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Interactive comment on "A revised map of volcanic units in the Oman ophiolite: insights into the architecture of an oceanic proto-arc volcanic sequence" *by* Thomas M. Belgrano et al.

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We are glad Dr Kusano enjoyed the read and thank her for her careful review. The comments are insightful and will greatly help to improve the manuscript. In the following, we discuss each point raised by the Reviewer and our proposed changes. We will then submit a detailed list of revisions arising from all the discussion comments with the revised manuscript.

The two main concerns raised by the Reviewer concern the genetic and tectonic context in which we have presented the volcanic map. Establishing this context was not the main aim of this study, and we hope that the map will be helpful regardless of which

consensus is eventually reached on the tectonic setting of each magmatic phase. Accordingly, we rely principally on previous studies for contextualizing the mapping work. These studies, especially those utilizing magmatic geochemistry and geochronology, have found many similarities between the IBM proto-arc sequence and the Semail sequence. We reviewed and added to this evidence more thoroughly in a recent paper Belgrano and Diamond (2019).

Regarding the issue of equivalence between the Semail and IBM units raised by the Reviewer, we agree that supporting evidence for this equivalence should be more clearly shown. We believe noting this equivalence will help workers unfamiliar with the Semail ophiolite to understand and use our map for comparison with other settings. To support the claims of equivalence, we will add the relevant IBM references to Table 1, and add a dedicated section to the Discussion, including a Figure based on Fig. 5a–d showing the Semail unit fields in comparison to the IBM lavas. We will also clarify that these are not exact equivalents, but rather the closest-available equivalents in a potentially analogous proto-arc sequence.

The Reviewer also points out that, in our Figures 5 and 6 the IBM boninite compositions are scattered and that the similarity between the Semail and IBM boninites, mentioned in Section 7.6, is not apparent. Our intention was to highlight the rather similar MgO contents of the Semail and IBM boninites (< 17 wt% MgO), and to contrast them with the remarkably magnesian Tonga boninites (up to 22 wt% MgO). We agree that in our other Figures, compositional distinctions are not apparent. Also, both the Semail and W. Pacific boninite compositions are rather scattered, in part due to the low concentrations. We have moved this discussion to a new dedicated discussion section (Section 8) with its own figure, and will clarify this text to point out the scatter and specify that the compositional similarity is limited to MgO content.

In the second main point, the Reviewer notes that a history of both axial and subduction phase magmatism is broadly accepted for the ophiolite. We agree on this point, with the important qualifier that we believe the available evidence, reviewed in Belgrano and Diamond (2019), indicates that the axial phase also occurred above a subduction zone. This subduction influence then became more pronounced during Phase 2 Alley magmatism (Alabaster et al., 1982; Kusano et al., 2014, 2017; Umino et al., 1990).

The Reviewer also points out that the mantle diapirs are tectonic features, and that our data cannot deny the existence of such features. We agree completely on these points and had not intended to argue otherwise. We would, however, argue that mantle diapirs are linked to the magmatic system through their roles in melt generation and as sites of melt migration (Nicolas, 1986; Rabinowicz et al., 1987). We did not intend to call into question the existence of these carefully-mapped diapiric structures (e.g., Nicolas et al., 2000; Rabinowicz et al., 1987). Our point is that the significant Phase 2 volcanism documented in this study implies that melt-generating structures in the mantle (diapirs) may conceivably be coeval with and related to Phase 2 magmatism. Therefore, unless independent evidence suggests otherwise, it should not necessarily be assumed that all of the mantle diapirs belong to the axial phase. Up to 14 such diapirs have been structurally mapped by Nicolas et al. (2000). Although an abundance of troctolites above the Maqsad diapir suggests that it at least partly formed during the MORB-like axial phase, the same is not clear for many of the other diapirs (Python and Ceuleneer, 2003). We will add a few sentences clarifying this to Section 7.2: Proportions of the upper crustal units, and cite the compatible findings of Python and Ceuleneer (2003), who found that only a minority ($\sim$25%) of mantle dykes and cumulates in Oman can be related to the axial, MORB-like phase.

The Reviewer also made several minor comments, which we respond to below.

We are grateful that the Reviewer spotted some typographical errors, and inconsistent Figure naming (e.g. 3a vs 3A). We will correct these errors and carefully proof-read the revised manuscript.

The Reviewer pointed out that some of the existing regional geological maps are inconsistent with one another and questioned how we dealt with this. Fortunately, these

differences were not found to be significant for the volcanic rocks. The only significant difference we noticed was between the BRGM Fizh and BME Wadi Bani Umar map sheets at the gabbro–SDC contact. As mapping this contact was not the aim of our study, we simply drew a compromise between the two maps onto our map. However, in light of the Reviewer's comment, we reconsidered this area, and have used the more recent field map of Adachi and Miyashita (2003) to draw the contact in this area.

The Reviewer requested the addition of V2 type I and II (Godard et al., 2003) compositions to Fig 4, as was erroneously indicated in the caption, but had been cut in the Figure.. To support our discussion and grouping of these units, we will add a third panel of MORB-normalized trace element plots with the Godard et al. (2003) units to Fig. 4.

The Reviewer questioned the meaning of the (-) symbol for Field Character in Table S1. Indeed, this symbol indicates that the field character was ambiguous or not clear. We will add a footnote to the table to clarify this.

The Reviewer asked whether the transitional features are mappable or geochemical features. So far, we mainly recognized the transitional nature of these rocks in terms of geochemistry and stratigraphic position. The field character of the units is indeed often intermediate between the units in question, though this is typically easier to recognize in hindsight, with the knowledge of their geochemistry. In any case, the differences are subtle and difficult to map, especially without continuous outcrop. We will add this explanation to Section 4.5: Interpretation of transitional compositions, to clarify this.

The Reviewer found the bar chart in Fig. 9 difficult to understand, questioning whether we needed to show each unit and whether we counted minerals multiple times. Showing the magnetic carriers on both an 'all unit' and unit basis takes up little extra space and shows the interesting progression from dominantly secondary to primary magnetic carriers upwards through the stratigraphy. As shown in Figs. 9e & d, several magnetic carrier minerals can exist within a sample. In the bar chart, the minerals are counted more than once if more than one is present in a single sample. We will add a note to the Fig. caption explaining this.

In Fig. 10, the Reviewer asks for references for each zone. The extents of these zones are all our own interpretations based on the aeromagnetic map, and we have updated the inset caption with (this study) to reflect this. The interpretation of the repeated ophiolite blocks and overlying Batinah Complex is supported by the gravity modelling of (Shelton, 1990), the structural observations of Woodcock and Robertson (1982b, 1982a) and outcrop mapping on the Yanqul and As Suwayq map sheets (BRGM, 1986b, 1986a). This is mentioned in the text, but we will also add these supporting references to the caption.

The Reviewer asked for a more concrete definition of what is meant by weak and strongly magnetic, in terms of colours on the RTP map. Outlining all these zones in Fig. 10 would render the underlying magnetic map unreadable. Instead, in the Section 5.4 (Observed reduced-to-pole anomalies) text we will define the typical values of weakly and strongly magnetic zones in RTP μT, which should allow the reader to more easily follow our references to Fig. 10.

The Reviewer also suggests that topography and volcanic bedrock units be added to Fig. 10. We agree that these layers would better help show how these datasets were integrated. Regarding the topography, the freely-available digital elevation models (NASA space shuttle and ASTER) are unfortunately noisy over the low relief of much of the volcanic terrain, which results in somewhat messy contouring and hill-shading. Any shading would also interfere with the interpretation of the magnetic colour scale. We hope that topographic data will be included into new editions of the regional map set if our map is incorporated. Until then, in Section 1.4.3 we have suggested that our map be used in tandem with the existing regional maps for this reason. Regarding the volcanic bedrock units, such details are difficult to show at the scale of the map shown in Fig. 10. We opted to show the outline of all the volcanic bedrock for this reason, as it can be shown with a single line. Adding shaded or hatched units would

hinder reading the underlying magnetic colour scale. To maintain the readability of Fig. 10, we show close-up examples of differently magnetized zones in Figs. 13 and 14. To address the Reviewer's concerns and aid the interested reader in understanding the integration of these datasets, we will add the aeromagnetic map as a layer to the supplementary Geospatial PDF, and mention this in Section 6: Map Construction and Presentation and in the Fig. 10 caption. This way, Fig. 10 remains readable, and the different datasets and maps can be easily viewed as layers at different transparencies, and at different scales, by interested readers.

With reference to Fig. 16, the Reviewer asks whether the type locality for boninites (Ishikawa et al., 2002) has been reclassified as Tholeiitic Alley. We have not reclassified the type locality for boninites. The type locality is not visible in Fig. 16; it lies just outside the Figure to the east, in an area that we have mapped as Boninitic Alley, in agreement with Ishikawa et al. (2002). We used the boninite sample OM16-46C and other samples from Kusano et al.,(2017), as well as our own field observations, to define the extent of boninites in this area.

The Reviewer also mentions that the representative field photo of a Tholeiitic Alley pillow in Fig. 3c resembles Geotimes lavas in the area. We agree that the overall shape and brownish-red colour of the pillow do resemble Geotimes lavas, and that is probably why this outcrop was originally mapped as Geotimes. However, the high vesicularity and black spherules of the pillows at this locality and intercalated andesite massive flows (not pictured) are in our experience diagnostic of Tholeiitic Alley. To confirm this, we collected five samples from along the Suhalylah volcanic section and mapped these outcrops in detail, so as be certain that Geotimes is not in fact the uppermost unit, as previously reported. The stratigraphically lower two samples were proven geochemically to be Geotimes, and the upper three were found to be Tholeiitic and Transitional Alley. For us, this unequivocally confirms that Alley lavas occupy the top of the Suhaylah volcanic section. To further support this point, we will add another representative photo of Tholeiitic Alley to Figure 3, and make a small close-up map figure of the

Suhaylah section in the discussion Section 7.4, as suggested by the Reviewer.

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