**Authors' reply to RC #1**

We would like to thank the reviewer for the insightful comments.

In the first half of this response we address the main comments raised by Referee 1 (Dr. Scott Whattam).

Referee comments are in black and our responses are in blue.

The main problem I have with the ms concerns the treatment of the tectonic configuration at and after subduction initiation (SI) and the concept of the "doubly-vergent" SI. This (doubly-vergent SI) is mentioned in the title and addressed in the final section of the manuscript. However, the data provided in this ms (essentially geochemical) cannot address this. I think the authors should probably just drop this section altogether or consider/provide alternative models for SI. An alternative title could be: Boninite and boninite-series volcanics in the northern Zambales ophiolite: Implications for subduction initiation along Philippine Sea Plate margins.

We use "doubly-vergent SI along Philippine Sea Plate margins" in a descriptive way, synonymous to "subduction initiation on both sides of the PSP". If doubly-vergent SI will be sustained it will lead to "double in-dip subduction" (Holt et al., 2017).

We disagree with the reviewer that our data cannot address "doubly-vergent SI". As detailed in our MS; the petrological, geochemical and most importantly field characterization of boninite, and the recognition of subduction initiation stratigraphy in the Zambales ophiolite when considered with available published geological and geophysical data (Section 6.4) support the initiation of subduction on both sides of the PSP.

Subduction along the margins of the Philippine Sea Plate is already well established (Deschamps and Lallemand, 2002, 2003; Lallemand, 2016) and a NE-verging subduction zone on the western margin of the PSP is reflected in both global and regional plate reconstructions of SE Asia and the western Pacific region (Wu et al., 2016; Zahirovic et al., 2014, 2016)
What we provide in our MS is evidence from the rock record for the inception of such a subduction zone.

We use and highlight "doubly-vergent SI" in the title because this configuration is distinct from SI scenarios solely based on the IBM forearc which mainly focuses on the problem of whether subduction initiation is spontaneous or induced (e.g. Arculus et al., 2016; Keenan and Encarnación, 2016).
Geodynamic models of SI should be based on robust geologic data and a "doubly-vergent SI" configuration based on the Zambales ophiolite provides another "boundary condition" for refined models of SI along PSP margins.

The alternative would be placing the Zambales ophiolite along the IBM forearc, similar to Fig. 4 of Pearce et al., (1992). From geologic arguments alone, (i.e. the opening of the West Philippine Basin at 55-46 Ma and presence of the Philippine archipelago in between) this would be untenable.

For example, there is no mention of a possible plume-induced SI scenario yet Figure 10a shows the Oki-Daito Plume smack in the middle (beneath) the WPB at (and probably just before?) subduction initiation. I believe that Wu et al. posit that the WPB formed as the result of plume emplacement pretty much at the same time or just before SI. An explanation for the cause of doubly-vergent subduction is not provided in the ms; I find the similar timing of SI on either sides of the WPB very difficult to explain without a plume-induced origin. Evidence for this (plume-contaimnation and hence a possible plume-induced SI scenario) would be in the form of isotopes from the proto-arc basalts and boninites which should record plume-contamination if there was a plume-induced origin (I am pretty certain that IBM FAB do not record evidence of plume-contamination). In any event, the SI scenario at the WPB appears to be similar to that of the Late Cretaceous along the Caribbean Large Igneous Province (CLIP). Whattam and Stern (2015) suggested that SI was likely plume-induced and resulted in subduction along a great portion of the periphery of the CLIP. The difference for this ms however, was that evidence for plume contamination was shown. Even if the authors do not address the doubly-vergent SI, the ms is still of great value as it documents a chemostratigraphy/chemotemporality identical to that of the IBM forearc.

What our data cannot directly address is the association of the "doubly-vergent SI" configuration along PSP margins to "plume-induced subduction initiation/ PISI". Thus, it is not mentioned in the MS and we make no attempt to link the two.

The location of Oki-Daito plume (based on Wu et al., 2016) is indicated in Fig. 10b for the only reason that in Page 14 Line 14-15 (original MS) we suggest that "the interplay between plate forces and mantle upwelling (e.g. Oki-Daito plume of Ishizuka et al., 2013) should be explored by geodynamic models".

We agree with the reviewer that a "doubly-vergent SI" configuration might provide a link to "plume-induced subduction initiation". However, at this point, to explicitly associate the two would be highly speculative and it is more appropriate to address this problem in a separate paper. As an example, the geodynamic models of Baes et al., (2016) can be modified to specifically test/demonstrate PISI in the PSP. The size and spreading history of the West Philippine Basin and the crustal structure of Cretaceous and Eocene terranes are constrained well enough that these can be incorporated in the models. The model dimensions can be set-up as 2640 km x 1525 km x 2640 km, and the crustal thickness of the overriding Cretaceous arc terranes-Eocene intra-arc basins as 15-25 km and 10-15 km, respectively (Nishizawa et al., 2014; Wu et al., 2016). But in our opinion and as demonstrated by Baes et al., (2016) the most critical parameter will be the size and shape of the Oki-Daito plume which is, so far, unknown. The age difference of the overriding and subducting plates might also be a key.

Using PRIMELT3 (Herzberg and Asimow, 2015), the primary magma composition and olivine liquidus temperature of a depleted (proto-arc) basalt (PAB) from the Coto Block that precedes boninite from the Acoje Block can likewise be estimated. The olivine liquidus temperature of a depleted PAB from Coto Block lies in an adiabatic upwelling path similar to Mariana Trench PAB with a $T_p$ of ~1370 ˚C (Perez et al., in prep). Ambient (MORB) and plume (Mauna Loa) mantle potential temperatures are estimated by Putirka, (2016) as 1330-1450 ˚C and 1560-1670 ˚C, respectively. So far, we find no evidence for the excess heat, thermal anomaly and plume head size invoked by Macpherson and Hall, (2001); hence our reticence to discuss PISI.

Isotopic investigations are in agreement that a mantle plume is likely associated with the oceanic plateaus and bathymetric highs emplaced at the same time as the opening of the West Philippine Basin (Hickey-Vargas et al., 2006.; Ishizuka et al., 2013). However, these studies also have ruled out this plume as the source of excess heat for boninite generation and concluded that a link between the Oki-Daito mantle plume and subduction initiation is rather unlikely. It is not the case that we didn't show any evidence of plume-induced SI but it is a question of whether there is one, at least from a petrological point of view.

Our main point is that characterization of the Eocene SI along Philippine Sea Plate margins as "doubly-vergent" is still valid whether it is plume-induced or not.

In our view, the most prudent way to address this is to note that the geometry of incipient subduction with convergent subduction zones on both sides/ along its margins as deduced from the Eocene tectonic reconstruction of the Philippine Sea Plate resembles the results of geodynamic models of plume-induced subduction initiation (Baes et al., 2016; Gerya et al., 2015). And we have added new lines in the revised manuscript to reflect this.

*" Although current terrestrial subduction is dominantly asymmetric, it is interesting to note that two-sided subduction is what is essentially produced in 2-D and 3-D models of mantle convection (Gerya et al., 2008; Wada & King, 2015). Likewise, we discern that doubly-vergent SI geometry or subduction initiation with oppositely-dipping subduction zones along its margins is characteristic of 3-D thermo-mechanical models of plume-induced subduction initiation (Baes et al., 2016; Gerya et al., 2015). Plume-induced subduction initiation was first recognized by Whattam and Stern, (2015) to describe the temporal association of late Cretaceous plume-related oceanic plateaus in Central America that was followed by arc volcanism with oppositely dipping slab dispositions. In the case of the Eocene Western Pacific, a mantle plume centered on the Manus Basin had originally been invoked by Macpherson and Hall, (2001) to account for an inferred thermal anomaly in IBM boninite mantle source, implicitly connecting the initiation of the IBM arc to the presence of a mantle plume. Isotopic studies are in agreement that a mantle plume is likely associated with the oceanic plateaus and bathymetric highs emplaced at the same time as the opening of the West Philippine Basin (Hickey-Vargas et al., 2006; Ishizuka et al., 2013); however, thermal anomalies in excess of the ambient mantle are not reflected in mantle potential temperature estimates of proto-arc basalt from the IBM forearc (Umino et al., 2017). A causative link between a mantle plume and the doubly-vergent SI configuration along Philippine Sea Plate margins is yet to be established. We speculate that the location of Philippine Sea Plate (PSP) in the nexus of Pacific, Indo-Australian and Eurasian plates and their long-term Cenozoic plate motion makes doubly-vergent subduction initiation along its margins feasible. The northwestward translation and clockwise rotation of the Philippine Sea Plate starting in the early Eocene had to be accommodated by the adjoining oceanic domain east of southern Eurasia (e.g. East Asian Sea); hence, its interaction with the oceanic leading edge of the Philippine Sea Plate is expected (Wu et al., 2016; Zahirovic et al., 2016) and likely led to incipient subduction (Fig. 10). The dynamics of sustained double-vergent subduction is*

*examined by Holt et al. (2017) but doubly-vergent subduction initiation is yet to be explored by numerical modelling. Field and petrologic data presented here demonstrate that models of subduction initiation based on the IBM forearc are currently simplistic. Geodynamic models of subduction initiation should be based on robust geologic data and a doubly-vergent SI configuration based on the Zambales ophiolite provides another boundary condition for refined models of SI along PSP margins. We advocate that geodynamic models of subduction initiation along Philippine Sea Plate margins incorporate a pre-Eocene, N/NE-dipping subduction zone (the proto West Philippine Trench of Faccenna et al., 2010) associated with Cretaceous terranes forming the overriding plate, doubly-vergent subduction initiation along its margins as well as the interplay of plate forces and mantle upwelling (e.g. Oki-Daito plume of Ishizuka et al. 2013) during incipient subduction (Fig. 10b).is a testable mechanism that can be explored by geodynamic modelling."*

In the next section, we address the points given by the reviewer in the "Specific points" section. We've adopted most of the suggestions given by the reviewer.

SPECIFIC POINTS

1. Page 1, Abstract: Line 15: as this is the first discovery of hi-Si boninite in the Zambales ophiolite, this should be explicitly stated.
Sentence modified → "We report for the first time…"

2. Page 1, Abstract: Line 18: place "the" before "Zambales ophiolite"; this has to be done in many instances throughout the ms
We reviewed the manuscript and placed "the" in places we've missed.

3. Page 1, towards bottom of the abstract: Perhaps should state that work on the Coto Block was done by others and not by this study; I had to subsequently look through the ms to see if work was done on both the Coto and Acoje blocks
Reference added

4. Page 1, Line 31: would insert "and vice versa" at end of first sentence
"and vice versa" added

5. Page 2, Line 2: after "plume-induced subduction initiation" should reference Whattam and Stern (2015) and Gerya et al. (2016) (I believe Whattam and Stern (2015) were the first to specifically coin this)
Added Whattam and Stern (2015). Gerya et al. (2016) is probably Gerya et al. (2015)? which was cited previously

6. Page 2, Line 6: Don't understand what "Challenge No. 11" means
As explicitly stated in the IODP (International Ocean Discovery Program) Science Plan for 2013-2013 "Challenge No. 11" is "How do subduction zones initiate, cycles volatiles, and generate continental crust?"

7. Page 2, Line 7: Would replace "including" with "with the exception of"

replaced with “with the exception of”

8. Page 2, Line 23: replace “verified” with “suggested”; we believe this to be the case, yes, but can’t outright verify it
replaced “verified” with “suggested”

9. Introduction focuses almost exclusively of subduction initiation (SI) at the IBM; relevant, but SI has also been discussed elsewhere; as well, different ideas of how SI transpires → e.g., spontaneous vs. induced (Stern, 2004)
This is briefly discussed in Page 2 Lines 24-31

10. Page 3, Line 27: don’t understand what “transitional” MORB means; transitional to MORB and IAT? If so, state this. As well, the IBM FAB which may be analogous to Coto Block MORB-like lavas, have characteristics intermediate to and which overlap MORB and IAT (e.g., Whattam et al., submitted). For example, whole-rock chemistry documents an arc-like Ti-V ratio <20 and evidence of melting of a source more oxidized than MORB (higher $Fe^{3+}/Fe_T$, Brounce et al., 2015). More on relation between Coto Block lavas and FAB later.
Sentence modified
Addressed fully in #56

11. Page 4, Line 7: sentence ending with “transition zone” needs references.
References added

12. Page 4, Line 24: I believe boninitic basalts was also mentioned earlier? These need to be defined at first instance (i.e., lavas which record MgO >8 wt. % and $TiO_2$ <0.5 wt. % as per IUGS boninite definition but $SiO_2$ <52 wt. %)
Sentence modified

13. Page 5, Line 17: Again, confused as to whether paper included Coto Block; Maybe state in first sentence of this paragraph that study was conducted on Acoje Block (only)
Sentence changed to “For this study, a subset of forty-four (44) samples located along NW-SE transects of the Acoje Block volcanic sequence at Barlo were selected for whole rock geochemical analyses and screened through visual and microscopic assessment of secondary alteration.”

14. Page 5, Line 24: change “lost weight”→ weight lost
changed to “weight lost”

15. Page 5, Line 31: Spell out GSJ/AIST
now “Geological Survey of Japan/National Institute of Advanced Industrial Science and Technology”

16. Page 6: Section 5.1: This section is very “dense” and difficult to read. I suggest making a table showing the different lithologies and their mineralogy and textures, and then significantly shortening the written description here
Seems ok to us

17. Page 6, Line 15: Should of probably brought this up earlier, but similar to

point 12 above, perhaps all the different categories of boninite (low-Si, high-Si, basaltic etc.) should be explained in the introduction or at least at an earlier point in ms

18. Page 6, Line 20: I think this is the first mention of Ogasawara? Mention where this is- Japan → part of IBM forearc?
Sentence changed to "This assemblage corresponds to Type II boninite of Umino (1986) in samples described from the type locality at Chichijima island, Ogasawara (Bonin) Archipelago in southern Japan."

19. Page 6, Line 24: change is → are
changed

20. Page 7 Line 6: Insert "A" before peculiar
changed

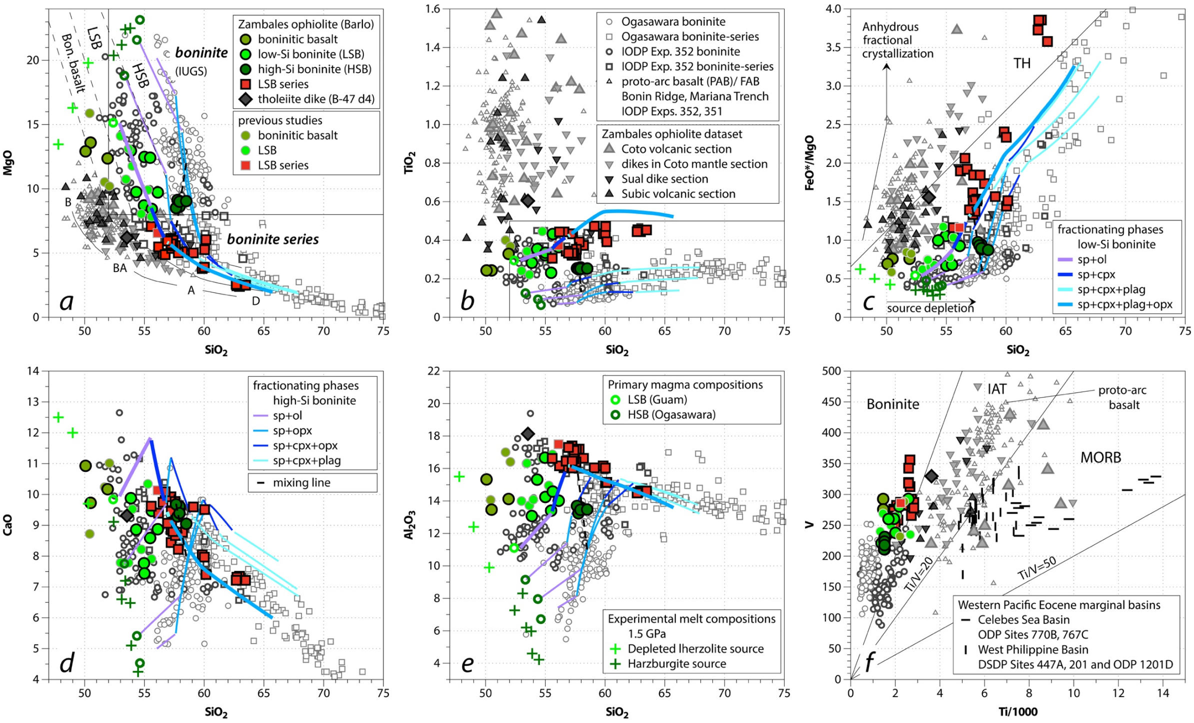
21. Page 7, Line 15: These LOI are very high. And what rock types exhibit these values? All or just high-Si boninites? These should be in the Table in Supp. Doc., correct? So put Supp. Doc. X at end of sentence. Maybe a plot of LOI versus various potentially mobile elements (e.g., MgO, K2O, Na2O, Ba) is warranted? Or at least some sort of statement like "though the LOI are high our petrologic arguments are based primarily on trace elements known to be immobile up to greenschist-facies conditions". Are any filters being applied to your samples? For example, using only samples which yield 98-102 wt.% oxides or ones with <3 % LOI?
Yes, indeed the LOI values are a bit high. We note that in general boninites have LOI values that are greater than 3 wt%.
IODP Expedition 352 shipboard boninite samples have LOI values that are much higher (mostly between 5-11 wt%) and samples from Haugen, 2017 which are the "cleanest, least weathered pieces" of the shipboard cores have LOI values that are just as much as the samples from this study.
Concerning element mobility, this is addressed in Page 7 lines 15-19.
Filters have already been applied in the current dataset. The current dataset was screened not only based on LOI values but primarily based on element immobility judged using diagrams such as CaO vs. $Na_2O$, major oxide vs LOI, and major oxide/trace element vs. immobile element (e.g. Zr). Samples with mobile element enrichment (e.g. total alkalis, Rb) followed a vertical vector and were excluded.

22. Page 7, Line 18: What is primary? And I note here that primary is used later on but not defined. Do you mean primary lavas such that exhibit high MgO, high Mg# (≅65), high Cr and high Ni?
Sentence modified.

23. Page 7, Line 22: I think a reference is needed after "boninitic basalts". Maybe Pearce and Robinson, or Reagan et al. (2017)? Not sure.
Reagan et al. (2015) added

24. Page 7, Line 23: As mentioned at the beginning, Figure 6 is very "busy". Would suggest plotting only samples from this study.
We slightly modified Figure 6. We removed objects that are not discussed in the text such the primary PAB and PAB fractionation. However, we've chosen to still include

the Ogasawara and Expedition 352 boninite and boninite series datasets. Our point is to compare Zambales boninite with the dataset from the IBM forerac. Another point is to show the major element variation vis-à-vis the modelled fractional crystallization path. We believe these are better addressed if our data are shown with the Ogasawara and Expedition 352 datasets.
The panels are now marked as “a, b, c”




25. Page 7, Line 24: change second “and” → or
changed

26. Page 7, Line 29: pristine? You mention above LOI values of 4-7 wt.%
Addressed fully in #21

27. Page 7, Line 31: ug/g? usually reported in ppm
Indeed, trace elements are traditionally reported in ppm but as per Wiedenbeck, (2017) ug/g is better

28. Page 8, Line 3: replace within with → between
changed to between

29. Page 8, Line 15: Change so reads: Compositions of Zambales boninite. . ...are marked by low incompatible trace element abundances. . .
changed to “Compositions of Zambales boninite and boninite series volcanics are marked by low incompatible trace element abundances relative to MORB”

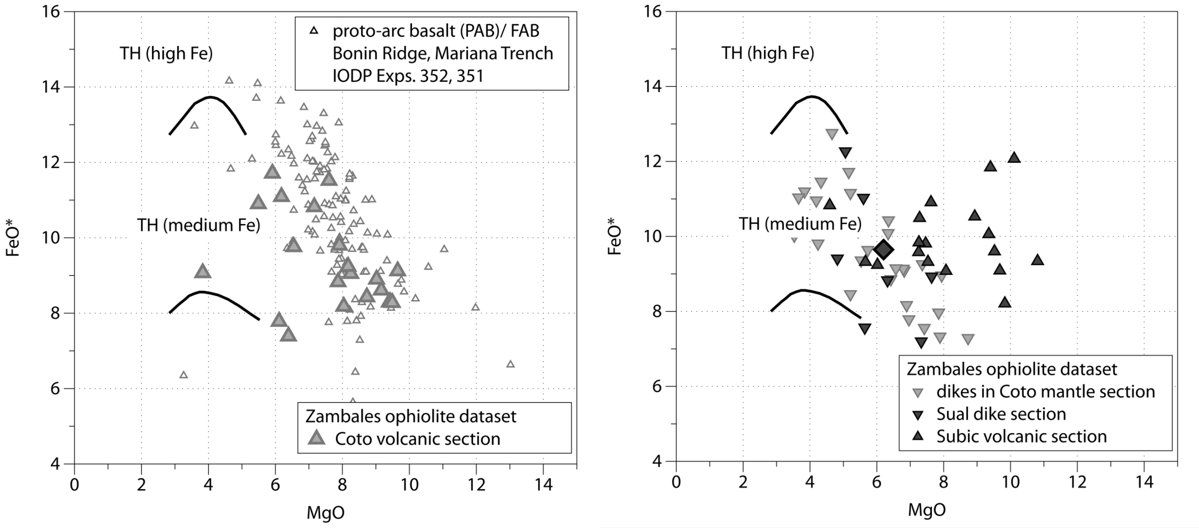
30. Page 8, Line 23: replace times with → x
replaced with x

31. Page 8, Line 25: insert boninite between Zambales and ophiolite

now “The effect of fractionation in Zambales boninite samples”

32. Page 8, Line 32: descending order is unclear; perhaps describe from base → top which is probably standard convention
now “The volcanic sequence at Barlo, based on observed local stratigraphic relationships, is LSB series volcanics-boninitic basalt-LSB-HSB from base to top.”

33. Page 9, Line 6: Is unclear how can be classified as moderate-Fe tholeiites without the Miyahsiro plot overlain by Arculus’ low-med-high Fe series fields
Figure will be added as Supplementary Figure 1




34. Page 9, Line 20: Haugen (2017): Is this a MSc or PhD thesis (not indicated in references).
References have been updated.

35. Page 10, Line 4: I think a paragraph at least is warranted to explain how the modelling was done using MELTS (supplementary document probably appropriate).
The modelling is fairly straightforward and the parameters are given in the figure captions. Figure 6 captions is now modified

36. Page 10, Line 5: Ghiorso and Gualda (2015) not in references
Ghiorso and Gualda (2015) is in the references

37. Page 10, Line 16: change “in the base” to → at the base 5
change to “at the base”

38. Page 10, Line 17: break sentence; add “;” after “at depth”
now “at depth; influx”

39. Page 10, Line 20: change slightly deviate → deviate slightly
changed to “deviate slightly”

40. Page 10, Line 24: change does → do
changed

41. Page 10, bottom of page: Would change Section 6.2 title to → Slab contributions
now “6.2 Slab contributions to a less depleted mantle source”

42. Page 10, bottom of page: Would include more up-to-date references for boninite petrogenesis
additional references added

43. Page 11, Line 7-8: OK, but they are equally LREE-depleted. What is the explanation for the spoon-shaped REE patterns? I think for the “classical” U-shaped signatures that the explanation is high-degree partial melting (which produces low MREE) which is subsequently slab-fluid modified to produce LREE enrichment; not sure of explanation for the high HREE
less depleted source

44. Page 11, Line 13: Maybe explain at beginning of section that Ba/Th is a marker/gauge of shallow slab-contributions and reference (Pearce et al., ?) Why Ba liberated at shallow conditions? Low temperature (I think); low P as well?
45. Page 11, Line 14: insert “increasing” after “mirrored by”
now “mirrored by increasing”

46. Page 11, Line 15: reference Fig. 8b after Th/Yb
Fig. 8b added

47. Page 11, Line 16: what is decoupled?
48. Page 11, Line 18: Insert “A” before “high U/Th ratio”
now “A high U/Th ratio”

49. Page 11, Line 19: change ratio ! ratios; change by ! if; add “to source” at end of sentence
changed

50. Page 11, Line 21: the La/Th vs. Sm/La is not shown so have to indicate this
“not shown” now indicated

51. Page 11, Lines 30-31: Why mention slab melts? This is not mentioned previously and I don’t think anyone familiar with boninite petrogenesis would consider slab melts as part of the equation.
Sentence now modified

52. Page 11, Line 2: change so reads: transitional between MORB and IAT
changed

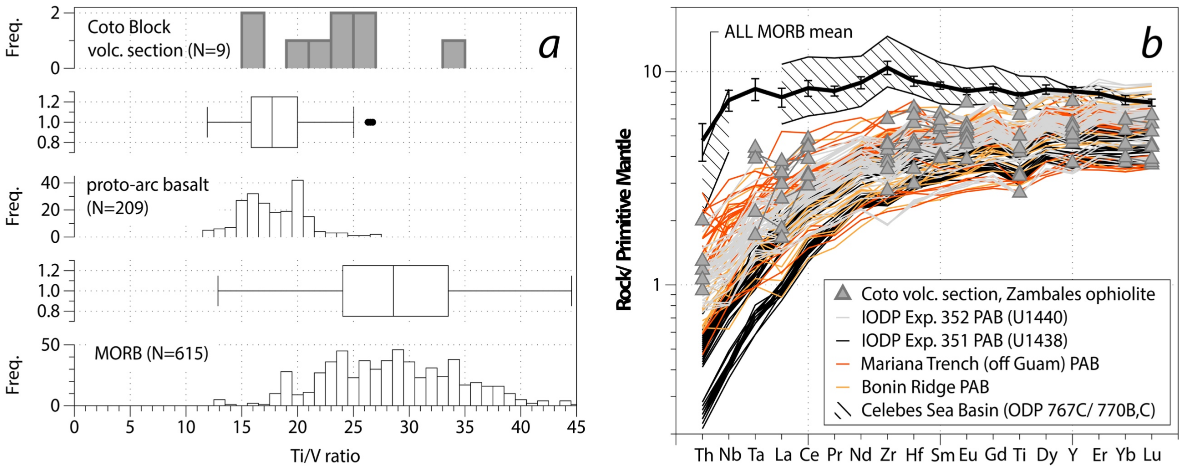
53. Page 12, Line 3: Change “in” → on the basis of
changed

54. Page 12, Lines 3-4: Confusing sentence; why mention distinct from Mariana BAB?
Mariana BAB is mentioned because originally Hawkins and Evans, 1983 characterized Coto Block as back-arc basin oceanic crust. By comparing certain

immobile elements (e.g Hf, Ta, Th), Geary et al., 1989 was able to show that it is distinct from Mariana BAB and that is has composition transitional to MORB and IAT.

55. Page 12, Line 5: Another confusing sentence; have to get point across that depletion in REEs, TiO2, Zr and Y of Acoje relative to Coto documents the progressive depletion of. . .what about LILE enrichments? These should increase from Coto ! Acoje
LILE enrichments are not discussed for two simple reasons- (1) paucity of reliable LILE analyses in the compiled dataset (Hawkins and Evans, 1983; Geary et al., 1989; Yumul, 1990, Evans et al., 1991, Tamayo, 2001), in fact no samples from the Coto Block crustal section have been analyzed by ICP-MS and (2) the altered nature of most samples, with most samples lacking petrographic characterization, makes it difficult to assess the primary nature of LILE enrichment.

56. Page 12, Line 10: Can't readily see where Coto lavas plot in Ti/V space but this is a very important point as FAB can be distinguished by MORB on the basis of Ti/V which is arc-like (>20) and by virtue of elevated Fe3+/FeT indicative of a more oxidized (arc-like) source. Suggest you state what the Ti/V ratios of the Coto lavas are and compare these with those of IBM. Are they similar or not?
We increased the symbol size of Coto volcanics in Fig 6.
Coto Block volcanics (n=9) have variable Ti/V ratios, mostly between 20-26 (Fig. 6f). Recognizing the small sample size, this range is higher than proto-arc basalts (PAB) and overlaps with MORB (Supplementary Fig. 2a); suggesting a less oxidized source compared to IBM PAB.
We also added the primitive mantle normalized immobile element pattern for Coto Block volcanics together with IBM PAB (Supplementary Figure 2b) to highlight its depleted nature.



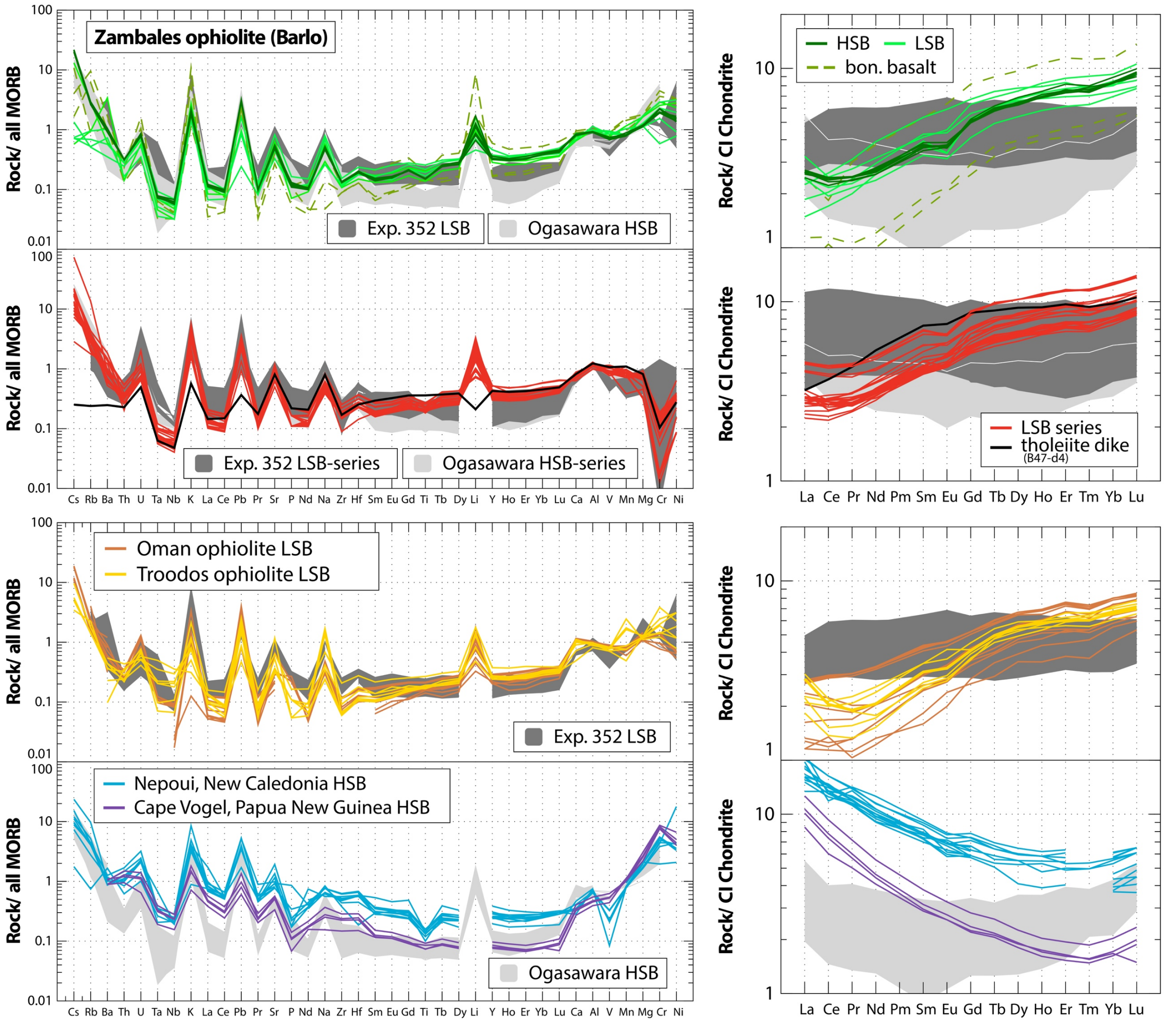

57. Page 12, Section 6.4. See the Presentation and Scientific Interpretations section
58. Page 13, Line 12: insert "above a west-dipping subduction zone" after (Ishizuka et al., 2011)
changed

59. Page 13, Lines 15-20: sounds perhaps like a plume-induced SI scenario
addressed in first-half of response

60. Page 14, Lines 6-8: Why feasible? No explanation for this (doubly-vergent subduction)
addressed in first-half of response

61. Page 14, Line 11: Change north-verging → NE-verging
changed to NE-verging

In addition, we slightly modified Fig. 7 because Nepoui and Cape Vogel HSB were mislabeled. Fig. 7 is now updated

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