

Ten kilometers ascent of porphyry Cu (Au, Mo)-forming fluids in the Sanjiang region, China

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

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Review of Chang et al., on June 6th, 2024

Chang et al. report melt inclusion data from the Sanjiang region to constrain the depth at which fluids escaped from felsic magmas. Their main findings are:

- 1) Fluids were saturated at depths of 10-20 km, rather than 5-10 km.
- 2) The shallow porphyry stock is too small to provide enough fluids for the formation of large deposits.
- 3) The formation of deposits is controlled by the volume of fluid and mineralization efficiency rather than by high metal and sulfur content.

However, their major conclusion is largely based on pressure estimates with considerable uncertainties and false assumptions, making these estimates unreliable. The remaining conclusions, which I agree with, recycle well-known knowledge and are not even incremental.

As such, the manuscript does not contain sufficient new information to warrant publication.

Below, I outline some major problems in the manuscript:

For pressure estimates, differentiating whether the depth is 5-10 km (0.15-0.25 GPa) or 10-20 km (0.25-0.52 GPa) requires a barometer with a precision of 0.2 GPa, or 0.1 GPa as stated in the title (5 km vs 10 km). Unfortunately, no known barometers provide such precision. For example, the extended data figure 5 clearly shows a scatter of 0.5-1 GPa on the y-axis. It should be noted that this is the best-case scenario, and when applied to natural samples, which likely will not behave exactly the same as experimental samples, the uncertainties will be much larger. To summarize, the barometry they use does not support their argument.

I am unsure why these authors believe that 5-10 km is the site of the fluid source. A classical review paper on this topic is "Porphyry Copper Systems" by Sillitoe (2010, Economic Geology), which has been cited 3770 times in the past 14 years. In his Figure 4, the source pluton is clearly situated at a depth of 5-15 km, and the source pluton itself extends vertically for 5-15 km. The authors have cited this classical paper, so it is unclear why they ignored this information.

They further claim that the shallow porphyry stock is too small to provide enough fluids for deposit formation, and they conducted a very simplified mass balance calculation. This is textbook knowledge, and there are many mass balance studies on this topic from the early 2000s. It would be surprising if these authors are unaware of this. Again, Figure 4 from Sillitoe shows that it is the source pluton at depth, rather than the shallow ones, that provides the fluids.

Their final finding that the volume of fluids, and hence the size of the source pluton, controls deposit formation is correct. However, this is nothing new as discussed above.

Reviewer #2

(Remarks to the Author)

I found this manuscript very interesting and very well executed. In recent years, several researchers have emphasized the importance of deep crustal magma differentiation in generating magmatic hydrothermal ore fertility and some proposed fluid saturation already at lower crustal depths. However, the proposition of mid crustal magma reservoirs directly donating the ore-forming fluids to porphyry systems is still provocative. This manuscript provides direct evidence for this in the form of a detailed silicate melt inclusion / geothermobarometry study in the Sanjiang region. I found the dataset of high quality and the arguments based on it logical. As the findings are very important, novel and provocative, I think the manuscript is ideally suited for Nature Communications. I only have few points to raise that might help to further strengthen the manuscript during the revision stage:

1) The authors discuss it the methods section, but further reinforcement of the argument that the studied magmas are coeval would be beneficiary. The locations are fairly spread out, and it is unclear which mafic-intermediate magmas are coeval with which felsic ones. E.g. existing ages could be referred to more specifically and be listed in the supplementary tables. Geochemical links are convincingly presented in the cited 2023 Geology paper.

2) The authors mention that magmatic sulfides are scarce. I wonder, was any magmatic anhydrite found/specifically looked for?

3) The evidence appears convincing that the magma emplaced in the porphyry fingers has mostly crystallized at 10-20 km depth and that the silicate melt lost at that stage most of its budget of ore-forming constituents to an already exsolving fluid phase, but the mechanism of the fluid transfer still puzzles me a little bit. As the authors also pointed out, the limited volume of magma transferred into the shallow crust could have only carried a limited mass of fluid phase. Thus, if I understand correctly, the basic idea is that once the fingers are emplaced, they serve as a channelway to transport a large volume of fluid from the large magma reservoir at 10-20 km depth. However, due to their small volume, the fingers would cool rather quickly and solidify as the top 10 km of the ambient crust is not very hot. How do the fluid percolation channels remain open for extended periods? Or, is the advective heat transfer by the abundant fluid sufficient to keep the magma hot for a bit more extended time period? Even then, each finger could provide probably one pulse of fluid to the ore-forming system. Are the observations in the mineralized zones consistent with this? Is the total amount of heat transferred this way sufficient to establish a typical porphyry ore forming hydrothermal system? I am not trying to suggest that the answer to these questions is no, but I think it would be interesting to add a few sentences of discussion of these points. The transfer of fluids through an already cooled and solidified finger seems unlikely as this would lead to spatially spread-out ore precipitation under a less steep temperature gradient (leading to sub-economic mineralization). Also, the authors pointed out the lack of hydrothermal alteration in the deeper section of the porphyry stocks.

Specific comments:

Line 166: It is not the solubility, just the activity of water that is reduced by CO₂

Line 216-228: Adding a bit more detailed explanation of these calculations in the methods section at the end would be useful.

Line 299-308: If similar high-Al, high P amphibole is common in porphyry stocks worldwide, this would be interesting to mention here.

Line 394-397: This sentence needs clarification. Or it could be elaborated more in the methods or at least in the footnote of the supplementary data tables how the replacement values were chosen.

Line 478: The beam size on the EMPA should not affect signal intensities. The same number of electrons are spread out over a larger area.

Reviewer #3

(Remarks to the Author)

Overall this paper reads well and this is a good contribution of natural data to the existing literature and compliments existing experimental work and modelling on this topic. Sufficient evidence is provided in support of the authors hypothesis and main arguments. The calculations presented in the main text regarding the mass of metals and S in the fluid need to be detailed either in the manuscript or in the supplementary material – this evidence is essentially the crux of the paper and need to be reproducible to those who read it. To improve this manuscript, the author should consider restructuring some of the text and also addressing literature that discusses an almost parallel set of processes but argue a different emphasis on the importance of deep versus shallow systems.

Below I detail 3 themes that could be improved throughout the manuscript, followed by a line-by-line review of the text.

1. Transfer of melt and fluids from deeper to shallower crustal levels

One of the main findings of the manuscript is that >90% of S and Cu and high proportions of other metals are partitioned into fluids exsolving at depths of up to 0.5 GPa. I think the author needs to explain more clearly why this is so significant relative

to other models that are favoured in the literature. For example, because 90% of total system Cu has partitioned to the fluid phase at depths of 0.5 GPa, the significant increase in the mass of fluid exsolved during decompression bears minimal influence to the overall 'fertility' or 'ore-forming potential' of the fluid since most of its metal inventory was determined at deeper levels in the crust.

This challenges prevailing models, which like you are in support of a 'single-stage' model for the formation of giant porphyries, but claim that the transfer of water-rich magmas from the lower crust is an essential mechanism for exsolving high masses of metal-rich fluids that determine the formation and size of the deposit (Chelle-Michou, 2017); the importance of deep fractionation is attributed to the accumulation of H₂O and metals whilst remaining predominantly volatile undersaturated (Chiaradia and Caricchi, 2017; Richards 2015; Rezeau and Jagoutz, 2020).

I do not dispute that deep fractionation of magmas plays a highly important role however I was surprised at how high the proportion of Cu and S partitioning in to these fluids were and hence how subordinate degassing on ascent was to adding to the mass of metals conveyed by these fluids. To consolidate your conclusions I think some reference to the aforementioned models in addition to detailing the method of your calculations would be recommended.

I think to strengthen the argument, the author should state more clearly the decoupling in concentration versus mass (Hogg et al., 2023). A simple calculation comparing the mass of fluid required to generate the drop in concentration observed in the intended evolved MIs in Fig.1 at 0.5 GPa and compare this to the mass of fluid exsolved at 0.1-0.2 GPa (from your Al barometry) would clearly emphasise to the reader that although a higher mass of FLUID exsolves at shallow levels, the higher fluid-melt partition coefficient at depth, supersedes the relatively small mass of fluids at Shanjiaoguo, resulting in 90% of the metal inventory being partitioned into a smaller fluid reservoir at 0.5 GPa.

2. Importance of sulfide saturation for the fate of S and Cu

The author mentions that intermediate samples are saturated in MSS which cause depletions in S and Cu and Au relative to the incompatible models (Fig.1).

The Cu MSS-melt partition coefficient ranges reaches up to 1000-10000 (Kiseeva and Wood 2013, Li and Audetat, 2015) and is highest in evolved melts. Whereas Cu fluid-melt partition coefficients generally do not exceed ~300-400 (Zajacz et al., 2008; Tattitch and Blundy, 2017) in evolved melts. Hence, formation of sulfides prior to degassing will cause S and Cu and Au to drop in the melt, as the author states.

Although you conclude that fluids are causing most of the depletion in volatiles and metals listed in Fig.1 some acknowledgement of how 90% of Cu and S are still being delivered to fluids despite the presence of MSS is recommended. Without modelling I appreciate that the above conclusions are rather speculative and not the primary focus of the paper. However I do feel it is important to comment on the state of the magma – if it is sulfide saturated, then address that e.g. mass of fluids degassing outweighs the effect that high K_d of Cu and Au and low mass of formation of Cu-Au-rich sulfides; or that perhaps degassing may have transitioned the melt into a sulfide undersaturated state (Nicholson et al., 2024).

The author states that only a small amount of MSS is precipitated – but could the author go a step further and perhaps compare the mass of fluid versus sulfide needed to partition a given mass of Cu – keeping in line with the theme of mass being important? My suggestion would be to start with something like $DCu_{MSS-melt} / DCu_{fluid-melt}$ to give how much more [Cu] is sequestered in sulfides versus fluids. I do appreciate the limited word count of the manuscript, so please consider this as a suggestion.

3. Stocks

Space permitting, some description of relation of stocks to magmatic system. This could be a figure in the supplement and description in the caption. You refer frequently to the fluid mass capabilities of the stock versus the magma. I think a visual aid would be nice to show that stocks can be fed transiently by fluxing magmas – hence further emphasises your point about the importance of deeper magmatic processes.

Line-by-line review

Line 38 – There are two prevailing models for formation of porphyries:

- Single stage – favoured for big porphyries as discussed here, where metals are predominantly conveyed in fluids exsolved typically from CA magmas despite early forming sulfides (Richards, 2015; Rezeau and Jagoutz, 2020).
- Multistage – more typical for small deposits, involves the storage of sulfides and subsequent remobilisation by later infiltrating magmas (Chiaradia, 2014 and refs therein) and/or fluids (Heinrich and Connelly, 2022).

I recommend a comment on the two prevailing models of ore formation before this sentence – with your focus being on the single stage where metals and S are mainly carried by fluids. Then this sentence highlighting importance of fluids but uncertainty on whether high pressure fractionation is important etc, this will bring the manuscript's main exploratory questions to the forefront.

Line 48 – mention somewhere the models that highlight the importance of deep and shallow reservoirs e.g. Rezeau and Jagoutz (2020); Chiaradia and Caricchi (2017). I think these are valuable citations since you mention both fractionation in deep and the transfer to shallow levels of the crust.

Line 69 – what do you mean by entire? Do you mean to say homogenous? Glassy?

Line 77: missing 'with' after well?

Line 76 – line 80: reword slightly – there are only experimental data for Cl and S so MIs and experimental data fit well for these elements; and MI data for S and Cl and metals correspond well to models of incompatible behaviour during magma differentiation. Also were the incompatible models based on LLDs at 1 GPa too?

Line 80: reorganise sentence slightly – important exceptions are S, Cu and Au, which, in contrast to Ag, Mo, Pb, and W, are relatively depleted compared to incompatible trends. Experimental and natural data show evidence of monosulfide solid solution MSS, in which S Cu and Au are strongly compatible (Kiseeva and Wood 2013; Li and Audetat 2015).

Line 87 – replace 'amount' with 'proportion'

Line 88 – theoretical trends presented in Fig.1;

Line 115 – 118: sentence structure, I suggest something like “since Cl and metal (*state metals specifically here because need to emphasise that its not Cu and Au*) concentrations in trachydacitic MIs (67–70 wt% SiO₂) in the intermediate rocks deviate negligibly from incompatible behaviour, we conclude the onset of fluid exsolution was not yet reached”.

Line 120: delete generally substantially

Line 121: address S and Cu and Au – you have mentioned their concentrations trace magmas to be sulfide saturated in early stages (mafic to intmed compositions) but then conclude depletion is due to degassing. I do not disagree but just a line clarifying that either correlation in depletion across all metals suggests sulfide saturation is not dominant and then perhaps elaborate briefly on a suggestion listed in aforementioned review.

Line 146: I think it would be nice to show some fluidity with Fig.1 and use the same colors for samples from mafic, intmed and felsic samples. For example, H₂O-saturated apatite inclusions in the rhyolites will be red instead of yellow?

Line 150: quantify the differences in Cl and F partition coefficients.

Line 174: sentence requires citations.

Line 175 – 178: sentence could be reworded. Do you need to state hornblende-based pressures? Could not just say this comparison relies on pressures obtained from hornblende inclusions in phenocrysts to avoid resetting ...

Line 179 – 183: combine into one sentence.

Line 199 – 202: “Below we show that such deep fluid exsolution was achieved by substantial phenocryst growth in water-rich felsic magmas, and that the volumes of shallowly-emplaced felsic porphyry stocks would have been far too small to provide all the ore-forming fluids.” I would move sentence. First give the evidence.

Need to describe what stocks are and their relevance in the magmatic system and porphyries before this.

Line 205 – 207: move this sentence to earlier when you introduce the model and replace no significant with minimal or none, “We assume that no significant amounts of these elements were incorporated into the crystallizing minerals.”

Line 207 – 209: repetition of point made earlier, I don't think you need this or remove it from earlier: Except for S, Cu, and Au, the good match between theoretically predicted (for 100% incompatibility) and actually observed element concentrations at 67–70 wt% SiO₂ in the melt (Fig. 1) suggests that this assumption is reasonable.

Line 209 – 213: Author states Cu and Au and S are less reliable in models but these are essentially what the paper is about so I think more attention needs to be paid as to why sulfide saturation is not affecting the composition of fluids (i.e undersaturated) or that it is but because the mass of fluids exsolving (despite lower DCu fluid-melt) is more significant than the mass of Cu partitioning into sulfides (despite high DCu MSS-melt) that it doesn't matter.

Line 221: measured 'in' not 'on'

Line 220 – 228: How did you compute these fluid compositions? Could you add this calculation to the supplement? I think to prove to the reader that the composition of deep fluids are a good match to natural data, there needs to be a comparison to computed fluids at e.g. 0.1 – 0.2 GPa as measured by some of the amphiboles in Fig. 4.

“The mass balance calculations also suggest that the aqueous fluids extracted 97 ± 4% of the S, 92 ± 7% of the Cl, 91 ± 14% of the Cu, 88 ± 10% of the Mo, 87 ± 14% of the Ag, 84 ± 12% of the W, 72 ± 19% of the Pb, and 63 ± 26% of the Au that was present in the trachydacitic melts (67–70 wt% SiO₂).”

This sentence is really important for the validity of your research and I think it requires some evidence based in a figure

(subplot of Fig. 5) showing the mass equivalent of metals in the fluids relative to natural data. a further suggestion might be to then compare the mass of metals in fluids exsolved at this shallower level (0.1 – 0.2 GPa) to show it would be inadequate perhaps?

Fig.5 should highlight that yes compositions agree (a) but so do the masses (b) – making the reader aware of the decoupling in concentration and mass of fluids and how this is a really important thing to consider in what is required from fluids to acquire ore-forming potential (Hogg et al., 2023).

Is this mass the cumulative mass of fluid exsolved from onset of volatile saturation to records in felsic samples?

Some comment on existing models that describe similar mechanisms but de-emphasises the importance of deeper fractionation for generating high mass fluids but instead for accumulating water and metals that are subsequently degassing at shallower levels – in contrast to your conclusions (Chiaradia and Caricchi 2017; Rezeau and Jagoutz, 2020; Chelle-Michou, 2017 etc).

Line 238: used a density of 2.4 g/cm³ but in Fig.3 a density of 2.7 g/cm³ is mentioned. How come?

Line 239: what are the average concentrations that you use? Perhaps detail in a figure caption?

Line 244: need to be more specific – conclusion is that the highest MASS of fluids, of suspected ore-forming potential, are exsolved at depth of 10-20 km.

Line 273 – 275: reference to some studies that have also suggested this and demonstrated that its feasible.

Line 276 – 285: restructure paragraph. You establish from mass balance that fluids exsolved at high pressures host 90% mass of Cu and S, which do not equate to the highest mass fluids. Then explain how these fluids were transferred to shallower levels – refer to previous studies here. Then use evidence of abundant fluid bubbles (are these in your samples?) to suggest initial mechanism of ascent of fluids.

Line 292: need citations and comment on similar studies e.g. not a new concept, it is widely appreciated that the mass of S and Cu in porphyries is 'anomalous'– Chiaradia and Caricchi (2017), show that periodic fluxing of hydrous magmas over ~2Ma facilitates generation of Cu-S rich fluids.

Line 311 - 330: this paragraph needs to pull out main points more clearly. It is important to discriminate the composition of computed fluids, existing natural data and then this study, more clearly. Which FI data in Fig. 5 are mineralised and non-mineralised?

Fig 5 highlights that mineralised and non-mineralised have the same concentrations – this would be a nice opportunity explicitly say that concentration decouples from ore-forming potential – instead mass is important (in line with your paper).

Bingham and Sanjiang have the same range in concentrations and the same volume stock but Cu and Au mass higher at Bingham – leads on from first conclusion, proving that mass of fluids is really important. This would be another reason to have a subplot of mass here too.

You mention metal ratios but don't say which? Are they in fluids or melts? And they are not quantified or shown in figures? I suggest remove or elaborate.

General comment – importance of the mass of the magma reservoir at the onset of volatile saturation is not addressed– I think this is important to consider and briefly note in the introduction.

Line 331: Need a final sentence that synthesises findings and main themes of paper.

References:

- Chelle-Michou, C., Rottier, B., Caricchi, L. and Simpson, G., 2017. Tempo of magma degassing and the genesis of porphyry copper deposits. *Scientific reports*, 7(1), p.40566.
- Chiaradia, M., 2014. Copper enrichment in arc magmas controlled by overriding plate thickness. *Nature Geoscience*, 7(1), pp.43-46.
- Chiaradia, M. and Caricchi, L., 2017. Stochastic modelling of deep magmatic controls on porphyry copper deposit endowment. *Scientific reports*, 7(1), p.44523.
- Heinrich, C.A. and Connolly, J.A., 2022. Physical transport of magmatic sulfides promotes copper enrichment in hydrothermal ore fluids. *Geology*, 50(10), pp.1101-1105.
- Hogg, O.R., Edmonds, M. and Blundy, J., 2023. Water-rich magmas optimise volcanic chalcophile element outgassing fluxes. *Earth and Planetary Science Letters*, 611, p.118153.
- Kiseeva, E.S. and Wood, B.J., 2013. A simple model for chalcophile element partitioning between sulphide and silicate liquids with geochemical applications. *Earth and Planetary Science Letters*, 383, pp.68-81.
- Li, Y. and Audétat, A., 2015. Effects of temperature, silicate melt composition, and oxygen fugacity on the partitioning of V, Mn, Co, Ni, Cu, Zn, As, Mo, Ag, Sn, Sb, W, Au, Pb, and Bi between sulfide phases and silicate melt. *Geochimica et*

Cosmochimica Acta, 162, pp.25-45.

Rezeau, H. and Jagoutz, O., 2020. The importance of H₂O in arc magmas for the formation of porphyry Cu deposits. *Ore Geology Reviews*, 126, p.103744.

Tattitch, B.C. and Blundy, J.D., 2017. Cu-Mo partitioning between felsic melts and saline-aqueous fluids as a function of X_{NaCleq}, f_{O2}, and f_{S2}. *American Mineralogist*, 102(10), pp.1987-2006.

Zajacz, Z., Halter, W.E., Pettke, T. and Guillong, M., 2008. Determination of fluid/melt partition coefficients by LA-ICPMS analysis of co-existing fluid and silicate melt inclusions: Controls on element partitioning. *Geochimica et Cosmochimica Acta*, 72(8), pp.2169-2197.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Reviewer #2

(Remarks to the Author)

I am overall satisfied with the authors' responses to my comments and the changes implemented in the manuscript. I maintain my opinion that this is an important contribution, and I recommend publication without further modifications.

Reviewer #3

(Remarks to the Author)

The revised version of this manuscript incorporates several of the review suggestions and has justified plausibly areas where my suggestions were not wholly relevant. I have no further comments and believe this paper now represents a strong contribution to *Nature Communications*.

The introduction is much clearer and effectively outlines prior research, key research questions, and consequently what this paper will address. The additional supplement provides evidence supporting the calculations and has strengthened the manuscript, and the new figures are excellent visual aids that enhance the overall clarity of the author's findings.

This paper is an important contribution to the field, and I look forward to seeing it published.

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Response to reviewer comments

We sincerely acknowledge the three Reviewers (R1 – an anonymous reviewer, R2 – Zoltan Zajacz, and R3 – Olivia R. Hogg) for their comments, which were very helpful to improve the manuscript. Below we show the reviewer comments in black, and our responses in blue.

Reviewer #1 (Remarks to the Author):

Review of Chang et al., on June 6th, 2024

Chang et al. report melt inclusion data from the Sanjiang region to constrain the depth at which fluids escaped from felsic magmas. Their main findings are:

- 1) Fluids were saturated at depths of 10-20 km, rather than 5-10 km.
- 2) The shallow porphyry stock is too small to provide enough fluids for the formation of large deposits.
- 3) The formation of deposits is controlled by the volume of fluid and mineralization efficiency rather than by high metal and sulfur content.

To more clearly point out the main findings of this study, a summary section was added on lines 243–267 (line numbers referring to the revised manuscript; the same below).

However, their major conclusion is largely based on pressure estimates with considerable uncertainties and false assumptions, making these estimates unreliable.

We clearly disagree with this comment. It is unclear to us how this argument was reached. Tests of the relevant barometers (i.e., Al-in-hornblende; Ti-in-quartz; H₂O content of melt inclusions) on natural systems in this and previous studies clearly suggest that their uncertainties are not significantly higher than ± 0.1 GPa (please see below for more details). Such uncertainties should be precise enough to reach our conclusions with regard to (1) the depth of the fertile magma crystallization, (2) the depth of exsolution of the ore-forming fluids, and (3) the vertical distance of fluid transfer to the site of ore precipitation.

The remaining conclusions, which I agree with, recycle well-known knowledge and are not even incremental.

We disagree also with this comment. One of our remaining conclusions is that the size of porphyry Cu (Au, Mo) deposits was mainly controlled by the mass of ore-forming fluids, rather than by the metal concentrations in the fluids or the magmas. There are certainly different views regarding this aspect, for example that large ore deposits formed from unusually metal-rich magmas (Core et al., 2006; Stern et al., 2007; Lamont et al., 2024). Importantly, our conclusion is well supported by the new fluid- and melt inclusion data from mineralized systems in the Sanjiang region. Such datasets rarely exist in the literature.

As such, the manuscript does not contain sufficient new information to warrant publication. We demonstrate that the porphyry Cu (Au, Cu) deposits in the Sanjiang region formed by fluids that exsolved from mid- to upper crustal magma reservoirs. This topic is currently highly debated (Sillitoe and Perelló, 2024; Heinrich, 2024; and references therein), and our study provides the first quantitative data to solve this issue. The two 2024 papers were added on line 39 to further emphasize the importance of the present study.

Below, I outline some major problems in the manuscript:

For pressure estimates, differentiating whether the depth is 5-10 km (0.15-0.25 GPa) or 10-20 km (0.25-0.52 GPa) requires a barometer with a precision of 0.2 GPa, or 0.1 GPa as stated in the title (5 km vs 10 km). Unfortunately, no known barometers provide such precision. For example, the extended data figure 5 clearly shows a scatter of 0.5-1 GPa on the y-axis. It should be noted that this is the best-case scenario, and when applied to natural samples, which likely will not behave exactly the same as experimental samples, the uncertainties will be much larger. To summarize, the barometry they use does not support their argument.

It is not true that "no known barometers provide such precision" (i.e., an overall precision of 0.1 to 0.2 GPa). As we pointed out on lines 466–467, the Al-in-hornblende barometer of Mutch et al. (2016) should have an overall uncertainty of 0.06–0.1 GPa when it is applied to quartz-saturated felsic magmas. Since this Al-in-hornblende barometer was calibrated also on a large dataset from natural granitic samples with independent pressure constraints, the uncertainties of our pressure estimates should be not higher than the cited values. Also, as was written on lines 487–489, the Ti-in-quartz barometer of Huang and Audétat (2012) was rigorously tested on natural magmatic systems by comparing with independent pressure constraints, which gave an overall uncertainty of better than ± 0.1 GPa (Audétat, 2013; Kularatne and Audétat, 2014; Audétat et al., 2023). Hence, our pressure estimates based on these two well-calibrated barometers should have uncertainties smaller than ± 0.1 –0.2 GPa.

This argument is also supported by the study of our own samples. For the Jianchuan rhyolite tuff sample, the three *independent* barometers (Al-in-hbl, Ti-in-quartz, and H₂O content of glassy melt inclusions) yielded consistent pressures within an uncertainty of ± 0.1 GPa. Also for the remaining porphyry samples, consistent pressures of 0.3–0.5 GPa for the crystallization of phenocrysts were obtained based on the two *independent* barometers (Al-in-hbl and Ti-in-quartz). As was stressed on lines 135–137, 204–208 and 358–361, only hornblende that co-crystallized with other matrix minerals of the porphyry samples yielded significantly lower pressures at ~ 0.1 –0.2 GPa. This is simply because the phenocrysts crystallized in deep magma reservoirs, whereas the fine-grained matrix crystallized after the final emplacement of the porphyry magmas (see Fig. 7). The overall large pressure range of all the analyses thus should not be taken as evidence for the argument that the barometric results are of poor precision – in fact it is the careful petrography that allows to delineate the sequential crystallization evolution starting from deeper magma crystallization to shallow solidification after magma emplacement.

Therefore, our conclusions based on the pressure estimates are robust. We revised or added the text on lines 118–137 and 467–470 to make the above points clearer.

I am unsure why these authors believe that 5-10 km is the site of the fluid source. A classical review paper on this topic is "Porphyry Copper Systems" by Sillitoe (2010, Economic Geology), which has been cited 3770 times in the past 14 years. In his Figure 4, the source pluton is clearly situated at a depth of 5-15 km, and the source pluton itself extends vertically for 5-15 km. The authors have cited this classical paper, so it is unclear why they ignored this information.

The widely cited source pluton depth of 5–15 km given by Sillitoe (2010) and the two references therein (Cloos, 2001; Richards, 2005) seems to be an empirical estimate rather than based on hard quantitative data. We made a summary of the few existing hard data, which indicate paleodepths of about 4–10 km (see Supplementary Table 1, which is also provided below). These values were reconstructed based on detailed geological mapping or drill core logging of tilted or differentially uplifted porphyry systems, where the source pluton underlying porphyry-type Cu deposits can be mapped directly. It is unclear where the 15 km limit in the Sillitoe (2010) paper originates from. On the other hand, since the reconstruction requires some interpretations (e.g., how to identify the true source pluton), it leaves room for alternative models. Some recent studies argue that the ore-forming fluids could start to exsolve already at lower- to mid-crustal depths in response to the ascent of porphyry magmas from the lower crust, without the need to construct mid- to upper crustal magma reservoirs (e.g., Loucks, 2021). Even Richard Sillitoe, in their recent publication (Sillitoe and Perelló, 2024), pointed out that these two models are equally possible. Our barometric + melt inclusion study provides the first direct evidence that porphyry Cu (Au, Mo)-forming fluids exsolved from mid- to upper crustal magma reservoirs situated at depths of up to ~20 km. We revised the text on lines 35–48 to better emphasize this controversy.

Supplementary Table 1 Depth of source plutons underlying porphyry Cu deposits

Occurrence	Depth of source pluton (km)	Reference
Chuquicamata deposit (Chile)	7–?	Sillitoe (1973)
Ann-Mason deposit (Navada, USA)	5–8	Dilles (1987), Dilles and Proffett (1995)
Miami Inspiration deposit (Arizona, USA)	7–10	Seedorff et al. (2008), (2019)
Sierrita-Esperanza deposit (Arizona, USA)	5–7	Seedorff et al. (2008), (2019)
Ray deposit (Arizona, USA)	6–9	Seedorff et al. (2008), (2019)
Kelvin prospect (Arizona, USA)	6–8	Seedorff et al. (2008), (2019)
Pebble deposit (Alaska, USA)	4–7	Olson (2015)

They further claim that the shallow porphyry stock is too small to provide enough fluids for deposit formation, and they conducted a very simplified mass balance calculation. This is textbook knowledge, and there are many mass balance studies on this topic from the early 2000s. It would be surprising if these authors are unaware of this. Again, Figure 4 from Sillitoe shows that it is the source pluton at depth, rather than the shallow ones, that provides the fluids.

Sure, we know about this, but the result depends very much on the size of the porphyry stocks and the size of the deposits, so this mass balance calculation has to be done for each deposit individually.

Their final finding that the volume of fluids, and hence the size of the source pluton, controls deposit formation is correct. However, this is nothing new as discussed above.

As replied above, there are many different views about what are the key factors in controlling porphyry copper deposit formation. Here, we provide strong evidence based on fluid- and melt inclusion compositions that the size of the source pluton was the key in the Sanjiang region.

Reviewer #2 (Remarks to the Author):

I found this manuscript very interesting and very well executed. In recent years, several researchers have emphasized the importance of deep crustal magma differentiation in generating magmatic hydrothermal ore fertility and some proposed fluid saturation already at lower crustal depths. However, the proposition of mid crustal magma reservoirs directly donating the ore-forming fluids to porphyry systems is still provocative. This manuscript provides direct evidence for this in the form of a detailed silicate melt inclusion / geothermobarometry study in the Sanjiang region. I found the dataset of high quality and the arguments based on it logical. As the findings are very important, novel and provocative, I think the manuscript is ideally suited for Nature Communications. I only have few points to raise that might help to further strengthen the manuscript during the revision stage:

[Many thanks for the nice words! We addressed the comments below.](#)

1) The authors discuss it the methods section, but further reinforcement of the argument that the studied magmas are coeval would be beneficiary. The locations are fairly spread out, and it is unclear which mafic-intermediate magmas are coeval with which felsic ones. E.g. existing ages could be referred to more specifically and be listed in the supplementary tables. Geochemical links are convincingly presented in the cited 2023 Geology paper.

[Done. Existing ages for the studied magmas were added in the Supplementary Data 1.](#)

2) The authors mention that magmatic sulfides are scarce. I wonder, was any magmatic anhydrite found/specifically looked for?

[We did specifically search for magmatic anhydrite in the studied samples, but it was only found in a syn-mineralization felsic porphyry at Yulong \(also reported in Chang et al., 2018\) and a post-mineralization felsic porphyry dike at Beiya, and the abundances seem to be extremely low. We revised the text on lines 153–156 to clarify this point.](#)

3) The evidence appears convincing that the magma emplaced in the porphyry fingers has mostly crystallized at 10-20 km depth and that the silicate melt lost at that stage most of its budget of ore-forming constituents to an already exsolving fluid phase, but the mechanism of the fluid transfer still puzzles me a little bit. As the authors also pointed out, the limited volume of magma transferred into the shallow crust could have only carried a limited mass of fluid phase. Thus, if I understand correctly, the basic idea is that once the fingers are emplaced, they serve as a channelway to transport a large volume of fluid from the large magma reservoir at 10-20 km depth. However, due to their small volume, the fingers would cool rather quickly and solidify as the top 10 km of the ambient crust is not very hot. How do the fluid percolation channels remain open for extended periods? Or, is the advective heat transfer by the abundant fluid sufficient to keep the magma hot for a bit more extended time period? Even then, each finger could provide probably one pulse of fluid to the ore-forming system. Are the observations in the mineralized zones consistent with this? Is the total amount of heat transferred this way sufficient to establish a typical porphyry ore forming

hydrothermal system? I am not trying to suggest that the answer to these questions is no, but I think it would be interesting to add a few sentences of discussion of these points. The transfer of fluids through an already cooled and solidified finger seems unlikely as this would lead to spatially spread-out ore precipitation under a less steep temperature gradient (leading to sub-economic mineralization). Also, the authors pointed out the lack of hydrothermal alteration in the deeper section of the porphyry stocks.

Yes, the mechanism of the fluid transport is indeed puzzling, but the presented evidence (including the simple mass balance) does not leave much room for other interpretations. We agree that each porphyry finger providing one pulse of fluid would cool quickly, but the fluid could also percolate rather fast to the top of the porphyry finger (e.g., Cathles and Shannon, 2007). Hence, we cannot answer whether additional heat transferred by the ascending fluid would be needed to retain an interconnected fluid network, or how much would be needed. The same is true for the amount of heat required to establish ore-forming hydrothermal systems. These are of course very interesting questions that warrant future studies. We added text on lines 231–233 to suggest that continuous input of fluids could ensure the fluid network to have remained open. We also added Fig. 7b, c to visualize our preferred model about how the porphyry stock could serve as a fluid network to transport large volume of fluid from the underlying magma reservoir. This model well accounts for the observations in mineralized systems that are listed on lines 235–241. Intermittent repetition of the process explains the common presence of multiple pulses of porphyry fingers and associated mineralization in individual ore deposits (e.g., Sillitoe, 2010; Chang et al., 2017).

Specific comments:

Line 166: It is not the solubility, just the activity of water that is reduced by CO₂

Corrected. Thanks for pointing out the mistake.

Line 216-228: Adding a bit more detailed explanation of these calculations in the methods section at the end would be useful.

A table in Supplementary Data 6 was added to show how the calculations were done.

Line 299-308: If similar high-Al, high P amphibole is common in porphyry stocks worldwide, this would be interesting to mention here.

We expect that it is common, but almost all previous studies focused only on unshielded amphibole phenocrysts in mineralized systems that likely experienced late recrystallization and alteration. More studies focusing on amphibole inclusions that are fully enclosed in other phenocrysts from quartz-saturated porphyries are required to allow for a solid summary.

Line 394-397: This sentence needs clarification. Or it could be elaborated more in the methods or at least in the footnote of the supplementary data tables how the replacement values were chosen.

Done. The following text was added on lines 324–325: "please see the footnote of Supplementary Data 2 for details about how the replacement values were chosen".

Line 478: The beam size on the EMPA should not affect signal intensities. The same number of electrons are spread out over a larger area.

Thank you for pointing it out. We wanted to say that a low beam energy or a large beam size should be used to reduce the effect of halogen migration. The corresponding text was changed on lines 397-398.

Zoltan Zajacz

Reviewer #3 (Remarks to the Author):

Thank you very much for your wealthy ideas and suggestions. We tried our best to incorporate most of your suggestions into the manuscript, while not adding too much text in order to keep the focus on the main story.

Overall this paper reads well and this is a good contribution of natural data to the existing literature and compliments existing experimental work and modelling on this topic. Sufficient evidence is provided in support of the authors hypothesis and main arguments. The calculations presented in the main text regarding the mass of metals and S in the fluid need to be detailed either in the manuscript or in the supplementary material – this evidence is essentially the crux of the paper and need to be reproducible to those who read it. To improve this manuscript, the author should consider restructuring some of the text and also addressing literature that discusses an almost parallel set of processes but argue a different emphasis on the importance of deep versus shallow systems.

The principle of the mass balance was elaborated by adding a calculation template in Supplementary Data 6. We rewrote part of the Introduction, and added the Fig. 7 and a summary section to better present the main focus of this study.

Below I detail 3 themes that could be improved throughout the manuscript, followed by a line-by-line review of the text.

1. Transfer of melt and fluids from deeper to shallower crustal levels

One of the main findings of the manuscript is that >90% of S and Cu and high proportions of other metals are partitioned into fluids exsolving at depths of up to 0.5 GPa. I think the author needs to explain more clearly why this is so significant relative to other models that are favoured in the literature.

The second paragraph of the Introduction was considerably revised to more clearly present our main focus and to show why it is topical and provocative. Fig. 7 and a concluding section was added to summarize the main findings.

For example, because 90% of total system Cu has partitioned to the fluid phase at depths of 0.5 GPa, the significant increase in the mass of fluid exsolved during decompression bears minimal influence to the overall ‘fertility’ or ‘ore-forming potential’ of the fluid since most of its metal inventory was determined at deeper levels in the crust.

Done. The corresponding text was added on lines 174–178.

This challenges prevailing models, which like you are in support of a ‘single-stage’ model for

the formation of giant porphyries, but claim that the transfer of water-rich magmas from the lower crust is an essential mechanism for exsolving high masses of metal-rich fluids that determine the formation and size of the deposit (Chelle-Michou, 2017); the importance of deep fractionation is attributed to the accumulation of H₂O and metals whilst remaining predominantly volatile undersaturated (Chiaradia and Caricchi, 2017; Richards 2015; Rezeau and Jagoutz, 2020).

There seems to be a misunderstanding. Our study actually supports prevailing models that involve (1) the formation of hydrous but $\pm$ H₂O-undersaturated magmas in the lower crust and (2) the exsolution of ore-forming fluids from magmas accumulated in mid- to upper crustal reservoirs (Richards, 2011; Chelle-Michou et al., 2017; Chiaradia and Caricchi, 2017). Our reconstructed pressures of 0.3–0.5 GPa (crustal depths of 10–20 km) indeed correspond to mid- to upper crustal levels in the Sanjiang region because the crust was up to 60 km thick at that time. What's really new in the present study is that we provide direct evidence to substantiate this model. In addition, we find that the ore-forming fluids in the Sanjiang region must have ascended over a surprisingly long distance of ~10 km to the site of ore precipitation.

I do not dispute that deep fractionation of magmas plays a highly important role however I was surprised at how high the proportion of Cu and S partitioning in to these fluids were and hence how subordinate degassing on ascent was to adding to the mass of metals conveyed by these fluids. To consolidate your conclusions I think some reference to the aforementioned models in addition to detailing the method of your calculations would be recommended.

Two more references, Chelle-Michou et al. (2017) and Chiaradia and Caricchi (2017), were added on line 42. The calculation table was added in the Supplementary Data 6.

I think to strengthen the argument, the author should state more clearly the decoupling in concentration versus mass (Hogg et al., 2023). A simple calculation comparing the mass of fluid required to generate the drop in concentration observed in the intmed to evolved MIs in Fig.1 at 0.5 GPa and compare this to the mass of fluid exsolved at 0.1-0.2 GPa (from your Al barometry) would clearly emphasise to the reader that although a higher masses of FLUID exsolves at shallow levels, the higher fluid-melt partition coefficient at depth, supersedes the relatively small mass of fluids at Shanjiang, resulting in 90% of the metal inventory being partitioned into a smaller fluid reservoir at 0.5 GPa.

Fig. 8 was added to highlight that the size of ore deposits was mainly controlled by the mass of ore-forming fluids, rather than the metal concentrations in the fluids. Since we are able to demonstrate that most of the ore-forming fluids exsolved at 0.3–0.5 GPa, the suggested comparison to the mass of fluid exsolved at 0.1–0.2 GPa is not necessary anymore.

2. Importance of sulfide saturation for the fate of S and Cu

The author mentions that intermediate samples are saturated in MSS which cause depletions in S and Cu and Au relative to the incompatible models (Fig.1).

The Cu MSS-melt partition coefficient ranges reaches up to 1000-10000 (Kiseeva and Wood 2013, Li and Audetat, 2015) and is highest in evolved melts. Whereas Cu fluid-melt partition coefficients generally to do not exceed ~300-400 (Zajacz et al., 2008; Tattitch and Blundy, 2017) in evolved melts. Hence, formation of sulfides prior to degassing will cause S and Cu

and Au to drop in the melt, as the author states.

Although you conclude that fluids are causing most of the depletion in volatiles and metals listed in Fig.1 some acknowledgement of how 90% of Cu and S are still being delivered to fluids despite the presence of MSS is recommended. Without modelling I appreciate that the above conclusions are rather speculative and not the primary focus of the paper. However I do feel it is important to comment on the state of the magma – if it is sulfide saturated, then address that e.g. mass of fluids degassing outweighs the effect that high Kd of Cu and Au and low mass of formation of Cu-Au-rich sulfides; or that perhaps degassing may have transitioned the melt into a sulfide undersaturated state (Nicholson et al., 2024).

The author states that only a small amount of MSS is precipitated – but could the author go a step further and perhaps compare the mass of fluid versus sulfide needed to partition a given mass of Cu – keeping in line with the theme of mass being important? My suggestion would be to start with something like $DCu_{MSS-melt} / DCu_{fluid-melt}$ to give how much more [Cu] is sequestered in sulfides versus fluids. I do appreciate the limited word count of the manuscript, so please consider this as a suggestion.

Thanks for the suggestion. This issue is now detailed on lines 153–161: "S, Cu, and Au could be affected by the presence of sulfides (S also by anhydrite), but the abundance of sulfides and anhydrite in the natural intermediate to felsic magmas is extremely low (typically, only 1–2 sulfide inclusions per 150- μ m-thick section; anhydrite has been observed only sporadically). Since the mass of fluids exsolved from the felsic magma significantly outweighs the mass of potentially precipitated sulfides (~5–10 wt% fluid vs. <<0.2 wt% pyrrhotite; Supplementary Data 6), and because sulfides that are not fully enclosed within other minerals decompose during fluid exsolution (Halter et al., 2002a; Nicholson et al., 2024), we argue that the effect of sulfides on the composition of the exsolved fluids was likely insignificant."

3. Stocks

Space permitting, some description of relation of stocks to magmatic system. This could be a figure in the supplement and description in the caption. You refer frequently to the fluid mass capabilities of the stock versus the magma. I think a visual aid would be nice to show that stocks can be fed transiently by fluxing magmas – hence further emphasises your point about the importance of deeper magmatic processes.

Good suggestion! We added Fig. 7 to visualize the relationship of the deep crustal mafic magma reservoir, the shallow crustal felsic magma reservoir, the porphyry stock, and the ore deposit. The cartoon in Fig. 7a highlights the presence of a mid- to upper crustal magma reservoir that is substantially crystallized and thus already saturated in aqueous fluids, and the ones in Fig. 7b, c shows the possible mechanism of efficient transfer and focusing of the ore-forming fluids from the magma reservoir to the top of the porphyry stock.

Line-by-line review

Line 38 – There are two prevailing models for formation of porphyries:

a. Single stage – favoured for big porphyries as discussed here, where metals are predominantly conveyed in fluids exsolved typically from CA magmas despite early forming

sulfides (Richards, 2015; Rezeau and Jagoutz, 2020).

b. Multistage – more typical for small deposits, involves the storage of sulfides and subsequent remobilisation by later infiltrating magmas (Chiaradia, 2014 and refs therein) and/or fluids (Heinrich and Connelly, 2022).

I recommend a comment on the two prevailing models of ore formation before this sentence – with your focus being on the single stage where metals and S are mainly carried by fluids. Then this sentence highlighting importance of fluids but uncertainty on whether high pressure fractionation is important etc, this will bring the manuscripts main exploratory questions to the forefront.

We acknowledge that it is still under debate whether early saturation of magmatic sulfides is detrimental or whether remobilization of these sulfides is beneficial for the formation of porphyry Cu deposits, but these topics are beyond the scope of the present study. We are reluctant to comment too much on the role of sulfides.

Based on the high Sr/Y ratio of Cu-mineralized porphyry intrusions, it seems clear that high-pressure differentiation of mafic arc magmas in the lower crust is a critical prerequisite for the formation of fertile magmas. The main purpose of our study is to provide evidence for where and how the fertile magmas exsolved porphyry Cu-forming fluids. The new evidence allows us to explicitly resolve the two prevailing models (Sillitoe and Perelló, 2024; Heinrich, 2024; Loucks, 2021; Sillitoe, 2010): (1) the ore-forming fluids exsolved from mid- to upper crustal magma reservoirs during substantial, isobaric crystallization of the fertile magmas; (2) the ore-forming fluids exsolved at lower- to mid-crustal depths in response to the ascent of the fertile magmas from the lower crust, with no need to construct mid- to upper crystal magma reservoirs. We rewrote lines 35–48 in the Introduction to more clearly set the stage for this discussion, which does not have much to do with the role of magmatic sulfides.

Line 48 – mention somewhere the models that highlight the importance of deep and shallow reservoirs e.g. Rezeau and Jagoutz (2020); Chiaradia and Caricchi (2017). I think these are valuable citations since you mention both fractionation in deep and the transfer to shallow levels of the crust.

Done. We referred to two more numerical modeling papers on line 42 (Chiaradia and Caricchi, 2017; Chelle-Michou et al., 2017) that highlight the importance of both deep and shallow reservoirs.

Line 69 – what do you mean by entire? Do you mean to say homogenous? Glassy?

We mean that individual, unexposed melt inclusions were ablated and analyzed entirely (together with part of the surrounding host mineral) by LA-ICP-MS, regardless of whether they are glassy or crystallized. The text on lines 69–77 was reworded to avoid misunderstanding.

Line 77: missing 'with' after well?

Corrected.

Line 76 – line 80: reword slightly – there are only experimental data for Cl and S so MIs and

experimental data fit well for these elements; and MI data for S and Cl and metals correspond well to models of incompatible behaviour during magma differentiation.

Done. See lines 78–82.

Also were the incompatible models based on LLDs at 1 GPa too?

Yes – “1 GPa” was added on line 749 to make it clear.

Line 80: reorganise sentence slightly – important exceptions are S, Cu and Au, which, in contrast to Ag, Mo, Pb, and W, are relatively depleted compared to incompatible trends. Experimental and natural data show evidence of monosulfide solid solution MSS, in which S Cu and Au are strongly compatible (Kiseeva and Wood 2013; Li and Audetat 2015).

The sentence was rewritten (lines 83–86).

Line 87 – replace ‘amount’ with ‘proportion ‘

Done.

Line 88 – theoretical trends presented in Fig.1;

Revised.

Line 115 – 118: sentence structure, I suggest something like “ since Cl and metal (*state metals specifically here because need to emphasise that its not Cu and Au*) concentrations in trachydacitic MIs (67–70 wt% SiO₂) in the intermediate rocks deviate negligibly from incompatible behaviour, we conclude the onset of fluid exsolution was not yet reached”.

The sentence was restructured.

Line 120: delete generally substantially

Done.

Line 121: address S and Cu and Au – you have mentioned their concentrations trace magmas to be sulfide saturated in early stages (mafic to intmed compositions) but then conclude depletion is due to degassing. I do not disagree but just a line clarifying that either correlation in depletion across all metals suggests sulfide saturation is not dominant and then perhaps elaborate briefly on a suggestion listed in aforementioned review.

This issue will be elaborated on lines 153–161. We kept the original text here for fluidity.

Line 146: I think it would be nice to show some fluidity with Fig.1 and use the same colors for samples form mafic, intmed and felsic samples. For example, H₂O-saturated apatite inclusions in the rhyolites will be red instead of yellow?

Done.

Line 150: quantify the differences in Cl and F partition coefficients.

Done.

Line 174: sentence requires citations.

Done. See line 133.

Line 175 – 178: sentence could be reworded. Do you need to state hornblende-based pressures? Could not just say this comparison relies on pressures obtained from hornblende inclusions in phenocrysts to avoid resetting ...

Done.

Line 179 – 183: combine into one sentence.

We like to stick to the two-sentence expression, as there is too much information to put it into one sentence.

Line 199 – 202: “Below we show that such deep fluid exsolution was achieved by substantial phenocryst growth in water-rich felsic magmas, and that the volumes of shallowly-emplaced felsic porphyry stocks would have been far too small to provide all the ore-forming fluids.” I would move sentence. First give the evidence.

Done. This and the following two paragraphs were restructured and partly rewritten.

Need to describe what stocks are and their relevance in the magmatic system and porphyries before this.

We added the new Fig. 7 to show what is porphyry stock and its relationship to the mid- to upper crustal magma reservoir and the ore deposit.

Line 205 – 207: move this sentence to earlier when you introduce the model and replace no significant with minimal or none, “We assume that no significant amounts of these elements were incorporated into the crystallizing minerals.”

"no significant" was replaced by "minimal", and the text in this part was restructured and partly reworded.

Line 207 – 209: repetition of point made earlier, I don't think you need this or remove it from earlier: Except for S, Cu, and Au, the good match between theoretically predicted (for 100% incompatibility) and actually observed element concentrations at 67–70 wt% SiO₂ in the melt (Fig. 1) suggests that this assumption is reasonable.

We kept this sentence to emphasize that the assumption for the mass balance is valid.

Line 209 – 213: Author states Cu and Au and S are less reliable in models but these are essentially what the paper is about so I think more attention needs to be paid as to why sulfide saturation is not affecting the composition of fluids (i.e undersaturated) or that it is but because the mass of fluids exsolving (despite lower DCu fluid-melt) is more significant than the mass of Cu partitioning into sulfides (despite high DCu MSS-melt) that it doesn't matter.

Thank you for this suggestion! This was detailed on lines 150–161.

Line 221: measured 'in' not 'on'

Corrected.

Line 220 – 228: How did you compute these fluid compositions? Could you add this calculation to the supplement?

The calculation table was added in the Supplementary Data 6.

I think to prove to the reader that the composition of deep fluids are a good match to natural data, there needs to be a comparison to computed fluids at e.g. 0.1 – 0.2 GPa as measured by some of the amphiboles in Fig. 4.

Since our aim is to evaluate the significance of the fluids exsolved in the mid- to upper crustal magma reservoir, and also for the sake of brevity, only the composition of the deep fluids is presented in Fig. 6a to compare with the natural fluid inclusion data.

“The mass balance calculations also suggest that the aqueous fluids extracted $97 \pm 4\%$ of the S, $92 \pm 7\%$ of the Cl, $91 \pm 14\%$ of the Cu, $88 \pm 10\%$ of the Mo, $87 \pm 14\%$ of the Ag, $84 \pm 12\%$ of the W, $72 \pm 19\%$ of the Pb, and $63 \pm 26\%$ of the Au that was present in the trachydacitic melts (67–70 wt% SiO₂).”

This sentence is really important for the validity of your research and I think it requires some evidence based in a figure (subplot of Fig. 5) showing the mass equivalent of metals in the fluids relative to natural data.

A subplot of the original Fig. 5 (now Fig. 6b) was added to visually show that the computed deep fluids extracted most of the Cl, S, and metals originally present in the felsic magmas.

a further suggestion might be to then compare the mass of metals in fluids exsolved at this shallower level (0.1 – 0.2 GPa) to show it would be inadequate perhaps?

Done. The corresponding text was added on lines 174–178.

Fig.5 should highlight that yes compositions agree (a) but so do the masses (b) – making the reader aware of the decoupling in concentration and mass of fluids and how this is a really important thing to consider in what is required from fluids to acquire ore-forming potential (Hogg et al., 2023).

As mentioned above, Fig. 6b was added to stress the importance of the fluids exsolved at 0.3–0.5 GPa in extracting S, Cl, and metals from fertile magmas. In addition, Fig. 8 was added to highlight that the size of ore deposits was mainly controlled by the mass of ore-forming fluids, rather than the metal concentrations in the fluids.

Is this mass the cumulative mass of fluid exsolved from onset of volatile saturation to records in felsic samples?

Yes.

Some comment on existing models that describe similar mechanisms but de-emphasises the importance of deeper fractionation for generating high mass fluids but instead for accumulating water and metals that are subsequently degassing at shallower levels – in contrast to your conclusions (Chiaradia and Caricchi 2017; Rezeau and Jagoutz, 2020; Chelle-Michou, 2017 etc).

As elaborated above, our conclusion is not significantly different from the proposition of these papers. However, in contrast to these papers we provide hard evidence for the

presence of mid- to upper crustal magma reservoirs underlying porphyry Cu (Mo, Au) deposits in the Sanjiang region, and that the fertile magmas experienced considerable crystallization in the magma reservoir and thus exsolved the majority of the ore-forming fluids. We rewrote part of the introduction, added Fig. 7 and a summary section to more clearly present the main focus and findings.

Line 238: used a density of 2.4 g/cm³ but in Fig.3 a density of 2.7 g/cm³ is mentioned. How come?

Changed to the same density of 2.7 g/cm³.

Line 239: what are the average concentrations that you use? Perhaps detail in a figure caption?

The concentrations were added on line 187.

Line 244: need to be more specific – conclusion is that the highest MASS of fluids, of suspected ore-forming potential, are exsolved at depth of 10-20 km.

Done. Please see lines 193–195.

Line 273 – 275: reference to some studies that have also suggested this and demonstrated that its feasible.

The feasibility is demonstrated in the coming text. We are not aware of previous studies that have drawn conclusions specifically with regard to this issue. This actually represents one of the main new findings of the present study.

Line 276 – 285: restructure paragraph. You establish from mass balance that fluids exsolved at high pressures host 90% mass of Cu and S, which do not equate to the highest mass fluids. Then explain how these fluids were transferred to shallower levels – refer to previous studies here. Then use evidence of abundant fluid bubbles (are these in your samples?) to suggest initial mechanism of ascent of fluids.

The paragraph was revised, with referring to the paper of Tosdal and Dilles (2020). In the original text we already referred to the paper of Pacey et al. (2019) in which it is suggested that abundant fluid bubbles were required for the transport of crystal-crowded porphyry magmas.

Line 292: need citations and comment on similar studies e.g. not a new concept, it is widely appreciated that the mass of S and Cu in porphyries is ‘anomalous’ – Chiaradia and Caricchi (2017), show that periodic fluxing of hydrous magmas over ~2Ma facilitates generation of Cu-S rich fluids.

Two references, Parmigiani et al. (2017) and Heinrich (2024), were cited to support the conclusion that most of the fluids could not have ascended simultaneously with the porphyry magma.

Line 311 - 330: this paragraph needs to pull out main points more clearly. It is important to discriminate the composition of computed fluids, existing natural data and then this study,

more clearly. Which FI data in Fig. 5 are mineralised and non-mineralised?

This paragraph was shortened and moved to the conclusion section. We added Fig. 8 to visually support our conclusion that the size of ore deposits was mainly controlled by the mass of ore-forming fluids, rather than by the metal content of the fluids or the magmas.

Fig 5 highlights that mineralised and non-mineralised have the same concentrations – this would be a nice opportunity explicitly say that concentration decouples from ore-forming potential – instead mass is important (in line with your paper).

Bingham and Sanjiang have the same range in concentrations and the same volume stock but Cu and Au mass higher at Bingham – leads on from first conclusion, proving that mass of fluids is really important. This would be another reason to have a subplot of mass here too.

Done. We added Fig. 8 to make this point more clear.

You mention metal ratios but don't say which? Are they in fluids or melts? And they are not quantified or shown in figures? I suggest remove or elaborate.

We mean metal ratios in the orebodies. The corresponding text was reworded on line 261.

General comment – importance of the mass of the magma reservoir at the onset of volatile saturation is not addressed– I think this is important to consider and briefly note in the introduction.

This point was already addressed on lines 183–185.

Line 331: Need a final sentence that synthesises findings and main themes of paper.

Done. A concluding section was added on lines 243–267.

Review by Olivia R Hogg.

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