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Interactive comment on "Magmatic sulphides in high-K calc-alkaline to shoshonitic and alkaline rocks" *by* Ariadni Georgatou and Massimo Chiaradia

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General comments Overall, the manuscript presents a useful dataset and has a good analysis and interpretation of that data. Some of the interpretation and conclusions are a little bit of a stretch from the data as presented; I felt that the comparison between the volcanic centres was too narrowly focussed on the sulphide inclusions and didn't consider them in the context of the silicate host, and in my opinion some of the conclusions made around post-subduction metal budgets draw too much from a small sample set. However, these are aspects that should be open for discussion – at least as speculative interpretation, they should help to advance the community's thinking on

subduction and post subduction metal budgets. One issue that isn't discussed in detail here is whether total metal abundance is actually a component of fertility. Although it's a logical assumption that more copper = more copper deposits, that isn't really supported by data on e.g. arc magmas. They very rarely (if ever) show Cu contents outside of a narrow band, and typically have sub-MORB concentrations of Cu. Cline and Bodnar's 1991 paper argued that the key to forming a porphyry was the mass of magma, rather than any remarkable pre-ore enrichment of Cu – which has hitherto not been observed.

Specific comments DOI links to additional data: None of the DOI links worked for me. If the data are published elsewhere they should be formally cited. If they are as yet unpublished they should be included as supplementary information with this manuscript.

Line 52 describes porphyries as being associated with Andean-type subduction, then contrasts post-subduction. I would strike the descriptor "Andean-type". It's too narrow to be correct. There is porphyry-type mineralisation associated with the Laramide orogeny – not exactly analogous to the Andes – and significant deposits elsewhere in the Pacific Rim that are associated with non-Andean, intraoceanic arc settings. I would leave the distinction between syn-subduction and post-subduction.

Line 347: Konya anhydrite inclusions – possibility of water saturation dismissed – why? On what basis? Some of the Konya samples are evolved compositions (dacites) and water saturation is entirely feasible. Sulphur speciation and hence mineral stability is also modified by pressure (see Matjushkin et al 2016).

Line 385: Various authors argue that the Cu is "lost" during arc magma differentiation, but not within continental crust (see Cin-Ty Lee et al 2012, Jenner's work on the topic too, esp. Nat Geoscience 2017). A more common argument is that the continental crust is Cu depleted - more so than expected - and that this occurs through deep fractionation and periodic delamination of Cu-rich cumulates. Both of these authors have invoked those Cu-enriched lithologies being reworked to produce porphyries, but this is at odds with the body of data on Cu-enrichment in arc magmas (we don't really see "failed porphyries" with anomalous Cu – arc andesites are all typically lower Cu than MORB). Richards (2009, Geology) argued that copper is lost to cumulates with other metals, and that reworking of cumulates could effectively enrich resulting deposits in those metals – but not particularly the Cu. This seems to be supported by the data from Howell et al 2019 (Nat Comms). Post-subduction systems show an enrichment in Au and Te, but Cu is similar to syn-subduction.

Line 400: An alternative implication is that porphyries are associated with intermediate rocks, and iss-inclusions are also found in intermediate rocks. The formation of iss and saturation of dense, Cu rich phase is unlikely to have a positive impact on "fertility" - and many authors link Cu compatibility during petrogenesis to the rarity of porphyries rather than their formation. My view is that porphyry-forming magmas need to be reasonably evolved in order to saturate with water, which subsequently extracts the metals. The degree of evolution required to reach water saturation would also be sufficient for iss stability / dominance.

Line 415: I'm not sure that I would conclude that post-subduction systems have the same initial metal budgets based on these data. Kula is intraplate, the text states that Konya has both syn- and post-subduction complexes, and Ecuador is arc related. Using Table 2 for the reconstructed and summarised inclusions, if Beydagi is the clearest example of post-subduction magmatism, then its stats are poor (low N), but it has only one mss-rich inclusion, and rather more that are iss-rich. This is the opposite of the other locations. Given the weak stats on Beydagi, I don't think you can confidently say that post-subduction magmas start with the same metal balance as the others.

If this argument is being made by restricting the comparison to type 2 inclusions, then it's a circular argument. Type 2 sulfides are by definition, mss>iss, so the predominance of Ni over Cu in Type inclusions is expected.

Line 425: This is a comparison between basic (Kula) and intermediate (Konya) suites), rather than a like-for-like comparison of subduction vs. OIB. A more direct comparison between suites with equivalent amounts of differentiation would better support the argument here. ISS-rich inclusions are perhaps better considered a phase associated with lower temperatures and more evolved magmas – indirectly associated with water and the subduction setting (given that water-rich melts have a lower liquidus and subduction magmas have more protracted crustal histories). What would happen if you compared the highly evolved phonolites of the Canary Islands? Does comparing an OIB with similar % crystallisation to a dacite yield ISS? As per fig 10 it should: the sulphide sequence is controlled by P and T.

Fig.7: Inclusion of SEM data with Cu below detection limits does not seem appropriate to me. They cannot be plotted appropriately on the axes, and this may be a consequence of the analytical technique rather than a fundamental mineral chemical control (i.e. detection limit for Cu by SEM-EDS is probably closer to 1 wt% than 0.01 wt%). I would rather the extreme Ni/Cu SEM-derived points are just removed.

Technical corrections Corrections to text and figures made directly on manuscript.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-106/se-2019-106-RC1-supplement.pdf

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-106, 2019.