Forum Reply

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Subsiding Sundaland

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We appreciate the Comment of Parham (2019) regarding the geological evidence of subsidence of the Sunda shelf, Southeast Asia. This Comment essentially supports our analysis (Sarr et al., 2019) by reviewing recent observations by himself (Parham, 2016) and some earlier studies, and regretfully posits that we apparently ignored some of this literature. We thank Parham for pointing at those articles, which we acknowledge provide seminal data that are useful for a broad conceptualization. We were aware of some, and ignored others. Parham's Comment sparked a few reflections on our selection.

The first one is commonplace: the main reason for not citing those is certainly space constraints. Scores of publications are relevant and we had to make choices. Yet, our selection is not arbitrary: we retained articles that either give quantitative estimates of vertical land motion, or were relevant at the global scale. The second reflection only merely enlarges the focus: we advocate that Parham's expectations cannot be matched by the current means of dissemination of scientific results, which favor conciseness and sharpness, before comprehensive and accurate descriptions, as those of Parham (2016). This is even reinforced by the fact that a large range of these articles are difficult to reach, either because they are ancient or because they do not belong to the list of mainstream journals commonly available in academic libraries.

The following remarks are based on more scientific grounds. We first note that our analysis is not redundant with Parham's and the aforementioned literature: we contest the statement that we "reached similar conclusions", because our study is quantitative, unlike the qualitative analysis of Parham; this shall not be regarded as a semantic argument, as we explain in the following. It is certainly true that our analysis is based on the same first-order geological observations that in our article (Sarr et al., 2019), we take back to Evans et al. (1936; not cited by Parham, 2016; 2019). However, the severe limitations that such analyses invariably bear call for different methodologies.

For example, Parham (2019) hypothesized that "if the Sundaland were tectonically stable during the late Cenozoic, there should be emergent evidence of higher-than-PMSL sea-level highstands that occurred during that time, particularly the last interglacial sea-level highstand (ca. 120 ka)". This reasoning is appealing, and it is specifically this observation that also prompted our research (see our figure 1). However, sea-level curves are the nemesis of coastal geomorphologists, and often jeopardize their interpretations, in particular when it comes to estimating the elevation of local sea-level highstands. Several reasons explain the uncertainties; for example, the fact that geochemical proxies yield uncertain estimates of past sea levels, or that changes in absolute sea level are spatially variable at each glacial cycle (e.g., Spada, 2017). Only for the last interglacial, de Gelder et al. (2018) reported published estimates ranging from −16 to +13 m above current sea level, with a standard deviation of 8 m, and the most conservative estimates leave room for the possibility that sea level was never higher than present-day during the past 2.5 m.y. Of course, commonly accepted curves do not predict such a range of uncertainty. Such a possibility is unlikely and, along with Parham, we do not favor this interpretation. However, it points to an unescapable subjectivity, if the reasoning is only reliant on the absence of emerged coastal landforms: while it may reveal the absence of uplift, it by no means warrants subsidence.

Sea-level curves are, in principle, more insightful in uplifting regions, for multiple fingerprints (typically, in Southeast Asia, flights of uplifted coral reefs) are often present, at a range of elevations, and can be dated. But even these more-favorable settings are often difficult to interpret in tropical environments, because of the uncertainty of sea-level curves (e.g., Pedoja et al., 2018), but also from the complexity inherent to reef constructions (e.g., Husson et al., 2018). These complexities become even more striking when attempting to interpret the geological record in subsiding or submerged regions, in the absence of radiometric dates (^{14}C, U/Th), offshore morphologies, and quantitative (numerical) models.

Other specificities distinguish our work from the previous studies mentioned by Parham (2019). An important point is that we focus on the core of Sundaland, while all others focus on Peninsular Malaysia, (besides Parham [2016] who expanded his investigations to the Malaysian part of Borneo). In addition, a large share of the descriptions is devoted to the deforming parts of Malaysia, and not as much on the tectonically quiet coastlines of Sundaland. Amongst the early literature, which compares to that mentioned by Parham, while being more focused on Sundaland, we instead point to the articles of Aleva et al. (1973) and Ben Avraham and Emery (1973), that were inspiring to Sarr et al. (2019).

These remarks are meant to explain why we could not refer to all available literature, and to contest any form of redundancy between the work presented in Sarr et al. (2019) and previous studies. We nevertheless reiterate that these studies are extremely valuable, and influenced our reasoning, and that we are grateful to Peter Parham for his Comment.

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