Response to Referee#1- Dan Smith

Comments by the reviewers have been indicated with ***black***
Replies by the authors have been indicated with ***blue***
Changes by authors have been indicated with ***red***

Answers to General comments

Comment: I felt that the comparison between the volcanic centres was too narrowly focussed on the sulfide inclusions and didn't consider them in the context of the silicate host

Reply: Thank you for this suggestion. The main focus of this study is indeed to provide a background on the sulfide occurrence and chemistry between the different study areas. Regarding the sulfide host minerals, you are right, in the present study we only mention the percentages of the main host minerals for the different study areas, (see line 179). This is due to the fact that in our previous work (Georgatou et al., 2018) we realised a detailed petrographic study as well as EPMA analysis on the silicate sulfide hosts for the case of Ecuador. The results obtained indicated that the sulfide occurrence within the host did not seem to coincide with any chemical anomaly in the zonation of the crystal (amphiboles, pyroxenes and plagioclases). In addition, sulfides were found before, within and after recharge zones in plagioclase hosts.
In this study we have observed a correlation between the sulfide composition (Ni/Cu content) and the nature of the host mineral (sequence of crystallisation) as we mention in line 314 and in the legend of Fig.6. In addition, although we have not shown these data because we are at an initial stage, we have conducted EPMA analysis (single points and profiles) on both silicates and oxides and we have not observed any systematic chemical correlation between the host's major element and the sulfide presence, as in the case of Ecuador. However, because in some cases detectable/determinable Cu amounts were measured in the host, we intend to investigate in detail by analysing both silicate and oxide host minerals with LA-ICP-MS for their trace element composition in order to check whether these Cu spikes are cause by nano-inclusions or possibly Cu diffusion.

Comment: in my opinion some of the conclusions made around post-subduction metal budgets draw too much from a small sample set.

Reply: We are aware that, despite the efforts made in this study to investigate a large number of thin sections, the population studied remains likely under sampled. However, the aim of this work is to evaluate first order variations in the textural and compositional characteristics of magmatic sulfides collected from different geodynamic contexts and it is likely that the investigated population is able to provide this. Moreover, we understand that this is a 'pilot' study and by investigating a limited number of volcanoes/samples we cannot generalise for all volcanic centers characterised by the same geodynamic setting.
We would like to clarify that the study areas characterised by a post-subduction setting are; the Usak basin including three volcanic centers (Beydagi, Itecektepe and Elmadag) and the Konya volcanic belt. Although, for the area of Konya, as also mentioned in the text, both subduction and post-subduction geodynamic setting have been suggested in the past, according to a very recent study by Rabayrol et al. (2019), the results show clearly that Konya represents a post-subduction rather than a subduction setting. Based on those findings we consider Konya one of the areas characterised by a post-subduction geodynamic regime.
Change: We have added a section in the end of Supplementary 2 discussing the limitations of the methods including the fact that this is a 'pilot' study and by investigating a limited number of volcanoes/samples we cannot generalise for all volcanic centers characterised by the same geodynamic setting. In addition, we have re-phrased and corrected transitional setting by post-

subduction setting adding this recent study (Rabayrol et al., 2019) in line 91 and 96. We have also changed the descriptor 'transitional' wherever needed.

If by 'some of the conclusions' the reviewer refers to a similar and more specific comment regarding our findings on similar initial metal abundances of the different study areas based on ImageJ modal abundances of the relative proportions of the mss and iss, please see our answer below for comment/Line415 (which now reads 482).

Comment: 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.

Reply: In order to be able to discuss in detail total metal abundances, a degree of modelling would be required, which goes beyond the purpose of the present study. Nevertheless, we agree with the comment, there is indeed a debate between studies suggesting that one can make deposits from normal or even "depleted" Cu magmas (Cline and Bodnar, Chiaradia and Caricchi) and others that claim that metal enrichment is a fertility asset (Core et al. 2006; Stern et al., 2007; Park et al., 2015 etc.). In order to emphasize more the importance of the volume of magma vs the total metal abundances as the referee suggested, we have made the following changes;
Changes: We have re-phrased and extended in the discussion the section dedicated to the implications for ore formation adding also in line 454, the paragraph below:
'Cline and Bodnar (1991) and more recently Chiaradia and Caricchi (2017) and Chelle-Michou et al. (2017) have shown that there is a correlation between the size of the magma reservoirs providing metals and fluids to the ore system and the size of the deposit and that also the duration of the ore process might play a role in this sense.'

Answers to Specific comments

Comment/Line 52/Now reads 58: 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.

Reply: Thank you for the suggestion.
Change: the descriptor "Andean-type" has been removed and replaced by syn-subduction. In addition as suggested we have used everywhere the distinction syn- and post- subduction to discriminate the two environments.

Comment/Line 347/Now reads 385: 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).

Reply: Thank you for this interesting observation, it is something that we had to consider as well. Indeed the iss-only/Cu-rich Type-4 sulfides co-existing with anhydrite are mostly seen in evolved samples (andesites and dacites, median SiO_2>60 wt%, see Tabs.1-2). This sulfate-sulfide co-existence

itself does not necessarily point out solely to an either water saturated or water-under saturated system since anhydrite can crystallise in both environments. However, if the system was indeed water saturated we probably wouldn't be seeing any magmatic sulfides, since the metals would have been mostly scavenged by the hydrothermal fluid, we would rather find hydrothermal sulphides. Whereas if the system reached sulfide saturation prior to fluid exsolution we would expect for the metals to partition mostly in the sulfide phase. These Type-4 sulfides co-exist in the same magnetite host with melt inclusions and apatite crystals. In addition their angular shape indicates that the solution was trapped initially as a Cu-rich liquid which solidified following the host mineral crystallisation planes and later unmixed. Nonetheless, as we mention in line 431 what we may be seeing here is the initial state of a magma saturating in water. The increased presence of vesicles in contact with the anhydrite-sulfide pairs, may be an additional fact for the above statement. We are currently working on the apatite and magnetite chemistry in order to investigate this possibility.

Change: We have added this reason in line 380, the sentence now reads;

'In addition, the system is not expected to be already water saturated since we would expect for the metals to partition into the fluid phase and result in hydrothermal and not magmatic sulfides, and therefore the temperature ranges in which anhydrite will be stable can differ.'.

Comment/Line 385/Now reads 429: 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.

Reply: We agree with the extended explanation and scientific content of the above references mentioned. However, since with the present study we do not have any quantitative and precise restraints on the pressure (and therefore depth) we have removed the phrase "within the continental crust". Our ongoing work on pressure, temperature of sulfide saturation in enclaves and their host rock should help answer this question.

Change: We have removed the phrase "within the continental crust" as requested.

Comment/Line 400/Now reads 519: 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.

Reply: Thank you for the suggestion.

Change: We have included the felsic nature of the lithologies as a possible contributing factor to ore forming process in lines 512 and 490.

Comment/Line 415/Now reads 482: 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.

Reply: Thank you for your comment, we realise that the method was not sufficiently explained in addition there was a mistake in line 256 that read '100' instead of 126 sulfides. The statement regarding similar initial metal magma contents between the different study areas is a result mainly based on the comparison of the relative proportions of the mss and iss composing Type-2 sulfide inclusions (Fig.9) and to a lesser degree on the bulk area reconstruction (Tab.2). We agree with the referee that it would be a circular argument to compare the Ni and Cu values between mss and iss and this is why we have not applied this method. For details regarding the methodology as well as similar results obtained from other studies which applied the same methodology, please see answer to first general comment of referee#2.
In total 163 sulfides (of all sulfide types) were processed by ImajeJ in order to obtain the modal abundances (area %) of the mineral phases composing the sulfide inclusions. Of those (163), 126 corresponded to Type-2 sulfides (see Tab.4 and 5 in doi:10.6084/m9.Figshare.8230787) and 84 account for the post-subduction setting specifically, (Beydagi-12, Itecektepe-16, Elmadag-10 and Konya-26/which is considered post-subduction rather syn-subduction, see answer to 2nd general comment above). Between them all those areas show similar mss area % (and iss area %), Beydagi=86.9±3.2 %, Itecektepe =84.8±4.9 %, Elmadag=86.9±4.8 % and Konya=88.1±2.6 %. As also stated in lines 251-254, the mss area % for the Kula/Intraplate-OIB and for the Ecuador/Subduction setting are also similar corresponding to 82.0±7.4 % and 82.0±4.8%, respectively. In total 126 Type-2 sulfides present relative proportions of mss= 84.2 and iss=15.7 with 2se=± 2.2.
Change: We have re-phrased, in the discussion (lines 463), the paragraph dedicated to the source fertility and the argument for initial metal contents of the different magmas, in order to state clearer the main results on which we are basing our argument. In addition we have extended the methodology part of ImageJ of the Supplementary 2 (as requested by referee#2) and we have added the number of sulfides considered for their mss and iss relative proportions in box-plot Fig.9. The value '100' has been corrected since the total number of Type-2 sulfides that have been processed by ImageJ is 126 as shown also in Tables 4 and 5 in doi:10.6084/m9.Figshare.8230787. Please see text for changes.

Comment/Line 425/Now reads 500: 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.

Reply: We agree with the reviewer. Please see answers to specific comments of line 400 (which now reads) and comment in lines 393-395 (which now read 512) from referee#1 regarding similar argument on the importance of lithology/melt evolution.
Change: We have removed the part of the sentence referencing Kula.

Comment/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.

Reply: We understand that the SEM values are not quantitative and since we could not calibrate our SEM (because of software problems with JEOL) we may as well indeed have an error close to 1 wt%.
Change: We have removed the data from the graph as requested.

Answers to Specific comments marked on main text by Dan Smith

The comments/answers below have been also posted directly on the main text of the manuscript

Comments/Lines 96 and 116/Now read 110 and 130: This should be tonnes of ore @ Au g/t. These numbers need checking. The resource should be expressed in tonnes or million tonnes, grade in g/t.

Reply: Thank you for the observation.
Change: The grades have been converted from Moz to tonnes. Please see text.

Comment/Line 157/Now reads 173: The statement isn't wrong, but I don't think the decreasing Sr should really be listed as a defining feature. In a number of post-subduction suites worldwide, anomalously high Sr is a notable feature (as mentioned in intro for Ecuador).

Reply: We agree with the comment.
Change: We have removed the element “Sr” from the sentence as requested.

Comment/Line 277/Now reads 308: Do you mean "There are two main stage of sulphide evolution observed..."

Reply: Yes this is what we meant to say.
Changes: After recommendation also from the referee#2 we have changed the sentence as follows, “Two main stages of sulfide evolution were observed in this study”.

Comment/Line 205/Now reads 219: A simple table would better summarise the inclusions types, their mineral hosts, and the volcanic centre hosts (and their classification with regards to syn-/ post-subduction). Perhaps expand fig 4?

Reply: We would rather keep the textural information of the figure and expand as suggested.
Change: We have expanded figure 4, which now besides the study area, the sulfide type with their mineral host also depicts the geodynamic setting at the top.

Comment/Line 387/Now reads 430: This content is covered in the background sections. I think you can cut this paragraph.

Reply: We agree with the reviewer.
Change: We have removed the indicated paragraph.

Comment/Line 393-395/Now reads 512: How much of this is because the samples from other areas (e.g. Kula) are in lithologies typically excluded from porphyry exploration programmes - mostly for being too primitive? Are we really seeing implications for ore formation, or are both the sulphide assemblage and presence of mineralization a consequence of the melt evolution stage?

Reply: Thank you for the suggestion.
Change: We have included the evolved nature of the lithologies as a possible factor for ore forming process and have re-phrased, in the discussion, the paragraph dedicated to the source fertility. Please see text.

Comment/Line 418-421/Now reads 486: This needs rephrasing. It reads as if magmas with insufficient water to strip metals are infertile - when logically, that cannot be correct. A porphyry deposit is a hydrothermal feature - the stripping of metals by a fluid is a vital part of the process. A magma with insufficient water will be copper rich yet still infertile.

Reply: Thank you for noticing.
Change: We have re-phrased as requested. Please see paragraph dedicated to water as a contributing factor for ore formation.

Comment/Line 427/Now reads 459: Key reference here is Richards 2009 (for post-subduction). Lee et al. 2012 (Science) argue similarly for normal arcs. I don't really see any good examples of Cu-rich magmas (representing recycled magmas that would be parental to porphyries) in compilations like Georoc.

Reply: Thank you for the suggestions.
Change: We have added both references.

Comment/Line 471/Now reads 571: Does it really suggest Cu enrichment? Is this not just Ni depletion? Fig 3 doesn't support Konya being Cu-enriched (one sample above 30 ppm whole rock).

Reply: We agree with the comment, indeed the sentence needed re-phrasing, since the implication does not mean Cu-enrichment in the whole rock, rather in the sulphides.
Change: We have re-phrased accordingly.

Comment/Line 487/Now reads 594: Many author entries in the reference list only have one initial - despite them actually having more than one.

Reply: Thank you for the observation.

Change: After checking the journal's reference format, we have added the second initial wherever needed.