The paradox of tropical karst morphology in the coral reefs of the arid Middle East: REPLY

REPLY: doi: 10.1130/G31454Y.1

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We appreciate the Comment of Shinn and Purser (2010) to our paper and value the opportunity to expand the discussion about type-2 structures in the Middle East. In their Comment, Shinn and Purser state that work conducted more than four decades ago “examined and explained” type-2 polygonal structures in the Arabian Gulf. However, the topic of creation of seafloor relief was not central to the quoted studies; Purser and Evans (1973) dedicate but a single sentence to polygonal fractures in submarine pavements, with a similarly cursory mention of a couple of sentences in Shinn (1969). Kendall and Skipwith (1969) do not cover polygonal buckling in marine hardgrounds at all. The point being that such fleeting mention is not tantamount to a defensible explanation of type-2 features. Our work (Purkis et al., 2010) tackles this discrepancy.

While our view on the formation of type-2 structures differs from Shinn and Purser, we do not dispute that submarine hardground acts as a locus for coral settlement. Nor do we contest that fractures in the hardgrounds extending between Bahrain and Qatar may be polygonal, though our experience in the southeastern Gulf is that they are linear. We also recognize that the terminology we used, specifically the term polygonal, may be inadequate to describe the complex maze of reticulated, anatomi- zing sills surrounding sediment-filled ponds that we term as type-2. As Shinn and Purser’s Comment shows, the term polygonal apparently raises associations with fairly equilateral shapes such as encountered in the supratidal sabkhas of the Gulf. While the analogy is tempting, we will show this to be an inadequate model.

We do not solely concentrate on the Arabian Gulf, but also consider seafloor structure across 25,000 km² of the Red Sea. While the Comment of Shinn and Purser pertains to the Gulf, this second arm of the study, when considered in unison with the Gulf observations, shows the explanation proposed in the Comment to be unsatisfactory. It is important to recognize that the Gulf is exceptional in the expansive formation of submarine hardground pavements; its waters are uniquely rich in the minerals that enable rapid lithification of grainstones through abiotic precipita- tion of carbonate cements. Unlike the Gulf, the Red Sea does not boast the necessary supersaturated seawater mineralogy to promote substantial hardground formation. Despite this disparity, the type-2 reef structures recognized in the Red Sea, which are widespread, are statistically inseparable to those in the Gulf (Purkis et al., 2010, our figure 3). If, as Shinn and Purser suggest, the type-2 structures are related to rapid submarine lithification and compressional fracturing, the accordance in patterning between the Gulf and the Red Sea cannot be expected. Furthermore, comparable reef formations to type-2 have also been reported in other tropical settings, which, like the Red Sea, also do not possess water chemistry conducive to widespread hardground formation. In our manuscript, this point is highlighted with reference to papers from Belize, Kirimitami, Pearl and Hermes Atoll, Cocos, Tuamotu, the Maldives, and the Great Barrier Reef. In addition, the fact that reticulated topography can form via karst has been adequately demonstrated by Purdy (1974).

Further plausible methods for the formation of type-2 patterning come in the form of growth structures (Wyrwoll et al., 2006). However, as for the folded-hardground hypothesis of Shinn and Purser, if such causative mechanisms were at play for the features we describe, the strong relationship between depth distribution and sea level would not be anticipated. As reported by Shinn (1969), Gulf hardgrounds form in depths down to 30 m, well within the range of coral growth. type-2 patterning is limited, however, to a maximum depth of 10 m (Purkis et al., 2010, our figure 4). As per Purdy (1974), the depth distribution of reticulated structures we describe in the Gulf and Red Sea are in concert with solution pockets formed during an interval of lower sea level. These features are not the same as those discussed by Shinn and Purser and cannot satisfactorily be described to arise from fractured hardgrounds. The structure we detail is built to sea level and, unlike the overthrust-teepees pictured in Shinn (1969), has vertical relief of several meters. Furthermore, even in the necessarily simplified world of computer simulation, we are successful in emulating the patterning recorded in the satellite imagery using a model of karst denudation. In the book to which Shinn and Purser so frequently refer, the chapter by Kassler (1973) makes reference to the bedrock below the Holocene of the Gulf as being extensively leached and altered by fresh water, evidence the author takes to be “probably indicative of sub-aerial exposure” (p. 24)—an observation very much aligned with our karst hypothesis.

We continue to assert that the link between reticulated reefs and underlying karst is particularly strong given the depth limit of the phenomenon and the direct connection to sea level and well-constrained perturbations to the Middle East climate in the early Holocene. We therefore concur with Shinn and Purser in their assessment of the importance of hardground expansion for the formation of teepees; however, there clearly exists a genetically different type of polygonal structure on the shallow seafloors of the Middle East. It is this that we have termed as type-2.

REFERENCES CITED


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