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Paradigm shift for controls on basalt magmatism: Discussion with Lustrino et al on the paper I recently published in *Earth-Science Reviews*

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ABSTRACT

I recently published "*Lithosphere thickness controls the extent of mantle melting, depth of melt extraction and basalt compositions in all tectonic settings on Earth – A review and new perspectives*" in this journal to demonstratively summarize a major component of my 30-year dedicated research on the basalt problem with the emphasis that the paradigm "mantle potential temperature controls the extent and pressure of mantle melting and basalt compositions" that is inconsistent with observations and experimental petrology must be abandoned. As the change from "temperature control" to "lithosphere thickness control" is fundamental and unfamiliar to many, I encourage members of the community for debate. The community interest in the theme of the paper is reflected by more than 1000 reads and 50 recommendations in R^G within 10 days of its publication. In addition to a few email exchanges, the commentary by Lustrino et al. (2022) is the first written discussion with interesting comments of both breadth and depth. Overall, their commentary is more highly affirmative rather than criticism. Most of the comments are not about Niu (2021) but more general and may be shared by the scientific community. Therefore, my response is intended to be thorough, in particular on issues and concepts that I consider the community needs to be correctly informed of. My reply follows four basic principles: [1] I maintain "*Objectiveness and open-mindedness (vs. 'confirmation bias') are requisite twins for insights and discoveries.*"; [2] I express my views based on observations, experimental petrology, well-understood concepts, logical reasoning and objective analysis, rather than personal opinions to "*believe*" or "*disbelieve*"; [3] I emphasize that some misattributions may be innocent, but should be avoided because they serve no favor to our science and scientific discussion; and [4] I try to use plain language to ensure my writing readily understood by the reader. For clarity, my reply follows the same numbered questions as in the commentary and I do not discuss issues on which the commenters agree with me. Figures referenced in quotations are those in Niu (2021). For consistency, mantle potential temperature is denoted with T_{MP} (vs. T_P)

1. Introduction

"*A consequence of this is rejection of mantle plumes in controlling the compositions of MORB and OIB … …*" (Lustrino et al., 2022)

Niu (2021) neither rejects mantle plumes nor implies so. The idea of "mantle plumes" has been a *good* hypothesis since its inception because it can help explain large igneous provinces (LIPs) and other localized intra-plate basaltic magmatism although this hypothesis needs testing.

A hypothesis should not be opinionated as being right or wrong but should be judged whether it is reasonable or not. A hypothesis is deemed reasonable if it can be tested, but it has no significance in the first place if it is not testable. If a hypothesis is proved to be true, then it can be developed into a theory. If a hypothesis is rejected, then we must formulate a new testable hypothesis, and repeat the process, until such newly developed hypothesis/theory is proven to stand the test of time (before it may be found to fail to explain newer observations). In my view, this is the essence of scientific research, an understanding gained from studying the history of science. For example, I agree with the community that *plate tectonics* is a mature theory although the potential of this theory continues to be explored, including its efficacies of further predictions (Niu, 2014, 2018, 2020). On the other hand, despite some persuasive arguments in favor of mantle plume derivation from deep

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mantle thermal boundary layers like the core-mantle boundary (e.g., Campbell and Griffiths, 1990; Davies, 1999, 2005) and its convenience to explain the origin of large igneous provinces (LIPs) since the late Paleozoic (e.g., Coffin and Eldholm, 1994; Coutillot et al., 2003), mantle plumes cannot yet be detected with confidence (e.g., Julian, 2005). Therefore, "*mantle plumes*" is not yet a theory, but a hypothesis. This hypothesis would be reasonable if it can be tested, yet the currently heated mantle plume debate, especially their presence or not in the Earth (e.g., Davies, 2005; Foulger, 2005, 2010; Foulger and Natland, 2003; Foulger and Anderson, 2005; Niu, 2005a; Campbell and Davies, 2006), albeit useful, offers little prospect for effectively testing the hypothesis. Given the importance in resolving the mantle plume debate, Niu et al. (2017) endeavor to offer a geologically executable approach "*Testing the mantle plume hypothesis: An IODP effort to drill into the Kamchatka-Okhotsk Sea basement*", in which interested readers can find details.

> "Niu *(2021) focuses … … in controlling basalt compositions and volumes … …*" (Lustrino et al., 2022)

Niu (2021) does not discuss basalt melt volumes.

The commenters may be confused between the extent of mantle melting (F) that Niu (2021) discusses and the volume of melt production (V_M) that Niu (2021) does not. These are different concepts with entirely different geological significance. The extent of mantle melting F is the mass fraction of melt (M_M) produced from partial melting of the fertile mantle (M_{FM}), i.e., $F = M_M/M_{FM}$ or $M_M = F^*M_{FM}$, in which $M_M = V_M\ ^*\rho_{[melt\ density]}$. Indeed, $V_M \propto F$, but $V_M = f(F,M_{FM})$ and the amount of M_{FM} melted (fertile mantle flux into the "melting zones" above the solidus) is geologically far more important for V_M. The variable F can be estimated from highly incompatible element abundances in basalts by assuming reasonable abundances of these elements in M_{FM}. V_M is not a basalt property and cannot be estimated from basalt compositions but is a geologically measurable quantity that can be evaluated using geological observations (see p. 1447–1448,1453 of Niu et al., 2011). For example, because of the lithospheric *lid effect*, intra-plate ocean island basalts (OIB) erupted on thin lithosphere have the geochemical property of high F, whereas OIB erupted on thick lithosphere have the geochemical property of low F, resulting in the inverse correlation of F with lithosphere thickness (i.e., the depth of lithosphere-asthenosphere boundary, LAB; Niu and Green, 2018). However, no correlation is expected between the size of OIB islands/island groups and LAB depth. This simply means that M_{FM} material flux into "melting zones" (above the solidus) is an important factor (as well as F) when discussing/comparing melt volumes ($V_M \propto M_{FM}$) between intra-plate ocean islands and island groups. "*This concept is relevant to the 'mantle plume' debate*" (p. 1449 of Niu et al., 2011). This concept applies to mantle melting in all tectonic settings.

2. Mantle potential temperature

> "Niu *(2021) questions the need to invoke different mantle potential temperatures (T_{MP}) to explain differences in compositions and eruptive depths of MORB … …*" (Lustrino et al., 2022)

Niu (2021) does not question the need, but shows no evidence for the need to invoke … …

The correct statement is following. There is no evidence for large $\Delta T_{MP} = \sim 250$ K beneath global ocean ridges away from Iceland (Niu and O'Hara, 2008; Niu, 2004a, 2016) and $\Delta T_{MP} = \sim 50$ K is possible if any (Niu and O'Hara, 2008). The invoked $\Delta T_{MP} = \sim 250$ K results from using invalid parameter Fe_8 (Niu, 2016, 2021). The T_{MP} beneath Iceland on the North Atlantic Ridge may be high if Iceland represents a deep-rooted thermal mantle plume, but this is unknown and is highly debated (Foulger and Anderson, 2005; Foulger, 2010). Elsewhere beneath intra-plate ocean islands or continental interiors, there could be T_{MP} variation that I have no evidence to rule out, but erupted basalts record no information on T_{MP} or initial decompression melting depth P_O, let alone T_{MP} variation. Erupted basalts do record information on final depth of melting P_F (LAB depth), which is the depth of melt extraction. The latter is manifested by the correlated compositional variations of basalts with LAB depth. So, in this case, yes, I do not invoke T_{MP} variation because basalts do not tell us the effect of T_{MP} and we thus do not know the role of T_{MP} or its variation from the erupted basalts. We do observe the effect of P_F, i.e., the *lid effect* without making any assumption.

> "*He writes that mantle potential temperature, 'refers to the surface projection of the mantle temperature along the adiabat'… … In fact, however, T_{MP} can be measured anywhere in the mantle, including the lithosphere, when partial melts find pathways and sufficient buoyancy to reach the surface (e.g.,* Anderson, *2011)*" (Lustrino et al., 2022)

Yes, Niu (2021) prefers to use the definition by McKenzie and Bickle (1988) because this definition is conceptually simple, physically straightforward and gives a convenient temperature reference in discussing mantle melting although there is not yet an agreed and consistent method in determining T_{MP} values.

The difficulty in reaching any agreed T_{MP} value for sub-ridge mantle is irrelevant to, and does not negate, the definition. The definition of T_{MP} is meant to compare possible mantle temperature variation at a given depth in the mantle unaffected by conductive cooling to the surface (or seafloor). The latter has varying high conductive geothermal gradient ($dT/dP_{[conductive]}$), depending on the thickness of the lithosphere (i.e., $dT/dP_{[thin\ lith]} > dT/dP_{[thick\ lith]}$). Therefore, T_{MP} is relevant to geothermal gradient of the asthenosphere, which is known as the adiabat or adiabatic geothermal gradient (i.e., $dT/dP_{[adiabat]} << dT/dP_{[conductive]}$). Choosing the surface projection of the asthenosphere temperature along the adiabat is meant to compare lateral asthenosphere temperature variation at a given depth and the "surface" is the unique "depth" to choose for conceptual simplicity and clarity.

By saying "*T_{MP} can be measured anywhere in the mantle, including the lithosphere*" (Lustrino et al., 2022), it causes confusion and makes any discussion on mantle melting incomprehensible. I agree that the issue is how to come up with a benchmark method in estimating T_{MP} beneath ocean ridges and in other settings.

> "Niu *(2021) reduces the maximum T_{MP} variation of oceanic and continental basalts to ±50°C from an average of 1350°C.*" (Lustrino et al., 2022)

Niu (2021) uses the average value of $T_{MP} \approx 1350$ °C considered to be appropriate for "normal" asthenospheric mantle (sub-ridge or sub-continental away from thermal plumes; e.g., McKenzie et al., 2005; Green and Falloon, 2005, 2015; Niu and Green, 2018).

Different authors would defend their own chosen T_{MP} values with their own reasoning and justifications. I do not wish to enter the debate that will not influence my understanding. I only focus on issues that I have new insights, e.g.,the erupted basalts show the *lid effect*, but not T_{MP} effect. This does not mean, and I do not assume, that T_{MP} is universally constant, but we just do not observe the effect of T_{MP} and its variation from erupted basalts although many people may choose to believe so. I emphasize what the data show, especially the correlated variations of petrological/geochemical parameters with physical observables such as the lithosphere thickness. If there were no discovery of the *lid effect* on basalt magmatism in all tectonic settings on Earth, we would probably forever be trapped in the "myth" of T_{MP} control and T_{MP}-related debate! Nevertheless, I maintain that T_{MP} is a useful concept and it is unlikely that T_{MP} is globally constant, but how to develop a benchmark method in estimating T_{MP} values is an important task.

3. Mantle plumes or not?

"Niu *(2021) offers a third hypothesis… … he precludes any inferences or any role for the possible temperature variation in the upper mantle, excluding the effect of the adiabatic thermal gradient effect. The diversity of tectonic environments in which* Niu *(2021) proposes the "LID effect" to be the controlling factor seems to leave nothing for mantle plumes.*" (Lustrino et al., 2022)

Niu (2021) [1] does not advocate a globally single uniform T_{MP}, [2] does not preclude at all any inferences or any role for possible temperature variation in the upper mantle, and [3] does not exclude the effect of adiabatic thermal gradient effect. These misattributions by a group of prominent scientists may be totally innocent but should be avoided because they do not serve any favor to our science and scientific debate. I hope the community holds an objective mind in reading the commentary and this response. To address the misattributions, I quote the following:

(1) "*Basalts and basaltic rocks are the most abundant igneous rocks on the earth and their petrologic and geochemical studies have formed our knowledge base on the thermal structure and composition of the mantle with which we have developed workable models on the chemical differentiation of the earth.*" (p. 1 of Niu, 2021). Hence, it is the petrological and geochemical studies (including experimental and theoretical) that tell us the composition and thermal structure of the upper mantle, for which I make no claims of any kind.

(2) "*The significantly correlated variations of OIB compositions with the thickness of oceanic lithosphere at the time of volcanism (Fig. 10a) is the simple manifestation of the lid effect with OIB erupted on thick lithosphere having the petrological signature of low extent of melting (low F ∝ P_O-P_F) and high pressure (high P_F = deep LAB) of melt extraction, whereas the opposite is true for OIB erupted on thin lithosphere (Fig. 10b). The same is true for CIB as demonstrated in Figs. 12-13.* ***We should note that the compositional scatter about the systematic trends must be combined effect of mantle source compositional variation, initial depth (P_O) of melting due to T_{MP} variation or source fertility variation, and errors associated with fractionation correction. We do not at all ignore all these factors, but they are secondary and insignificant because they are overshadowed by the lid effect.***" (p. 22 of Niu, 2021)

(3) "*[1] Global MORB (Figs. 8,9), OIB (Fig. 10), VAB (Figs. 11) and CIB (Fig. 13) compositions all show the lid effect (i.e., the P_F control), but do not show the effect of 'temperature control' (i.e., P_O or T_{MP}). The latter may not be important at all in reality or, if any, must have been obliterated because of effective and efficient melt-solid equilibration in the melting mantle. [2] Objectiveness and open-mindedness (vs. 'Confirmation bias') are requisite twins for insights and discoveries.*" (p. 22 of Niu, 2021).

What we observe in basalts is the *lid effect*, not effect of "T_{MP}". This does not mean T_{MP} has no effect, but its effect, if any, is overshadowed by the *lid effect*. I suggest that the reader hold an objective and critical mind in reading the literature without being misled by innocent misattributions.

4. The solidus

"*In his diagrams,* Niu *(2021) assumes a single, fertile, C-H-free solidus, namely that of* McKenzie and Bickle *(1988)*." (Lustrino et al., 2022)

Yes, Niu (2021) uses a single solidus for conceptual and discussion clarity. Niu (2021) does not ignore, but does acknowledge the relevance of the matter:

(1) "*The effect of minute volatiles on the solidus is insignificant for volumetrically important magma generation and can thus be neglected here for clarity (*Niu and Green, 2018*).*" (p. 3 of Niu, 2021) This simplification is petrologically justified at least for two simple reasons: [1] H_2O and CO_2 are the most abundant volatiles in the mantle, yet they are only in trace amount in MORB, OIB and CIB settings. They do have effect on the topology of the solidus (see Fig. 4 of Niu and Green, 2018), but because of their "incompatible element" behavior, they quickly enter the melt phase upon near solidus melting, **making all the subsequent and dominant decompression melting occurring on the anhydrous (dry) solidus**; [2] H_2O and CO_2 are expected to be more important for metasomatized deep portions of oceanic and continental lithosphere (e.g., Niu and O'Hara, 2003), whose low-degree melting, when thermally perturbed on localized scales or in the asthenosphere after being recycled, can produce H_2O and CO_2 rich SiO_2-undersaturared melts, but such melts are volumetrically unimportant compared to volumetrically significant basalt magmatism at ocean ridges (e.g., entire ocean crust occupying ~70% of earth surface) and intra-plate settings we discuss (more H_2O in subduction-zone settings). This is stated as "*Melting cannot happen in the lithosphere as it is under subsolidus conditions* ***(except for volumetrically small metasomatic veins or veinlets of lower solidus temperature)***." (p. 18 of Niu, 2021)

In this context, it is worth to emphasize that the effect of volatiles such as H_2O and CO_2 on mantle melting is certainly important (e.g., Wyllie, 1977), but should not be overly stretched for reasons discussed above. In the case of CO_2 effect on magmatism, we should not be overly influenced by the currently hot discussion on crust-mantle carbon recycling, carbon capture and storage in the crust and carbon emission into the atmosphere. Their discussion is important only if relevance, objectiveness and rigor are warranted.

(2) "*The mantle solidus ($dT/dP_{[solidus]}$) is a material property and its position and topology in P-T space depends on the composition of mantle rocks, especially the effect of volatiles such as H_2O and CO_2 (see [e])*" (p. 5 of Niu, 2021). The effect of varying mantle Mg# and alkalis ($Na_2O + K_2O$) on the solidus is evaluated by Niu and O'Hara (2008; p. 657), suggesting "*that the net effect of source composition and small T_{MP} contributions on the initial depth of melting is likely to be small … …*")

(3) "*But the understood mantle melting mechanisms (Fig. 3) do not require excess heat or temperature but require conditions that place the asthenospheric mantle onto or above the solidus* ***(dry or wet solidus)*** *in P-T space although the initial depth of melting (P_O) may vary.*" (p. 20 of Niu, 2021)

(4) "*Modified after* Niu (2020) *and* Niu et al. (2015) *(with the wet solidus and dehydration solidus adapted from* Green et al. (2010) *and* Green and Falloon (2015)*, showing in P-T space the mantle solidus (*McKenzie and Bickle, 1988*) … …*" (p. 21 of Niu, 2021)

5. Lithosphere

"*The core issue of* Niu *(2021) is what he defines as the "lid effect", a concept also promoted by* Anderson *(2011)*" (Lustrino et al., 2022)

The potential lithospheric thickness control has been mentioned by several authors with the obvious earlier ones being Ellam (1992) and Haase (1996). Recognizing that the sub-ridge mantle melting stops at depth variably deeper than the Moho, Niu and Batiza (1991) proposed the final depth of sub-ridge mantle melting P_F. Recognizing from the global MORB data and abyssal peridotites (MORB melting residues; Dick et al., 1984), Niu and co-authors emphasized the presence and significance of the conductive (or cold) thermal boundary layer (CTBL) beneath ocean ridges (Niu, 1997, 2004a; Niu and Hékinian, 1997a, 1997b; Niu et al., 1997), which led to the examination of the *lid effect* on

OIB (Humphreys and Niu, 2009; Niu et al., 2011).

Anderson (2011) may have talked about, but the lid effect was ***NOT*** proposed by Anderson (2011). The record shows that Don enjoyed reading Humphreys and Niu (2009), and this is what Don emailed to me (May 14th, 2009) "***Yaoling, I always take one important long paper with me when I go on a trip. I would like to study your paper with Humphreys when I sail to Alaska on Sunday. Could you send me a printable file? Thanks. Don***".

Yes, there have been different definitions on lithosphere, mechanical, thermal or compositional, but in terms of magmatism, the lithosphere is consistent with the conductive thermal boundary layer and the lithosphere-asthenosphere boundary (LAB) is most consistent with amphibole dehydration solidus, which is discussed by Niu et al. (2011) and elaborated in great detail by Niu and Green (2018) "*The petrological control on the lithosphere-asthenosphere boundary (LAB) beneath ocean basins*", which I recommend the community to read because it offers in simple clarity a unifying solution that not only explains why LAB depth increases with increasing seafloor age from beneath ocean ridges (∼ 10 km) to beneath seafloor of up to ∼70 Ma (the square root of age relationship) on a global scale, but also explains the intrinsic control on the globally constant LAB depth (∼ 90 km) beneath seafloor older than ∼70 Ma (isobaric solidus at ∼90 km).

6. Asthenosphere

"Niu *(2021) considers the mantle underlying lithosphere to be a convective layer with homogeneous T_{MP} and an adiabatic geotherm.* Niu *(2021) holds that the T_{MP} of the sublithospheric mantle is virtually the same, everywhere, whether the lithosphere is thin (oceanic ridges) or thick (mature oceanic basins and midplate continental settings)*." (Lustrino et al., 2022)

Yes, Niu (2021) agrees with the community that the sub-lithospheric mantle can be reasonably considered as convective mantle with adiabatic geothermal gradient ($dT/dP_{[adiabat]}$), but does not consider anywhere in any way a homogeneous T_{MP}, which is another innocent misattribution. If mantle plumes do exist and are indeed derived from the deep thermal boundary layer at or near the core-mantle boundary in the mantle thermal plume hypothesis, then T_{MP} [plumes] > T_{MP} ["normal"] (Niu, 2005a, 2020). The seismic images of subducting slabs in the upper mantle, in the mantle transition zone and even in the lower mantle are informative that there is no uniform T_{MP}. By definition (McKenzie and Bickle, 1988) as discussed above, T_{MP} is the property of the convective mantle (with $dT/dP_{[adiabat]}$), has nothing to do with the lithosphere (with $dT/dP_{[conductive]}$) and is irrelevant to lithosphere thickness variation (with varying $dT/dP_{[conductive]}$).

"*We question the assumption of a fully convective asthenosphere, especially given the low melt fraction (∼2%) hypothesized by* Niu *(2021) himself. The presence of small amounts of melts (∼1–2%;* Niu, *2021) in what is considered "asthenosphere" is not sufficient to allow a fully convecting system*." (Lustrino et al., 2022)

Niu (2021) does not assume 1–2% melt in the convective asthenosphere. The 1–2% melt is a conservative estimate in the sub-ridge decompression melting region between the onset of melting at P_O and depth of melting cessation at P_F (i.e., the melting region P_O-P_F) by assuming near-fractional decompression melting (e.g., McKenzie, 1985a, 1985b; Johnson et al., 1990). Away from ocean ridges and deeper than the dry solidus (P_O), there can be incipient melt <0.5% or less (Green et al., 2010; Niu and Green, 2018). In the upper mantle deeper than the seismic low velocity zone (LVZ), there could be even less melt or no melt (e.g., Green et al., 2010; Green and Falloon, 2015).

The view of the commenters is that there should be no convection in the mantle because there is not enough melt in the mantle. This view is likely incorrect because it is inconsistent with observations, inconsistent with many aspects of our present-day whole-earth knowledge and inconsistent with their own opinions that seem to be incoherent.

In a plain language, mantle convection is a phenomenon of mass in motion (slow creeping) driven by pressure difference (Niu, 2020). In the grand gravitational field in the Earth, the pressure difference is dictated by buoyancy contrast due to density contrast at any given depth. Both compositional difference and temperature difference can create density difference and thus buoyancy contrast, responsible for mantle convection on various scales. Note that such mass-in-motion is vertical (up and/or down) in the Earth's gravitational field, but horizontal motion (e.g., surface plate motion) readily happens as a passive response to vertical motion elsewhere. Thermal convection requires thermal boundary layers (TBLs) across which large temperature contrast exists. Our current understanding is that there are two thermal boundary layers in the Earth. The top cold thermal boundary layer (TCTBL) is the lithospheric plates, which cools the mantle and drives plate tectonics through subduction. This is well established and well understood as one of the key tenets of the plate tectonics theory. Because of the mass conservation and continuity principles, oceanic lithosphere subduction into the deep mantle must be accompanied by massive upflow elsewhere, forming Holmes-type convection ("convection current"). The Holmes-type convection may be too simplistic, but nevertheless, it correctly depicts the thermal convection of the mantle (see Niu, 2018, 2020 for detailed discussion). The basal hot thermal boundary layer (BHTBL) is at the core-mantle boundary (or the D″ region), which cools the core and is responsible for mantle plumes (Davies and Richards, 1992; Davies, 1993, 1999; Bercovici et al., 2000; Niu, 2005a, 2014, 2018). The commenters are my respected mantle plume debaters although they deny mantle plumes sometimes yet advocate mantle plumes in other times as above. Nevertheless, mantle convection through slow creeping is a simple fact as manifested by seafloor plate motion and subduction (observable limbs of convecting mantle), about which I have no doubt; there is no need and no evidence for volumetrically significant melt present anywhere in the deep mantle as constrained by mantle seismic velocity variation (see Section 2.1 of Niu, 2021).

Also, if there were no mantle convection, we would never understand why the Earth is 4.55 Ga old (radiometrically dated with geological support) rather than mush much younger as Lord Kelvin calculated using his conductive thermal model. This is beyond the scope here, but the interested readers can consult the references given here (Harrison, 1987; England et al., 2007a, 2007b).

7. Adiabatic gradient

"Niu *(2021) considers the existence of two super-adiabatic volumes, one in the shallowest Earth and the other close to the contact with the core, with the bulk of the remaining mantle volume having a homogeneous adiabatic gradient*." (Lustrino et al., 2022)

The commenters correctly understand the concept of the adiabat, but I would not use "super-adiabatic volumes", but thermal boundary layers (TBLs) discussed above, the top cold conductive thermal boundary layer (TCTBL), represented by the lithospheric plates, that drives plate tectonics and the basal hot thermal boundary layer (BHTBL) at the core mantle boundary (or D" region) that is responsible for mantle plumes (see above and Niu, 2018, 2020).

In the entire mantle between these two TBLs, there is no heat loss, but the small T decrease with decreasing P (depth) is simply a consequence of volume expansion due to decompression: $[dT/dP]_S = T\alpha/\rho C_P$ by assuming reasonable thermal expansion coefficient (α), density (ρ) and heat capacity (C_P) of the mantle mineralogy (varying with depth and phase changes). So, it is reasonable/convenient to assume a constant dT/dP [adiabat] for the convective upper mantle. The adiabat values estimated for the upper mantle seem to vary to some extent (1.2–1.8 K/kbar) because of the uncertainties of these variables, which are also one of the causes of varying estimated T_{MP} values. I do not think it is productive to criticise the approximation/assumption by the community if we do not have better data to improve the uncertainties.

Because of the likely mantle compositional heterogeneity on all scales due to unknown but possibly inherited primordial heterogeneities or plate tectonics recycling, there could be geochemically enriched heterogeneities with concentrated heat-producing elements (e.g., K, Th, U) such as subducted deep portions of lithosphere of metasomatic origin (e.g., Niu and O'Hara, 2003), there could be dT/dP different from the adiabat or even inverted dT/dP on some large "local" scales in the mantle. The latter is likely to be true, but cannot be quantified without knowing the "space" and "time" of these heterogeneities. We thus cannot use such uncertainties to argue against constant adiabat assumption/approximation by the community, which is not productive and cause unnecessary confusions.

In this context, we should emphasize that some people may consider the 660-km seismic discontinuity (i.e., 600-D; the base of the mantle translon zone), which is the lower-upper mantle boundary, as a thermal boundary layer, but this is unlikely because heat transfer (or thermal "homogenization") across the 660-D is effectively accomplished through "convective" processes as globally evidenced by penetration of many subducting slabs into the lower mantle. Likewise, mass-balance requires the same amount of lower mantle material rising into the upper mantle (see Niu, 2018). Stagnation of slabs in the mantle transition zone above the 660-D in some places may prevent localized mass and heat exchange across these slabs, but they are not permanent features, and they are certainly not heat source, but heat sink (e.g., Niu et al., 2017). Hence, the 660-D is intrinsically not a permanent thermal boundary layer to generate anomalously hot plumes, but the slab dehydration can cause upper mantle melting and intraplate basaltic magmatism such as in eastern continental China since the Mesozoic (e.g., Niu, 2005b, 2014, 2018, 2020; Sun et al., 2021). The latter is a special consequence (far away from surface plate boundaries) of plate tectonics (Niu, 2005b) and cannot be attributed as products resulting from thermal mantle plumes of BHTBL origin.

8. Where is magma generated?

"Niu *(2021) suggests that the lithosphere has no role in producing basaltic magmatism, being too cold, because of conductive cooling …… His Figures 3, 5, 6-10 and 14 assume a topology of a single solidus (with the exclusion of mantle wedge systems)*. (Lustrino et al., 2022)

Niu (2021) says "*Melting cannot happen in the lithosphere as it is under subsolidus conditions* ***(except for volumetrically small metasomatic veins or veinlets of lower solidus temperature)***." (p. 18 of Niu, 2021). This differs from the extreme statement by the commenters and is in fact correct in both concept and reality. The point is [1] if the material in question is placed onto or above the relevant solidus, melting takes place; and [2] the "lithosphere" could melt if it is converted to "asthenosphere" at the time of melting, in which case melting takes place in the newly converted asthenosphere, not lithosphere. The best example is eastern continental China, where the lower portions of the thick cratonic lithosphere prior to the Mesozoic (e.g., Menzies et al., 2007) had been converted to asthenosphere in the Mesozoic because of basal hydration weakening with the water coming from the dehydrating paleo-Pacific slab stagnant in the mantle transition zone, accompanied by melting of the asthenosphere newly transformed from the prior lithosphere (e.g., Niu, 2005b, 2006, 2014; Niu et al., 2015; Sun et al., 2021). Also, because deep portions of the oceanic lithosphere are predicated to consist of enriched dikes and veins of metasomatic origin developed at the LAB (Niu et al., 2002a; Niu and O'Hara, 2003), these dikes and veins with low solidus can undergo melting subsequently caused by localized thermal perturbation associated with OIB volcanism (e.g., Niu, 2008; Humphreys and Niu, 2009; Niu et al., 2012).

"*According to* Niu *(2021), this means that partial melting is never expected beneath over-thickened cratonic lithosphere, even if temperatures are high, e.g., T_{MP} = 1600 °C, assumed by* Niu *(2021) to reflect the arrival of a mantle plume. In his Fig. 14d, the solidus would be encountered at depths of ~5 GPa, and for this reason no melting can occur within cratonic lithosphere*." (Lustrino et al., 2022)

It is correct that no melting takes place for any material at temperatures **below its solidus** (dry or wet). That is, compositionally depleted and physically buoyant cratonic mantle lithosphere is refractory and will not melt unless its physical property can be changed to that of asthenosphere by hydration such as eastern continental China in the Mesozoic (see above and Figs. 12,13 and discussion of Niu, 2014). Again, if there exist low-solidus dikes, veins and veinlets of metasomatic origin in the cratonic mantle lithosphere, they can melt and only do so if they are thermally perturbed to make the temperatures above their own solidi.

9. OIB variability

"*According to* Niu *(2021), the chemical differences among the poorly defined group of OIB are related to the "lid effect" …… This solution does not consider the several decades of isotope geochemistry and petrologic studies …… A single-LID hypothesis cannot explain the variation of $^{87}Sr/^{86}Sr$ from 0.7021-0.7075, $^{143}Nd/^{144}Nd$ from ~ 0.5123–0.5131, and $^{206}Pb/^{204}Pb$ from ~17 to ~22 …… These are not simple isotopic differences, but reflect lithologic heterogeneity as well. These differences must have an effect on the solidus temperature.*" (Lustrino et al., 2022)

Niu (2021) does not discuss radiogenic isotopes although the latter has been the focus of a number of my studies of oceanic basalts (e.g., Niu et al., 1996, 1999, 2002a, 2002b; Niu and O'Hara, 2003; Niu and Hékinian, 2004; Niu, 2009, 2018) and continental basalts with my studnets and collaborators.

It is conceptually important to note, however, that mantle melting is a physical process and melt composition is a consequence of physical response of constituent mineralogy, major elements, minor elements, trace elements and volatiles that determine the physical properties of the source rock and melt. Hence, the *lid effect*, which is the very physical control on F and P_F is recorded in major, minor and trace element compositions of the melts. Radiogenic isotopes (and stable isotopes) do not determine the physical properties of rocks and melts and thus do not show the effects of F and P_F controls. Hence, contrary to the statement by the commenters, radiogenic isotopes do not have effect on solidus, but reflect variations of mantle sources and source histories. This reinforces the understanding that the *lid effect* exerts the primary *physical* control on the extent of melting, depth of melt extraction and compositions of melt expressed by chemical elements (not isotopes) that determine the physical properties of the mantle source rock and melts (basalts). The contents of volatiles (e.g., H_2O and CO_2) that behave like "incompatible elements" are likely more enriched along with incompatible elements in basalts erupted on thicker lithosphere than those erupted on thin lithosphere in both OIB and in basalts from continental interiors although such data are lacking because of degassing during subaerial eruption.

It is important to note that there can be correlated variations of radiogenic isotopes with melting parameters such as the extent of melting when basalts are derived from heterogeneous mantle sources containing ancient enriched lithologies of metasomatic origin (e.g., Niu et al., 1996; Niu et al., 2002a, 2002b; Niu and Hékinian, 2004). For example, we can observe $^{87}Sr/^{86}Sr$ positively and $^{143}Nd/^{144}Nd$ negatively correlate with the abundances and ratios of more-to-less incompatible elements (e.g., La/Sm), not because these isotopes are physically sensitive to the melting process, but because of inherited radiogenic daughter isotope (D: ^{87}Sr, ^{143}Nd) ingrowth from decay of their respective radioactive parent isotopes (P: ^{87}Rb, ^{147}Sm). It is thus the P/D ratio in the source [1] that determines the radiogenic isotope ratios, i.e.,

$^{87}Sr/^{86}Sr \propto Rb/Sr$ and $^{143}Nd/^{144}Nd \propto Sm/Nd$, and that [2] may contribute indirectly to the possible effectc on the solidus (e.g., $La/Sm \propto Rb/Sr \propto 1/[Sm/Nd] \propto$ high volatile contents and high abundances of progressively more incompatible elements in mantle sources). We will not see such correlations [1] if the mantle source of basalts under study is relatively uniform or [2] if the source heterogeneities were developed more recently without enough time for radiogenic ingrowth (Mahoney et al., 1994; Niu and O'Hara, 2003).

10. Olivine-basalt geothermobarometry

"Niu *(2021) questions one of the fundamentals of basaltic petrogenesis,* i.e.*, that it is possible to estimate (albeit very difficult to prove correctness) the depth and temperature of initial partial melting on the basis of FeOtot and MgO of the melt as well as its composition with respect to that of olivine liquidus crystals… … We believe that the memory of* P_O *is not (completely) obliterated by re-equilibration. A number of studies of melt inclusions in olivine (… …) show that, during decompression melting, melt droplets show variable FeOtot but the same MgO at a given* T_{MP}" (Lustrino et al., 2022)

If the correctness of a scientific view is decided by popularity voting, then I will probably receive only one vote from myself. Following my methodology, I do not choose to "believe" or "disbelieve", but choose to carry out a logical, consistent and rigorous analysis on the basis of observations, experimental petrology and understood concepts and principles, which can be reflected in a brief "evolution history" of my own published views. For the purpose of clarity, I elaborate the analysis using numbered points below:

(1) Like many, I would accept the view that erupted basalts recorded the initial depth of decompression melting beneath ocean ridges (e.g., Niu and Batiza, 1991) following the authority idea by Klein and Langmuir (1987) although I had been puzzled because this popularly accepted model interpretation that is based on experimental petrology is in fact inconsistent with experimental petrology data.

(2) Experimental petrology (e.g., Jaques and Green, 1980) shows that for a given fertile mantle composition, MgO and FeO increase whereas SiO_2 decreases in the partial melt with increasing pressure of melting. To apply the experimental results to MORB melts is not straightforward because experimental melts produced in tiny capsules are "primary" magmas without undergoing any cooling-induced fractional crystallization, whereas erupted MORB melts are not primary melts, but variably evolved at crustal level pressures dominated by fractional crystallization (e.g., O'Hara, 1968a, 1968b; Walker et al., 1979; Stolper, 1980; Perfit and Fornari, 1983; Christie and Sinton, 1986). So, it is logical if we could somehow correct the low-pressure fractionation effect so as to compare such corrected MORB melts with experimental melts to gain insights on the pressures of MORB mantle melting.

(3) Because the liquidus temperatures of basaltic melts are near linearly related to MgO contents, Klein and Langmuir (1987) creatively correct MORB FeO for the fractionation effect to constant MgO = 8.0 wt%, thus obtaining a pressure parameter Fe_8, used to infer mantle potential temperature because of the assumption of $Fe_8 \propto P_O \propto T_{MP}$. My earlier research benefited from this creative approach, but the puzzle remained because global MORB Fe_8 varies from 6.5 to 11.5 (~ 5-unit difference), equivalent to MORB Mg# = 56–68 that is far more evolved than Mg# ≥ 72 required to be in equilibrium with mantle olivine with $F_O \geq 90$. Hence, Fe_8 provides no information on P_O and T_{MP} unless the FeO is corrected to Mg# = 72 (e.g., Mg_{72}, Fe_{72}, Si_{72}; see my later publications; Niu and O'Hara, 2008; Niu, 2016, 2021).

(4) Abyssal peridotites (AP) are MORB melting residues (Dick et al., 1984; Dick, 1989). My AP studies indicate that AP are not simple residues, but have excess olivines and incompatible element enrichments as the result of MORB melt cooling and crystallization/refertilization in the advanced residues in the conductive thermal boundary layer beneath the Moho (Niu, 1997, 2004a; Niu and Hékinian, 1997a; Niu et al., 1997). I was eventually convinced that MORB melt evolution also takes place in the mantle as well as in the crust and that the erupted MORB melts cannot in any way remember P_O and T_{MP} in terms of pressure-sensitive elements Fe, Mg and Si, which are the very elements that make up olivine ($[Mg,Fe]_2SiO_4$), the most abundant mantle mineral and earliest liquidus silicate phase of MORB melt evolution.

(5) The (4) above led to Niu et al. (1997) "*Recognizing that [1] porous flow is the primary means of melt transport, [2] the time scale for melt transport from melting region to the crust is of the order of thousands of years (e.g.,* McKenzie, 1984, 1985a, 1985b *;* Spiegelman and McKenzie, 1987 *;* Rubin and Macdougall, 1988, 1990*), and [3] no more than a few tens of hours are sufficient for attending solid-melt equilibrium in peridotite melting experiments requires that low-pressure melt-solid equilibration is inevitable during melt ascent*" (p. 1067 of Niu, 1997).

(6) The "depth" signature may not be erased for olivine-insensitive and incompatible elements, but the signature of olivine ($[Mg,Fe]_2SiO_4$) making elements Mg, Fe and Si, **which are exactly the pressure-sensitive elements** (see (2–4) above), will be entirely erased because olivine is the dominant mineral in the decompression melting mantle and in the conductive thermal boundary layer consisting of advanced melting residues (sampled as AP) atop the mantle through which melt passes towards the crust.

Note again: [1] olivine-making elements (Mg, Fe and Si) are the very pressure sensitive elements; [2] sub-ridge decompression melting (mantle upwelling and pressure decreasing) is largely taking place in the spinel peridotite stability field characterized by the incongruent melting reaction of the form a Cpx + b Opx + c Spinel = 1.0 Melt + d Ol (Niu, 1997, 2004; b > a), in which both olivine and melt are products and there is continued melt-olivine equilibration/re-equilibration; [3] olivine crystallization from the ascending melt through the thermal boundary layer is inevitable as observed (sampled as AP; see (4–5) above). This conclusion is not a personal belief, but is based on observations, experimental petrology, and rigorous analysis (Niu, 1997, 2004a, 2016, 2021).

(7) When MORB FeO is corrected to Mg# = 72 to be in equilibrium mantle olivine Fo_{90}, we can use Fe_{72} to discuss mantle sources and processes. The global MORB Fe_{72} varies in a small range (~ 7.5–8.5; $\Delta Fe_{72} \approx 1.0$ unit; vs. $\Delta Fe_8 \approx 5$ units). The unity range ΔFe_{72} has no significance for T_{MP} but is consistent with fertile mantle compositional variation) (Niu and O'Hara, 2008).

(8) While olivine-basalt based thermobarometry is often not used for MORB, the basic principle is the same as the above on the basis of $K_D = [Fe_{[Ol]}/Fe_{[Melt]}/[Mg_{[Ol]}/Mg_{[Melt]}] \approx 0.3$–$0.35$ as a function of melt composition (e.g., Baker and Stolper, 1994; Matzen et al., 2011; see discussion in Niu, 2016). Nevertheless, melt temperatures are independent of K_D or Mg/Fe, but proportional to MgO contents because varying MgO (or FeO) can give the same K_D (see Herzberg et al., 2007).

(9) Olivine-basalt based thermobarometry is widely used for "mantle plume" magmatism (e.g., ocean island basalts, large igneous provinces on land and in ocean basins and komatiites) and active basaltic magmatism in the continental interiors (Herzberg et al., 2007; Putirka, 2008; Lee et al., 2009). Most researchers would agree that mantle plume melting takes place at depth significantly deeper than sub-ridge mantle melting. The initial depth of plume melting (P_O) is proportional to T_{MP} and is likely varying

between plumes of different T_{MP} as many would choose to agree (e.g., Herzberg et al., 2007).

(10) The significant positive correlations of Mg_{72} and Fe_{72} and negative correlation of Si_{72} in global OIB (Niu et al., 2011; Niu and Green, 2018) and continental interior basalts (Guo et al., 2020; Sun et al., 2020, 2021) with the lithosphere thickness (i.e., the LAB depth) at the time of volcanism demonstrate in simple clarity that the pressure signatures preserved in all these basalts are depth signatures of the LAB. I could choose to "believe" different mantle plumes have different T_{MP} and also wish to "believe" such T_{MP} signatures must be preserved in erupted basalts, but I cannot because we do not see any possible T_{MP} signature in these basalts. What we see is the irrefutable and smoking gun evidence for the lithospheric thickness control (i.e., P_F or LAB depth), which is the very *lid effect*.

(11) All the observations and rigorous analysis in (10) above do not mean that T_{MP} is the same for all plumes, but the erupted basalts do not preserve the T_{MP} signature and thus have no memory of T_{MP} and P_O mostly because of effective and efficient olivine-melt equilibration and re-equilibration in the melting mantle until melting cessation at the LAB, at least for the pressure-sensitive elements Mg, Fe and Si in basaltic melts that are exactly the olivine-making elements. We should note that mantle melting beneath most OIB islands and continents take place in the garnet peridotite stability field characterized by the incongruent melting reaction of the form a Cpx + b Ol + c Gnt = 1.0 Melt + d Opx (Herzberg, 1992), in which olivine contributes to the melt as a reactant, but the concept of melt-olivine equilibration in terms of K_D ($[Fe_{[Ol]}/Fe_{[Melt]}]/[Mg_{[Ol]}/Mg_{[Melt]}]$) applies as (6,8) above.

(12) On the other hand, if we could agree that different mantle plumes are expected to have different T_{MP} and thus different initial melting depths P_O, then we should expect varying extent of melting $F \propto [P_O\text{-}P_F]$ because of the varying P_O. If the olivine-making elements Si, Mg and Fe only record P_F but not P_O due to effective melt-olivine equilibration in the melting mantle as elaborated in (11) above, then the extent of mantle melting ($F \propto [P_O\text{-}P_F]$) recorded by olivine-insensitive elements (e.g., incompatible elements) should preserve the signature of varying P_O. For example, for a given P_F (LAB depth), plumes with higher T_{MP} would begin to melt at deeper P_O, and will melt more with higher $F \propto [P_O\text{-}P_F]$. That is, abundances of trace elements incompatible in olivine would decrease with increasing T_{MP} and P_O. However, both pressure sensitive elements (Mg, Fe and Si) and abundances of olivine-incompatible elements all consistently show only P_F (LAB) signatures, but do not show signatures of P_O or T_{MP} for global OIB and continental interior basalts (Niu et al., 2011; Niu and Green, 2018; Niu, 2021).

(13) The analysis in (12) above raises serious questions: [1] Do all mantle plumes have similar T_{MP} and P_O? [2] Is globally T_{MP} of mantle plumes all similar? Or simply [3] may T_{MP} variation between mantle plumes vary very little and be negligible? It is possible that between-plume T_{MP} does indeed vary (we do not know), but the variation is probably very small, and its effect, if any, is far smaller than the *lid effect* (Niu et al., 2011).

(14) It is necessary to emphasize that many people treat melt inclusions in olivines as representing pristine "primary" magmas. This is a useful way of thinking but is mostly incorrect as it violates well-understood principles of experimental petrology. This is because in most cases the host olivines are phenocrysts with Fo < 90 crystallized in crustal magma chambers with their parental melts having been significantly evolved after leaving the mantle melting region at the LAB depth (P_F). This approach would be meaningful if olivines in the erupted basalts were "xenocrysts" (or "source-crysts") formed at the depth of P_O, but this is unlikely. Mantle melting in the garnet peridotite stability field (e.g., conditions of intra-plate settings beneath continents and seafloor of > 70 Ma), olivine is neither a liquidus phase nor incongruent melting product, but a melting reactant (a Cpx + b Ol + c Gnt = 1.0 Melt + d Opx; Herzberg, 1992). Thus, no P_O-depth olivine forms in the first place, let alone to travel through the melting region, lithospheric mantle, bypass crustal magma chambers and show up in the erupted basalts.

The mantle melt may begin to crystallize at depth close beneath the LAB, where clinopyroxene and garnet (*Not* olivine) are on the liquidus as shown experimentally at $P \geq 3$ GPa (O'Hara and Yoder, 1967; O'Hara, 1968b, 1969; O'Hara et al., 1975) and crystallize in melt layers (or lenses) closely beneath the LAB (Niu, 2008; Niu and Green, 2018) evidenced by their megacrysts. Importantly, these megacrysts only effectively record the conditions of LAB, not any deeper (Sun et al., 2020, 2021, 2022). In such case at the depth of LAB beneath the thickened lithosphere (vs. sub-ridge melting scenario), olivine is not on the liquidus, but co-precipitation of clinopyroxene and garnet in the melt lenses can similarly produce the more evolved residual melt with lowered Mg# (see Sun et al., 2022) that again and further invalidates the olivine-basalt-based thermobarometry using erupted basalts with olivine phenocrysts for P_O and T_{MP}.

To make the point simpler and clearer, mantle melting beneath most intra-plate ocean islands and continents takes place in the garnet peridotite stability field that produces melt while consuming olivine in equilibrium with the melt. The melt "reservoirs" (layers or lenses) close beneath the LAB conditions produce no olivine, but coprecipitate garnet and clinopyroxene. These geological observations and experimental petrology understanding indicate that most olivines in OIB are low-pressure phenocrysts crystallized from variably evolved melts at varying shallow depths (crustal level or shallow lithospheric mantle magma reservoirs) with no information on P_O and T_{MP}.

(15) In summary and to answer the question, "*Do erupted basalts preserve the signature of T_{MP} and Po?*" My humble, objective, and rigorous answer in simple clarity is: "***No, erupted basalts have no memory of P_O and T_{MP}!***" This is not a personal belief, but an objective demonstration and analysis. To repeat, this answer proper does not mean globally constant Po and T_{MP}, but does mean that the data do not preserve and thus do not convincingly show the effects of varying P_O and T_{MP}, if any. I suggest that the community be prepared to objectively answer the questions raised in (13) above and to think about the observations and analysis given in (14) above.

11. The link between MORB chemistry and oceanic spreading rates

"*Contrary to* Niu *(2021), there are no systematic differences between chemical compositions of basalts erupted at ridges spreading at different rates.*" (Lustrino et al., 2022)

This question is a personal belief by the commenters without studying the data.

Using the then available global MORB data, Niu and Hékinian (1997b) showed that the MORB data normalized with respect to spreading rate windows show correlated variations with spreading rate, i.e., Ca_8/Al_8 increases whereas Al_8 and K_8/Ti_8 (the latter was not published in that paper due to print space limit) decrease with increasing spreading rate (the subscript "8" means the same as for Fe_8, but these elements here are olivine-irrelevant and generally pressure-insensitive for sub-ridge mantle melting), which is in simple clarity consistent with "*the extent of sub-ridge mantle melting increases with increasing spreading rate*". This MORB based understanding is independently substantiated by the then available abyssal peridotite (AP; MORB melting residues) data with AP minerals from slow-spreading ridges being much less depleted than those from fast-spreading ridges (Niu and Hékinian, 1997b; Niu, 1997).

In defence of Klein and Langmuir (1987), Langmuir and co-workers had long denied the demonstrations by Niu and Hékinian (1997b), including Gale et al. (2014) that state '*There is no correlation between the chemical parameters and spreading rate*". Despite the thorough and rigorous demonstrations (p. 2093-2095 of Niu, 2016) and reiteration (Niu, 2021), the commenters choose to believe the "authority" assertions without looking at the data and observations. Given the new and more comprehensive global MORB dataset compiled by Gale et al. (2014), Niu (2016) examines these new data and reaffirms the discovery made 18 years earlier that the extent of sub-ridge mantle melting increases with increasing spreading rate (p. 2094 of Niu, 2016). Importantly, Regelous et al. (2016) further reaffirm that the extent of sub-ridge mantle melting increases with increasing spreading rate using newly available large dataset of global abyssal peridotites.

As the commenters choose to believe what Gale et al. (2014) say while choosing to ignore what the data show as specifically detailed in Niu (2016), it is necessary and constructive that we objectively look into the relevant statements and data by Gale et al. (2014). Niu and O'Hara (2008) demonstrate that Fe_8 is petrologically invalid as a parameter to discuss mantle processes unless one uses Fe_{72} (MORB melts corrected to Mg# = 72 to be in equilibrium with mantle olivine Fo_{90}). To defend the parameter Fe_8, Gale et al. (2014) re-corrected the MORB data to Fe_{90}, where subscript "90" refers to olivine Fo_{90}, meaning MORB FeO is corrected to be in equilibrium with mantle olivine of Fo_{90}, which is the same as MORB Fe_{72} (see Niu and O'Hara, 2008). The central idea by Gale et al. (2014) is to convince the reader that Fe_8 is valid because it is exactly the same as Fe_{90} (= Fe_{72}).

Examining the updated new data with corrections by Gale et al. (2014), Niu (2016) discovers that $Fe_8 = 1.0545 \times Fe_{90}–0.9570$ ($R^2 = 0.947$), which is apparently *convincing* because the slope is close to unity ≈ 1.0545 with the intercept being close to zero ≈ − 0.9570 relative to Fe_8 or Fe_{90} in the range of 5–13. However, the claim "$Fe_8 = Fe_{90}$" is petrologically impossible simply because with Fe_{90}, we are dealing with a MORB melt with a single unique Mg# = ~ 72, but with Fe_8, we are dealing with the MORB melt having a range of possible Mg# from ~54 to 72. How is it possible that a single MORB sample with a unique Mg# = ~ 72 in terms of Fe_{90} can simultaneously have any of Mg# values varying from 54 to 72 in terms of Fe_8? That is, it is impossible that a MORB melt with unique FeO and unique MgO can simultaneously have more than one Mg# (=100 x Mg/[Mg + Fe]). Therefore, conclusions based on Fe_8 or their Fe_{90} have no petrological foundation because they are subjective assertions resulting from arbitrary data manipulation with no significance (Niu, 2016, 2021). I suggest that the commenters and interested readers consult Fig. 3 and related discussion by Niu (2016; p. 2085).

The most pertinent question is why the commenters choose to believe what Gale et al. (2014) say, but choose not to examine what the data show? This question may be of general significance for the community and for young readers. Hence, the denial of the spreading rate-controlled extent of sub-ridge mantle melting demonstrated by Niu and Hékinian (1997b) and reaffirmed by Niu (2016) and Regelous et al. (2016) is simply quoted from arbitrary assertions in the recent literature. The commenters also give a long list of the literature going back to 1965. I have no doubt that these papers have had petrological contributions in one way or another, but they serve no purpose for or against "*Spreading rate dependence of the extent of mantle melting beneath ocean ridges*" (Niu and Hékinian, 1997b).

12. Estimating primary magma compositions

"Niu *(2021) does not explain the primary melt calculation method, but presumably assumes olivine as the only liquidus phase.*" (Lustrino et al., 2022)

Niu and co-authors introduced the method over 20 years ago (Niu et al., 1999, 2002a, 2011; Niu and O'Hara, 2008; Humphreys and Niu, 2009; Niu, 2016, 2021; Niu and Green, 2018).

We do not wish to obtain "primary" magmas as doing so would require many assumptions out of control and the resultant "primary magma" compositions would have little significance. We choose not to use the popular olivine addition method because most MORB melts are residual melts after varying extent of fractional crystallization of spinel, olivine, plagioclase and clinopyroxene (plus orthopyroxene, Ti—Fe oxides, apatite, titanite, zircon etc. in the highly evolved felsic melts) (see Niu et al., 2002b; Chen et al., 2019). Despite the large MORB major element compositional variation as a function of Mg#, the general trends are consistent with crustal level magma evolution processes (including magma mixing and melt-rock interaction etc.) dominated by fractional crystallization, i.e., the liquid lines of descent (LLDs). We thus use polynomial regression to express the LLDs and correct MORB samples by back-tracking to Mg# = 72 constrained by the most primitive samples (Niu, 2016; see Fig. 4 in p. 2086). To minimize correction errors, we use MORB samples with MgO > 7.0 wt% (Niu and O'Hara, 2008). Importantly, melts with Mg# ≥72 may not necessarily represent primary melts but are in equilibrium with mantle olivine with Fo ≥ 90. Thus, the corrected melts at Mg# ≥ 72 (i.e., Si_{72}, Ti_{72}, Al_{72}, Fe_{72}, Mg_{72}, Ca_{72}, Na_{72}, K_{72} and P_{72}) can be used to discuss mantle sources and processes such as the extent of melting (F) and final depth of melting (P_F) (Niu et al., 1999, 2002a; also see Guo et al., 2020; Sun et al., 2020).

13. Ridge axial depth, mantle fertility and degree of melting

"Niu *(2021) relates ridge axial depth to source fertility, with Fe- and Al-enriched peridotitic mantle being denser (because richer in iron and in modal pyrope garnet) and less buoyant than more refractory peridotite … … These show that the amount of basaltic melts that can be extracted is function of the fertility of the sources.*" (Lustrino et al., 2022)

Much of the comment under this heading has been effectively addressed above (1−12).

To remind the commenters and to reinform the reader, we need to reiterate here (p. 22 of Niu, 2021): "*MORB compositional systematics as a function of ridge axial depth variation (Fig. 9a) is also a consequence of the lid effect (Figs. 8,9). These correlated variations are very informative that fertile mantle source compositional variation plays a dynamic role (*Niu et al., 2001 *;* Niu and O'Hara, 2008*). The ridge depth variation is the manifestation of sub-ridge mantle density variation because of major element compositional variation, from compositionally depleted and physically buoyant mantle beneath shallow ridges to less depleted (or enriched) and denser mantle beneath deep ridges. Dense fertile mantle beneath deep ridges upwells reluctantly in response to plate separation, which leads to limited extent/amplitude of upwelling, allowing conductive cooling to penetrate to a great depth (P_F), shortening melting interval (P_O-P_F) and melting less relative to the more refractory and buoyant mantle beneath shallow ridges (Fig. 9b). MORB compositional variations in Fig. 9a are consequence of the lid effect and **the source compositional inheritance, both working in the same way.***"

"*It is also worth to emphasize that it is straightforward that the enriched (or less depleted) mantle with higher Fe/Mg (e.g., low Mg# = Mg/[Mg + Fe^{2+}] < 0.89) is readily understood to be denser than the depleted (or less enriched) mantle with slightly higher Mg/Fe (e.g., high Mg# = Mg/[Mg + Fe^{2+}] > 0.90), but the effect of higher Al_2O_3 in the enriched (or less depleted) mantle is far more important because of the formation and stability of garnet, which has the greatest density of all the upper mantle minerals; 1.0 wt. % Al_2O_3 can make up to 5 wt. % garnet (see* Niu et al., 2003 *;* Niu and O'Hara, 2008*) as illustrated in Fig. 9c.*" (p. 9 of Niu, 2021).

14. Komatiites

"… … *Following his model, requiring a correlation between degree of melting and lithospheric thickness under a constant mantle solidus with* *constant T_{MP}, Niu (2021) concludes that komatiites are/were generated under anomalously thin lithosphere. … …*" (Lustrino et al., 2022)

Niu (2021) does not assume a constant solidus with constant T_{MP}. Niu (2021; p. 22) states "*Following all the above, we can add here that komatiite as a result of very high extent of mantle melting (high $F \propto P_O - P_F$) requires **not only deep initial melting (high Po)** but also shallow melting cessation (low P_F) under thin or very thin lithospheric lid although Archean komatiites are often preserved in association with cratonic shields of thick lithosphere. This inference offers an additional perspective on understanding the petrogenesis of yet mysterious komatiites (see* McKenzie, 2020*).*" How could it be possible for the commenters to ascribe "***high P_O***" as "constant mantle solidus with constant T_{MP}"? Clearly, high P_O means deep initial depth of melting due to either low solidus (e.g., wet solidus) or high T_{MP} or both.

"*For example, East Anatolia is thought to be completely lacking a lithospheric mantle, with asthenosphere lying just below the Moho* (e.g., Şengör et al., *2003). However, no komatiites are found there.*" (Lustrino et al., 2022)

This statement is somewhat curious because to our present understanding, the ~60,000 km long globe-encircling ocean ridges have the thinnest lithosphere, where no komatiites are produced. See above, in addition to thin or thinned lithosphere with shallow P_F, deep initial melting (high Po) is required and "*Clearly, high P_O means deep initial depth of melting due to either low solidus (e.g., wet solidus) or high T_{MP} or both.*"

15. Magma volumes

"… … Niu *(2021) makes the powerful point that if mantle rises and if melt is extracted from it, the residuum becomes highly viscous and buoyant, and underplates the lithosphere. It thus thickens the lithosphere – it does not thin it.* Niu *(2021) does not develop this line of reasoning further, to explain large-volume intraplate flood-basalt provinces. Indeed, in the latter part of the paper he suggests that large-volume basalt provinces in the geological record must indicate thin lithosphere, and that there were many more plumes that did not produce any surface volcanism. It is not clear how a hypothesis can be tested that does not feature any potential observables.*" (Lustrino et al., 2022)

The commenters agree with me essentially on all elements under this heading. The so-called hypothesis here is not my hypothesis, but the hotly debated mantle plume hypothesis (see above).

Niu (2021) states "… … *We thus cannot avoid the conclusions: [1] mantle plumes, no matter how hot and how big a plume head may be, cannot melt by decompression to produce LIPs beneath thickened cratonic lithospheric lid … … [5] it follows that **if there are/were** many more mantle plumes and plume heads beneath continents at present and probably also in Earth's history, only those arriving beneath thin or thinned lithosphere could be recognized through LIPs … … [6] Hence, it is the size, thickness and strength of the continental lithosphere that determines whether a mantle plume can surface and whether a mantle plume can break up the continents, not the other way around; [7] Arrival of mantle plume heads beneath coherent continental lithosphere may in fact facilitate cratonization*".

The above is the summary of both demonstrations and rigorous analysis in Niu (2021), and is an insightful contribution about the understanding of continental breakup relevant to the mantle plume debate. As discussed in Section 1 above, my humble view is that the current debate on whether mantle plumes exist or not in the earth offers no prospect for effectively testing the mantle plume hypothesis because interpretations about LIPs or mantle melting anomalies continue to be *reinterpretations of interpretations* without a consensus view on what a mantle plume is, let alone to settle the debate (see Niu, 2005a). A geologically testable hypothesis is needed, such as "*Testing the mantle plume hypothesis: An IODP effort to drill into the Kamchatka-Okhotsk Sea basement*" (Niu et al., 2017), which is geologically executable. This requires that the mantle plume hypothesis being tested assumes that [1] The Hawaiian volcanism that is the classic hotspot (Wilson, 1963) represents a typical mantle hotspot; [2] this hotspot is the surface expression of a deep-rooted thermal mantle plume originated from the lower mantle (Morgan, 1971), probably at the core-mantle thermal boundary layer (CMB or D″) (e.g., Davies, 1999; Campbell and Griffiths, 1990; Sleep, 1992); [3] a large growing mantle plume head is required to carry the material from the deep mantle to the surface according to the Stokes law (e.g., Davies, 1999; Campbell, 2005); [4] the plume head product in ocean basins is an oceanic plateau, which is a lithospheric terrane that is large (1000's km across), thick (>200 km), shallow (2–4 km high above the surrounding seafloors), buoyant (~1% less dense than the surrounding lithosphere – both basaltic crust and the underlain melting residues) (e.g., Campbell, 2005; Niu et al., 2003), and thus must be unsubductable and preserved in the surface geology (Niu et al., 2003, 2017). The Hawaiian volcanism has been considered as the surface expression of a type hot thermal mantle plume, but it does not seem to have a (known) plume head product. If this is true, then the Hawaiian mantle plume in particular and the mantle plume hypothesis in general must be questioned. Therefore, whether there is an oceanic plateau-like product for the Hawaiian volcanism is key to testing the mantle plume hypothesis, and the Kamchatka-Okhotsk Sea basement is the best candidate to find out if it is indeed the Hawaiian mantle plume head product or not (Niu et al., 2003, 2017; Niu, 2004b).

In this context, I agree that debaters on LIPs (mafic to ultramafic magmas $>0.1 \times 10^6$ km^3; frequently $>1.0 \times 10^6$ km^3; Ernst et al., 2021) ought to pay attention to the "volume" issue, not only about the magma volumes, but the volumes of fertile mantle ($V_{FM} = M_{FM}/\rho_{[fertile\ mantle\ density]}$) whose partial melting produces the LIP volumes ($V_M = M_M/\rho_{[melt\ density]}$) in order to contribute to the debate. Given the *lid effect* and the varying decompression melting intervals, the extent of melting for a given V_{FM} is likely highly variable with F = 0–20% (educated guess). Assuming an average extent of melting F ≈ 10% (educated guess) and $\rho_{[melt\ density]}/\rho_{[fertile\ mantle\ density]} = 3.0/3.3 \approx 0.91$, then for an LIP with $V_M \approx 1.0 \times 10^6$ km^3, we are talking about $V_{FM} = 0.91{*}V_M/F \approx 9.1 \times 10^6$ km^3 (~ 10×10^6 km^3). Simply put, a factor of 10 (or more if F < 10%) fertile mantle needs considering when discussing LIP volumes, especially if the LIP is emplaced in a rather short time interval. To place a volume of ~10 million km^3 fertile mantle material onto relevant/appropriate solidi in any short time interval is not a simple matter. If we assume a spheric plume head with a radius of *R* = 500 km (e.g., Campbell, 2007), which has a volume of ~524×10^6 km^3. This volume would be about 50 times larger than the above scenario of $V_{FM} \approx 10 \times 10^6$ km^3, which seems more than adequate to produce an LIP volume of $V_M \approx 1.0 \times 10^6$ km^3. However, when this plume head flattens upon reaching the lithosphere into a disk with radius of *r* = 1000 km (e.g., Campbell, 2007), then this flattened plume head would have thickness of 167 km.

Can such plume head undergo F ≈ 10% melting? The answer is simply *NO*. With realistic effects of lithosphere thickness, solidus and T_{MP} variations all considered, it is possible that only top small portions of this plume head could undergo limited extent of melting because no matter how hot the plume head may be and how low the solidus may be, much of the 167 km thick plume head will be under subsolidus conditions as illustrated in simple clarity in Fig. 14 of Niu, 2021 (also see Niu, 2020). Even if the plume head arrives beneath an ocean ridge, much of the 167 km thick "plume head disk" would likely remain under subsolidus conditions unless it is highly hydrated. One may wish to choose to believe the plume head is made up of low-solidus eclogite, but eclogites are too dense to rise ($\rho_{eclogite}/\rho_{peridotite} \approx 1.06$; ~ 6% denser) at least in the upper mantle conditions (Ringwood and Green, 1966).

I should state here that the above "mantle plume hypothesis" refers to the classic hot thermal mantle plumes originating from the basal hot thermal boundary layer (BHTBL) at the core-mantle boundary (see Section 6 above) and such mantle source materials (also compositionally fertile) would have high T_{MP}. To avoid confusions, mantle melting anomalies originated elsewhere such as the mantle transition zone (MTZ) should not be considered as thermal mantle plumes because the MTZ is not a thermal boundary layer (see Section 6 above), but dehydration of subducted slabs in it may trigger upper mantle melting and intraplate basaltic magmatism (e.g., Niu, 2005a, 2005b, 2006, 2014, 2020; Sun et al., 2021). Without an agreed definition for hotspots (e.g., Wilson, 1963) and mantle plumes (e.g., Morgan, 1971), it has little significance to classify mantle "melting anomalies" (away from plate boundary zones) "as hot hotspots", "warm hotspots" or "cold hotspots" (e.g., Bao et al., 2022) unless these "mantle melting anomalies" are conceptually disconnected from the mantle plume hypothesis that has been under intense debate (see above). Nevertheless, the recent work by Bao et al. (2022) is an interesting exercise.

The above examples and analyses are meant to recommend interested members of the community to conduct logical and rigorous analysis rather than choose to "believe" or "disbelieve". I use the word "assume" or "assuming" quite a few times, of which I am not guilty, but to illustrate in simple clarity some physical scenarios that may be possible/impossible or likely/unlikely. For me, logical and rigorous analysis continues rather than choosing to "believe" or "disbelieve" someone's personal opinions or beliefs.

16. Concluding remarks

I thank the commenters for their commending words and for their affirmation of the paradigm shift from "mantle temperature control" to "lithosphere thickness control" on mantle melting and basalt magmatism. A concise version is published in the *mantleplume.org* website (http://www.mantleplumes.org/LithosphericLid.html).

I agree with the commenters that "a wide range of variables explain igneous activity on Earth and, likely, the other rocky planets" (Lustrino et al., 2022) in addition to the lithospheric *lid effect*. These variables include "bulk- and trace-element chemical heterogeneities, the distribution and speciation of volatiles, and large wavelength variations in temperature caused by radiogenic decay" (Lustrino et al., 2022). In fact, all these have been important aspects of my basalt research over the past 30 years, but such "geochemistry-foccused" studies have not formed a paradigm to have influenced the subject field and to have prevented the community from open-mined and insightful research. The theme of Niu (2021) is not to concentrate on these petrological and geochemical aspects, but to demonstrate the need of a paradigm change, i.e., it is the "*lithospheric lid thickness, not mantle potential temperature, that controls the extent of mantle melting, depth of melt extraction and basalt compositions in all tectonic settings on Earth*".

The "compositions" in discussion here refer to properly averaged major and trace element abundances in basalts to show the first-order systematics of global significance with "local" variabilities being averaged out. My motto has been "Think big, but never overlook every detail", and details form foundations for big problems that I have been endeavoring to deal with. In fact, MORB major element compositional correlation with ridge axial depth is a straightforward consequence of fertile mantle major element compositional variation that imparts the signature of MORB major element compositional variation and also controls the ridge depth because of composition-controlled sub-ridge mantle density variation. Radiogenic isotopes (and stable isotopes) are not the theme to emphasize in Niu (2021) because they do not determine the physical properties of mantle source rocks, and thus record no physical conditions related to extent and pressure of melting. Nevertheless, radiogenic isotopes can provide information on mantle sources and source histories and could be indirectly correlated with major and trace elements in some basalt suites inherited from source heterogeneities of ancient origin (e.g., Niu et al., 2002a).

It is important to note, Niu (2021) neither denies mantle potential temperature (T_{MP}) variation in the earth nor ignores possible effects of T_{MP} variation on initial depth of mantle melting. Niu (2021) demonstrates clearly that erupted basalts do not show T_{MP} effect, i.e., erupted basalts do not correlate with any physical observables that are uniquely and unequivocally indicative of T_{MP} control or T_{MP} variation. However, erupted basalts do show correlated compositional variations with lithosphere thickness (or conductive thermal boundary layers or CTBL) in all settings.

The statement that we do not see T_{MP} effects does not mean that T_{MP} has no effect, but we cannot scientifically justify the claimed *paradigm* that "T_{MP} controls the extent and pressure of mantle melting and basalt compositions on Earth" without unique and unequivocal observational evidence. We can say with observational evidence that "*lithospheric lid thickness variation controls [1] the extent of mantle melting, [2] depth of melt extraction and [3] basalt compositions in ALL tectonic settings on Earth (MORB, OIB, VAB, CIB).*" (Niu (2021), which represents the *New* and *Unifying* understanding on basalt petrogenesis in the context of global tectonics.

The commenters are a group of my highly regarded leading scientists of the international community with highly acclaimed geophysical, petrological and geochemical experiences and expertise. Their misunderstanding (also misattributions) on the above issues may very well reflect similar misunderstanding by the international community. In this case, to repeat the same demonstrations (e.g., Niu and O'Hara, 2008; Niu et al., 2011; Niu, 1997, 2004, 2016, 2021; Niu and Green, 2018) may not satisfy some or many who are used to choosing to "believe" or "disbelieve". So, it should be more productive and informative by sharing my scientific approach in an email response to a comment (November 25, 2017):

"*Recently, I had some exchanges with a friend, who is an influential and preeminent scientist in my field, whom I respect and admire although we differ in views on certain scientific issues. This friend said something like 'Some of your work is getting a great deal of attention, though I am sure that it must also be controversial.' I take this as a compliment although this statement is incorrect! The correct statement is this: I choose to work on 'controversial' problems or choose to work on important problems about which the prevailing views or standard models are 'controversial' (i.e., they are in error, contradict observations, and thus need correction). The reason why I choose to write what I write is because I choose not to write papers that agree with standard models or with previous work by others – doing that I would be copying or repeating without making new contributions. As a geoscientist, I am committed to make original contributions with insights (e.g., make new discoveries, offer logical solutions to unsolved problems, identify flaws in popular models and make efforts to do correctly etc.). My contributions are ensured to be consistent with geological, geophysical, petrological and geochemical observations as well as chemical and physical concepts and principles. Reading the histories of scientific developments, I know it will take time before much of the community begins to happily accept that the results of my original contributions are in fact all correct! Of course, this time will come sooner if the reader chooses to think objectively with an open mind rather than accepting standard or prevailing models as facts without thinking. I also have the view that scientific research is a self-correcting process, and we must be willing to change or revise when new data available require change and revision. It is such attitude and scientific approach that have truly advanced our science and technology as lucidly written in the book 'A short history of nearly everything' by Bill Bryson.*"

Declaration of Competing Interest

The author declares no conflict of interest nor competing financial interests.

Acknowledgments

I thank Professor Yan Wang for editorial handling of this response and thank Michele Lustrino, Gillian Foulger, Malcolm Hole and James Natland for their commentary. I consider the latter to be important because it may represent a general query of the scientific community, to which this reply is essential and informative in terms of both content and style. My research themes and topics are varied, but mantle melting and basalt petrogenesis in the context of global tectonics is one of my primary research efforts. My research career over the years has benefited from interactions with many people, including colleagues, friends, students, journal reviewers and editors. My research career developments have been highly influenced by, in chronological order, Rodey Batiza (my PhD supervisor), Charlie Langmuir, Henry Dick, Roger Hékinian, late Shen-su Sun, Tony Ewart, David Green, late Michael O'Hara, Peter Wyllie and Marge Wilson, whose encouragements and debate, agreeing or not, have trained me running on the road towards a critical observer, an independent thinker and an original and rigorous scientific contributor. Scientific debate, while sailing on research cruises, with Jamie Allen, Wolfgang Bach, Rodey Batiza, Pat Castillo, Henry Dick, Bob Duncan, Godfrey Fitton, David Graham, Roger Hékinian, Jill Karsten, Jim Natland, Andy Saunders and many others has been enjoyable. *E*-mail exchanges with late Don Anderson, Richard Arculus, Ian Campbell, Pat Castillo, Tim Elliott, Gillian Foulger, Jim Gill, Bill Griffin, Bill McDonough, Dan McKenzie, Jim Natland, Sue O'Reilly, Roberta Rudnick, Bob Stern and others have been both stimulating and entertaining. I confess that my papers must have annoyed many (I was privately told so many times), but I am grateful to all those mentioned above and many others, including those being annoyed, who have elevated my scientific curiosity to this day, and the curiosity continues. My research has been financially supported, over the years, by US NSF, Australian ARC, UK NERC, Chinese NSF, The Royal Society, The Leverhulme Trust, China Geological Survey, Chinese Academy of Sciences, The University of Queensland, Cardiff University, University of Houston, Lanzhou University, China University of Geosciences in Beijing and Durham University. This contribution is supported by grants from NSFC (91958215, 41630968), NSFC-Shandong Joint Fund for Marine Science Research Centers (U1606401) and 111 Project (B18048).

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