

I have read the original paper by He et al. (2003) in EPSL; the re-evaluation in Nature Geoscience by Ukstins Peate and Bryan (2008); the draft Comment by He et al.; and the draft reply by Ukstins Peate and Bryan.

The key stratigraphic observations (He et al., 2003) are 1) that the middle Permian Maokou Formation displays marked thickness variations in the vicinity of the Emeishan igneous province of southern China; 2) that isopachs for the Maokou display a roughly elliptical pattern; and 3) that radial thickening of the Maokou is accompanied by younging of preserved strata. He et al. (2003) take these data to imply doming and differential erosion following deposition. This interpretation is supported by the presence at the base of the overlying Emeishan basalts of up to 600 m of conglomerate overlying a karstic erosion surface (unconformity) with as much as 230 m of relief, and more typically, up to 50 m. Development of the unconformity is attributed to in excess of 900 m of plume-induced uplift and exhumation. This figure is based upon an estimate of the thickness of eroded carbonate at the center of the inferred dome (at least 300 m) combined with the thickness of overlying conglomerate at the periphery of the dome. The conglomerate is interpreted as alluvial fans covering an area of 400 km by as much as 70 km. The duration of uplift, based on biostratigraphic constraints, is said to be less than 2.5 m.y. The radius of the inferred dome is 800 km.

Ukstins Peate and Bryan (2008) argue that conglomerate taken by He et al. (2003) to represent alluvial fan deposits are of hydromagmatic origin, consistent with emplacement close to sea level, and hence not with the concept of plume-generated uplift. Thickness variations in the Maokou Formation are attributed to syn-depositional normal faulting. Comparable thickness trends are observed within the Emeishan basalts. The uneven contact between these units may reflect depositional topography associated with carbonate accumulation, at least in part.

Review of Comment by He et al.

He et al. claim that domal uplift is an “unambiguous prediction of the mantle plume hypothesis.” I agree with Ukstins Peate and Bryan (UP&B): uplift is an expectation of a particular model that is not borne out by field data from numerous other large igneous provinces. The main difficulty for He et al. is that they do not adequately deal with alternative interpretations. UP&B are correct in suggesting that thickness and age data for the Maokou Formation are explicable also in terms of growth and progradation, modified by relatively minor erosion. That growth may or may not be fault-related. The existence of an unconformity is not sufficient evidence for inferring kilometer-scale uplift, particularly given that the interpreted erosional relief is a) modest, and b) not adequately documented. He et al. provide no documentation of carbonate facies in the Maokou. We are led to understand that the Maokou displays sheet-like geometry, but if a platform edge is involved, that may not be the case at all. The top-Maokou unconformity is said to be symmetrical. This is not borne out by the data presented in He et al. (2003), which are way too sparse to be sure of actual stratigraphic trends. The best way to document physical stratigraphic relations is not with a series of isolated measured sections but with a map. Some, and perhaps many, unconformities involve subaerial exposure and erosion.

However, that isn't the only mechanism. Marine bypass also results in offlap geometrically similar to the stratigraphic pattern evident in the Maokou. The key to making such distinctions, and it should be relatively straightforward, relates to stratigraphic details not available in the sources I have read for this review. It is also not the case that all subaerial unconformities result from uplift. While I do not necessarily agree, many of my stratigraphic colleagues would argue that sea-level change is the primary driver. I also have a hard time accepting kilometer-scale uplift, erosion and subsidence back to sea level within a span of less than 3 m.y. He et al. indicate that evidence for hydromagmatism is spatially limited (a single locality), with evidence also for subaerial eruption. UP&B indicate that hydromagmatic features have a strike length of ~400 km, hardly that limited. The argument of He et al. that eruption into a lake would account for many of the features documented by UP&B is inconsistent with the presence of marine fossils.

Review of Reply by Ukstins Peate and Bryan

UP&B present compelling evidence for hydromagmatic activity. I appreciate that depositional complexities (reefs, etc.) may be present in the Maokou, and that they're potentially important. However, to be fair, UP&B provide no evidence in support of such an interpretation. The clastic features described in the Emeishan basalts do not strike me as remotely like an alluvial fan (the interpretation of He et al.). The asserted scale of alluvial fan deposits is also inconsistent with virtually everything we know about fans. I agree that it is important in ancient carbonate rocks to document that karst features date from the time of deposition, and not to more modern weathering. I accept statements favoring a significant role for Himalayan deformation. Indeed, if the deformation is as strong as UP&B imply, I would be concerned about the significance of measured stratigraphic thicknesses (the data of He et al.). It is incumbent on He et al. to show, based on appropriate mapping, that thicknesses do not include structural repetitions. A minor point on paragraph one: While I accept the argument that large-scale uplift is not after all a common feature of plumes, it is not true that there are no other asserted examples. Williams and Gostin postulated such an origin for the kilometer-scale Wonoka canyons in the Neoproterozoic of South Australia. I think that better explanations are available, but Williams and Gostin might be acknowledged in this context (*Journal of the Geological Society of London*, v. 157, p. 759-768; *Journal of Geological Society of London*, v. 158, p. 573-576).

Recommendation

Based on the information available to me, I think that UP&B's (2008) reinterpretation of plume-induced uplift in the Emeishan LIP is well founded, and publication in *Nature Geoscience* justified by the importance of this example in plume literature. With respect to numbered questions:

1) Is the criticism valid? Mostly No. However, I would justify publication on the basis of interest in this example.

2) Is it likely to be of general interest? Yes.

3) Would you say that the Reply is persuasive? Mostly Yes.

4) Could it be made more concisely? If anything, it would be better to deal with additional issues for which data may not be available. So No.

If the Comment is accepted for publication, it would be appropriate to publish the Reply also.

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