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# REPLY

## Rejoinder to: Hearty, P.J. and Kindler, P., 1994. Reply—Straw Men, Glass Houses, Apples and Oranges: A Response to Carew and Mylroie's Comment on Hearty and Kindler (1993). *Journal of Coastal Research,* 10(4), 1095–1105.

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This rejoinder is written to address a few of the issues raised in HEARTY and KINDLER's (1994) reply to our discussion (CAREW and MYLROIE, 1994) of their 1993 paper on the geology of San Salvador Island, Bahamas (HEARTY and KINDLER, 1993). We stand by our comments (CAREW and MYLROIE, 1994) on HEARTY and KINDLER (1993), and we do not wish to engage in a lengthy and point by point rebuttal of their reply (HEARTY and KINDLER, 1994). However, there are several issues raised and questions posed by them (HEARTY and KINDLER, 1994), that we feel compelled to address.

(1) HEARTY and KINDLER (1994, p. 1,098) comment on our blind AAR study of *Cerion* (MIRECKI *et al.*, 1993). They say,

> "Their purpose was to discredit the whole-rock racemization results in HEARTY and KINDLER (1993a), . . . ."

Our publication (MIRECKI *et al.*, 1993, p. 97) states,

> "In an attempt to determine the utility of aminostratigraphy in these carbonate rocks, we undertook a blind study using the pulmonate gastropod *Cerion*. The goals of this study were to estimate the precision of *Cerion* shell amino acid data produced by different geochemists, and to determine if aminostratigraphy can be used reliably to understand sea-level data preserved in the geologic record of San Salvador Island."

That study was blind only in the sense that the geochemist (Mirecki) doing the AAR analyses did not know the ages or locales of the specimens being tested; we (Carew and Mylroie), or course, did. Our purpose was to test the results (analyses by Wehmiller) we ourselves had published because we had come to suspect the reliability of those data. However, in light of the problems encountered utilizing shells of a single taxon, we do at this time also question the utility of other AAR data from the Bahamas.

(2) Later on page 1,098, and earlier in their reply, HEARTY and KINDLER (1994) suggest that we obtained erroneous results in our AAR studies of *Cerion* because we sampled improperly and conducted less than rigorous AAR analyses. We invited Hearty to San Salvador, and we provided him with an introduction to the geology of San Salvador by taking him in the field and showing him significant outcrops. We had ample opportunity to observe his field methods and his collection procedures, and we found no significant differences between his and our procedures. HEARTY and KINDLER (1994) provide the following statement concerning the need for appropriate procedures.

> "These extensive AAR investigations have resulted in several guidelines that must be scrupulously adhered to in order to yield consistent ratios (HEARTY, 1986; HEARTY *et al.*, 1986; WEHMILLER *et al.*, 1988; . . .)" (HEARTY and KINDLER, 1994, p. 1,095).

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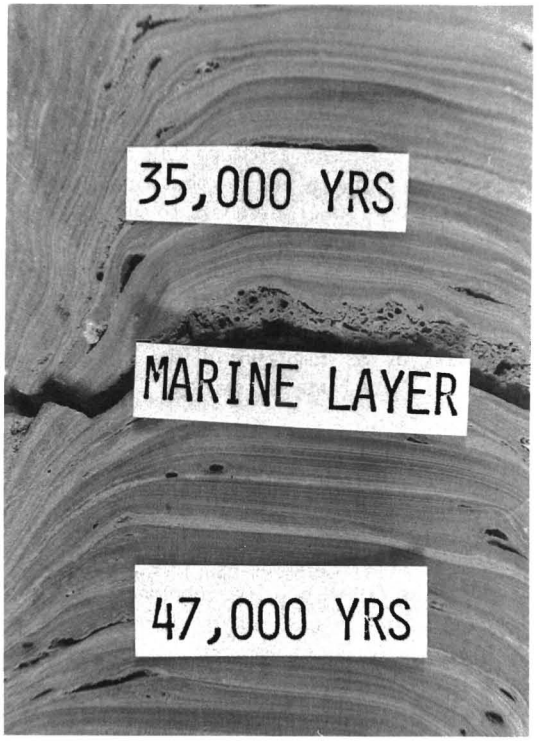


Figure 1. Reprint of photograph of a stalagmite from Lighthouse Cave on San Salvador Island, Bahamas that appeared as Figure 4 in CAREW (1983, p. 167). Note that the lower boundary of the serpulid (marine layer) is covered by the "marine layer" label. The calcite growth laminae immediately above the top of the serpulid layer follow the topography of that encrustation, thereby indicating that the serpulid layer predates the second stage of growth of this stalagmite. The stalagmite cracked when it was sawed in half.

*Note*: The full list of authors for the citation of WEHMILLER *et al.*, 1988 includes Wehmiller, Belknap, Boutin, Mirecki, Rahaim, and York (see literature cited). Thus, it appears that Hearty and Kindler recognize both of our colleagues (Wehmiller and Mirecki), who have conducted all of the amino acid analyses on which we have published, as appropriate AAR experts.

(3) We are encouraged by the new data reported in HEARTY and KINDLER (1994, p. 1,097, and Tables 2a and 2b) that seem to indicate that by using just the thick apertural lip of adult *Cerion* consistent and reliable AAR results may be obtained. We are currently conducting a study using only *Cerion* apertural lip. If that investigation supports HEARTY and KINDLER's (1994) findings we will be extremely pleased, for it has always been our stated intention to continue research on *Cerion* in hopes of obtaining a reliable chronological tool, as can be seen from the quote below.

> "The results of this study call into question, until further research identifies the source of error, or a way to correct for it, the ages of Bahamian eolianites derived from amino acid analyses of *Cerion*. We intend to continue to investigate this problem with the goal of developing a methodology that is demonstrably reliable." (MIRECKI *et al.*, 1993, p. 100).

(4) HEARTY and KINDLER (1994) ask a question about a stalagmite containing an *in situ* serpulid worm tube layer, on which we have published,

> "Photos and our observations of the speleothem show that the serpulids grow in a crack that cross-cuts time lines (in CAREW, 1983, p. 167). Could not the growth of (*sic*) the marine encrustations have occurred in a 'fresh' crack in the stalagmite during Recent inundation?" (HEARTY and KINDLER, 1994, p. 1,101)

*Answer*: Figure 1 is a reprint of that figure as it appeared in the 1983 publication (CAREW, 1983, p. 167) cited by HEARTY and KINDLER (1994, p. 1,101). As is readily seen, the layers of calcite deposited on top of the serpulid layer follow the topography of the serpulid encrustation, and it is not until several growth layers later that the topography is smoothed out. Clearly the serpulid layer predates the second growth phase of the stalagmite. Furthermore, the stalagmite was collected whole, that is, uncracked. The stalagmite was cracked, along the surface of weakness caused by the serpulid layer, when it was sawed in half.

(5) In their discussion of our assignment of an age to the deposits of Dixon Hill on San Salvador Island, Hearty and Kindler imply that we did not publish an earlier (before AAR analyses) interpretation. They state,

> ". . . to our knowledge, there is no published 'earlier interpretation' of Dixon Hill." (HEARTY and KINDLER, 1994, p. 1,101)

There is such a publication, and they cite it in the lower right column on page 1,100 (HEARTY and KINDLER, 1994). In that publication (CAREW *et al.*, 1982) we said, as they correctly quoted, that Lighthouse Cave (which

is within Dixon Hill) must be older than 47,000 years (because the stalagmite seen in Figure 1 had begun to grow in the air-filled cave by then). We also said,

> "Cave systems which would have been adjusted to that higher sea level have been found on San Salvador, Andros, New Providence and other Bahamian islands. Lighthouse Cave on San Salvador is perhaps the best example, and it is reasonably certain that its minimum age is thus 125,000 years; but it may have formed at an even earlier high sea level position." (CAREW *et al.*, 1982, p. 12)

The deposits of Dixon Hill, which contains Lighthouse Cave, clearly must be that age or older.

(6) HEARTY and KINDLER (1994, p. 1,101) state,

> "1991 (Spring): Draft of Hearty and Kindler SSI manuscript sent to C&M for review. No review offered to date."

Both Carew and Mylroie separately provided extensive reviews and editing to an earlier version of Hearty and Kindler's manuscript. Those reviewed manuscripts were delivered in person to Hearty at the 1991 GSA Annual Meeting in San Diego. We did not receive or review any later manuscript by Hearty and Kindler.

(7) HEARTY and KINDLER (1994, p. 1,102) question our petrologic acumen. They say,

> "C&M have not demonstrated their proficient use of petrology. Suffice it to say, they once stated that the middle Holocene North Point member (*sic*), 'revealed a preponderance of skeletal material' (HUTTO and CAREW, 1984, p. 201). Observers can easily recognize that this unit essentially contains ooids and peloids (*e.g.* WHITE and WHITE, 1990 [*sic*])."

First, Hutto and Carew's 1984 publication represents the earliest report on the petrology of the eolianites on San Salvador, and it was the result of work largely done by Hutto, then a Senior undergraduate student, and supervised by Carew. Second, a complete quote of what Hearty and Kindler only partially quoted is as follows.

> "Samples from many offshore cays, North Point, and modern dunes reveal a preponderance of skeletal material. Although ooids are present in amounts varying to as much as 50% of a sample, they are neither as generally well developed nor in as great an abundance as found in most rocks of the interior of the island." (HUTTO and CAREW, 1984, p. 201)

Note that our comment was not about the North Point *Member* rocks (our italic for emphasis), nor exclusively about the rocks of North Point (a geographic locale on San Salvador Island). In fact, at that time (HUTTO and CAREW, 1984) there was no such thing as the North Point Member. It was not until 1985 that we developed our island stratigraphy and created the North Point Member (CAREW and MYLROIE, 1985). In that publication, in the discussion of the rocks of the North Point Member, we said,

> "Petrographically these rocks are a variety of pelsparites." (CAREW and MYLROIE, 1985, p. 21).

Third, the following quote from WHITE and WHITE (1991), made note of by HEARTY and KINDLER (1994, p. 1,102; incorrectly cited as WHITE and WHITE, 1990), shows that their 1991 study confirmed our earlier analyses (HUTTO and CAREW, 1984; CAREW and MYLROIE, 1985).

> "The data show that the Holocene rocks all have very similar compositions. Peloids are dominant, occurring usually as 70 to 80% of the grains, but ranging from 50 to 100%. Ooids are the second most abundant grain type. They usually make up 20 to 30% of the grains, but in some case (*sic*) may be 40 to 50% of the grains. Bioclasts are not always present in the Holocene rocks." (WHITE and WHITE, 1991, p. 244)

(8) HEARTY and KINDLER (1994, p. 1,102) state,

> "In the Bahamas, phreatic caves never occur within Holocene and Sangamonian rocks."

There are no enterable phreatic caves in Holocene rock, because they are at this time developing in the subsurface; but there are phreatic caves in Sangamonian rocks on San Salvador (*e.g.*, Altar Cave and Chinese Fire Drill Cave, VOGEL *et al.*, 1990), and on other islands in the Bahamas (*e.g.*, South Andros Island, CAREW and MYLROIE, 1989).

(9) We note that HEARTY and KINDLER (1994) had no comment concerning the errors that we pointed out in their map (CAREW and MYLROIE, 1994).

In conclusion, we reiterate that we have studied the geology of San Salvador Island and the Bahamas for nearly two decades, we stand by our previously published comments (CAREW and MYLROIE, 1994) on HEARTY and KINDLER (1993). As noted by HEARTY and KINDLER (1994, p. 1,103) we have a long publication record, some of which is in what *they* term "gray literature" of GSA, SEPM, and IGC field guides, and the biennial Geology of the Bahamas Symposium series (all of which involved peer review and editing). Those publications record the data and interpretations that have evolved as we (and our many students and colleagues) learned more about, and further investigated, the geology of San Salvador Island, and the Bahamas. For example, HEARTY and KINDLER (1994, p. 1,101) comment that we, and colleagues with whom we have published, have changed our opinions concerning the interpretation of paleomagnetic data. That is true. We invoked paleomagnetic analyses as one of the many analytical techniques that we were the first to utilize on San Salvador Island (including: AAR of *Cerion*, whole-rock $^{14}C$, $^{234}U/^{230}Th$ of corals and speleothems) in order to help us unravel the geochronology of the deposits. Our record of reassessment of these paleomagnetic data as they developed over time is the appropriate scientific result. Only now, after about ten years of continued work on this subject, does it appear that we (and our paleomagnetist collaborators) may finally have gathered enough data to make this technique truly useful (PANUSKA *et al.*, 1994; KIRKOVA *et al.*, 1994).

We have regularly shared our ideas with colleagues (orally, as posters, and in abstract) at regional, national, and international meetings; on GSA, SEPM, and IGC sponsored field excursions that we co-led; and we have also published in peer-reviewed publications when we accumulated sufficient data to support important interpretations and conclusions (*e.g.*, CAREW and MYLROIE, 1991, 1995; MYLROIE and CAREW, 1988, 1990, 1995; MYLROIE *et al.*, 1995). In contrast, HEARTY and KINDLER (1993) published their interpretation of the geology of San Salvador (HEARTY and KINDLER, 1993: Figure 4) based on a few field sessions and a sampling regime that was inadequate for the extrapolation to the whole-island geology they published.

If any readers have further interest in the geology of the Bahamas and have difficulty obtaining any of the literature please contact either of us and we will be happy to provide copies of articles or addresses of sources.

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