Solid Earth
Discussions

[SED]

Interactive
comment

Interactive comment on "Tempo-spatial variation of the late Mesozoic volcanism in Southeast China testing the western Paleo-Pacific Plate subduction models" *by* Xianghui Li et al.

Anonymous Referee #1

This article reports 48 new and robust U-Pb age dates for Mesozoic volcanic rocks from South China. I highly support its publication in a highly modified form.

While the data are plentiful and important, the interpretation of the data is presented in a non-scientific fashion, so the logic behind the conclusions and the importance of the data are lost. The combined dataset shows a spectrum of dates that range mostly from ca. 150-160 Ma to 90 Ma with one well defined peak at 132.9 Ma and another subordinate peak at 99.9 Ma. A bimodal distribution in ages seems robust, but this simple message is lost by (1) trying to divide South China into sub-blocks and examining age distributions independently by block, (2) too finely discussing potential

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age-modes (up to five), and (3) over-interpreting the spatial and temporal distribution of the ages in a tectonic reference frame.

(1) Splitting South China into two tectonic terrains, SHTB and CZ, for a Mesozoic problem is highly questionable. There is no difference in Nd model ages between the two, suggesting their basement histories are identical. If there is a difference, then it was inherited long prior to the Mesozoic, so the designation has little meaning for the present study. Like the authors show themselves in Figs 2a and 2b, there is no difference in the general age spectra between the two. I recommend taking this out completely, otherwise you need to justify it.

(2) I cannot understand why the ages were separated into five discrete populations. Is there any statistical reason for this? If you insist on so finely interpreting the data, then you need to have statistical grounds to defend it. If one had five dates that were distinct by 1 Ma each, could this be used to argue for five separate age populations? How much data are needed before designating that a single age population has meaning? What seems robust to me is that when enough (N = ?) data are acquired, one very robust peak appears at ca. 133 Ma and a secondary one at ca. 100 Ma. How much is this conclusion biased by arbitrary (geologically speaking) sample selection? What if you obtained 48 new dates from other basins? Applying more rigorous statistical methods would significantly improve the paper as a scientific contribution.

Since southeast China had a basin and range-type setting in the Mesozoic, it seems likely the basins would preserve extrusive volcanic rocks, whereas these rocks would be eroded at the horsts (positive topography) to reveal the subterranean feeders (magma chambers) that fed the extrusive rocks. If true, can one also use the dates from the plutonic rocks to get an even better idea of the age distribution of volcanism + plutonism in South China? It would be very interesting if you could compare the two (volcanic vs. plutonic rocks) age distributions.

I highly recommend that the authors examine the data, besides just the ages, (e.g.,

C2

U and Pb concentrations, etc.) in more detail. Are there any trends in these variable trough time/place?

(3) Figures 5, 6 and 7, together with the text in sections 5.2 and 5.3, should be omitted. It completely detracts from the important message of the paper and has no justification.

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