in the discussion and addition of Figure 3 was much needed in the first version of this manuscript, and is a beneficial addition. There are a few figures that would benefit from additional edits as previously suggested. However, based on the changes I would recommend the revised manuscript for publication.

Reviewer #2 (Remarks to the Author):

My comments, corrections, and suggestions are indicated in the attached PDF file.

Good luck!

Reviewer #4 (Remarks to the Author):

Review by Jens Hopp of manuscript “Shallow asthenosphere with pristine helium from the superdeep mantle beneath Patagonia” by Jalowitzki et al., submitted to Nature Communications.

The thematic issue of this manuscript is identifying a superdeep (>300 km) mantle component by its elevated 3He/4He-ratio in mantle-derived rocks from Patagonia. For this purpose high 3He/4He-ratios found in mantle xenoliths from various locations of various age (but predominantly Pliocene) in Patagonia are interpreted within a geotectonic reference frame encompassing the last hundreds of millions of years. If the postulated scenario is true, this would be the first evidence for a superdeep wet plume.

In order to link a superdeep component to the Patagonian magmatism in time the authors propose (1) a geodynamic model for the geologic evolution of the respective crust-mantle evolution, (2) the presence of water in the transition zone that is responsible for formation of hydrous minerals observed in the mantle xenoliths and (3) a link of the helium isotope record to the postulated upwelling of superdeep material, which in turn link the water with the transition zone. To manage this the authors have to propose a quite controversial topic, namely the presence of significant volumes of water within the transition zone. But this cannot be proven by the data (a sponge might be dry, no matter of its storage capabilities). The only what the authors can prove is that water plays a role in formation of hydrous minerals without any inference of its origin. The helium could have other sources as well. Other explanations might also work, in particular the introduction of water by dehydration of an oceanic slab. This situation – hydrous minerals and subduction – is frequent and even might lead to a coexistence of primary minerals (from a "dry" mantle region and "wet" slab fluids). Another objection here is the expected lateral extension of this upwelling component – this is not visible in a 2D figure. From the authors geodynamic model a line of volcanic units might be characterized by this superdeep signature (its not a "hotspot" sensu strictu) – is there any evidence for this?

My main objection of this topic is not the water itself but the presented He data, which are the basement of their conclusions – no high 3He/4He, no superdeep component. The authors made a lot of analyses, also of heated complementary samples to their crushed specimens. A lot of work, I appreciate that. Their heating He data show a clear presence of cosmogenic He in the mineral lattices – if not, the signature is typical lithospheric helium. There are some studies showing that such cosmogenic He is not relevant if a stepwise crushing method is applied for gas extraction. But this is not the entire truth. Clearly, He diffusion might redistribute He from lattice sites into fluid inclusions, in particular in old samples like the Cerro Chenque xenoliths, and in addition there is a certain probability to redistribute the helium via recoil into fluid inclusions. Commonly, small amounts of cosmogenic He are hardly recognized within an "ocean" of mantle helium. But the mantle xenoliths investigated for this study are quite low in gas concentrations. A given range of 4He of <10-9 ccm STP/g is equivalent of about E-14 ccm STP/g 3He. To account for the reported high 3He/4He-ratios requires not really much cosmogenic He. For example, the highest values observed during crushing of samples from the PM12 suite (single strokes) from Cerro de los Chenques (is this the same as Cerro Chenques?) are associated with lowest 4He concentrations, i.e. these samples are most prone to contamination with cosmogenic helium. I would not build a quite substantial model of geochemistry upon samples which are suspect of containg a minor, but in the end significant, cosmogenic helium component. Other examples of stepwise crushing show increasing 3He/4He with increasing number of strokes, e.g., both series of PM10. Sample PM 7 shows within errors a constant ratio of about 6.2 Ra, i.e. as soon as we enter the field of lithospheric signatures no systematic increase is apparent. But for PM7 4He concentrations are one order of magnitude larger, as is the case for PM4 with asthenospheric 3He/4He. From all samples in this study only one extraction is a potential candidate for a mantle helium signature, PM7-B1 Ol 2000x (single crush). In this case 4He concentrations are higher and 3He/4He is about 14. Which is peculiar given the non-existence of such a component in the stepwise crushing experiment. And sorry, one single – only possibly – correct mantle value is not enough to constrain a superdeep wet plume model. Note, that in the latter sample no clear evidence of cosmogenic neon is visible (I would not necessarily expect that anyway).

Provided the He is truely of mantle origin (which I doubt): It could be also possible that the Eocene Cerro Chenque volcanics might be related to an enrichment in 3He within the local lithosphere during the earlier rifting process, documented in Fig 3c, but unrelated to formation of hydrous phases from subduction related fluids.

As probably is clear from above I unfortunately cannot really support publication in its current form though I like the general geodynamic model. But the data appear not robust enough to support the conclusions presented by the authors.

Detailed comments:

L. 150: "… a pristine helium composition …" or "…pristine helium compositions…"

Same paragraph: I don't think that the existence of low T xenoliths excludes a high T hotspot. If an ascending melt transports helium upwards this He (together with CO2 and water) can be incorporated en passant in colder host rocks.

L. 230ff: This sentence is too long and should be shortened in two pieces at least.

L. 233: " allow for"

L. 237: "… geometry of the subducting slab…"

L. 243: "… peak of magmatism …"

L. 289: It is difficult to judge from the outside, but websterite could be a former magmatic vein or, alternatively, have a cumulate origin. To conclude, its presence supports a hybridization model, is at least ambivalent.

Figure 1: Write " Antarctic Plate", "Inferred Collision Zone"

Figure 3: Might it be possible to mark the positions of the xenoliths in time by a number or "x" (or something similar) in c), d) and e) ?

Supplementary information:

Corrections: Correction for atmospheric He is commonly done with the air He/Ne ratio, like here. This is of course a possible way to do it, but since the trapped atmospheric component not necessarily is equivalent to the air element ratio this is a bit random. It gives an order of magnitude of a possible interaction with air, but I am not sure if this is a well-defined correction. Nevermind, the extent of correction is negligible in these samples.

More complicated is the correction for cosmogenic 3He. The authors use the crush results as representative of a true mantle 3He/4He and then calculate the cosmogenic 3He for the heated samples. But this requires that the crushed helium data are not corrupted by cosmogenic helium in itself… Note that during crushing cosmogenic He is likely overrepresented in the fluid inclusions relative to Ne (see above), so I have doubt that a correction will correctly ever reproduce the real amount of cosmogenic helium in crushed samples. This is in this study crucial.

In the supplementary information, paragraph L. 146ff, the authors calculate cosmogenic 3He concentrations. I can't follow this. Their given 3He(cos) is about 2 x 10E-10 ccm STP in PM10-B2ol, but total measured 3He is about 6 x E-13 ccm STP/g. The weight is about 280 mg, so a factor of ca. 4 is still needed to make the numbers comparable (sample 3He is ca. 1.5 x E-13 ccm STP), but the discrepancy is three orders of magnitude… In turn, to get a high 3He/4He-value (from an asthenospheric base value of 7 Ra) requires only addition of an absolute 3He(cos) concentration of ca. 8 x 10E-14. Which is not really much. Looking at Ne is not really helpful, because the 3He/21Ne-ratio in inclusions might differ from production ratio. And their given value of 0.001 seems to support this (however, I am not sure if this value has any relevance if the 3He(cos) is already not correct). Note, that in their given example Ne blank correction is reported to be 198%., which doesn't facilitate a comparison. Is it possible that the authors forgot to include an exponent in their calculations (in Suppl Table 2 at least 3He is given in E-6 in the value fields, but with E-8 for overall columns, which gives E-14…). Looking at their 3He/21Ne(cos) ratios in heated samples with a denoted 70-100% cosmogenic component provides values of 8-12, whereas in the crushing samples the ratio is quite small. I guess, most Ne is in fact non-cosmogenic… which would fit my expectations and explains the ca. air composition in most samples.

Revision notes

We would like to thank the three reviewers for their constructive comments and suggestions. They provided different improvements according with their expertise's. Based on these comments, we carefully revised all results and the calculations involved in the treatment of data. We will address all the major and minor comments of the reviewers as follows. A word file containing the track changes also is resubmitted. The numbers of lines indicate the modifications made on the updated file considering the track changes.

(*Text highlighted in italic and blue color = Reviewer's comments*)

Reviewer #1 (Remarks to the Author):

It appears that the authors have addressed the reviewers' suggestions and clarified the interpretation in the context of the data and previous understanding of regional tectonic events for the given time period. The geodynamic interpretation in the discussion and addition of Figure 3 was much needed in the first version of this manuscript, and is a beneficial addition. There are a few figures that would benefit from additional edits as previously suggested. However, based on the changes I would recommend the revised manuscript for publication.

Response to Reviewer #1

We appreciate very much the positive feedback given by Reviewer #1 and we would like to apologize for does not properly respond to the first round of reviewers. It was a consequence of our decision of completely reformulate the manuscript. However, here we will address the answers for all comments that are related to the data and/or interpretation maintained after the reformulation of our manuscript. All modifications made in response to the comments given by Reviewer #1 were applied in the revised manuscript.

Proofreading of grammar and corrected case is needed in a few instances (for example, in the abstract). Both "Juan Fernandez" and "Juan Fernández" appear in the text and *figure captions. "Somuncura" and "Sumuncura" are also present in different places. Perhaps these are interchangeable in this case but I am unsure.*

Response. We carefully adjusted all occurrences along the text, as well as in the figure captions.

Consider revising word choice or provide evidence when using particular phrases. For example, in the abstract:

"undoubtful" is perhaps not an effective word here.

Response. This word was deleted.

Including some detail about FOZO or "C" may be useful here. Arguably, Helium isotopes are not straightforward tracers for mantle endmembers in some instances. This limitation should be discussed briefly.

Response. As suggested, we detailed the concept of FOZO, PREMA, and C components between the lines 268-286 and 406-415. Regarding the assumption about the limitation of helium isotopes to constrain mantle endmembers, we believe that they in fact are a powerful toll to this purpose. We understand as a limitation the situation in which there is a mixing between endmembers, but this complexity is usually solved through neon isotopes (and sometimes using argon). Of course, radiogenic isotopes and other geochemical proxies also are useful to make sure about mantle endmembers. An example of this limitation to our work is the high-$^{3}He/^{4}He$ ratios that would be primordial or cosmogenic in origin. This possibility was deeply explored in the main text (lines 120-136, 224-251), as well as in the Supplementary Information (lines 56-59, 134-206) based on the considerations given by Reviewer #4.

Do errors vary for samples or for the type of material? Is there an explanation for the sample with ±7 RA?

Response. In terms of noble gas isotopes, the mantle endmembers are especially well-defined for the oceanic basalts (MORB and OIB) because the volcanic glasses and phenocrysts usually are gas-rich samples compared to the SCLM, which is represented by mantle xenoliths (gas-poor xenocrysts). The subduction-related reservoir has mainly been defined based on phenocrysts and/or volcanic gas samples. Therefore, the representative samples of each tectonic environment define the compositional variation among them. In this case, the type of material (i.e., glass, phenocryst, xenocryst, gas) will determine the amount of gas. A sample with $^{3}He/^{4}He$ isotopic ratio of 7 R_A can be defined as belongs to MORB or SCLM reservoirs and, in this case, it is necessary additional evidence to elucidate this result. For instance, neon isotopic ratios are essential solve this doubt. Alternatively, additional measurements are necessary to verify if the predominance of results indicates MORB or SCLM.

Assuming these are used to indicate the pristine and deep component, are there any characteristics about the samples with 26-27.74 RA samples that are noteworthy? Phase, location, etc. Is it significant that the highest 3He/4He is from a websterite?

Response. This question was properly discussed in the second version of our manuscript, where we associated this notable high-$^{3}He/^{4}He$ ratio to a hypothetical trans-lithospheric suture zone related to the Carboniferous-Permian collision of two continental blocks (Deseado and the North Patagonian massifs). This suture zone coincides with a region characterized by low values of elastic thickness estimations, as well as by a shallow lithosphere-asthenosphere boundary (~50 km). This discussion was added to the abstract (lines 32-35), as well as to the main text (lines 170-186, 256-259, 290-293, 315-318, 367-376, 410-415). Furthermore, based on this study, we consider that there is an association between pyroxene-rich lithologies and the high-$^{3}He/^{4}He$ ratios (lines 388-391).

Are there any further constraints on this component, e.g. noble gas signature and depth? Could He-Ar results be used to assess mixing or contributions for air, upper mantle, and deep mantle for samples with the highest 3He/4He?

Response. We invoked the existence of a shallow asthenosphere beneath the inferred collision zone, which favored the upwelling of primordial ^{3}He from the superdeep mantle (lines 170-186, 317-318, 367-376). Concerning the argon results, unfortunately they are not useful to our discussion due to the significant contribution from the air.

Regarding "the original helium composition of the SCLM was superimposed". How much Helium was transferred, what concentrations are needed to reflect MORB and/or OIB contributions?

Response. It is important to consider that the noble gases are chemically inert, being modified by physical (mechanical) processes. Hence, here we assume that the original helium composition of most SCLM mantle xenoliths investigated in this study was replaced by subsequent events during to the long-lived tectonic evolution recorded by the

SCLM beneath Patagonia. These events potentially include the influence of subduction-related components (radiogenic $^3He/^4He$ ratios) and the events associated to the consumption of oceanic slabs throughout the geological time. For example, the development of slab-windows that favors the asthenospheric upwelling to the mantle wedge and induces the overprint of the SCLM by MORB-like signature. Finally, the helium ratios containing OIB-like composition are a challenge to explain in the context of Patagonian back-arc. However, it is our main objective in this work. With respect to how much helium was transferred to justify the replacement of the SCLM composition by other endmember (i.e., MORB, radiogenic, OIB), it is quite difficult to estimate considering that the concentration of helium varies significantly depending on the analyzed material (i.e., volcanic glass, volcanic gas, phenocryst, and xenocryst), which can exhibit any composition.

Where do the thermometry estimates come from?

Response. I am sorry, but probably I does not understand the question. However, I will try to answer. The temperature estimate mentioned here (line 213) was taken from Bertotto et al. (2021). Considering the suggestions given by Reviewer #4, this part of the main text was reformulated, and the estimates previously mentioned were disregarded.

How can you tell it is stagnant? And is there a strongly high-velocity anomaly between 80-100 km depth that is east of Canquel?

Response. In this case, we are considering the paper published by Navarrete et al. (2020) as reference to affirm that the high-velocity anomaly observed between 300 and 600 km represent the stagnated (fossil) Farallon oceanic plate. I am sorry because I didn't indicate this reference in the first version of our manuscript.

Unfortunately, due to the limited number of seismographs acquiring data in this region, we have a limited resolution that unfeasible the certainty of the existence of tomographic anomalies with less than 100 km. However, the high-velocity anomaly observed between 80 and 100 km east of Canquel represent the oceanic crust (in the Atlantic Ocean).

Not sure if I would say I see the similar anomalies if profiles at the other latitudes are not shown. Perhaps the profiles can be included in a supplementary figure.

Response. I completely understand your point-of-view. Here we were comparing the low-velocity anomaly between 50 and 180 km based on the image (a), which represent the tomographic result in 100 km depth. For the second version of our manuscript, we don't stablish this comparison. Hence, we decided don't modify this figure including the vertical section related to the region of Juan Fernandez and San Félix.

I find the 3He/4He error bars are light colored and difficult to see.

Response. We modified the intensity of color in the error bars.

It is difficult to discern if the cluster of samples with the highest 3He/4He are PM10-B2 OL or PM10-Q77 WR. These may be confused easily with PM-7 B1 WR. Perhaps include labels for the most important samples.

Response. Considering the comments given by Reviewer #4, we have decided to assume as primordial only the samples PM10-Q77 WR and PM10-B2 Ol. For this reason, we moved the other samples with high-$^{3}He/^{4}He$ ratios to the Supplementary Figure S1, which includes the high-$^{3}He/^{4}He$ ratios related to the cosmogenic contribution.

I suggest including a San Felix value here. Sample 99654 has an olivine crushing measurement reported in Jackson and Steinberger (2021) in G3, Table S2. 10.1029/2020GC009525

Response. We appreciate very much for this recommendation, but we consider that that this result ($^{3}He/^{4}He$ = 6.97 ± 0.21 R_A) isn't essential to our discussion. Also, it would represent an additional citation in the reference list due to only one isotopic ratio.

It may be misleading to show the arrow trending towards "Pristine component" starting at >10 RA, as this suggests 10 RA qualifies as pristine, even though that is somewhat in the realm of MORB. A pristine component would be better portrayed if the arrow points off the diagram to much higher values. As is, perhaps "OIB source", or "Less degassed mantle" is more appropriate.

Response. We agree with this comment, although usually it is assumed that a primordial component has >10 R_A. Thus, based on this comment, we adjusted the information related to the arrow as "less degassed mantle".

Are SE Pacific hotspot data all from lavas, i.e., whole rock? Are you distinguishing between mineral separates and whole rock? Juan Fernandez only has Helium isotope measurements on olivine and clinopyroxene.

Response. In our opinion, there is no difference between the type of material employed to noble gas measurements. Thus, we didn't select a specific type of sample to plot in the $^{3}He/^{4}He$ vs. ^{4}He diagram. However, the helium isotopic data from Farley et al. (1993) are from olivine separates, except for the sample PF-20 CPX (clinopyroxene). Differently, Poreda et al. (1993) discussed the helium results of basaltic glasses. The results reported by Yamamoto et al. (2020) for petit-spots also are from basaltic glasses. On the other hand, the paper of Kurz et al. (2009) includes helium isotopic data from basaltic glasses and olivine separates.

I am not a seismologist so I cannot comment on the use of the tomographic slices given for the region. This figure would benefit from annotations that could connect the proposed sequence of events and components with the view from seismic tomography. Some suggestions below:

- *Label y-axes on a) and b)*

Response. As suggested by Reviewer #1, the labels were inserted.

- *Index contour should be specified in the caption and perhaps labeled in diagram*

Response. Aiming to facilitate the identification of the variation of tomographic anomalies, the indices for each contour were included, labeling their respective values.

- *In a) it could be useful to superimpose island/seamount outlines with present day hotspot for San Felix and Juan Fernandez*

Response. The contours of the islands and the main seamounts were included in Figure 3a, representing the current positions of the hotspots of São Felix and Juan Fernandez.

- *Instead of a vertical solid black line on b), label Canquel at the surface (0 km) with the same yellow diamond symbols used in a)*

Response. This suggestion was done.

- *Show convergent boundary, lithosphere-asthenosphere boundary, trench, or other important features of the subduction zone in a) or b).*

Response. Based on the model proposed by Priestley et al. (2019), we included the depth of the lithosphere-asthenosphere boundary (LAB) along the profile (dashed black line) in Figure 3b. Based on the model proposed by Rivadeneyra-Vera et al. (2019), we also included the depth of the crust-mantle boundary (Moho) along the profile (white dashed line). Finally, based on the model proposed by Portner et al. (2020), we included the outline of the top of the Nazca Plate (green dashed line). It was not possible to obtain this contour along the profile because the slab model does not reach the latitude of Cerro Chenque (-43.8), and therefore the information from the closest latitude (-40) was used. We understand that the contour used is representative for the top of the slab for the region of Cerro Chenque.

Portner, D.E. (2020). https://doi.org/10.1029/2019JB017884

Priestley, K. (2018). https://doi.org/10.1002/9781119249740.ch6

Rivadeneyra-Vera, C. (2019). https://doi.org/10.1029/2018JB016811

PM12-15 Ol+Opx 2000x 1 - 1.07g, is there a reason as to why olivine and opx were measured together? How can you discern if the trapped Helium originates from the same stage?

Response. This sample (PM12-15 Ol+Opx) is composed by olivine and orthopyroxene because the amount and size of clinopyroxene are too small. For this reason, we decided to verify the consistency of the noble gas results from this sample by comparing its composition with whole-rock samples. Considering the analytical uncertainties, the $^3He/^4He$ ratios of samples PM12-15 Ol+Opx (6.98 ± 0.19), PM12-26 WR (6.88 ± 0.69), and PM12-48 WR (7.21 ± 0.18) are identical. Thus, we conclude that there is no significant compositional variation between the mineral phases (olivine, orthopyroxene, and clinopyroxene) from this locality (Cerro de los Chenques).

Reviewer #2 (Remarks to the Author):

My comments, corrections, and suggestions are indicated in the attached PDF file.

Good luck!

Response to Reviewer #2

We would like to thank the suggestions provided by Reviewer #2. They surely contributed to the substantial improvement of our work, especially regarding the tectonic evolution of Patagonia. All modifications made in response to the comments given by Reviewer #2 in the pdf file were applied.

Line 30. undergassed?

Response. Here the correct word is really "undegassed" because we are referring to a mantle source that selectively retained the volatiles (in this case, noble gases) in the deep mantle. This work is widely used in works focusing on noble gas isotopes to mentioning less depleted (and less degassed) lower mantle.

Line 33. hypothetical

Response. As suggested, we inserted this word before "trans-lithospheric suture zone".

Line 41. the mantle transition zone is not a superdeep mantle region

Response. Here we are assuming as a superdeep mantle that deeper than 300 km. It is a consensus between several publications involving superdeep diamonds (e.g., Anzolini et al., 2016, 2018, 2019, 2020; Brenker et al., 2021; Smith and Nestola, 2022; Nestola et al., 2023). Therefore, we understand that the mantle transition zone (410-660 km) represents a superdeep mantle.

Anzolini et al. (2016). http://dx.doi.org/10.1016/j.lithos.2016.09.025

Anzolini et al. (2018). http://dx.doi.org/10.2138/am-2018-6184

Anzolini et al. (2019). https:// doi .org /10 .1130/G45605.1

Anzolini et al. (2020). www.pnas.org/cgi/doi/10.1073/pnas.2004269117

Brenker et al. (2021). https://doi.org/10.2138/am-2020-7513

Smith and Nestola (2022). https://doi.org/10.1002/9781119528609.ch7

Nestola et al. (2023). https://doi.org/10.1038/s41586-022-05392-8

Line 79. undergassed

Response. Please, consider the answer above.

Line 91. hypothetical suture

Response. The proposed changed was applied.

Line 91. would have possibly been

Response. The proposed changed was applied as "would have been".

Line 133. Zaffarana et al. (2017) have established that the Gastre Fault is not a transcontinental fault. The authors should read in detail this paper.

Response. In fact, the Reviewer #2 is correct. We carefully read the work of Zaffarana et al. (2017), as well as the publications of Zaffarana et al. (2012) and von Gosen and Loske (2004). We are sorry for this misunderstood related to the Gastre Fault System. Due to this information, we removed the Gastre Fault from Figure 1, as well as adjusted the main text of our manuscript (lines 174, 176, 291, 317, 458). As a consequence, the reference number 25 (Zaffarana et al., 2017) was deleted.

Gosen and Loske (2004). https://doi:10.1016/j.jsames.2004.08.007

Zaffarana et al. (2012). https://revista.geologica.org.ar/raga/article/view/560

Line 143-146. According to multiple previous studies, the slab window related to the subduction of the Chile ridge is currently located (opened) in the Santa Cruz province (southern Patagonia). The low-velocity anomaly beneath the central and northern Patagonia was interpreted as a ~N-S slab tearing since there is a slab gap identified in seismic tomographies exactly beneath this anomaly (see Navarrete et al., 2020). This slab tearing would be connected with the slab window of the Chile ridge. Authors should mention this previous interpretation.

Response. We appreciate very much for this information. As suggested, we verified the paper published by Navarrete et al. (2020) and improved our discussion (lines 180-183).

Line 157-160. As was interpreted by Navarrete et al. (2020).

Response. As suggested, we added this information (line 218).

Line 201-202. Authors should carefully read the work by Zaffarana et al. (2017), since they are assuming that the Gastre fault is linked to the hypothetical suture.

Response. Based on the previous comment given by Reviewer #2, as well as in the contribution of Zaffarana et al. (2017) and references therein, we have decided to remove any discussion about the Gastre Fault System from the manuscript.

Line 217-218. A Permian (and Triassic) slab break-off was proposed by Pankhurst et al. (2006) and Gianni and Navarrete (2022).

Response. Yes, it is true that the authors mentioned above also proposed a slab break-off for the studied region. For this reason, we included these references in the text (line 315).

Line 223-227. It is worthwhile for the authors to know that there is no consensus on the origin of the Golfo San Jorge basin. There are proposals that indicate that during the Cretaceous, this basin was a foreland basin (see Gianni et al., 2015. Synorogenic foreland rifts and...), which would explain, among other things, why there was no magmatism in this basin during the Cretaceous.

It is true that during the Jurassic there were multiple extensional-related depocenters that allow indicating a lithospheric thinning.

Response. We would like to thank the opportunity to have additional knowledge related to the understanding of the Golfo San Jorge Basin through the contribution of Gianni et al. (2015). We didn’t know this publication. Based on it, we included additional information to our discussion (lines 325-327).

Line 294. undergassed?

Response. Please, consider the answer above.

Reviewer #4 (Remarks to the Author):

Review by Jens Hopp of manuscript "Shallow asthenosphere with pristine helium from the superdeep mantle beneath Patagonia" by Jalowitzki et al., submitted to Nature Communications.

The thematic issue of this manuscript is identifying a superdeep (>300 km) mantle component by its elevated 3He/4He-ratio in mantle-derived rocks from Patagonia. For this purpose high 3He/4He-ratios found in mantle xenoliths from various locations of various age (but predominantly Pliocene) in Patagonia are interpreted within a geotectonic reference frame encompassing the last hundreds of millions of years. If the postulated scenario is true, this would be the first evidence for a superdeep wet plume.

Response to Reviewer #4 (Prof. Jens Hopp)

We really want to thank very much the contribution given by Prof. Hopp. Apparently, he was the specialist in noble gas isotopes involved in the reviewer process of our manuscript. Although we are confident with respect to the obtention of our data due to the method employed to the extraction of the noble gases from Patagonian mantle xenoliths, the aspects questioned by Prof. Hopp were fundamental to ensure that we really found the first primordial component beneath Patagonia. Therefore, his contribution allowed the substantial improvement of our work. For instance, we verified if the high-$^{3}He/^{4}He$ ratios of samples PM10-Q77 and PM10-Q109 (both analyzed by stepwise crushing extraction) are in fact primordial or if they have cosmogenic contribution by applying the heating extraction of their rock powders. Based on these new results, we are convinced that sample PM10-Q77 is primordial (see details below), whereas sample PM10-Q109 showed cosmogenic contribution (it is unexpected for us). Unfortunately, due to the doubt generated by this controversial result, we decided to discard the other high-$^{3}He/^{4}He$ ratios of samples "PM7-B1 Ol", "PM12-01 WR", and "PM12-17 WR". It is important to clarify that it was a preventive decision because we don't have these rock powders anymore to apply the same verification. Concerning the general and detailed comments from Prof. Hopp, they will be answered as follow:

In order to link a superdeep component to the Patagonian magmatism in time the authors propose (1) a geodynamic model for the geologic evolution of the respective crust-mantle *evolution, (2) the presence of water in the transition zone that is responsible for formation of hydrous minerals observed in the mantle xenoliths and (3) a link of the helium isotope record to the postulated upwelling of superdeep material, which in turn link the water with the transition zone. To manage this the authors have to propose a quite controversial topic, namely the presence of significant volumes of water within the transition zone. But this cannot be proven by the data (a sponge might be dry, no matter of its storage capabilities). The only what the authors can prove is that water plays a role in formation of hydrous minerals without any inference of its origin. The helium could have other sources as well. Other explanations might also work, in particular the introduction of water by dehydration of an oceanic slab. This situation – hydrous minerals and subduction – is frequent and even might lead to a coexistence of primary minerals (from a "dry" mantle region and "wet" slab fluids).*

Response. We would like to elucidate that differently of indicated by Prof. Hopp, we are attributing the formation of hydrated minerals (phlogopite and pargasite) in the SCLM beneath Patagonia to two main processes. (1) the small-scale edge-drive convection, which favored the mantle hybridization and the preferential generation of pyroxene-rich lithologies due to the thermo-mechanical erosion in the base of the lithosphere (please, see lines 382-391). (2) the opening of slab-windows and the consequent asthenosphere upwelling throughout the Phanerozoic. An example of the latter process can be observed in a new suite of mantle xenoliths from Patagonia that contains well-developed glass veins with adakite composition associated to the crystallization of phlogopite (manuscript in preparation by our research group). In this case, the partial melting associated to the dehydration of the metamorphized subducted slab (eclogite or garnet amphibolite) plus overlying sediments provided the components necessary to the generation of phlogopite in the mantle wedge. Therefore, both processes occur in the lithosphere/asthenosphere instead in the mantle transition zone (please, see Fig. 3c for additional information).

We still would like to clarify that the tectonic model proposed to explain the origin of the high-$^{3}He/^{4}He$ (up to 27.74 R_A) observed in the mantle xenolith PM10-Q77 essentially includes the contribution from the thermomechanical erosion promoted by the small-scale mantle convection in a rifting system (Fig. 3c), but it also would be related to the development of slab-windows (Figs. b and d), as well as to the mantle transition zone (Fig. 3c). In this case, the wet plume as a response to the mechanical perturbation promoted by the reaching of the Farallon oceanic slab into the mantle transition zone would be a possible candidate only to the mantle xenoliths brought to the surface from Miocene to Present (it is not our case).

Here we consider crucial clarify that we are not correlating the high-$^3He/^4He$ ratios to the influence of water, but to volatiles (helium) containing deep origin (probably from the asthenosphere, but also from the mantle transition zone). This assumption is based on the existence of primordial helium component in mantle xenoliths from Cerro Chenque (PM10), which is located in the inferred collision zone between the Deseado and North Patagonian massifs. Thus, we conclude that the primordial helium was preferentially channelized throughout this suture zone. In this context, it is expected that this component be rare (limited) compared to the influence of other slab-derived sources (i.e., radiogenic and MORB-like), or even the preservation of the original SCLM component of mantle xenoliths. Actually, it is exemplified by our data that essentially record SLCM and MORB-like helium signatures (Supplementary Table S1), with only two samples recording true primordial composition.

Regarding the presence of significant volume of water in the mantle transition zone, it is well-constrained through mineral inclusions from superdeep diamonds, as well as based on experimental studies, that there is a substantial amount of water in this Earth's layer. For example, ringwoodite and wadsleyite, both capable to incorporate up to ~3 wt.% of water in their structures, represent the high-pressure polymorphs of forsteritic olivine in the mantle transition zone and lower mantle, that is, they are the most abundant phases in this part of the Earth. (Chen et al., 2004; Chamorro Pérez et al., 2006; Pearson et al., 2014; Chang et al., 2015; Thomas et al., 2015; Bolfan-Casanova et al., 2018). Furthermore, currently our research group is investigating the existence of water in iron oxides hosted by diamonds from the superdeep mantle.

Chen et al. (2004). https://doi.org/10.1073/pnas.0405048101

Chamorro Pérez et al. (2006). https://doi.org/10.1007/s00269-006-0096-8

Pearson et al. (2014). https://doi.org/10.1038/nature13080

Chang et al. (2015). https://doi.org/10.1002/2015JB012123

Thomas et al. (2015). https://doi.org/10.3389/feart.2014.00038

Bolfan-Casanova et al. (2018). https://doi.org/10.3389/feart.2018.00075

Another objection here is the expected lateral extension of this upwelling component – this is not visible in a 2D figure. From the authors geodynamic model a line of volcanic *units might be characterized by this superdeep signature (its not a "hotspot" sensu strictu) – is there any evidence for this?*

Response. We consider the existence of a volcanic line with superdeep signature unlikely in Patagonia because in our study we verified that the rising of a pristine helium component is conditioned to the suture zone (inferred collision zone) that probably is more effective in the deeper samples (or source) than in shallower samples (e.g., spinel-bearing xenoliths). In Patagonian back-arc, it is a fact that all mechanisms involved in the generation of magmatism has been attributed to the development of the subduction systems, with no conclusive evidence about the role of a hotspot sensu stricto. The progressive increase of ages, as well as chemical heterogeneity, is only observed in localities that record the change of the dip angle of the oceanic plates. It is important to observe that Navarrete et al. (2020) provided a comprehensive revision about several aspects related to the geological evolution of Patagonia, where they improved the models proposed by Honda et al. (2006) and Orihashi et al. (2006). These authors associated the eruption of the large volume of lava from Somuncura and Canquel plateaus, the most important basaltic plateaus of Patagonia, to the existence of a wet plume triggered by the mechanical perturbation promoted by the Farallon oceanic plate when it reached the mantle transition zone during the Oligocene-Miocene. Even in this context, there is no evidence of a line with a hotspot-like signature.

Moreover, there is a scarce number of studies about mantle xenoliths from Patagonia (Jalowitzki et al., 2016; Novais-Rodrigues et al., 2021), whereas still there is no publication about alkaline basalts. We are sure that with the advance of noble gas investigations of mantle xenoliths and alkaline basalts from Patagonia, this pristine component will be better characterized and understood.

Honda et al. (2006). https://doi.org/10.1186/BF03352614

Orihashi et al. (2006). https://doi.org/10.1016/j.gca.2006.06.932

Jalowitzki et al. (2016). http://dx.doi.org/10.1016/j.epsl.2016.06.034

Navarrete et al. (2020). https://doi.org/10.1016/j.earscirev.2020.103379

Novais-Rodrigues et al. (2021). https://doi.org/10.1016/j.gr.2021.06.003

My main objection of this topic is not the water itself but the presented He data, which are the basement of their conclusions – no high 3He/4He, no superdeep component. The authors made a lot of analyses, also of heated complementary samples to their crushed specimens. A lot of work, I appreciate that. Their heating He data show a clear presence *of cosmogenic He in the mineral lattices – if not, the signature is typical lithospheric helium. There are some studies showing that such cosmogenic He is not relevant if a stepwise crushing method is applied for gas extraction. But this is not the entire truth. Clearly, He diffusion might redistribute He from lattice sites into fluid inclusions, in particular in old samples like the Cerro Chenque xenoliths, and in addition there is a certain probability to redistribute the helium via recoil into fluid inclusions. Commonly, small amounts of cosmogenic He are hardly recognized within an "ocean" of mantle helium. But the mantle xenoliths investigated for this study are quite low in gas concentrations. A given range of 4He of <10-9 ccm STP/g is equivalent of about E-14 ccm STP/g 3He. To account for the reported high 3He/4He-ratios requires not really much cosmogenic He. For example, the highest values observed during crushing of samples from the PM12 suite (single strokes) from Cerro de los Chenques (is this the same as Cerro Chenques?) are associated with lowest 4He concentrations, i.e. these samples are most prone to contamination with cosmogenic helium. I would not build a quite substantial model of geochemistry upon samples which are suspect of containg a minor, but in the end significant, cosmogenic helium component. Other examples of stepwise crushing show increasing 3He/4He with increasing number of strokes, e.g., both series of PM10. Sample PM 7 shows within errors a constant ratio of about 6.2 Ra, i.e. as soon as we enter the field of lithospheric signatures no systematic increase is apparent. But for PM7 4He concentrations are one order of magnitude larger, as is the case for PM4 with asthenospheric 3He/4He. From all samples in this study only one extraction is a potential candidate for a mantle helium signature, PM7-B1 Ol 2000x (single crush). In this case 4He concentrations are higher and 3He/4He is about 14. Which is peculiar given the non-existence of such a component in the stepwise crushing experiment. And sorry, one single – only possibly – correct mantle value is not enough to constrain a superdeep wet plume model. Note, that in the latter sample no clear evidence of cosmogenic neon is visible (I would not necessarily expect that anyway).*

Provided the He is truely of mantle origin (which I doubt): It could be also possible that the Eocene Cerro Chenque volcanics might be related to an enrichment in 3He within the local lithosphere during the earlier rifting process, documented in Fig 3c, but unrelated to formation of hydrous phases from subduction related fluids.

As probably is clear from above I unfortunately cannot really support publication in its current form though I like the general geodynamic model. But the data appear not robust enough to support the conclusions presented by the authors.

Response. We would like to initiate the response to this concern explaining that the heating extraction measurements (from separated minerals) were performed before the crushing in order to verify which samples are gas-rich. Based on this information, we selected the gas-rich samples to apply the stepwise crushing. Also, it is important to clarify that in this work we are integrating two moments of generation of data. In a first moment, the single measurements applying 2000 strokes (produced by Prof. Conceição), whereas the heating as well as the stepwise crushing belong to a second stage (produced by the first author). It is undoubtable that unfortunately our heating measurements show strong influence of a cosmogenic component.

However, we definitely believe that our high $^3He/^4He$ isotopic ratios obtained by stepwise crushing represent a primordial rather than a cosmogenic component. In support of our interpretation of the relevant samples, we would like to emphasize the following points:

1. It is well-defined that helium is preferentially extracted by mineral crushing under high-vacuum conditions while cosmogenic and/or radiogenic helium isotopes are released by mineral melting (heating) as they are produced and remain within the mineral's matrix (e.g., Scarsi, 2000; Sumino et al., 2006, 2011; Yokochi et al., 2005). Since radiogenic/cosmogenic helium should be homogeneously distributed in the sample, and the pristine component preserved in fluid inclusions should be released in the earlier steps during crushing, we would expect to observe the increase in $^3He/^4He$ with the progress of crushing if the high $^3He/^4He$ is derived from insignificant cosmogenic ^3He. However, it should be noted that there is no constant progressive increase of $^3He/^4He$ ratios in both stepwise crushing extractions for samples PM10-Q77 and PM10-Q109.

References

Scarsi, P. (2000). Fractional extraction of helium by crushing of olivine and clinopyroxene phenocrysts: Effects on the $^3He/^4He$ measured ratio. Geochim. Cosmochim. Acta. 64, 3751-3762. https://doi.org/10.1016/S0016-7037(00)00419-1

Sumino, H., Kaneoka, I., Matsufuji, K. and Sobolev, A. V. (2006). Deep mantle origin of kimberlite magmas revealed by neon isotopes. Geophys. Res. Lett. 33, L16318, https://doi:16310.11029/12006GL027144.

Sumino, H., Dobrzhinetskaya, L. F., Burgess, R. and Kagi, H. (2011). Deep-mantle-derived noble gases in metamorphic diamonds from the Kokchetav massif, Kazakhstan. Earth Planet. Sci. Lett. 307, 439-449. https://doi.org/10.1016/j.epsl.2011.05.018

Yokochi, R., Marty, B., Pik, R. and Burnard, P. (2005). High $^{3}He/^{4}He$ ratios in peridotite xenoliths from SW Japan revisited: Evidence for cosmogenic ^{3}He released by vacuum crushing. Geochemistry Geophysics Geosystems. 6. https://doi.org/10.1029/2004GC000836

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2. Additionally, if the high $^{3}He/^{4}He$ ratios were of cosmogenic origin, its contribution should inevitably be detected in neon isotopes, resulting in high $^{21}Ne/^{22}Ne$ ratios (^{21}Ne enrichment) coupled with $^{20}Ne/^{22}Ne$ ratios lower than the air. However, excluding the samples with high blank, $^{40}Ar^{++}$ on $^{20}Ne^{+}$, and CO_2^{++} on $^{22}Ne^{+}$ contributions (>10 %), we essentially observe the air component in our samples. The only exception is the sample PM7-B1, which plot within the MORB-air mixing line. Nevertheless, it is essential to note that we have decided to disregard this specific result due to its high analytical uncertainty considering 2σ. As demonstrated by Sumino et al. (2000), we also estimated the potential cosmogenic neon contribution associated with possible cosmogenic helium. The results are presented in the Supplementary Table 2, as well as the calculations are thoroughly described in the Supplementary Information. Based on the information above, we confidently conclude that the helium component of our xenoliths is primordial, rather than cosmogenic.

Reference

Sumino, H., Nakai, S., Nagao, K. and Notsu, K. (2000). High $^{3}He/^{4}He$ ratio in xenoliths from Takashima: evidence for plume type volcanism in southwestern Japan. Geophys. Res. Lett. 27, 1211-1214. https://doi.org/10.1029/1999GL008438

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3. Even considering a hypothetical scenario in which the selective release of cosmogenic helium occurs without a significant release of cosmogenic neon (Yokochi et al., 2005), we refute this possibility based on findings by Blard et al. (2008). These authors demonstrated that the crushers used in the former publication were too much effective in releasing only helium due to heat generated during the crushing extraction. To strengthen our argument that our crushers are not effective in releasing matrix-sited cosmogenic/radiogenic helium, we generated unpublished data showing that they didn't release cosmogenic helium during stepwise crushing by analyzing the same samples of Yokochi et al. (2005).

References

Blard, P.-H., Puchol, N. and Farley, K. A. (2008). Constraints on the loss of matrix-sited helium during vacuum crushing of mafic phenocrysts. Geochim. Cosmochim. Acta. 72, 3788-3803. https://doi.org/10.1016/j.gca.2008.05.044

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4. After Prof. Neely communicate the decision regarding our manuscript, we tried to attest the quality of our data. Thus, we analyzed the rock powders (after stepwise crushing) by heating extraction at the University of Tokyo to determine whether the crystal-lattice-hosted component is primordial or cosmogenic in samples PM10-Q77 and PM10-Q109. The new results confirmed that the high-$^{3}He/^{4}He$ ratios revealed by sample PM10-Q77 is truly pristine. The $^{3}He/^{4}He$ ratio obtained by heating (using the rock powder) for this sample is 21.64 ± 0.32 R_A, which is equivalent (slightly lower) to the previous results obtained by stepwise crushing (23.39 ± 0.59 R_A to 27.74 ± 0.21 R_A). Moreover, neon results do not differ from the atmospheric ratio considering the analytical uncertainties. Therefore, we conclude that there is no doubt about the primordial component of this sample, as well as about the insignificant addition of a radiogenic ^{4}He from U and Th. Based on these results, we assume that the high-$^{3}He/^{4}He$ ratio of sample PM10-B2 Ol 2000x (23.02 ± 6.84 R_A) also reflect a true pristine component. This value is identical to those obtained by stepwise crushing for sample PM10-Q77, but the analytical uncertainty is high. However, sample PM10-Q109 surprisingly shows the effect of cosmic ray irradiation as evidenced by a higher $^{3}He/^{4}He$ ratio (50.29 ± 0.71 R_A) compared to the stepwise crushing (14.32-19.25 R_A). Yet, neon data corroborate this finding because this sample has high $^{21}Ne/^{22}Ne$ (0.0494) coupled with low $^{20}Ne/^{22}Ne$ (9.55). We don't have an explanation to this unexpected result at this moment, but probably it would be related to the mechanism mentioned by Prof. Hopp, in which the "*He diffusion might redistribute He from lattice sites into fluid inclusions*". Also, it is evident that during the heating experiments of whole-rock samples (separated minerals), sample PM10-Q77 presented the lower cosmogenic effect (15 %; $^{3}He/^{4}He$ = 30.97 ± 0.93 R_A) among all analyzed samples from Cerro Chenque (where $^{3}He/^{4}He$ ratio of PM10-Q109 is 87.98 ± 1.61 R_A, which imply 70 % of cosmogenic contribution). In any case, we reformulated the manuscript based on this new information, which led us to the decision of ignore the other results with high-$^{3}He/^{4}He$ ratios (samples PM7-B1 Ol 2000x, PM12-01 WR 2000x, PM12-17 WR 2000x, and PM12-26 Ol 2000x). We prefer to abandon these suspected results to guarantee the quality of our data, although it is important to clarify that this decision is preventive because we don't have the rock powders of single crushing extraction (2000 strokes) anymore to apply the same verification.

Regarding the following argument "*the mantle xenoliths investigated for this study are quite low in gas concentrations*", we would like to consider that it is quite common to mantle xenoliths display low amount of gas (xenocrysts are gas-poor samples), especially compared to volcanic glasses but also to phenocrysts.

With respect to the number of samples (or results) to justify the publication, we consider that our discovery is impactful (relevant) to the tectonic evolution of Patagonia because there are few noble gas data available, as well as it is the first report of a pristine component to this region. Therefore, we believe that the six measurements (including stepwise crushing, single crushing, and powder heating) presented here are enough to justify our findings.

As expressed by Prof. Hopp, our preferred scenario to the origin of a primordial helium also is the rifting process exemplified by the opening of the Golfo San Jorge Basin during the Jurassic-Cretaceous (Fig. 3c), although we cannot exclude the possible contribution from the other mechanisms (i.e., opening of slab-windows and the perturbation of the mantle transition zone).

Detailed comments:

L. 150: "… a pristine helium composition …" or "…pristine helium compositions…"

Same paragraph: I don't think that the existence of low T xenoliths excludes a high T hotspot. If an ascending melt transports helium upwards this He (together with CO2 and water) can be incorporated en passant in colder host rocks.

Response. This modification was applied (line 189). Actually, we agree with this comment, but our objective in this sentence is reinforce that there are no high-T mantle xenoliths in this study.

L. 230ff: This sentence is too long and should be shortened in two pieces at least.

Response. This suggestion was accepted, and the sentence was shortened (lines 329-333).

L. 233: " allow for"

Response. We are sorry, but we disagree with this suggestion. For this reason, we maintained this sentence without alteration.

L. 237: "... geometry of the subducting slab..."

Response. This suggestion was applied (line 335).

L. 243: "... peak of magmatism ..."

Response. This suggestion was applied (line 350).

L. 289: It is difficult to judge from the outside, but websterite could be a former magmatic vein or, alternatively, have a cumulate origin. To conclude, its presence supports a hybridization model, is at least ambivalent.

Response. Yes, in fact veins or cumulates are plausive explanations to the occurrence of pyroxenites, such as to our websterite (PM10-Q77). According with our interpretation, this lithology is a product of mantle hybridization related to the mixing between SiO_2-rich melts (i.e., adakite melts), depleted mantle, and sediments. This reaction (mixing) favors the selective consumption of clinopyroxene to form orthopyroxene-rich lithologies, which can be represented by a layer (or vein) in the SCLM. Of course, this process is valid to pyroxenes in general, which will result in pyroxenitic lithologies. Based on our tectonic model (Fig. 3c), this hybridization probably is coeval with the Gondwana fragmentation (Jurassic to Cretaceous).

Figure 1: Write " Antarctic Plate", "Inferred Collision Zone"

Response. These adjusts were done. Thank you for indicating these errors!

Figure 3: Might it be possible to mark the positions of the xenoliths in time by a number or "x" (or something similar) in c), d) and e) ?

Response. It would be extremely useful, but the indication of where the xenoliths from Cerro Cenque (PM10) were in time would be totally speculative. For this reason, we prefer does not indicate its position.

Supplementary information:

Corrections: Correction for atmospheric He is commonly done with the air He/Ne ratio, like here. This is of course a possible way to do it, but since the trapped atmospheric component not necessarily is equivalent to the air element ratio this is a bit random. It gives an order of magnitude of a possible interaction with air, but I am not sure if this is a well-defined correction. Nevermind, the extent of correction is negligible in these samples.

Response. In our opinion, some details always will confer some uncertainty to noble gas data. However, as mentioned by Prof. Hopp, we assume that the atmospheric contribution to helium ratios is negligible. Moreover, we would like to make it clear that our objective in this work is guarantee the quality of our results. For this reason, we verified the contribution of different components to our samples.

More complicated is the correction for cosmogenic 3He. The authors use the crush results as representative of a true mantle 3He/4He and then calculate the cosmogenic 3He for the heated samples. But this requires that the crushed helium data are not corrupted by cosmogenic helium in itself… Note that during crushing cosmogenic He is likely overrepresented in the fluid inclusions relative to Ne (see above), so I have doubt that a correction will correctly ever reproduce the real amount of cosmogenic helium in crushed samples. This is in this study crucial.

Response. As mentioned above, we understand that due to limitations imposed by the samples or even by the results, sometimes it is complicated to have the perfect condition to define the data. However, here we used the available tools to ensure that they are reliable. In our study, the effect of cosmogenic contribution was verified in stepwise crushing results because it is widely assumed that they undoubtably release the noble gases from fluid inclusions with minor contributions from the mineral matrix. In any case, we have decided to exclude from the discussion all high-$^{3}He/^{4}He$ results that were not well-defined as pristine. Therefore, based on this significant reformulation, we will consider the estimation of cosmogenic contribution especially for those samples from Cerro Chenque that truly are pristine (PM10-Q77 WR and PM10-B2 Ol). Yet, it is assumed that the heating extraction imply the release of noble gases from both fluid inclusions and the mineral matrix. Although the helium data of sample PM10-Q77 WR shows the effect of cosmogenic contribution when analyzed by heating ($^{3}He/^{4}He$ = 30.97 ± 0.93 R_A), subtracting its effect in our sample (15 %) and considering the 1σ analytical uncertainties, we achieve a value of 25.50 R_A, which overlaps the result of the total crushing (26.28 ± 0.19 R_A). Regarding the heating experiments, note that in this case we are discussing the disaggregated (separated) minerals, not to rock powder after stepwise crushing.

In the supplementary information, paragraph L. 146ff, the authors calculate cosmogenic 3He concentrations. I can't follow this. Their given 3He(cos) is about 2 x 10E-10 ccm STP in PM10-B2ol, but total measured 3He is about 6 x E-13 ccm STP/g. The weight is about 280 mg, so a factor of ca. 4 is still needed to make the numbers comparable (sample 3He is ca. 1.5 x E-13 ccm STP), but the discrepancy is three orders of magnitude… In turn, to get a high 3He/4He-value (from an asthenospheric base value of 7 Ra) requires only addition of an absolute 3He(cos) concentration of ca. 8 x 10E-14. Which is not really much. Looking at Ne is not really helpful, because the 3He/21Ne-ratio in inclusions might differ from production ratio. And their given value of 0.001 seems to support this (however, I am not sure if this value has any relevance if the 3He(cos) is already not correct). Note, that in their given example Ne blank correction is reported to be 198%., which doesn't facilitate a comparison. Is it possible that the authors forgot to include an exponent in their calculations (in Suppl Table 2 at least 3He is given in E-6 in the value fields, but with E-8 for overall columns, which gives E-14…). Looking at their 3He/21Ne(cos) ratios in heated samples with a denoted 70-100% cosmogenic component provides values of 8-12, whereas in the crushing samples the ratio is quite small. I guess, most Ne is in fact non-cosmogenic… which would fit my expectations and explains the ca. air composition in most samples.

Response. We completely understand the arguments of Prof. Hopp, but we would like to clarify some mistakes from our part that leading to an erroneous interpretation. For instance, the expressed results in Supplementary Table 2, as well as in the Supplementary Information are incorrect (confused) because we indicated exponent values, but the results were not adjusted. We corrected this mistake in the updated version of both files (text and table), and we would like to apologize for it. Using the example above, the correct value of [$^3He_{sample}$] for PM10-B2 Ol is 5.95×10^{-14}, whereas the [$^3He_{cosmogenic}$] is 1.90×10^{-18}. Therefore, the trapped ^{3}He concentration in this sample is four orders of magnitude higher than in the cosmogenic. In this case, the calculated cosmogenic contribution is 0.0032 %. The [$^3He_{sample}$] for each crushing measurement of sample PM10-Q77 WR varies from 4.79×10^{-14} to 3.29×10^{-14} while the [$^3He_{cosmogenic}$] varies from 6.12×10^{-16} to 1.42×10^{-15}. Consequently, the estimated cosmogenic contribution is from 2.04 to 4.43 %, where the first extraction (100 strokes) does not demonstrate the effect of cosmic ray irradiation. For calculations, we are assuming the single crushing $^3He/^4He$ value (23.02 R_A) to sample PM10-B2 Ol, and the total crushing $^3He/^4He$ (26.42 R_A) value to sample PM10-Q77 WR as representative of mantle. In our opinion, this assumption is reliable due to the several arguments presented above.

Concerning the application of cosmogenic corrections to neon isotopes, we agree that there is no reason to use this approach, especially because these results are strongly affected by the blank or mass interference ($^{40}Ar^{++}$ on $^{20}Ne^{+}$ and CO_2^{++} on $^{22}Ne^{+}$). The few samples that contain analytical quality enough to be considered usually do not differ from the atmospheric composition.

Considering the suggestions given by the three reviewers, it was necessary to modify some parts of the main text, as well as the Supplementary Information. These adjusts are indicated as follow:

Main text

Line 33. We replaced "Cenozoic" by "Eocene" due to the comments given by Reviewer #4. After them, we can ensure the existence of a pristine component with high helium only for samples from Cerro Chenque (PM10).

Lines 37-41. Based on the suggestion given by Reviewer #4, we decided to emphasize the influence of the thermomechanical erosion promoted by the small-scale mantle convection to the pristine ^{3}He observed in mantle xenoliths from Cerro Chenque (PM10).

Line 90. Based on the exclusion of the samples with some suspicious pristine $^{3}He/^{4}He$ ratios, we modified the information about the variation of these ratios.

Line 110. The information related to the high $^{3}He/^{4}He$ ratio of Estancia Alvarez (PM7) was deleted based on the considerations made by Reviewer #4.

Lines 120-139. The text regarding the extraction methods employed in this work was adjusted based on the considerations made by Reviewer #4.

Lines 142-150. We added the information needed to corroborate the existence of a pristine component recorded in mantle xenoliths PM10-Q77 WR and PM10-B2 Ol 2000x.

Lines 153-156. The order of these sentences was inverted to make the text clearer.

Line 172. It was necessary to correct the interval of values of high-$^{3}He/^{4}He$ ratios.

Line 174. The information about mantle xenoliths with high-$^{3}He/^{4}He$ ratios from Cerro de los Chenques (PM12) and Estancia Alvarez (PM7) were removed due to the considerations made by Reviewer #4.

Lines 184-186. As suggested by Reviewer #1, we inserted the lithosphere-asthenosphere boundary (LAB) in the Figure 3. The results reported by Priestley et al. (2019) reinforce the existence of a shallow asthenosphere beneath the inferred collision zone. In this case, also beneath Cerro Chenque (PM10).

Lines 210-211. As a consequence of the modifications suggested by Reviewer #4, we kept only the temperature estimate of sample PM10-Q77. As a consequence, we removed the other estimates, as well as their references.

Lines 227-230. As mentioned by Reviewer #4, we informed that our relevant samples (with high $^3He/^4He$) have low helium concentrations and based on this characteristic, we decided to perform additional measurements to verify the possible influence of a cosmogenic component.

Line 240. We removed the information regarding the high-$^3He/^4He$ ratio of sample PM7-B1 (from Estancia Alvarez).

Line 242-243. Here we updated the interval of values from samples with high-$^3He/^4He$ ratios.

Line 270. We decided to delete the word “some”.

Lines 297-298. The information about the thickness of the lithosphere was added to the text, as suggested by Reviewer #1.

Lines 343-344. We decided to invert the order of the tectonic events based on the opinion of Reviewer #4.

Line 374. The information regarding samples PM10-Q109 and PM12-17 were deleted due to the comments given by Reviewer #4.

Caption of Figure 1. The interval of values of high-$^3He/^4He$ ratios was updated.

Caption of Figure 2. We adjusted the text to be clearer.

Caption of Figure 3. We realized that the indication of (a), (b), (c), (d) and (e) was incorrect. Therefore, our mistake was corrected.

Caption of Supplementary Fig. 1. We inserted the information about the plot of samples analyzed by single and stepwise crushing experiments containing doubtable high-$^3He/^4He$.

Caption of Supplementary Fig. 2. As suggested by Reviewer #2, we inserted the information about the low-velocity anomaly, which represent the connection between a slab tearing widespread along Central-North Patagonia and the slab-window generated through the collision and subduction of the Chile Ridge. Moreover, we updated this figure, as well as this caption according with the suggestions given by Reviewer #1.

Supplementary Information

Line 45. We removed the indication about the number of samples studied here due to the ambiguity between measurements and samples.

Lines 54-59. It was necessary to adjust this part due to the suggestions given by Reviewer #4.

Line 76. We corrected this value after the careful verification of data.

Line 100. We corrected the values after the careful verification of data.

Line 103. We corrected this value after the careful verification of data.

Line 133-212. The topic “Cosmogenic component” was completely reformulated based on the suggestions given by Reviewer #4. Furthermore, the citation about the publication of Staudacher et al. (1993) was deleted, whereas Jalowitzki et al. (2016) was added.

In summary, based on the compelling evidence presented above, we cordially request that our manuscript be considered for publication in the Nature Communications. We definitely believe that the results obtained from samples PM10-Q77 WR and PM10-B2 Ol genuinely reflect a pristine component and that our interpretation is well-supported by them.

Please let us know if you require any further clarification or additional information. We believe that addressing these crucial points will strengthen the significance of our findings.

Thank you for considering our work for publication.

Sincerely,
Tiago

REVIEWER COMMENTS

Reviewer #4 (Remarks to the Author):

Re-review by Jens Hopp of manuscript "Shallow asthenosphere with pristine helium from the superdeep mantle beneath Patagonia" by Jalowitzki et al., submitted to Nature Communications.

As this is a re-review I will keep my comments rather short. The authors have made substantial changes in detail, which covers most of my concerns listed in my first review. Two critical issues remain, one is the interpretation of the helium data, the second is related to the source of the pristine helium component within a tectonic frame. See details in the comments further down.

1) New data added by the authors and a calculation by myself shows that in case of sample PM-Q77 a pristine helium signature might have be retained, though I still have some skepticism on this topic. There is surely some cosmogenic helium admixed, the question is how much. In case of doubt (I cannot prove that either) I would leave this issue to the reader and accept this interpretation.

2) Establishing of such a local pristine component of presumably deep mantle origin then leads to the question within which tectonic frame this helium was transported to the surface. The age precludes their straightforward interpretation of a wet plume transporting pristine helium through a slab window, because the age is younger then the inferred Eocene age. On the other hand, an alternative wet plume further back in time (Mesozoic) is not really convincing to me as it is a different tectonic situation, with a large-scale introduction of volatiles into the asthenosheric mantle. And some Nd-Sr isotopic data are also not in favor of a deep component present in Eocene magmatism. However, I am no expert in paleotectonics. It seems rather difficult to explain the findings within wet plume scenarios, maybe other alternatives (simple hotspot source?) could fit as well? Because of the complex situation I would suggest the wet plume scenario as a suggestion, which might be of broader interest and could induce further discussion. Therefore, I would recommend publication. But the authors should make some minor changes in describing their wet plume hypothesis with more emphasis on how in Eocene times this could have been possible, and discuss the pitfalls of their hypothesis.

Detailed comments:

I still have some reservations in interpretation of the high 3He/4He as pristine mantle component. But I also have to admit, that neither prove or disprove is successful here, as long as neon isotopes of sufficient quality are missing to clarify this – and this is something outside the influence of the authors, they did their best. I made a rough calculation of cosmogenic 3He/21Ne-ratios based on their existing neon isotope data. The heated samples listed at the end of their suppl. table 2 all have significant contributions of cosmogenic gases, with 3He/21Ne(cos) in a range between 2-17 (mostly below 10). This is a rather expected range, but suffers from precision of 21Ne(cos). Taking the most clearly extractions showing elevated 21Ne/22Ne and lowered 20Ne/22Ne gives a 3He/21Ne(cos) of about 8. Now, in turn, with this value I tried to calculate a potential corrected 3He/4He ratio of the most important sample PM10-Q77 and find lowered 3He/4He-ratios (14-22 RA), but still elevated in respect to MORB. As such, a pristine component in this specific xenolith cannot be ruled out definitely. The authors also added some more information by analyzing the powder residue by heating of two samples including Q77, with the latter showing no difference to the crushing extractions before. This may in fact indicate a trapped component (or a ubiquitous cosmogenic component…). Nonetheless, the authors should state explicitly in their manuscript that in this particular xenolith cosmogenic helium could still account for some, but not the whole 3He budget, making a pristine component still more likely, to make clear that they have cared for that.

In this context I would like to point out some errors again in their Suppl. Table 2. Their columns AO and AP are identical though telling the reader it should be 22Ne/21Ne and 21Ne/20Ne. In addition, their calculation of cosmogenic 21Ne in column AR is wrong. They calculated the excess 21Ne as 20Ne*(21Ne/22Ne – air 21Ne/20Ne). For simplicity, you simply could calculate 21Ne – 20Ne*air 21Ne/20Ne. Please check all your columns again for some more possibly undetected errors (I haven`t found more, but who knows). I also find their method to calculate cosmogenic 3He in part questionable, as they partly define the trapped component by their crushing result. This would be true if it is proven that during crushing NEVER cosmogenic 3He can be released – but this is not necessarily the case. Hence, a potential crushing extraction with notable cosmogenic 3He would never be characterized as such. Commonly, I would try to assess a cosmogenic contribution as worst case scenario, e.g., taking the local SCLM-values as they did for other samples.

L. 398-400: I am lost here … You argue here that upwelling of a wet plume with FOZO signature occurred during the Oligocene-Miocene. On the other hand, the FOZO type signature is observed in older Eocene xenoliths and actually you were mentioning this explicitly before, in L. 265-269, but not in the Fig. 4c. This is a crucial gap in your hypothesis. If a wet plume is responsible for high 3He/4He (supply of deep component) and this happened earlier, you need to adjust your model in Fig. 4. Otherwise, the wet plume is not responsible for high 3He/4He here. Then we have to face the question, which other possibilities are plausible? After your statement in L. 269 you added “Thus, the origin of the pristine helium must be older, probably during Paleozoic and Mesozoic. In this context, …” and now follows a short introduction to the capability of Wadsleyite / Ringwoodite for carrying water. But this is not related to the fundamental question, how this pristine helium arrived at Eocene age on the surface? In your Fig. 4c, a broad upward cycling of volatiles during edge driven convection could be the major source of helium but this refers to a Mesozoic situation. Do you believe, He is stored somewhere in the asthenosphere for about 100 Ma years, mixed with MORB-type helium and then very locally is remobilized in a new tectonic situation? Seems rather unrealistic, and in particular this FOZO component would have distributed in the asthensphere quite on a large scale, if looking at your Fig 4c. After Navarrete et al. 2020 the Paleocene-Eocene magmatic rocks have Nd-Sr isotope ratios more similar to depleted mantle (asthenospheric) which would fit an asthenospheric upwelling in a slab window during break-up of the Aluk slab. I am not an expert in paleotectonics, but I think an interested reader might find this interpretation contradictory.

Other points:

L.158-159: "… crust-mantle boundary has ca. 40 km, revealing extremely thin lithospheric mantle with ca. 10 km thick …" I don't understand this. Is 40km the depth of MOHO or a thickness? Similarly, you likely mean 10 km thickness?

L. 212: "primordial components" Be aware that your 3He in SCLM and MORB is also primordial… Since you have defined FOZO as having >10Ra you may choose this term instead of primordial here.

L. 232: You may delete "Similarly" here.

L. 252: "…also was allowing the rise of …"

L. 264: Instead "plausive" write "plausible".

L. 279: " …in an eroded …"

L. 317-322: This sentence is too long and complicated. Make more sentences out of it.

Revision notes

Once again, we want to thank you for the detailed review provided to our research. It is evident that you took a significant amount of time dedicated to the review and we sincerely appreciate the effort and enthusiasm. We have addressed all the comments provided below. We have accepted your suggestions regarding the tectonic model and the manuscript was reformulated accordingly. Furthermore, we reinforced our trust in the helium isotopic data of the sample PM10-Q77. As a consequence of these suggestions, the text and figures were extensively reformulated.

Based on the modifications promoted, we hope that this improved version be suitable for publication in the Nature Communications. All the researchers involved in this work are satisfied with this latest version.

(*Text highlighted in italic and blue color = Reviewer's comments*)

REVIEWER COMMENTS

Reviewer #4 (Remarks to the Author):

Re-review by Jens Hopp of manuscript "Shallow asthenosphere with pristine helium from the superdeep mantle beneath Patagonia" by Jalowitzki et al., submitted to Nature Communications.

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1) New data added by the authors and a calculation by myself shows that in case of sample PM-Q77 a pristine helium signature might have be retained, though I still have some skepticism on this topic. There is surely some cosmogenic helium admixed, the question *is how much. In case of doubt (I cannot prove that either) I would leave this issue to the reader and accept this interpretation.*

Response. As demonstrated through the calculations shown in the Table S2, we agree with Prof. Hopp about the existence of some amount of cosmogenic helium admixed in the sample from Cerro Chenque (PM10). However, we consider that this contribution was not enough large to modify the interpretation of our results (as pointed out by Prof. Hopp). Therefore, we would like to provide consistent arguments to support the existence of a primordial component with significantly less noble gas released from the mineral lattice of sample PM10-Q77.

First of all, we would like to emphasize the consensus among the scientific community on the isotopic ratios obtained by crushing extraction as representing the trapped (mantle) helium component (e.g., Kurz, 1986; Kurz et al., 1987; Hilton et al., 1993; Scarsi 2000; Gautheron and Moreira, 2002; Gautheron et al., 2005; Graham, 2002; Hopp et al., 2004, 2007; Blard et al., 2008; Mata et al., 2010; Sumino et al., 2011; Moreira et al., 2012; Madureira et al., 2014; Jalowitzki et al., 2016; Williams et al., 2019; Day, 2022; among others).

Sample PM10-Q77 was crushed in high-vacuum condition to preferentially releasing and measuring noble gases trapped in melt and fluid inclusions. Considering 2s analytical uncertainties of each step (100, 500, 1000, 2000 strokes), the $^{3}He/^{4}He$ ratios of this sample are equivalent. The exception is the first extraction (100 strokes), which probably reflect the measurement with less effect of the ^{3}He cosmogenic. However, all results are in the same order of magnitude (Tab. S2). Therefore, our extraction method does not offer the risk of releasing cosmogenic helium from the mineral lattice.

It has been documented that crushing experiments involving prolonged time promote the releasing of cosmogenic helium from the mineral lattice (e.g., Hilton et al., 1993; Scarsi, 2000; Moreira and Madureira, 2005; Yokochi et al., 2005). Hilton et al. (1993) proposed that the $^{3}He/^{4}He$ ratios are susceptible to increase due to the release of cosmogenic component after 60 seconds of crushing extraction. However, they found similar values for experiments carried out with different durations (30 s, 60 s, 90 s, and 10 minutes, see Tab. 2 of Hilton et al., 1993). Furthermore, in a recent investigation including samples from the same region (Andean Cordillera), Lages et al. (2021) reported similar results to those reported by Hilton et al. (1993), but without contribution of cosmogenic component. In Scarsi (2000), the effect of cosmogenic contribution in crushing experiments was identified only after 40 minutes of extraction. Hence, in our opinion, the cosmogenic component cannot be completely extracted by single crushing or even by stepwise crushing with shorter duration. In our study, the time necessary to crush the samples varies from 30 seconds (100 strokes) to 10 minutes (2000 strokes). In the contribution of Moreira and Madureira (2005), the high $^{3}He/^{4}He$ ratios are accompanied by nucleogenic neon isotopic ratios, which is not our case. As discussed in the last round of review, Blard et al. (2008) demonstrated that the crushers used in the work of Yokochi et al. (2005) are selectively effective in release helium cosmogenic due to the heat generated during the crushing extraction. Thus, there is a particularity related to this specific extraction system instead to the crushing extraction method or even to the samples. As said before, if it is necessary, we can provide the unpublished results generated in the same mass spectrometer used in our research (MS-IV) for the same samples analyzed by Yokochi et al. (2005). These results reveal the lack of cosmogenic helium during stepwise crushing, attesting that our crushers are not effective in releasing matrix-sited cosmogenic helium.

After crushed, sample PM10-Q77 was recovered and then melted in a furnace (heating extraction). The reason why the $^{3}He/^{4}He$ ratios are slightly lower in the heating experiment (21.64 ± 0.32 R_A) than in the crushing experiment (23.35-27.49 R_A) is due to the effect of radiogenic ^{4}He accumulated in the mineral lattice (Fig. S1). Hence, ^{3}He in the sample is contained in the fluid/melt inclusions. Also, it is important to emphasize that the stepwise extraction method employed in this work has been extensively tested in the analytical routine of the Noble Gas laboratory of the University of Tokyo (e.g., Sumino, 2000; Jalowitzki et al., 2016; Novais-Rodrigues et al., 2021; among others).

In addition, it is expected that the cosmogenic/nucleogenic contribution should be recorded by both helium and neon isotopes (e.g., Moreira and Madureira, 2005; Aka et al., 2009), which are canonic elements with very similar coefficient partitions (e.g., Heber et al., 2007). Therefore, mantle xenoliths significantly affected by cosmogenic helium component will show $^{20}Ne/^{22}Ne$ lower and $^{21}Ne/^{22}Ne$ higher than the atmospheric composition (9.8 and 0.029, respectively). In addition, due to the lower abundance of neon compared to helium, it makes the former more susceptible than the later to the nucleogenic/cosmogenic contribution. Considering only the reliable data of sample

PM10-Q77 (1000 and 2000 strokes of the stepwise crushing and the rock powder heating), the $^{20}Ne/^{22}Ne$ and $^{21}Ne/^{22}Ne$ isotopic rations are undistinguishable from the atmospheric values, indicating negligible contribution of nucleogenic ^{21}Ne (Table S2). Note that we are considering 2s analytical uncertainties and assuming as reliable those results that have less than 10 % of blank contribution (100 strokes has 14 %) and isobaric interferences of CO_2 on ^{22}Ne (100 and 500 strokes have 29 and 11 %, respectively). Sample PM10-B2 shows absurdly high blank contribution (198 %) and isobaric interferences (^{40}Ar on ^{20}Ne = 29 %, and CO_2 on ^{22}Ne = 52 %) and, therefore, cannot be considered for this assumption.

Aiming to endorse if the cosmogenic ^{3}He had acted to modify the composition of the sample PM10-Q77, the increase in $^{3}He/^{4}He$ would be more evident in samples with the lowest ^{4}He concentration, but this is not observed (Fig. 2). This confirms that the difference in $^{3}He/^{4}He$ observed in our dataset cannot be attributed to different inputs of cosmogenic ^{3}He.

Based on the information above, we tried to exclude any suspicion about the quality of the most important data for this manuscript, the stepwise crushing $^{3}He/^{4}He$ ratios for the sample PM10-Q77.

Aka et al. (2009) - https://doi.org/10.1016/j.jafrearsci.2009.04.002

Blard et al. (2008) - https://doi.org/10.1016/j.gca.2008.05.044

Correale et al. (2016) - https://doi.org/10.1016/j.gr.2016.01.003

Day (2022) - https://doi.org/10.1016/j.gca.2022.06.002

Gautheron and Moreira (2002) - https://doi.org/10.1016/S0012-821X(02)00563-0

Gautheron et al. (2005) - https://doi.org/10.1016/j.chemgeo.2004.12.009

Graham (2002) - https://doi.org/10.2138/rmg.2002.47.8

Heber et al. (2007) - https://doi.org/10.1016/j.gca.2006.11.010

Hilton et al. (1993 - https://doi.org/10.1016/0012-821X(93)90244-4

Hopp et al. (2004) - https://doi.org/10.1016/S0012-821X(03)00691-5

Hopp et al. (2007) - https://doi.org/10.1016/j.chemgeo.2007.01.004

Jalowitzki et al. (2016) - https://doi.org/10.1016/j.epsl.2016.06.034

Kurz (1986) - https://doi.org/10.1038/320435a0

Kurz et al. (1987) - https://doi.org/10.1016/0012-821X(87)90188-9

Lages et al. (2021) - doi: 10.7185/geochemlet.2134
Madureira et al. (2014) - https://doi.org/10.1016/j.chemgeo.2014.02.015
Mata et al. (2010) - https://doi.org/10.1016/j.epsl.2009.12.052
Moreira and Madureira (2005) - https://doi.org/10.1029/2005GC000939
Moreira et al. (2012) - https://doi.org/10.1016/j.chemgeo.2012.06.014
Novais-Rodrigues et al. (2021) - https://doi.org/10.1016/j.gr.2021.06.003
Scarsi (2000) - https://doi.org/10.1016/S0016-7037(00)00419-1
Sumino (2000) - PhD thesis, the University of Tokyo
Sumino et al. (2011) - https://doi.org/10.1016/j.epsl.2011.05.018
Williams et al. (2019) - https://doi.org/10.1029/2019GC008437
Yokochi et al. (2005) - https://doi.org/10.1029/2004GC000836

2) Establishing of such a local pristine component of presumably deep mantle origin then leads to the question within which tectonic frame this helium was transported to the surface. The age precludes their straightforward interpretation of a wet plume transporting pristine helium through a slab window, because the age is younger then the inferred Eocene age. On the other hand, an alternative wet plume further back in time (Mesozoic) is not really convincing to me as it is a different tectonic situation, with a large-scale introduction of volatiles into the asthenosheric mantle. And some Nd-Sr isotopic data are also not in favor of a deep component present in Eocene magmatism. However, I am no expert in paleotectonics. It seems rather difficult to explain the findings within wet plume scenarios, maybe other alternatives (simple hotspot source?) could fit as well? Because of the complex situation I would suggest the wet plume scenario as a suggestion, which might be of broader interest and could induce further discussion. Therefore, I would recommend publication. But the authors should make some minor changes in describing their wet plume hypothesis with more emphasis on how in Eocene times this could have been possible, and discuss the pitfalls of their hypothesis.

Response. Regarding the tectonic context in which the helium signature of the archetypical SCLM ($^3He/^4He$ = 6 ± 1 R_A; Gautheron and Moreira 2002; Gautheron et al., 2005; Jalowitzki et al., 2016) beneath Central-North Patagonia was replaced by the high $^3He/^4He$ ratios, we have decided to assume the influence of the Karoo mantle plume as a plausive candidate (see Navarrete et al., 2016, 2019a-b, 2021, 2024). In Patagonia, the early stages of the Gondwana break-up are coeval with the development of the Golfo de San Jorge rift basin and by the Chon-Aike rhyolitic to dacitic magmatism. The Chon-Aike

Silicic Large Igneous Province (CA-SLIP) has been widely associated to the Karoo mantle plume and the opening of the Weddell Sea prior to the opening of the South Atlantic (Pankhurst et al., 1998; Storey et al., 2013; Navarrete et al., 2016, 2019a-b, 2021, 2024). In southern South America and Antarctic Peninsula, the silicic volcanism of Chon-Aike province (~188-153 Ma; Féraud et al., 1999; Pankhurst et al., 2000; Bastias et al., 2021) has been associated to the Karoo-Ferrar large igneous province, especially due to the contemporaneous chronology recorded by V1 main stage (188-178 Ma) (e.g., Pankhurst et al., 1998; Storey et al., 2013). Furthermore, it is well-documented through Re-Os model ages that the SCLM beneath Patagonia has a common origin with Malvinas/Falkland Islands, southern South Africa, and Antarctica (Schilling et al., 2008, 2017; Mundl et al., 2015, 2016). The connection between the Karoo mantle plume (South Africa) and the Deseado Massif (Patagonia) also was proposed by Tassara et al. (2017), who correlated the epithermal Au-Ag deposits of the Deseado Massif auriferous province to the calc-alkaline stages of the CA-SLIP. Recently, the influence of hotspots from the South Atlantic Ocean (Shona, Bouvet, and Discovery) has been invoked to explain the compositional heterogeneity evidenced by lavas from Patagonian back-arc (Søager et al., 2021) and Chile Ridge (Mallick et al., 2023).

According with O'Connor et al. (2012), the hotspot trail of Bouvet, Shona, and Discovery began at ~44-41 Ma. However, the early surface expression of Bouvet-Shona, when the South America was connected to the Africa, is linked to the Jurassic Karoo-Ferrar mantle plume (including Falkland/Malvinas Island) (e.g., Fleming et al., 1995; Mitchell et al., 1999; Riley et al., 2006; Neumann et al., 2011; Hole et al., 2016; Reeves, 2020; Elliot et al., 2021). These hotspot lavas are characterized by pristine high $^3He/^4He$ isotopic ratios (12-15 R_A; Moreira et al., 1995; Kurz, 1998; Sarda et al., 2000; Day et al., 2020 and references therein). Regarding the lower values reported for the Bouvet, Shona, and Discovery hotspots compared to our results, they can be explained by the less occurrence of hotspots characterized by high $^3He/^4He$ ratios in the Atlantic large low-shear velocity province (LLSVP) (Jackson et al., 2021). According with these authors, Bouvet and Shona hotspots are connected to (supplied by) a mantle plume (in this case, Karoo mantle plume) while Discovery is defined as disconnected. Alternatively, the lower $^3He/^4He$ ratios observed in these hotspot lavas can be a consequence of the mixing between a deeper and undegassed component (OIB) with a shallower and degassed (MORB) mantle source.

Mantle xenoliths from the Ferrar large igneous province (Antarctic) show OIB-type $^3He/^4He$ isotopic ratios (Broadley et al., 2016). However, differently of our high helium results, two Antarctic xenoliths (MBM01202-Ol, and MBMO1203-Ol) contain primordial helium and neon ratios (10.60-11.40 R_A) (see Tab. 1 and Fig. 4 of Broadley et al., 2016). Although these authors have reported samples with higher $^3He/^4He$ ratios (12.50-26.80 R_A), they show different neon components (i.e., atmospheric, nucleogenic, and MORB-like).

In this context, the lithospheric thinning promoted by a sequence of events since ~300 Ma culminating with the development of the Golfo San Jorge basin (~180 Ma), which acted as a preferential conduit to the upwelling of the primordial $^3He/^4He$ within the shallow asthenosphere beneath the Central-North Patagonia. Therefore, the primordial helium (>20 R_A) found in the SCLM beneath Patagonia clearly represent vestiges of a common component related to the initial stages of the Gondwana fragmentation related to the Karoo mantle plume.

Bastias et al. (2021) - https://doi.org/10.1016/j.lithos.2021.106013
Day et al. (2022) - https://doi.org/10.1016/j.chemgeo.2021.120626
Elliot et al. (2021) - https://doi.org/10.1144/M55-2018-39
Féraud et al. (1999) - https://doi.org/10.1016/S0012-821X(99)00190-9
Fleming et al. (1995) - https://doi.org/10.1007/BF02688238
Gautheron and Moreira (2002) - https://doi.org/10.1016/S0012-821X(02)00563-0
Gautheron et al. (2005) - https://doi.org/10.1016/j.chemgeo.2004.12.009
Hole et al. (2016) - https://doi.org/10.1144/jgs2015-027
Jackson et al. (2021) - https://doi.org/10.1029/2020GC009525
Jalowitzki et al. (2016) - https://doi.org/10.1016/j.epsl.2016.06.034
Kurz (1998) - https://doi.org/10.1016/S0016-7037(97)00383-9
Mallick et al. (2023) - https://doi.org/10.1016/j.epsl.2023.118320
Mitchell et al. (1999) - https://doi.org/10.1144/gsjgs.156.5.0901
Moreira et al. (1995) - https://doi.org/10.1016/0012-821X(95)00080-V
Mundl et al. (2015) - https://doi.org/10.1130/G36344.1
Mundl et al. (2016) - https://doi.org/10.1093/petrology/egv048
Navarrete et al. (2016) - https://doi.org/10.1016/j.jog.2016.10.001

Navarrete et al. (2019a) - https://doi.org/10.1016/j.earscirev.2019.05.002
Navarrete et al. (2019b) - https://doi.org/10.1016/j.jsames.2019.102224
Navarrete et al. (2021) - https://doi.org/10.1016/j.jsames.2020.102886
Navarrete et al. (2024) - https://doi.org/10.1016/j.earscirev.2023.104651
Neumann et al. (2011) - https://doi.org/10.1093/petrology/egr011
O'Connor et al. (2012) - https://doi.org/10.1038/ngeo1583
Pankhurst et al. (1998) - https://doi.org/10.1016/S0377-0273(97)00070-X Pankhurst et al. (2000) - https://doi.org/10.1093/petrology/41.5.605
Reeves (2020) - https://doi.org/10.13140/RG.2.2.16620.72327
Riley et al. (2006) - https://doi.org/10.1093/petrology/egi085
Sarda et al. (2000) - https://doi.org/10.1029/1999JB900282
Schilling et al. (2008) - https://doi.org/10.1016/j.epsl.2008.01.005
Schilling et al. (2017) - https://doi.org/10.1016/j.precamres.2017.03.008
Søager et al. (2021) - https://doi.org/10.1016/j.gr.2020.12.008
Storey et al. (2001) - https://doi.org/10.1130/0-8137-2352-3.71
Storey et al. (2013) - https://doi.org/10.1017/S175569101300011X
Tassara et al. (2017) - https://doi.org/10.1038/s41467-017-00821-z

Detailed comments:

I still have some reservations in interpretation of the high 3He/4He as pristine mantle component. But I also have to admit, that neither prove or disprove is successful here, as long as neon isotopes of sufficient quality are missing to clarify this – and this is something outside the influence of the authors, they did their best. I made a rough calculation of cosmogenic 3He/21Ne-ratios based on their existing neon isotope data. The heated samples listed at the end of their suppl. table 2 all have significant contributions of cosmogenic gases, with 3He/21Ne(cos) in a range between 2-17 (mostly below 10). This is a rather expected range, but suffers from precision of 21Ne(cos). Taking the most clearly extractions showing elevated 21Ne/22Ne and lowered 20Ne/22Ne gives a 3He/21Ne(cos) of about 8. Now, in turn, with this value I tried to calculate a potential corrected 3He/4He ratio of the most important sample PM10-Q77 and find lowered 3He/4He-ratios (14-22 RA), but still elevated in respect to MORB. As such, a pristine component in this specific xenolith cannot be ruled out definitely. The authors also added some more information by analyzing the powder residue by heating of two samples including Q77, with the latter showing no difference to the crushing extractions before.

This may in fact indicate a trapped component (or a ubiquitous cosmogenic component...). Nonetheless, the authors should state explicitly in their manuscript that in this particular xenolith cosmogenic helium could still account for some, but not the whole 3He budget, making a pristine component still more likely, to make clear that they have cared for that.

Response. We would like to thank Prof. Hopp for recognize our effort to guarantee that the high $^3He/^4He$ data of Cerro Chenque (PM10) is truly primordial instead cosmogenic. We did our best in order to clarify any doubt concerning the quality of our results (please, see the arguments presented above). Although the ideal situation would be that where the neon results follow those for helium, the lack of nucleogenic neon isotopic ratios in the samples from PM10 indicates that the helium is mantle-derived.

Assuming the results obtained by stepwise crushing method as representative of a trapped (mantle) component, as well as the first extraction (100 strokes) as the less affected by the cosmogenic component, we have decided to use this $^3He/^4He$ ratio (23.35 R_A) for the estimation of the cosmogenic contribution. In this estimation, it is less than <15 %, implying in $^3He/^4He$ ratios of ~23 R_A (Table S2). Alternatively, considering the best possible scenarios, in which the trapped helium component is represented by the result of total crushing of the sample PM10-Q77 (26.28 R_A), the cosmogenic contribution is less than 5 %. However, as suggested by Prof. Hopp, to verify worse alternatives, we employed the higher $^3He/^4He$ ratio measured for the Shona/Bouvet hotspot lavas as trapped component (15 R_A; Kurz, 1982; Moreira et al., 1995; Moreira and Sarda, 2000; Day et al., 2022 and references therein). Thus, we achieved up to 44 % of cosmogenic contribution to the sample PM10-Q77 (Table S2), which imply $^3He/^4He$ ratios of ~15 R_A, similar to the pristine component observed in the Bouvet hotspot (Kurz, 1982) (Table S2). As expected, assuming the local SCLM as mantle component (PAVF = 6.91 R_A; Jalowitzki et al., 2016), we will have up to 70 % of cosmogenic influence. However, it will be a tendence for any sample containing high $^3He/^4He$ ratio. In this case, it is important to consider that the estimations achieved for the cosmogenic contribution represent the excess of ^3He, which should be cosmogenic or pristine. Therefore, we are attributing overestimated percentages to the cosmogenic component.

Considering all excess of ^{21}Ne as having cosmogenic origin and assuming the $^3He/^{21}Ne$ ratio of 3.5 (Staudacher and Allègre, 1993), the maximum cosmogenic ^3He concentration is 5.1´10^{-15} cm^3 STP/g, which represents less than 13 % of the ^3He measured in the higher $^3He/^4He$ ratios of the sample PM10-Q77. It results in the $^3He/^4He$ ratios varying from 21.28 to 24.94 R_A. Applying the same approach for the rock powder of this same sample, the cosmogenic contribution is 3 % ($^3He/^4He$ = 20.92 R_A). For the sample PM10-B2, due to the high analytical uncertainty in the $^{21}Ne/^{22}Ne$ ratios (0.0359 ± 0.0094), we assumed the atmospheric ratio (0.029) to estimate the cosmogenic ^3He contribution. Thus, we achieved a corrected $^3He/^4He$ ratio of 22.98 R_A. In his case, we would like to reinforce the comment provided by Prof. Hopp about "*the neon isotopes of sufficient quality are missing*". Unfortunately, we have no access to a better constrain about how much cosmogenic ^3He was added to our high $^3He/^4He$ ratios due to the limitation imposed by the neon results.

With respect to make it clear to the reader that the high $^3He/^4He$ ratios obtained for Cerro Chenque mantle xenoliths contain some influence of the cosmogenic component, as well as aiming to be responsible in the discussion of these results, we added the information described above in the manuscript, as well as in the supplementary information. We understand the responsibility involved in the publication of these results and we did our best to elucidate everything that could generate distrust. Again, we would like to thank very much Prof. Hopp for his detailed and committed contribution!

Day et al. (2022) - https://doi.org/10.1016/j.chemgeo.2021.120626
Kurz (1982) - https://hdl.handle.net/1912/2211
Moreira and Sarda (2000) - https://doi.org/10.1016/S0012-821X(00)00010-8
Moreira et al. (1995) - https://doi.org/10.1016/0012-821X(95)00080-V
Staudacher and Allègre (1993) - https://doi.org/10.1029/93GL01244

In this context I would like to point out some errors again in their Suppl. Table 2. Their columns AO and AP are identical though telling the reader it should be 22Ne/21Ne and 21Ne/20Ne. In addition, their calculation of cosmogenic 21Ne in column AR is wrong. They calculated the excess 21Ne as 20Ne(21Ne/22Ne – air 21Ne/20Ne). For simplicity, you simply could calculate 21Ne – 20Ne*air 21Ne/20Ne. Please check all your columns again for some more possibly undetected errors (I haven`t found more, but who knows). I also find their method to calculate cosmogenic 3He in part questionable, as they partly define the trapped component by their crushing result. This would be true if it is proven* *that during crushing NEVER cosmogenic 3He can be released – but this is not necessarily the case. Hence, a potential crushing extraction with notable cosmogenic 3He would never be characterized as such. Commonly, I would try to assess a cosmogenic contribution as worst case scenario, e.g., taking the local SCLM-values as they did for other samples.*

Response. We are sorry for the mistake involving the columns AO and AP of Table S2. It was corrected. In addition, we calculated the cosmogenic ^{21}Ne accordingly. Thank you for letting us know about these mistakes. The Table S2 was carefully revised (columns and lines). With respect to the trapped component, please, consider the arguments provided above. In this respect, and considering the tectonic context, we used the most reliable measurement (100 strokes) for estimate the effect of the cosmogenic component. The problem of employing the local SCLM (6.91 R_A) or even the MORB (8 R_A) endmembers for samples with high ^{3}He/^{4}He ratios is that we inevitably will attribute a strong cosmogenic contribution (and probably unrealistic) for any sample. For this reason, we adopted the estimation of the cosmogenic ^{3}He concentration based on the ^{3}He/^{21}Ne ratio of 3.5 (Staudacher and Allègre, 1993).

L. 398-400. I am lost here ... You argue here that upwelling of a wet plume with FOZO signature occurred during the Oligocene-Miocene. On the other hand, the FOZO type signature is observed in older Eocene xenoliths and actually you were mentioning this explicitly before, in L. 265-269, but not in the Fig. 4c. This is a crucial gap in your hypothesis. If a wet plume is responsible for high 3He/4He (supply of deep component) and this happened earlier, you need to adjust your model in Fig. 4. Otherwise, the wet plume is not responsible for high 3He/4He here. Then we have to face the question, which other possibilities are plausible? After your statement in L. 269 you added "Thus, the origin of the pristine helium must be older, probably during Paleozoic and Mesozoic. In this context, ..." and now follows a short introduction to the capability of Wadsleyite / Ringwoodite for carrying water. But this is not related to the fundamental question, how this pristine helium arrived at Eocene age on the surface? In your Fig. 4c, a broad upward cycling of volatiles during edge driven convection could be the major source of helium but this refers to a Mesozoic situation. Do you believe, He is stored somewhere in the asthenosphere for about 100 Ma years, mixed with MORB-type helium and then very locally is remobilized in a new tectonic situation? Seems rather unrealistic, and in particular this FOZO component would have distributed in the asthensphere quite on a *large scale, if looking at your Fig 4c. After Navarrete et al. 2020 the Paleocene-Eocene magmatic rocks have Nd-Sr isotope ratios more similar to depleted mantle (asthenospheric) which would fit an asthenospheric upwelling in a slab window during break-up of the Aluk slab. I am not an expert in paleotectonics, but I think an interested reader might find this interpretation contradictory.*

Response. We would like to apologize for generating a contradiction related to the caption of the Figure 4. Based on the improvement of the tectonic model, this figure was reformulated, as well as the main text. In addition, we realized that the following caption was inserted in the Figure 4C "Superdeep mantle upwelling and volatile influx from wet plume (Focal Zone - FOZO)". It was our mistake because we have no intention of correlate the high $^3He/^4He$ ratios from Patagonian mantle xenoliths to the influence of a wet plume. Hence, we have removed this wrong information and adjusted this figure according with the updated tectonic context in which there is the influence of the Karoo mantle plume.

Concerning how the pristine helium was preserved in the SCLM for ~120 Ma (from Jurassic to Eocene), we understand that although the tendency should be of replacement of this component by superimposed events as the South America plate migrate westward, the Central-North Patagonia has favorable conditions for storing ancient noble gas signature. Comparatively, it is expected that the lavas and mantle xenoliths outcropping in southern Patagonia (46°S) have MORB-like signature (e.g., Stern et al., 1999; Gorring and Kay, 2001; Gorring et al., 1997, 2003; Jalowitzki et al., 2016) due to the collision and subduction of the Chile Ridge, which produces the opening of successive slab-windows since ~16 Ma (e.g., Cande and Leslie, 1986; Breitsprecher and Thorkelson, 2009; Jalowitzki et al., 2016, 2017). This observation can be extrapolated to the Paleocene-Eocene Farallon-Aluk Ridge (and slab-window) because the record of its influence is restricted to lavas and mantle xenoliths of southern Patagonia (south of ~46°S) (e.g., Ramos and Kay, 1992; Parada et al., 2001; Espinoza et al., 2005; Jalowitzki et al., 2017). However, the Central-North Patagonia (including Paso de Indios region), were not affected by the subducted segments and their correspondent slab-windows (see Sanhueza et al., 2023). Therefore, considering that the noble gases are chemically inert and will change through of tectonic (pysical) events, after the influence of the Karoo mantle plume in the Jurassic (~190-180 Ma), besides the development of the active margin (Andes Cordillera) in the west of the South America, there are no significant events capable of modifying the SCLM beneath the Central-North Patagonia. It is exemplified by our own results, where excluding the pristine component, they mainly show a SLCM component in the mantle xenoliths from Somuncura and Canquel plateaus (Tab. S2).

Still related to the preservation of a given helium component throughout the geological time and to the plate tectonic, we consider plausive that the high $^{3}He/^{4}He$ ratios recorded by the mantle xenoliths from Cerro Chenque had survived during the last ~120 Ma if they are not completely modified by different components since that time. An exceptional example of the maintenance of noble gas isotopes is the SCLM beneath the Pali-Aike Volcanic Field (PAVF, Patagonia) (Jalowitzki et al., 2016). Assuming the youngest Re-Os T_{RD} model age of 260 Ma (Schilling et al., 2008) as the "formation" (stabilization) of the SCLM beneath PAVF, it means that this archetypical component for He, Ne, Ar, and Xe was retained after important tectonic events. Among these events, we can consider the Gondwana break-up, the development of the subduction zone related to the Andean Cordillera formation, as well as the passage of two oceanic ridges that promoted the development of slab-windows (Farallon-Aluk and Chile Ridge). Therefore, even facing a complex geodynamic since the Paleozoic, the PAVF SCLM remained unmodified.

It is important to make it clear that we are correlating the high $^{3}He/^{4}He$ ratios of Cerro Chenque mantle xenoliths to the mantle plume conduit advection linked to the western margin of the Karoo mantle plume (Jackson et al., 2021; Navarrete et al., 2024). Consequently, this mechanism, adding to the fact that the studied samples represent shallow fragments, the incipient (limited) recording of a pristine high $^{3}He/^{4}He$ ratios is expected.

The Paleocene-Eocene magmatism in the Paso de Indios region, where Cerro Chenque (PM10) is located, is poorly constrained through Sr-Nd isotopic ratios. Among the data plotted in the Figure 5 of Navarrete et al. (2020), we can consider two samples (CH and PAT4) from Stern et al. (1990). They have $^{87}Sr/^{86}Sr$ isotopic ratios varying from ~0.70489 to 0.70512 and εNd from 0.0 ($^{143}Nd/^{144}Nd$ = 0.51264) to 0.2 ($^{143}Nd/^{144}Nd$ = 0.51265). The Sr-Nd results showed in Navarrete et al. (2020) and attributed to the publication of Mahlburg Kay et al. (2007) for the Eocene magmatism are in fact from Ramos and Kay (1992) and Kay et al. (1993, 2002, 2004). The $^{87}Sr/^{86}Sr$ isotopic ratios of these works varies from 0.703233 to 0.704163 while the εNd values are from 1.4 ($^{143}Nd/^{144}Nd$ =

0.512709) to 5.8 ($^{143}Nd/^{144}Nd$ = 0.512937). However, these results are from the southern Patagonia (south of 46°S), as well as from Somuncura and Canquel plateaus instead of the Paso de Indios region (the study area). The samples discussed by Gorring et al. (1997), Gorring and Kay (2001), and Bruni et al. (2008) belong to the southern Patagonia (south of 46°S) and, therefore, cannot be compared with our samples. Moreover, our Sr-Nd unpublished data for the lavas from Cerro Chenque (PM10) display $^{87}Sr/^{86}Sr$ ~0.7045 and εNd ~4.5. Considering the samples of interest (those located in the Paso de Indios region), the Sr-Nd ratios clearly demonstrate an enriched character ($^{87}Sr/^{86}Sr$ = 0.7045-0.7051 and εNd = 0.0-4.6) compared to the depleted mantle ($^{87}Sr/^{86}Sr$ ~0.702 and εNd ~10; Hart et al., 1992).

Finally, we have decided to leave the definition of FOcal ZOne (FOZO) in the improved version of our manuscript. Therefore, the local pristine result revealed by the mantle xenoliths from Cerro Chenque is a product of the Karoo mantle plume in the shallow asthenosphere. Also, it is important to note that samples with a deeper source (i.e., olivine phenocrysts from alkaline basalts) would be more suitable to show this influence more clearly.

Breitsprecher and Thorkelson (2009) - https://doi.org/10.1016/j.tecto.2008.02.013

Bruni et al. (2008) - https://doi.org/10.1017/S0016756808004949

Cande and Leslie (1986) - https://doi.org/10.1029/JB091iB01p00471

Day et al. (2022) - https://doi.org/10.1016/j.chemgeo.2021.120626

Espinoza et al. (2005) - https://doi.org/10.1016/j.lithos.2004.09.024

Gorring et al. (1997) - https://doi.org/10.1029/96TC03368

Gorring et al. (2003) - https://doi.org/10.1016/S0009-2541(02)00249-8

Gorring and Kay (2001) - https://doi.org/10.1093/petrology/42.6.1067

Hart et al. (1992) - DOI: 10.1126/science.256.5056.517

Jackson et al. (2021) - https://doi.org/10.1029/2020GC009525

Jalowitzki et al. (2016) - https://doi.org/10.1016/j.epsl.2016.06.034

Jalowitzki et al. (2017) - https://doi.org/10.1016/j.lithos.2017.09.008

Kay et al. (1993) - XII Congreso Geológico Argentino

Kay et al. (2002) - XV Congreso Geológico Argentino

Kay et al. (2004) - https://revista.geologica.org.ar/raga/article/view/1432

Matthews et al. (2016) - https://doi.org/10.1016/j.gloplacha.2016.10.002
Mahlburg Kay et al. (2007) - https://doi.org/10.1093/petrology/egl053
Navarrete et al. (2020) - https://doi.org/10.1016/j.earscirev.2020.103379
Navarrete et al. (2024) - https://doi.org/10.1016/j.earscirev.2023.104651
Parada et al. (2001) - http://dx.doi.org/10.4067/S0716-02082001000100002
Ramos and Kay (1992) - https://doi.org/10.1016/0040-1951(92)90430-E
Sanhueza et al. (2023) - https://doi.org/10.1029/2023GC010977
Schilling et al. (2008) - https://doi.org/10.1016/j.epsl.2008.01.005
Stern et al. (1990) - https://doi.org/10.1007/BF00321486
Stern et al. (1999) - https://doi.org/10.1016/S0419-0254(99)80013-5

Other points:

L.158-159: "... crust-mantle boundary has ca. 40 km, revealing extremely thin lithospheric mantle with ca. 10 km thick ..." I don't understand this. Is 40km the depth of MOHO or a thickness? Similarly, you likely mean 10 km thickness?

Response. In fact, the elastic thickness estimations for the Paso de Indios region are quite unusual. However, these results were well-constrained and discussed in detail by Tassara et al. (2007) and Schilling et al. (2017). In this case, the continental crust has 40 km thick while the lithospheric mantle has only 10 km thick, which result in a lithosphere with 50 km thick. Therefore, the Moho (crust-mantle boundary) is 40 km deep. As discussed in the submitted manuscript, for this reason the asthenosphere is shallow due to the effective erosion of the SCLM.

Schilling et al. (2017) - https://doi.org/10.1016/j.precamres.2017.03.008
Tassara et a. (2017) - https://doi.org/10.1016/j.epsl.2006.10.008

L. 212: "primordial components" Be aware that your 3He in SCLM and MORB is also primordial... Since you have defined FOZO as having >10Ra you may choose this term instead of primordial here.

Response. In the updated version of our manuscript, we have decided to remove the previous reference to the FOZO reservoir.

L. 232: You may delete "Similarly" here.

Response. This part was removed due to the modifications made to the text.

L. 252: "...also was allowing the rise of ..."

Response. This part was removed due to the modifications made to the text.

L. 264: Instead "plausive" write "plausible".

Response. This part was removed due to the modifications made to the text.

L. 279: " ...in an eroded ..."

Response. This part was removed due to the modifications made to the text.

L. 317-322: This sentence is too long and complicated. Make more sentences out of it. **Response.** This part was removed due to the modifications made to the text.

Based on the clarifications above, we cordially request that our manuscript be considered for publication in the Nature Communications. We understand that this lasted version was considerably improved following the recommendations provided by all reviewers, especially by Prof. Hopp.

Thank you for considering our work for publication.

Sincerely,
Tiago

REVIEWERS' COMMENTS

Reviewer #4 (Remarks to the Author):

This is now my third review of this manuscript and apparently the authors optimized their manuscript quite satisfactory according to my previous suggestions. With these changes this improved manuscript seems ready for publication, with very minor issues mentioned below.

I found no errors in the main text now, but some minor issues in caption/legend to Fig 4a-e: In Fig 4c your header shows "Late Triassic-Early Jurassic" but in the caption you write "...triggered by the opening of the Golfo San Jorge Rift Basin during the Jurassic-Cretaceeous". Please, adjust accordingly. By the way, the header gives an age range 200-170 Ma, whereas the next two Figs d and e have ages of 190 Ma and 180 Ma. This does also not fit really. Maybe only state 200 Ma in Fig 4c? Check the ages carefully.

In Line 343: Write "flat-slab" instead "flat-lab".

Line 346: write "steep" instead of "step", and "sink" instead of "sinking".

I have recognized that you have removed wadsleyite and ringwoodite from the manuscript (also the term "wet plume"), but still you have included the phases in Fig.4. Though these minerals may stay there (it is not wrong) there is of course no connection to the text anymore - maybe you could remove them (I leave that to the authors)?

In general, Fig 4 is now much better displaying the authors basic ideas as before.

REVIEWERS' COMMENTS

Reviewer #4 (Remarks to the Author):

This is now my third review of this manuscript and apparently the authors optimized their manuscript quite satisfactory according to my previous suggestions. With these changes this improved manuscript seems ready for publication, with very minor issues mentioned below.

Response. After three rounds of revision, we are very grateful to Prof. Hopp due to his dedication and commitment to improve our work. This final version certainly is a product of a great joint effort between the authors and reviewers!

I found no errors in the main text now, but some minor issues in caption/legend to Fig 4a-e. In Fig 4c your header shows "Late Triassic-Early Jurassic" but in the caption you write "...triggered by the opening of the Golfo San Jorge Rift Basin during the Jurassic-Cretaceeous". Please, adjust accordingly. By the way, the header gives an age range 200-170 Ma, whereas the next two Figs d and e have ages of 190 Ma and 180 Ma. This does also not fit really. Maybe only state 200 Ma in Fig 4c? Check the ages carefully.

Response. We have mentioned Jurassic-Cretaceous in the caption of Figure 4c because it is the interval of time defined to the opening of the Golfo San Jorge Rift Basin (Renda et al., 2019). Concerning the information inserted in the header of this figure, we assumed a shorter interval (Late Triassic-Early Jurassic) to the lithospheric hybridization promoted by the edge-driving convection mechanism (SCLM erosion). Regarding the sequence of ages, we have tried to detail it as accurately as possible. However, in order to avoid misinterpretations for the readers, we have decided to adjust it maintaining only Early Jurassic (~200 Ma) in both caption and header. Thank you for this suggestion!

Renda et al. (2019) - https://doi.org/10.1016/j.tecto.2019.228232

In Line 343. Write "flat-slab" instead "flat-lab".

Response. Thank you for pointing out this error, which has been corrected!

Line 346. write "steep" instead of "step", and "sink" instead of "sinking".

Response. These modifications were applied.

I have recognized that you have removed wadsleyite and ringwoodite from the manuscript (also the term "wet plume"), but still you have included the phases in Fig.4. Though these minerals may stay there (it is not wrong) there is of course no connection to the text anymore - maybe you could remove them (I leave that to the authors)?

Response. We agree with this consideration and, therefore, we have decided to remove "ringwoodite" and "wadsleyite" from Figure 4.

In general, Fig 4 is now much better displaying the authors basic ideas as before. **Response.** Thank you!