Coral Reefs Doomed?

A Coral-List Server Discussion Thread

This thread is from the Coral List Server. Some of the writers included a previous posting in their message. For simplicity, the included messages have been replaced by a link to the previous message that was quoted. If you follow that link, moving back in your browser should bring you back to your original position. This should continue to work even if you download the document to your machine. If you have any difficulties navigating this document, send a message to the CHAMP WebMaster.

Date: Fri, 7 Sep 2001 08:51:29 -0400 (EDT)
From: Jim Hendee <hendee@aoml.noaa.gov>
To: Coral-List <coral-list@coral.aoml.noaa.gov>
Subject: coral reefs doomed?

Dear Coral Colleagues,

I know I'll get raked over the coals on this (especially because I don't have all the literature at my fingertips), but the content and tone of the news article below is troublesome to me, even though such a tone helps to gain attention, as well as funding, so that we can more thoroughly study the problem of coral bleacing and global warming. Of course I respect our colleague's right to a viewpoint, but when I see this, I can't help but have these thoughts:

Such a projection gives no "credit" to adapatation and natural selection, even though such adaptation would have to occur under a relatively short time span (50 years). I believe Ware et al (1996), among others, have addressed this.

As Dr. Al Strong and I have discussed, and as alluded to but unfortunately not expanded upon in the last sentence of the article, if the seas are warming, then you might expect the zoogeography of corals to expand (relocate?) into the cooler areas, as long as the substrate, circulation, light and water quality regimes are conducive. (I would imagine some coral researchers have modeled these possibilities, and I apologize for not referencing your work.)

Even though high sea temperatures are the primary cause and indicator of coral bleaching, that is not the only cause, and no credit is given to the evidence in the literature (e.g., Lesser 1996, among others) that high UV is also an agent in coral bleaching. Higher UV, especially in the tropics, is part of the problem as it relates to the earth's ozone layer.

There is evidence that high sea temperatures that elicited coral bleaching at some localities in the past did not elicit coral bleaching during extended cloudy periods (Mumby et al, in press). (Perhaps the cooler areas mentioned in the above paragraph might also have lower UV?)

There are other causes of coral bleaching (e.g., see Glynn 1993, 1996) and this manifestation of stress is complex and to my mind public statements on coral bleaching should emphasize this.

Would an annual update to the ITMEMS statement on coral bleaching (http://coral.aoml.noaa.gov/bulls/ITMEMS-bleach.html) be helpful for the public in this regard? It is my opinion that it would, that we should address the topics above (among others, e.g., coastal effects), and that it would behoove us to widely circulate the update among the press as a consensus opinion (if that is possible!).

Just my two cents worth...

Cheers,

Jim Hendee
NOAA/AOML
Miami, FL

Glynn, P. (1993). Coral reef bleaching: ecological perspectives. Coral Reefs 12, 1-17.

Glynn, P. (1996). Coral reef bleaching: facts, hypotheses and implications. Global Change Biology 2, 495-509.

Lesser, M.P. (1996). Elevated temperatures and ultraviolet radiation cause oxidative stress and inhibit photosynthesis in symbiotic dinoflagellates. Limnol Oceanogr. 41(2): 271-283.

Mumby, P.J., Chisholm, J.R.M., Edwards, A.J., Andrefouet, S. & Jaubert, J. 2001. Cloudy weather may have saved Society Island reef corals during the 1998 ENSO event. Mar Ecol Prog Ser (in press).

Ware, J.R., Fautin, D.G., & Buddemeier, R.W. (1996). Patterns of coral bleaching: modeling the adaptive bleaching hypothesis. Ecological Modelling 84, 199-214.

-------- Original Message --------

World coral reefs to die by 2050, scientist warns
By Ed Cropley, Reuters
Thursday, September 06, 2001

GLASGOW, Scotland =97 The world's coral reefs will be dead within 50 years because of global warming, and there is nothing we can do to save them, a scientist warned Wednesday.

"It is hard to avoid the conclusion that most coral in most areas will be lost," Rupert Ormond, a marine biologist from Glasgow University, told a science conference. "We are looking at a loss which is equivalent to the tropical rain forests."

Only the coral reefs in nontropical regions such as Egypt stand any chance of lasting beyond 2050, Ormond said, but even the days of the stunning marine parks of the Red Sea are numbered as sea temperatures continue to creep up.

In the past, reefs have suffered from sediment buildup and the coral-eating crown-of-thorns starfish, whose numbers have exploded due to the over-fishing of their predators.

Now the main threat to the delicate structures that harbor some of nature's most stunning creations comes from warmer seas, which cause coral bleaching.

Microscopic algae that support the coral polyps cannot live in the warmer water, and the polyps, the tiny creatures who actually create the reefs, die off within weeks.

Scientists agree the world's oceans are now warming at a rate of between one and two degrees Celsius every 100 years due to the increased amounts of greenhouse gases in the atmosphere which trap the sun's rays.

But even if humans stopped pumping out greenhouse gases such as carbon dioxide tomorrow in a bid to halt the process, it would still be too late to save the reefs, Ormond said. "I don't know what can be done, given that there's a 50-year time lag between trying to limit carbon dioxide levels and any effect on ocean temperature," he told the conference, held by the British Association for the Advancement of Science.

The implications stretch far beyond the death of the colorful coral structures themselves. The weird and wonderful eels and fish which inhabit the nooks and crannies will become homeless, and many species will die out. "We are looking at a gradual running down of the whole system. Over time, the diversity of coral fish will die," Ormond said.

Humankind will also suffer directly as the dead reefs are eroded and shorelines that have been protected for the last 10,000 years face the wrath of the oceans without their natural defenses.

In an age of relatively cheap scuba-diving holidays, this also means many developing countries in the tropics, such as Kenya or those in the Caribbean, face losing a major source of revenue.

The only cause for optimism was that new coral reefs could start to emerge in colder waters such as the north Atlantic Ocean and Mediterranean Sea.

Date: Fri, 07 Sep 2001 13:00:01 -0500
From: "Bob Buddemeier" <buddrw@kgs.ukans.edu>
To: Jim Hendee <hendee@aoml.noaa.gov>
CC: Coral-List <coral-list@coral.aoml.noaa.gov>
Subject: Re: coral reefs doomed?

Jim, et al.,

Good questions, good points, -- and like it or not, a pretty good if disturbing article.

On your question about range expansion to compensate for temperature increase

and inhospitably hot tropics -- there are unfortunately 3 geographic factors that work against that.
1. The available shallow water benthic area decreases rather significantly as you move to higher latitudes (no atolls, narrower shelves, etc.)
2. Light -- see the Kleypas et al analysis -- Kleypas, J.A., McManus, J.W. and Menez, L.A.B., 1999. Environmental limits to coral reef development: Where do we draw the line? American Zoologist, 39(1): 146-159. Maximum reef depth shoals dramatically at higher latitudes, even within the thermal mixed layer. This presumably reflects light limitations due to sunangle and day lenght variations -- which aren't going to change.
3. Carbonate saturation state decrease is squeezing from the high latitude sides -- see the US National Assessment, http://www.cop.noaa.gov/pubs/coastalclimate.PDF, section 4.4.

So there is little basis for optimism there.

With acknowledgment of the terminological problems, some form of adaptation/acclimatization probably does have real potential to ensure the survival of corals , but not necessarily "reefs as we know them." The Ware et al article and its precursor, Buddemeier, R.W. and Fautin, D.G., 1993. Coral Bleaching as an Adaptive Mechanism: A Testable Hypothesis. BioScience, 43: 320-326, are looking more solid as experimental tests come in (Kinzie et al in Biol. Bull. earlier this year, Baker in Nature more recently), but for some reason this concept has been anathema to some reef cology and conservation types. (see also Buddemeier, R.W., Fautin, D.G. and Ware, J.R., 1997. Acclimation, Adaptation, and Algal Symbiosis in Reef-Building Scleractinian Corals. In: J.C. den Hartog (Editor), Proceedings of the 6th International Conference on Coelenterate Biology (16-21 July 1995, Noordwijkerhout, The Netherlands). National Museum of Natural History, Leiden, pp. 71-76 for a related issue). This may be because it is seen as diminishing the seriousness of the bleaching problem, but in my view your position is the more valid -- without some mechanistic reason to believe that corals CAN survive, there is very little justification for investing money in research and conservation.

This also relates to my tired old hobby horse of the non-reef coral habitats -- I don't think we are getting the real picture, or doing ourselves any favors, by exclusive concentration on reefs; corals have survived many periods of non-reef-building, and we had better figure out how, why and where.

Thanks for bringing this up.

Bob Buddemeier

--
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Note: *Buddemeier had Hendee's whole message in his original message. Hendee's message is* *already displayed above.*

Date: Sat, 8 Sep 01 11:06:12 -0400
From: Stephen C Jameson <sjameson@coralseas.com>
To: "Bob Buddemeier" <buddrw@kgs.ukans.edu>,
"Jim Hendee" <hendee@aoml.noaa.gov>
cc: "Coral-List" <coral-list@coral.aoml.noaa.gov>
Subject: Re: coral reefs doomed?

Dear Jim and Bob,

Regarding Jim's:

>Such a projection gives no "credit" to adapatation and natural selection,
>even though such adaptation would have to occur under a relatively short
>time span (50 years).

In a nut shell, isn't the overriding problem (which Bob addressed in a plenary session at the NCRI symposium in Ft. Lauderdale) the fact that the increasing CO2 concentration in the atmosphere is changing the pH of the ocean (making it more acidic) and reducing the ability of corals to calcify properly (Bob's point number 3 stated in brief and in relation to high latitude)? So, no matter where a coral goes - it is going to have problems surviving.

Wasn't it also at the NCRI Symposium plenary session where Bob estimated coral reefs had only about 50 years to survive and this prediction was related to the change in pH not temperature (as stated in the press release)?

Best regards,

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Date: Sat, 8 Sep 2001 12:26:09 -0400
From: "Mike Risk" <riskmj@mcmail.cis.mcmaster.ca>
To: "Jim Hendee" <hendee@aoml.noaa.gov>,
"Coral-List" <coral-list@coral.aoml.noaa.gov>
Subject: Re: coral reefs doomed?

Hi Jim.

Although I share your concerns in general, the bad news is: the conclusion is probably correct. I don't read that as a funding ploy-Rupert clearly says there's stuff-all we can do about it, leading funding agencies to say why bother?

Notwithstanding the recent stimulating work by Jackson et al on overfishing, the hard evidence from the 20th century (and this one, too) is that land-based sources of pollution have ineradicably slain more coral reefs than all other causes put together. The references on this are close to countless. This trend continues unabated, and science seems slow to respond. (I invite other readers, perhaps offended by this comment, to submit examples of coral reef monitoring programs that are linked to legislation and enforcement by a proper detection/identification/amelioration process.)

Will reefs colonise new shelf areas? Sure. In fact, the rate at which this will occur may be estimated from the drilling work done long ago by Walter Adey, in the Virgin Islands. It takes the ocean about 1,000 years to clean up the shoreline and make it ready for new corals. Presumably, this same process in the future will take even longer, given the necessity for reworking condos and Hondas: plus, that ocean will not be nearly as clean as the advancing Holocene seas were. So: but don't hold your breath. For sure, it will happen after the next election.

Concern about ocean warming is well-placed. One of the best references to this is by Francis Rougerie, in...1988?. This is in French, and hence not as widely read and cited as it should be. Quelle honte.

Concern about oceanic pH is probably overblown:
1. we seem to have forgotten the seminal work of Sillen, in the 60's, showing that silicates, not carbonates, are the long-term oceanic buffers. Lord knows we have done lots to "protect" tropical coastlines from pH change by loading them with chemically-reactive silicates (feldspars, illite, montmorillonite, etc). Large quantities of these minerals are in fact bound up in coral skeletons, hence corals carry with them their own personal buffers (Cortes and Risk, 1985, BMS).
2. the pH of tropical coastlines will no doubt shift-after all the high-mag calcite has dissolved. As HMC makes up a large proportion of reef sediments, this may take some time.
3. as the climate changes and we shift to the other metastable condition of global climate, this will be accompanied by a fundamental reorganisation of the oceans. This will involve (far as we know) vertical mixing, which will put low-pH surface waters into contact with bottom sediments and bottom waters of higher pH. This process was outlined in Smith et al, 1997, April Nature. This process can occur within five years. None of the present ocean models allows for mixing on this vertical and temporal scale, hence all need recalibration. (Some of this work is under way now, using data from deep-water corals.)
4. McConnaughey and colleagues, and Barnes and colleagues, in separate publications within the last 12 months, have shown that corals calcify faster at elevated temps, and in the presence of fleshy algae.

My prediction (Risk, 1999) was that coral reefs, as some of us knew them (and you were one, Jim), will be eradicated by land-based sources from most of the world's shelves long before pH shifts appreciably-in fact, my prediction was even more dismal than Rupert's. I think I said 2020.

I am hesitant about statements, usually made (I'm afraid) by geologists, along the lines of "Corals have been around for a long time, they will survive." It's true, but misleading. Yes, coral relatives-burrowing sea anemones-are the oldest metazoan fossils yet found: Proterozoic, McKenzie Mountains, NWT. Such statements need to have appended to them the comment that large proportions of the geologic record are virtually barren of reefs, of any type. I consider these statements similar to: "The globe's been hot before, we survived", which we have also heard lately. The globe has been quite hot before, involving a fundamental rethinking of real estate values. Every North American Grade Six kid should do the exercise of drawing the +15-m sealevel contour onto the globe, and estimating the human population involved. Or perhaps we should start with those politicians whose development seems to have been arrested at Grade 6...

It may very well be that some of those we refer to as "deep-water" corals may be a recolonisation/biodiversity resource-let us hope so. This has recently become an extremely productive area of research, and interested persons should log on to the coolcoral site, or contact me for preprints.

This email is devoid of specific page #'s, etc, for refs: my office is being moved, I am fileless, and am celebrating by being a carpenter for a while. Another guy who tried it came back, so what have I got to lose?

Yours in gloom: Mike

From: "Ove Hoegh-Guldberg" <oveh@uq.edu.au>
To: "'Bob Buddemeier'" <buddrw@kgs.ukans.edu>,
"'Jim Hendee'" <hendee@aoml.noaa.gov>
Cc: "'Coral-List'" <coral-list@coral.aoml.noaa.gov>
Subject: RE: coral reefs doomed?
Date: Sun, 9 Sep 2001 09:01:31 +1000

Dear Bob and others,

I was triggered to respond by the inferences in your statement that some "reef ecology and conservation" types have trouble with the Adaptive Bleaching Hypothesis. Any practicing experimental scientist would have an issue with the state of play regarding support for this hypothesis. The basic problem at this point is nothing to do with "culture" - it is more to do with hard evidence, which is almost completely lacking to support this still very soft and hypothetical explanation for why coral bleach. While experimental tests have been coming in, they have had serious problems in terms of design and the conclusions they draw. Us "reef ecology and conservation types" still wait for the definitive data that shows corals will bleach, get rid of one dinoflagellate genotype and adopt another WHILE the thermal (or other) stress is still being applied to the coral-dinoflagellate association. This has never been shown. Showing diversity in rDNA is interesting but irrelevant if diversity here does not relate to relevant physiological differences. The recent paper by Baker (whom I greatly respect), for example, used light and could not prove (using RFLPs) that his corals had changed from one dinoflagellate genotype to another (simply up-regulating one strain over another is not sufficient - that is acclimation and is not surprising). The experimental design was also confounded by the fact that stressed corals were placed in the two contrasting and confounding (for the experiment) habitats (one, the deeper site, was at the extreme depth limit of the species concerned while the other was clearly more optimal after photo acclimation). It is therefore not surprising that the corals died more at deeper site - which has nothing to do with the fact that they did not bleach!).

Other issues abound and concern us "reef ecology and conservation types" - the idea of range of expansion is limited (as outlined by several people so far) by the fact that light may be a more important limiting than temperature. I also want to stress that the issue of the decline of reefs (as you, Bob, did state) has nothing to do with the extinction of corals. As the "geo types" (deliberate use here) tell us worse things have happened to corals and they have bounced back (but over thousands if not millions of years). The issue, however, is the current human dependency on coral reef ecosystems - reefs disappearing for even a few decades would present serious issues for several hundred million people. The idea of finding out how reefs survived major extinction events is interesting but largely irrelevant to the current discussion.

So - out I come on my old hobby horse - we still have no evidence of unusual adaptive abilities of corals that will match the fast rate of change. Us reef ecology types keep looking. While looking for this evidence - perhaps we also need to focus on how reefs will change and how we can "adapt" as human societies to these changes. This research direction, if the projections of the future are correct, will assume a major significance as we enter the next few decades.

Best wishes,

Ove

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Note: *Hoegh-Guldberg had Buddemeier's whole message in his original message. Budemeier's message is already displayed above.*

From: "Jeffrey Low" <jeffrey-low@mailhost.net>
To: "'Coral-List'" <coral-list@coral.aoml.noaa.gov>
Subject: RE: coral reefs doomed?
Date: Sun, 9 Sep 2001 10:37:06 +0800

Hi everyone,

I hestitate to air my views in this forum, which will be read by the "greats" in coral reef research. However, I beg your indulgence to add my questions and comments to the debate on the destruction of coral reefs.

Factors affecting coral reef survival. I think it is moot to say one factor overrides the other - unless we know ALL the factors, and how they relate to each other, even the "global" factors may only play a small part in coral survival in a specific regions, and at that point in time. Even then, these factors would probably change faster than science can determine to be of practical use.

Pollution. I use the term liberally here, to include CO2, sediment, sewerage etc. Most, if not all, of the problems related to coral reefs are man-made. While I hear a lot about the biology of corals, their reaction to certain influences, what is being done to link the biology with the "pollution management" sciences? My meaning is that should more be done to address the question of how do we keep our environment cleaner?

Conservation, preservation, protection. Are we trying to keep the coral reefs as they are? Even in the face of environmental change on a global scale? Maybe their "time" has come and we will be powerless to prevent it. Given that humans have caused premature termination of thousands of species, but species extinction has been going on for some time, no? Perhaps the overall degradation of the various ecosystems worldwide is an indication of the (eventual) demise of the human race as we know it.

Population. I would class this as the ultimate source of all our problems (not just for coral reefs). To paraphrase from the movie "Godzilla" - size does matter. 6 billion people ... I can't even imagine what that number constitutes. And it is set to top 7 billion by 2050? How do you manage the waste produced by so many people? How do you prevent overfishing when fish may be the main (and sometimes only) source of protein. How do you prevent over-exploitation of the oceans resources? I recall a funny anecdote in the newspapers about someone who calculated that if everyone of earth passed gas at the same time, it would cause an explosion that would destroy the world. It seemed funny at the time ....

Cheers,

Jeffrey Low
SINGAPORE
jeffrey-low@mailhost.net

From: "Ove Hoegh-Guldberg" <oveh@uq.edu.au>
To: "'Jeffrey Low'" <jeffrey-low@mailhost.net>,
"'Coral-List'" <coral-list@coral.aoml.noaa.gov>
Subject: RE: coral reefs doomed?
Date: Sun, 9 Sep 2001 16:47:45 +1000

Hi Jeffrey,

Let us hope synchrony in gas does not prevail!

People are key to both the problem and the solution. The same mass scale efforts you refer to in terms of the negative also apply to the other side of the equation. If all of us planted a tree, there would be 6 billion new trees. If everyone in the rich developed countries insulated their homes rather than use heating or air-conditioning, we would have a dramatic decline in the greenhouse gas problem. So - six billion people does not have to be a negative (yes - I know - it rarely is)

On the relative impacts of climate change versus "pollution". There has been a perception of a competition among us of "who has the worst factor for causing reef decline'. I find that silly. While the GCRMN data tend to indicate a dramatic impact of climate events like 1998 (16% loss of living coral in a single year), the truth is that the synergies and interactive effects are probably where the action is as opposed to an isolated and single factor.

Cheers,

Ove

Note: *Hoegh-Guldberg had Low's whole message in his original message. Low's message is already displayed above.*

Date: Sun, 09 Sep 2001 14:32:09 +0100
From: "Mark Spalding" <Mark.Spalding@unep-wcmc.org>
To: <coral-list@coral.aoml.noaa.gov>
Subject: RE: coral reefs doomed?

Just a few quick thoughts on this, because tommorrow and Tuesday I'm going to be facing quite a bit of national and international press regarding the launch of the World Atlas of Coral Reefs. I'm quite expecting a question such as "We heard last week that coral reefs will all be dead within 50 years and there's nothing we can do about it, so why should be bother trying?"

I think the answer is something like.

1 - this is a very extreme view, that is not to say impossible, but it lies at one end of a spectrum, while "no impact whatsoever lies at the other". The reality is somewhere in between

2 - We do not, therefore, give up while what we are talking about is still a remote chance.

3 - What can we do? Well perhaps we can ameliorate the impacts, for example by reducing the mix of other threats facing reefs. While this may not prevent coral death from bleaching, it seems highly likely that it would facilitate recovery. Detailed networks of protected areas may help, and more active management may become essential. For example, even the worst hit areas of the Indian Ocean showed very localised pockets of high survival. These may be critical for subsequent recovery of wider areas, and should be given high levels of protection following a bleaching event. Similarly overfishing of grazing fish may prevent coral settlement as algae grow up, so perhaps there are fisheries management controls we should consider.

4 - The jury is still out on the rates of adaptation of corals, given the timescales genetic adaptation may be out of the question (not completely), but there is also phenotypic plasticity. We need to watch, and to experiment.

If the doomsday scenario really starts to look likely there may still be more active management measures we could take, and research needs to think about these.

Cheers

Mark

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Date: Sun, 9 Sep 2001 17:37:09 +0100 (BST)
From: JM Kemp <jmk100@york.ac.uk>
To: "'Coral-List'" <coral-list@coral.aoml.noaa.gov>
Subject: RE: coral reefs doomed?

One small comment about range - expansion and survival of coral and other associated taxa in the face of climate change: Ignoring the details of arguments about acidity, etc, and just thinking geographically, if climate change does
force those taxa away from the equator into higher latitudes, a quick look at any atlas shows that the 'range expansion' argument is invalid for some large parts of the GLobe. Although it may hold water in the Tropical Atlantic, parts of the Pacific, and the densely - packed archipelagos of the Indo-Malay region, in the Indian Ocean (especially the northern INdian Ocean), and other areas this is not the case.

My own stamping ground of Arabia, including the Red Sea and the Arabian Sea, provides good examples: force the many hundreds of taxa endemic to that part of the world any further north and they'll have to develop legs and lungs (which may be taking the adaptation hypothesis a little too far): there's nowhere else for them to go except dry land.

Similarly, any of the numerous reef-coral taxa endemic to remote islands or island chains in the tropics of any of the worlds oceans are likely to have nowhere to go, simply because they are unable to disperse and colonise areas away from their present home ranges. For poorly dispersing taxa the distance involved may not even have to be very large before it becomes insuperable.

Just a thought.

Jerry Kemp

Date: Sun, 9 Sep 2001 13:16:24 -0400 (EDT)
From: Jim Hendee <hendee@aoml.noaa.gov>
X-Sender: hendee@blimpie
To: Coral-List <coral-list@coral.aoml.noaa.gov>

Subject: Re: coral reefs doomed?

I need to make something clear about my original message in "coral reefs doomed?": I was NOT intimating that R. Ormond's statements were made as a "ploy" (ref: colleague M. Risk's post) to gain funding. I can see how one might draw that inference from what I said, but that was definitely not my intent.

My overall intent in the message was that a more well-rounded statement on coral reef decline might be more helpful in public statements to the press. However, I am beginning to see that a consensus might be impossible, even if a desirable goal.

Cheers,
Jim

From: "Mike Risk" <riskmj@mcmail.cis.mcmaster.ca>
To: "Jim Hendee" <hendee@aoml.noaa.gov>,
"Coral-List" <coral-list@coral.aoml.noaa.gov>
References: <Pine.GSO.4.03.10109091307580.1664-100000@blimpie>
Subject: Re: coral reefs doomed?
Date: Sun, 9 Sep 2001 17:29:06 -0400

And, in turn, allow me to make myself clear.

Jim Hendee was not one of the people I hoped would take offense at my posting.

There has been more than a little game-playing by some reef scientists, re obtaining funding to save the world's reefs from disaster. Neither Jim nor I read Rupert's comments as pleas for more dough, but as the sad conclusions of an experienced scientist. I differ from those conclusions only in scale.

From: BTyler3@aol.com
Date: Sun, 9 Sep 2001 20:18:34 EDT
Subject: Re: coral reefs doomed?
To: coral-list@coral.aoml.noaa.gov ('Coral-List')

Re: Mark Spalding's comments and others...
<<Just a few quick thoughts on this, because tommorrow and Tuesday I'm going to be facing quite a bit of national and international press regarding the launch of the World Atlas of Coral Reefs. I'm quite expecting a question such as "We heard last week that coral reefs will all be dead within 50 years and there's nothing we can do about it, so why should be bother trying?">>

I'd like to throw in my two cents worth about why bothering to study/protect coral reefs IF(??) they are actually on there way to widespread decline as is being discussed here. This probably seems obvious to biologists and managers, but not necessarily to politicians/reporters controlling/influencing the purse strings.

There are other reasons to protect these areas and to maintain water quality in reef areas other than maintaining hard corals.

What would be the effect of hard coral die-offs from many of the worlds coral reefs? No doubt there would be a change in structure, both physical and ecological. Coralline algae, sponges, and possibly soft corals, would likely become the dominant structure-forming organisms. This change in structural characteristics would lead to community changes in composition, diversity and abundance, but not necessarily complete elimination of important marine resources in these areas.

In the worst case scenario, there may eventually be complete erosion of wave-dissipating functions of the resulting reefs, but this may take much longer. But it seems to me that these altered reef areas would still be valuable marine resources worthy of protection for the future, if nothing else then to help put off the possibly inevitable breakdown of the entire reef structure. Good water quality and management practices should hopefully enhance whatever takes place over the long-term.

Bill

Dr. Bill Tyler
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Date: Mon, 10 Sep 2001 13:59:40 -0400
From: "Alan E Strong" <Alan.E.Strong@noaa.gov>
To: Coral-List <coral-list@coral.aoml.noaa.gov>
Subject: Re: coral reefs doomed?

Dear Ove, Bob, and others,

It seems about the right time to correct a misimpression that we spoke to at Bali last October. Our Bali paper noted that NOAAs satellite SST data from around the tropics were believed to have been indicating an alarming increase (upward tendency hardly a trend!) over the past two decades latitudinally as high as 0.5 deg C at 5 N latitude! A re-evaluation of these data, through a program sponsored by NASA and NOAA, called Pathfinder has taken all the year-to-year improvements in making correct measurements over that time interval and reprocessed the data in an up-to-date and uniform fashion. More importantly, in-situ SST data from all the drifting and fixed buoys available were utilized to both validate and correct satellite calibrations on a regular basis. From Pathfinder we now believe that we have a more accurate set of NOAA satellite SST observations the best results for buoy comparisons are still seen when using only those Pathfinder satellite SSTs made at night.

>From Pathfinder nighttime SST observations (Paper will be presented at the upcoming Ocean Sciences AGU) it is seen that SSTs through most of the tropical

latitudes have not been rising but holding rather steady. In fact some regions have been showing steady DECLINES in SST. We still are finding greater declines in the southern hemisphere (reported at the Bali meeting) but even northern tropical locations show decreases: e.g., region around Midway; the region known as The Warm Pool both continue to trend downward during the 80s and 90s. Even though much of the Indian Ocean experienced devastating bleaching from high SSTs in the late 90s, this area is basically experiencing a downward SST tendency. There are several regions that may be showing statistically significant increases, but this final say will not be official until the Feb 2002 Ocean Sciences meeting when we expect to have Pathfinder 1999 and 2000 SST data fully incorporated. Regions that have been experiencing upward tendencies are: American Samoa Fiji Cook Islands; some regions of the Caribbean (especially eastern portions); Mexicans Pacific coast; Red Sea; Arabian Sea/Persian Gulf; and possibly the extreme southern regions of GBR. There are other regions in the northern Atlantic and Pacific, outside areas of interest to coral folks, that show upward trends. These upward tendencies may be starting to show effects of climate increases that, from the oceans standpoint seem to be mostly noted at higher latitudes in the Northern Hemisphere.see you at Oceans Sciences.

Footnote:
A much scarier scenario is seen when the 1997/98 El Nino period is incorporated, a scenario we believe that will be largely eliminated with the addition of 1999 and 2000 SST data. Any trends ending during such a significant event are statistically flawed. What some are concerned about for the future of coral reefs from the standpoint of temperature is what will El Ninos be like over the next 50 years So far I know of no reliable model with the answer to that question

Cheers,
Al

Date: Mon, 10 Sep 2001 21:18:37 -0400
To: Mike Risk <riskmj@mcmail.cis.mcmaster.ca>, coral-list@coral.aoml.noaa.gov
From: "Alina M. Szmant" <szmanta@uncwil.edu>
Subject: Re: coral reefs doomed?

Dear Mike:

In your recent Coral List message you made the following statement:
".... is that land-based sources of pollution have ineradicably slain more coral reefs than all other causes put together. The references on this are close to countless...."

Having tried to track down peer-reviewed published work on this subject and having found the Kaneohe Bay case, and some of Jorge Cortes and your work on Costa Rica reefs buried in sediments to be the only scientifically credible major studies of reef decline due to pollution, I'd greatly appreciate being directed to the "countless references". I am sure there are others on Coral List whom also would be interested. Hopefully you have a master list of such references on your computer you could send out as an attachment or post on a web site for our edification while your office is being remodeled.

Thanks,

Alina Szmant

****
Dr. Alina M. Szmant
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University of North Carolina at Wilmington
One Marvin K. Moss Lane
Wilmington NC 28409
TEL: (910)962-2362 FAX: (910)962-2410
email: szmanta@uncwil.edu
Presently in Key Largo: (305)453-4595
****

From: "Mike Risk" <riskmj@mcmail.cis.mcmaster.ca>
To: coral-list@coral.aoml.noaa.gov, "Alina M. Szmant" <szmanta@uncwil.edu>
Subject: Re: coral reefs doomed? Argh without refs
Date: Tue, 11 Sep 2001 15:45:39 -0400

Hi Alina.

I always have an excuse for not doing homework. In this case, my wife =
(Jodie Smith) is in surgery, I am taking a break to do email, but have =
no intention of doing science for several days. (She's OK.)

The largest problem here, as you are no doubt aware, is that, after 30 =
years of using the same survey techniques: we have damn few long-term =
records. So every argument that land-based sources cause stress may be =
met with the counterargument, that you have no basis for concluding =
that. (No matter that it's a BS argument-in these days of embracing =
traditional knowledge, the one source we refuse to acknowledge is the =
memory banks of aging reef scientists...)

BUt here's a start. One of the best/worst places to see this is in SE =
Asia. Tom Tomascik has documented disappearance of whole reefs in Pulau =
Seribu (Thousand Islands), off Jakarta, within historical times-used old =
data sets from the days of Umgrove. His work has appeared in various =
iterations, including his book, and the Ginsburg Miami volume. Edinger =
worked in several locations in Indonesia, with some of my other =
students-published 2000 (?), Mar Poll Bull, plus several other summary =
papers. The effect of a combo of sediments and sewage ranges from a =
large drop in biodiversity and coral cover, to (most often) complete =
extirpation. It classifies as a regional mass extinction: he estimated a =
loss of (?) 40% of generic diversity of corals in the past 15 years. =
Climate change had zip to do with it.

Sri Lanka lost almost all of its reefs over the past decade...

If I feel like doing science in a few weeks, I'll get back to you. =
Promise.

Date: Sat, 15 Sep 2001 16:28:16 -0500
From: buddrw <buddrw@kgs.ukans.edu>
To: <oveh@uq.edu.au>, Jim Hendee <hendee@aoml.noaa.gov>
Cc: Coral-List <coral-list@coral.aoml.noaa.gov>
Subject: RE: coral reefs doomed?

Ove, and others --

Part of the reason you are still waiting for hard experimental evidence regarding the ABH is that you consistently misstate and/or misunderstand what it is. Some specific examples:

"the definitive data that shows corals will bleach, get rid of one dinoflagellate genotype and adopt another WHILE the thermal (or other) stress is still being applied to the coral-dinoflagellate association." This is part of the ABH only to the extent of requiring continuance of the stressful REGIME (e.g., frequency of high temperature excursions), not of the stressful bleaching-inducing CONDITION (e.g., continuous high temperature). It seems to me that you are attacking the latter proposition, which is NOT what we proposed or modeled (Ware et al).

"used light and could not prove (using RFLPs) that his corals had changed from one dinoflagellate genotype to another (simply up-regulating one strain over another is not sufficient - that is acclimation and is not surprising)." Bleaching is a stress response, and we think that stress adaptation probably doesn't care that much about light, temperature or whatever -- besides which, there is certainly strong evidence for the synergism of light in temperature even in the bleaching episodes attributed primarily to temperature. Sorry if using light is a problem for you -- it's not for us. Further, we are willing to plead guilty to having accepted that which is not surprising -- what you refer to as 'up-regulation' we considered a shift in dominance or inertnal competitve abilities among the varieties of zoocxanthellae that could or did inhabit a host -- very much a part of ABH.

Rather than go on and nit-pick your counter-arguments, I'd like to suggest that this is a good opportunity to set up and broaden the debate as a discussion thread -- with the proviso that we rely on direct quotes in context (since the subject is a bit complicated for one-line summaries) rather than on strawman revisions to discuss what the ABH actually is or isn't.

Bob Buddemeier

Dr. Robert W. Buddemeier
Senior Scientist, Geohydrology
Kansas Geological Survey
University of Kansas
1930 Constant Avenue
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ph (785) 864-2112; fax (785) 864-5317
email: buddrw@kgs.ukans.edu

Note: *Buddemeier had Hoegh-Guldberg's whole message in his original message.* *Hoegh-Guldberg's message is already displayed above.*

Date: Sat, 15 Sep 2001 16:10:42 -0500

From: buddrw <buddrw@kgs.ukans.edu>
To: Coral-List <coral-list@coral.aoml.noaa.gov>,
"Mike Risk"@coral.aoml.noaa.gov
<riskmj@mcmail.cis.mcmaster.ca>, Jim Hendee <hendee@aoml.noaa.gov>
Subject: RE: coral reefs doomed?

It's interesting, if mildly depressing, to see so many reasons for pessimism.

I generally agree with most of Mike's points, but there are two that he raises that I think merit comment -- both related to the CO2 and saturation state issue, and both addressing issues of temporal scale and kinetics.

First, Mike raises the question of 'long-term' silicate buffering. True enough -- in the very long term, none of this is an issue, and even on the thousands of years time scale we are dealing with ocean DIC content that overwhelms the size of the atmospheric reservoir(and essentially all others but the mineral). The critical issue is that we are not dealing with scales of this magnitude -- the anthropogenic CO2 input has been on the scale of a century (more if you count the beginning of the industrial revolution, less if you start from the rapid rise post-WWII). The mixed layer of the ocean, however, contains DIC in an amount comparable to the atmospheric reservoir with a probably turnover time of a few centuries (cf. many radiocarbon studies of marine apparent ages). For the purpose of considering presewnt problems, it is a reasonable first approximation to treat the mixed layer (which is where all of the reef-building corals live) as an isolated compartment, and on that scale the CO2 effect is clearly dominant.

Second, the high-mag calcite issue -- I too am out of my office, but in 1986 June Oberdorfer and I published a chapter in Carbonate Diagensis book edited by Purser and Schroeder that pointed out that reef interstitial water is controlled at the saturation state of high-mag calcite. What is most definitely not true is that this has much effect on the saturation state of the overlying seawater. Here again, the issue is time scales -- in this case of advective open water exchange compared to the flushing of interstitial porewaters (see also the paper by same authors in the ICRS 6 proceedings). There are many orders of magnitude difference -- and in fact the possibility of equilibrating the sedimentary carbonate with the ocean water is on time scales equivalent to the silicate buffer controls, and basically insignificant on the 100 year scales dominated by gas and open water exchange reactions.

A question, Mike -- I didn't understand your point about vertical mixing replacing high pH bottom water with low pH suface water -- did that refer to some particular locale? Certainly for most of the ocean saturation state, pH etc are lower at depth than at the surface.

Bob Buddemeier

Dr. Robert W. Buddemeier
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email: buddrw@kgs.ukans.edu

Note: *Buddemeier had Risk's whole message in his original message. Risk's message is already displayed above.*

Date: Tue, 18 Sep 2001 00:58:14 -0500
From: buddrw <buddrw@kgs.ukans.edu>
To: Coral-List <coral-list@coral.aoml.noaa.gov>,
Jim Hendee <hendee@aoml.noaa.gov>
Subject: RE: coral reefs doomed -- and the ABH

Coral-listers;

I have received, in addition to this broadcast message from Ove, other personal communications that indicate that there is a fairly broad pool of misunderstanding about what the Adaptive Bleaching Hypothesis is and isn't. The comments below address primarily things that it isn't, and I have sent messages to Ove and others on an individual basis to try to get this sorted out so that a productive discussion can ensue.

In the meantime, I heartily recommend recourse to the original literature as a source of primary information -- I, Daphne Fautin, and John Ware will all be more than happy to answer questions or attempt to clear up confusion.

Bob Buddemeier

PS: I stand by my original statements.

Dr. Robert W. Buddemeier
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Note: *Buddemeier had Hoegh-Guldberg's whole message in his original message. Hoegh-Guldberg's message is already displayed above.*

Date: Tue, 18 Sep 2001 10:37:27 -1000
To: buddrw <buddrw@kgs.ukans.edu>, Coral-List <coral-list@coral.aoml.noaa.gov>,
Jim Hendee <hendee@aoml.noaa.gov>
From: Richard Grigg <rgrigg@soest.hawaii.edu>
Subject: RE: coral reefs doomed -- and the ABH and carbonate saturation

Dear Bob,

Thank you for shedding some more light on your adaptive bleaching hypothesis and as you point out, there is almost a complete absence of hard evidence either for or against the argument. In this regard, I don't have to remind you, that absence of evidence is not evidence of absence (of coral's adaptive abilities). Also, in this regard, I think we can infer more from the fossil record than most of us seem now willing to accept even though the adaptive responses have the benefit of thousand or even millions of years. BUT, over the millenia, there must have been some rapid bursts of sudden change such as the K-T event itself. Stephen J. Gould's view of evolution by punctuated equilibrium is, in fact, based on such bursts of change. And yet, we don't see much extinction in corals at least at the generic or Family level (Re: Veron's work). Doesn't this imply high adaptive ability? Perhaps we need to revisit the fossil record more often and pull in the views of John Pandolfi and Charley Veron (where are you guys?).

Also, while I am at it, let me ask you to shed some of your exceptional knowledge and experience in marine geo-chemistry on the problem of decreasing carbonate saturation state in the world's oceans as a result of increasing co2 globally. I think there is an equally broad pool of misunderstanding about the degree to which existing carbonate sediments in the world's oceans, can serve as a buffer to this effect??? I for one would appreciate hearing your insights on this question. Hope this question does not pose to great a burden but I'm sure the coral reef community will appreciate your views.

Rick Grigg
Dept. of Oceanography
University of Hawaii

Note: *Grigg had Buddemeier's whole message in his original message.* *Buddemeier's message is already displayed above.*

From: "Precht, Bill" <Bprecht@pbsj.com>
To: "'Richard Grigg'" <rgrigg@soest.hawaii.edu>,
buddrw
<buddrw@kgs.ukans.edu>,
Coral-List <coral-list@coral.aoml.noaa.gov>,
Jim Hendee <hendee@aoml.noaa.gov>
Subject: RE: coral reefs doomed -- and the ABH and carbonate saturation
Date: Tue, 18 Sep 2001 17:11:18 -0500

Rick, Bob & the List:

Food for thought...

I had the great fortune to work for the late Ceseare Emiliani of the Univ. Miami about ten years ago... one of the topics we often discussed over a few cold ones was the impact of warm global temperatures on the survival of life in the oceans, especially in the topics...

An interesting paper that may be germane to the argument is by Emiliani,
Kraus & Shoemaker (1981) Earth Planet. Sci. Lett. 55:317-334 - where they
show that about 20% of the late Cretaceous reef-building coral genera
survived an abrupt rise in temperature (about 10 degrees C in just a few
MONTHS) that was related to the mass extinction at the K/T boundary.

What is the important question here - the fact that 20% survived or that 80%
went extinct??

All the best,

Bill

William F. Precht
Ecological Sciences Program Manager
PBS&J
Miami

Note: *Precht had Grigg's whole message in his original message. Grigg's message is already displayed above.*

Date: Wed, 19 Sep 2001 08:32:34 -0400
From: John Ware <jware@erols.com>
Organization: SeaServices, Inc.
To: "coral-list@coral.aoml.noaa.gov" <coral-list@coral.aoml.noaa.gov>
Subject: Coral reefs doomed??

Dear List,

For a quantitative view of the effect of acclimation (or adaptation or
whatever), you might wish to consider the paper that I presented at the
8th ICRS, Vol 1:527-532; "The effect of global warming on coral reefs:
acclimate or die". This was, I believe, the first attempt to quantify
the effect of acclimation rate on the expected response of coral reefs.
In fact, this might have been the first *quantitative* prediction of the
effects of global warming on reefs.

One major conclusion is that even with acclimation rates that would be
considered long by human standards, say 25 - 50 yrs, the chances of
survival of coral reefs are dramatically increased. Acclimation with
such large time constants may not be detectable using currently
available data or experimental methods.

John

(Note: Despite the rather melodramatic title, this paper has repeatedly
been overlooked by even rather meticulous researchers such as Ove. Just
my Cinderella complex showing. jrw)

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****
* *
* John R. Ware, PhD *

* President *
* SeaServices, Inc. *
* 19572 Club House Road *
* Montgomery Village, MD, 20886 *
* 301 987-8507 *
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* |\/__ Undersea Technology for the 21st Century \ *
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Date: Wed, 19 Sep 2001 12:50:38 -0400
From: "Alan E Strong" <Alan.E.Strong@noaa.gov>
To: John Ware <jware@erols.com>
CC: "coral-list@coral.aoml.noaa.gov" <coral-list@coral.aoml.noaa.gov>
Subject: Re: Coral reefs doomed??

John et al.,

Watch our WebSite tomorrow for recent report from Okinawa on 2001 bleaching (they are finally recovering from) and information relative to 1998 recovery from massive event that year.

http://www.osdpd.noaa.gov/PSB/EPS/SST/climohot.html

Cheers,
Al

--
**** <>< ******* <>< ******* <>< ******* <>< *******
Alan E. Strong
Acting Chief, Oceanic Research & Applications Division
Team Leader, Marine Applications Science Team (MAST)
Phys Scientist/Oceanographer
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Note: *Strong had Ware's whole message in his original message. Ware's message is already displayed above.*

From: "Ove Hoegh-Guldberg" <oveh@uq.edu.au>
To: "'John Ware'" <jware@erols.com>, <coral-list@coral.aoml.noaa.gov>
Subject: Climate and corals
Date: Thu, 20 Sep 2001 13:37:00 +1000

Dear John,

Thanks for reminding me (again) about your paper from the Panama meeting, which I have now read. As you know, I tried (in 1999) to go from speculation about climate by interacting with three premier climate modelling groups in Australia, Europe and the USA. This allowed me access to models that simulated important aspects within the climate change debate such as El Nino variability, the impact of aerosols and the forcing due to IS92a greenhouse scenarios. By using several models, I was able to draw on experts in simulating climates and was able reduce the problem of the bias of one model.

As you know (somewhat depressingly), the scenarios for future patterns of bleaching did not different greatly between models. The issue of acclimation and adaptation is complex and I have a few comments that I will send through in a separate email. I feel this debate (as Bob has noted) is useful and will hopefully clear up some of the recent understandings.

Regards,

Ove

Note: Hoegh-Guldberg had Ware's whole message in his original message. Ware's message is already displayed above.

From: "Mike Risk" <riskmj@mcmail.cis.mcmaster.ca>
To: "buddrw" <buddrw@kgs.ukans.edu>,
"Coral-List" <coral-list@coral.aoml.noaa.gov>,
"Jim Hendee" <hendee@aoml.noaa.gov>,
"Richard Grigg" <rgrigg@soest.hawaii.edu>
Subject: Re: Fossil lessons
Date: Thu, 20 Sep 2001 09:44:22 -0400

Hi Rick (-list).

It's hard to concentrate on academic debates with the world in disarray, my office in cardboard boxes, my wife in recovery and my department in ruins. But I will stop whining.

Yes, I could not agree more-the fossil record has a great deal to say about survival and extinction.

We hear a lot about how "resilient" corals are. They aren't.

In general, Phyla are extremely robust. Now that Paleo has done the sensible thing and folded the Archeocyatha into the Porifera, we can observe that no phylum extant in the Cambrian has ever died out. So the trunks of the trees remain, while branches come and go.

Corals have contributed to reefs in varying proportions, from the Ordovician on-but how many Rugosa and Tabulata have you seen on reefs? The real survivors among the Coelenterata are the gorgonians, virtually unchanged since the Ordovician. Along with nereid polychaetes. Perhaps the largest barrier reef in the history of the planet (Guadalupian, W. Texas) is virtually devoid of corals.

Most of our view that corals are robust and omnipresent stems from our experience with Cenozoic reefs, which are well-exposed and preserved in many classical outcrops. Cenozoic reefs experienced three major extinction events: Eocene/Oligocene, Oligocene/Miocene, and Plio/Pleistocene. (See work by Stan Frost, Ann Budd, etc.) The Plio/Pleistocene event was a freeze-out, and not very relevant to what looms. Examination of the Oligo/Mio event, however, is illuminating.

This extinction event was likely caused by a shelf-edge upwelling, bringing in conditions of turbid water and high nutrients. These are the conditions that reefs face now-and I point out that grazing in the Oligocene was unaffected by people. Not even Alley Oop.

Half the corals in the Caribbean died (Edinger and Risk, 1994: PALAIOS 9: 576-598). Some other bad news: bioeroders, primarily filter-feeders, sailed through unchanged: so the balance was severely upset. (I have to point out here that any reef "model" that ignores bioerosion is dealing with less than 50% of the carbonate balance, and hence deserves less than 50% of our confidence.) I suggest that what we are seeing now precisely parallels what the record tells us: massive regional extinctions, shifting of the carbonate balance equation...This event remade the Caribbean coral fauna, reducing it to a fraction of previous biodiversity levels. Although Indo-Pacific representatives escaped the Caribbean event, they have yet to recolonise the Caribbean.

So I suggest that the fossil record allows us to estimate recovery times of reef coral faunas: between 1,000 years (Adey) to >25 million years. You and I won't see it!

Another view from SE Asia: Edinger et al., 2000: Diversity and Distributions 6: 113-127: "...land-based pollution was the primary determinant of coral species diversiity and species occufrrence on reefs."

I continue to be pessimistic. I feel that present fixation of the biological research community is at least partly driven by a reluctance to deal with the real problems: coastal development associated with population increases.

Mike

From: "Jeffrey Low" <jeffrey-low@mailhost.net>
To: "Coral-List" <coral-list@coral.aoml.noaa.gov>
Subject: RE: Fossil lessons

Date: Fri, 21 Sep 2001 07:07:15 +0800

Dear Mike,

Sorry to hear about the disarray in your life .... hope things work out (eventually). I totally agree with you on your last point - in fact, I came across an article in the newspapers on two papers published in Science (Alroy and Roberts) that claim "humans more lethal than climate change". Of course, they were looking mostly at land extinctions caused by human migration in prehistoric times, but the present day loss of coral reefs (and other coastal habitats) are directly related to population growth. I would hazard a guess that if we (ie the human race) can get our population growth under control, much of the existing problems of overfishing, caostal degradation, pollution and greenhouse gases would be drastically reduced or not exist.

What I don't hear much on this list are projects / research being done related to quantifying the human factor in the degradation. Not the blast fishing / cyanide problems, but more of the "if you have x% less people, then the damage will be y% less and restoration can proceed at z% rate". Perhaps some other list has this kind of on-going discussion?

One final comment - all countries seem to run on the thoery that you need to have replacement rates higher than death rates (in the human population) so that (economic) growth can be sustained. Now, if that is the case, doesn't that mean that there is a never-ending spiral of population increase? If I remember my basic biology - this consitutes a positive feedback system .... which will ultimately result in the breakdown of the system (as opposed to a negative feedback, which keeps the system in balance).

Before I end, let me just say that this is just my "coffe-shop" interpretation of the "big picture". I defer to more informaed minds on the subject, and would like to hear more on this. Thanks.

Jeffrey Low
SINGAPORE
Email: jeffrey-low@mailhost.net

Note: *Low had Risk's whole message in his original message. Risk's message is already displayed above.*

From: "Ove Hoegh-Guldberg" <oveh@uq.edu.au>
To: "'buddrw'" <buddrw@kgs.ukans.edu>, "'Jim Hendee'" <hendee@aoml.noaa.gov>
Cc: "'Coral-List'" <coral-list@coral.aoml.noaa.gov>
Subject: Adaptive Bleaching Hypothesis (1)
Date: Sat, 22 Sep 2001 09:41:51 +1000

Dear Bob,

With great respect to you and your colleagues, the effort to discuss the ABH should be seen not as an "attack" but as an attempt to clarify and expand on this interesting area (aka "spirit of debate"). My intention in responding to your broadcast message (Sep 16) was to also clarify the implication that the resistance to the ABH was somehow not on scientific terms. Given the interest in this area, I agree that it is important to keep the discussions open and visible on the coral-list forum.

To begin with, let us put one assertion to rest. You suggest that I have "consistently misstated" your hypothesis. I understand the hypothesis as encapsulated in your own words (Ware, Fautin and Buddemeier 1996) as: "Buddemeier and Fautin (1993) proposed that bleaching is not merely pathological, but is also adaptive, providing an opportunity for recombining hosts and algae to form symbioses better suited to altered circumstances."

To the first issue - recombination involves re-mixing as well as recombining. If part of the ABH involves shifts in the genotype frequencies of populations of pre-existing mixed dinoflagellate symbionts, then I would argue that "re-combining" as a term is not clear (and hence perhaps the greater confusion) and that "remixing" should be included in these descriptions of the ABH hypothesis. I spoke briefly (as I walked out of a talk in Bali) to Daphne about this distinction in regard to the "adaptation" versus "acclimation" (hence the recent reference to the re-mixing genotypes as "acclimation" not "adaptation"). By the way, this is the only time (prior to recent exchanges in September) that we (you, I or Daphne) have corresponded on this issue. I enjoyed the conversation and was unaware of any anxiety.

Secondly, according to your recent email, I need to also recognise the expanded definition of "altered circumstances" to include a changed regime (more frequent and/or intense bleaching events) as opposed to an on-going stress. I have and have no problems with this. It does not remove the problems, however. More on this in a second email to the list.

At the end of the day, however, we are left with a need (8 years after the ABH was first formulated) to go beyond the partial verification of assumptions and theoretical modelling (as per John Ware and co-authors) to the critical testing of this hypothesis. While there has been attempts to test the assumptions in at least one paper, the critical test for this hypothesis is that new combinations of host-symbiont genotypes with greater fitness arise from changed circumstances with respect to bleaching events (be that changing patterns of frequency and/or severity). "The key observations that corals, when heat stressed, expel one variety of zooxanthellae and take on another more heat-tolerant variety while the heat stress is still present, has never been made." (Hoegh-Guldberg 1999). That statement is still correct but does address a restricted set of ABH possibilities. This statement should be more inclusive given the above: "The key observation: that corals after heat stress or a changed sea temperature regime, shift toward more fit combinations of host-symbiont genotype combinations, has never been made." Unless I am mistaken, no observation like this has not been made. I suppose as a biologist, I would expect this to be a visible and obvious feature of coral-dinoflagellate symbioses, especially before and after the substantial selective pressure of recent bleaching events.

In the spirit of scientific debate, I want to also discuss (in detail as you request) your broadcast proposition (Sep 8 2001) that "Bleaching as an

Adaptive Mechanism: A Testable Hypothesis. BioScience, 43:320-326, are looking more solid as experimental tests come in (Kinzie et al in Biol. Bull. earlier this year, Baker in Nature more recently)." As requested, I will "rely on direct quotes in context" but will do this directly in a separate email to the list.

All the best,

Ove

Note: *Hoegh-Guldberg had Buddemeier's whole message in his original message. [Buddemeier's message] is already displayed above.*

From: "Ove Hoegh-Guldberg" <oveh@uq.edu.au>
To: "'Jim Hendee'" <hendee@aoml.noaa.gov>,
"'Coral-List'" <coral-list@coral.aoml.noaa.gov>
Subject: Adaptive Bleaching Hypothesis (2)
Date: Sat, 22 Sep 2001 10:15:07 +1000

Dear Coral-list,

I hope that it is not inappropriate to provoke discussion about this much talked about topic. My sole intention is to explore this important issue. I have chosen to deal with it as a series of carefully defined steps. As will you see, while the theory may have logical appeal, the critical assumptions upon which it is based are either false or unsubstantiated.

Before I begin, a clarification with respect to the biological terms "adaptation' and "acclimation". Adaptation is strictly used to describe genetic changes in a population that lead to genetically based characteristics of that population considered more optimal with respect to the local environment. Acclimation refers to phenotypic change whereby (through changes in gene expression and/or post-translational modification) the characteristics of an organism are made more optimal relative to the local environment. These definitions are held by most textbooks (e.g. Eckert and Randall etc) and are not mutable (as far as I know).

The Adaptive Bleaching Hypothesis (ABH)

In order to proceed logically, exploring the assumptions of the hypothesis makes good sense. These are listed by Ware, Fautin and Buddemeier (1996; Patterns of coral bleaching: modelling the adaptive bleaching hypothesis", Ecol. Modelling 84:199-214). I find this paper useful because it lists the five critical assumptions of the ABH and then builds a logical model from this grounding, the behaviour of which can be compared to nature. As with any model, however, the assumptions (assuming correct logical deductive processes) are critical for the truth of a model (to state the obvious, if the assumptions are wrong, then the model or argument fails).

Summary table (details below):

a.. Assumption 1 = true
b.. Assumption 2 = false at the time scale required

c.. Assumption 3 = true
d.. Assumption 4 = false
e.. Assumption 5 = false if assumption 4 is false

Conclusion (details below):

Critical assumptions 2 and 4 (5 depends on 4) are not currently supported and available evidence (little evidence to the contrary) suggests that they are false. From this analysis, the only conclusion is that the ABH is false.

Details:

What are the assumptions of Ware, Fautin and Buddemeier (1996) and are they true or false?

Assumption 1. "Multiple types of both zooxanthellae and host species commonly exist on a coral reef."

This is true for corals and work by Trench, Rowan, Loh, Baker, Loi, Carter and others have shown that it is true for zooxanthellae (i.e. diversity is high among zooxanthellae).

Assumption 2. 'Some types of zooxanthellae are able to live with more than one host species, and host species may form symbiotic relationships with more than one type of zooxanthella, either simultaneously or serially. The various combinations differ in their adaptation to the environment."

As you will see from the following, this is false at the timescale required. Other critical pieces of evidence do not exist.

What is true: Some types of zooxanthellae (distinguished via rDNA sequences - note - RFLPs do not have enough precision to distinguish species etc) appear in several corals while other coral species have their own dedicated zooxanthella type (Rowan, Wilcox, Baker, Loh and others, Loh et al. in press). Some hosts show several different rDNA sequences associated with their zooxanthellae (Rowan and Powers 1991, Rowan 1998). There is evidence that some zooxanthellae may specialise in high light or low light habitats (e.g. Rowan et al 1997, see also recent papers by K. Michalek-Wagner, A Banazak re: different zooxanthella biochemistries) - and it is likely that various combinations of host and symbiont differ in the type or quality of the environment that they are adapted for. Specific evidence about heat tolerance of different combinations is lacking although Kinzie et al 2001, Iglesias-Prieto and others have some evidence that different isolated zooxanthellae have different heat tolerances (but see Assumption 3 which states that the tolerance of the host-symbiont combination is all important).

What is unknown: How mutable (changed) are these relationships? An important part of this assumption for the ABH is that new symbiotic relationships can form and disband over very short periods of time. Without this rapid, dynamic feature bleaching will not be important mechanism for the evolution of new combinations. If they are not easily mutable then the long-term performance of different strain and host combinations under new conditions and their impact on reproductive success of both partners etc. through reduced energy and other inputs will be more important.

Evidence that this is assumption is largely untrue at the time scales needed: To my knowledge, no lab or field infection experiment using dinoflagellates from other hosts (like those of WK Fitt and others) have ever resulted in a new combination of symbiotic algae and host. In cases where foreign types of zooxanthellae were introduced, populations were eventually replaced by the original type of zooxanthella (see also Kinzie and partners 2001, who also obtained this result with field exposed, completely aposymbiotic anemones). Also - no one has seen a change in the types of zooxanthellae occupied by a coral following a bleaching event (i.e. new combinations arising from a bleaching event). Baker (2001)'s techniques do not have the necessary resolution to answer this question. He sees new bands arise within the zooxanthellae isolated within corals translocated to the shallows. However, he cannot say that the new bands are due to invasion of external zooxanthellae or a case of up-regulation of a small existing population of the particular type of zooxanthellae concerned (he would have to clone his PCR products and verify for a large number of transformed clones that there were no sequences - hence zooxanthellae cells - of the new RFLP band in his corals before treatment i.e. that the change is not a product of acclimation as opposed to adaptation).

Implications: The process of symbiont switching operates at a longer time scale making bleaching irrelevant to the process. This is not surprising if the complex requirements of integrating two genomes into a symbiosis are considered. Research on what is required reveals complex self-non-self recognition (McNeil, P. L., T. Colley, Trench, Hohman, et al. (1981). J. Cell Sci. 52: 243-270, Muscatine, Hohman and others), metabolite transfer and the host of other specific lock-and-key biochemical and physiological interactions. We need to think of transferring zooxanthellae between hosts as partly akin to transplanting chloroplasts or mitochondria between plant species. Remember also that the types of zooxanthellae that occupy different corals are quite separate genetically and may represent different species or even genera (Trench, McNally et al. 1994 and others) - hence are likely to have a large suite of different requirements and features that have to be integrated (evolved) in order for a symbiosis to function. Adopting life within another cellular environment is not trivial and may involve many coordinated changes in genetic makeup (aka it is not simple to swap from one host to another - hence this process is likely to constrained in terms of evolutionary speed).

If new zooxanthellae types cannot invade easily, then the ABH is restricted to the dynamics of the zooxanthella populations of a subset of corals which already have multiple strains of zooxanthellae in their tissues. That is, new combinations do not form "easily" (at the very least, they probably form over decades to centuries but not over the days and weeks required by the ABH). At this point, we are left with changes that occur in the relative frequency of existing genotypes within a coral. These are pre-existing genetic combinations. The question at this point becomes, is this "adaptation" or "acclimation"? At first cut - one might call this is "adaptation" because there is a change in the frequency of genotypes within the total zooxanthella population of an geographic area.

This is wrong, however, as populations of zooxanthellae within a host are largely clonal (asexual) populations of single individuals. If this is the case, then a multi-strain coral host is really an association of three or more individuals (the coral host individual, and 2 or more zooxanthella individuals). The change in the relative proportions of one zooxanthellae individual over another within a host is then a matter of a change in the size of individuals. This then is a phenotypic (acclimatory) not genotypic (adaptive) change. Being multistrained and responding to changed circumstances, then, is no different to a association that having a set range of phenotypic responses with definite limits (there is no such thing as unlimited acclimation). Perhaps in evolutionary time (at least decades to centuries), the switching of symbionts may allow a certain flexibility that is not inherent within a single genome. But the time scale and process do not involve bleaching (adaptive or acclimatory).

Assumption 3. "The upper temperature limit beyond which the symbiosis is disrupted is characteristic of the host-symbiont combination rather than of the host or symbiotic alga alone."

This is probably true given the highly integrated nature of symbiosis. Specific thermal tolerances of corals/zooxanthellae associations and their variance with thermal regimes were largely first identified by Steve Coles and Paul Jokiel. Many recent studies (Goreau, Strong, Hayes, Brown) culminating in the SST and HotSpot work by NOAA and others. New work by Ray Berkelmans (in press) further confirms that thermal tolerances vary on a geographic basis with water temperature.

Assumption 4. "Bleaching provides an opportunity for the host to be repopulated with a different type of partner."

This is unproven and most evidence suggests that it is false. As I have repeatedly stated, we have yet to see a single experiment that shows that a bleaching event or set of disturbances results in a change of the type of symbiont with corals (during or after). No one has evidence of a more fit recombination of host and symbiont as a result of changed circumstances. Even the recent Kinzie el al (2001) study with aposymbionts of the sea anemone (Aiptasia) found that they did not take up new types of zooxanthellae. Apart from the problem of having very limited genetic resolution due to limitations of the RFLP technique (same problem as with AC Baker's 2001 study), Kinzie and co.'s aposymbiotic anemone hosts only became infected by the original type (B) of zooxanthella (To quote them: "All Aiptasia that became infected when exposed to natural seawater were found to harbour clade B, which is the zooxanthellar clade normally found in this anemone").

Unfortunately for the ABH, other observations militate against this assumption being true:

Firstly, corals that appear totally white still have many zooxanthellae in their tissues (e.g. Hoegh-Guldberg and Salvat 1995 - bone white corals ranged as high as 1.0 x 104 cell/cm2). These are probably the source of repopulation of corals by zooxanthellae in the event of recovery after bleaching. If competition by the original zooxanthellae is so effective (i.e. "originals" win every time according to WK Fitt, D Schoenberg and others who have done the rigorous experiments in this regard), then it would appear that this is a major obstacle to the idea that "bleaching provides an opportunity for the host to be repopulated with a different type of partner." That is, bleaching does not make a coral or other cnidarian host an open slate. The inherent algae in recovering corals probably will always have the upper hand.

Secondly, as stated above, no one has seen a single case of bleaching providing "an opportunity for the host to be repopulated with a different type of partner". If this were a major forcing function within the evolution of coral reefs, shouldn't we see large scale examples of this? William Loh from my lab has been searching for changes in rDNA sequence types of zooxanthellae with corals and reefs after bleaching events in Okinawa with his Japanese colleagues. What he has seen is potential selection against some zooxanthella genotypes and associations (their coral host species died out) but never the advent of a new association of host and symbiont. That is, on the short term scales of bleaching events, William has seen a diminishing not increasing stock of combinations (not good for adaptation as you will appreciate). At risk of repeating myself, the advent of new combinations probably requires a longer time period (because of the biochemical complexities of symbiosis) than the few generation times required. See above.

An added assumption is added by the authors under assumption 4. They state: "We assume no mortality of bleached corals, regardless of the severity of bleaching or whether there is a zooxanthella type with which the coral is compatible under the existing temperature conditions."

I assume that this addition is a condition for the computer model to work. In the face of overwhelming field evidence, this is simply false (GCRMN, Wilkinson and many others). A model that requires this falls over heavily at this point. Perhaps John can explain how critical this element is and how dependent the ABH is on it.

Assumption 5. "Stress-sensitive combinations have competitive advantages in the absence of stress, which implies a reversion to stress-prone combinations under non-stressful conditions."

This remains unknown. However, if we haven't seen assumption 4 holding true (i.e. that bleaching leads to new fitter combinations), then we obviously don't have assumption 5 (the reversion of these combinations in periods of non-stress) in the bag.

In conclusion:

The ABH has more than a few problems in terms of the stated assumptions and should be discarded. It was a "nice" idea but now is largely falsified through the fact that critical assumptions like 2 and 4 above are (at the very least) false.

I hope that this helps progress the ABH debate in a positive way. I am very interested in engaging in discussions over the details above. Most of all - I want to strongly emphasize that this is not an attempt to denigrate the ABH authors but more an attempt to improve our understanding of mass bleaching by critically examining important ideas and suggestions. I am aware that coral-list members may have much to add and that I probably have not done justice to all authors (if there are critical pieces of literature, please bring them to the list's attention).

Regards to all,

Ove

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Date: Sat, 22 Sep 2001 20:48:36 -0500 (CDT)
From: FAUTIN DAPHNE G <fautin@falcon.cc.ku.edu>
To: coral-list@coral.aoml.noaa.gov
Subject: The Adaptive Bleaching Hypothesis

Dear Coral-Listers,

I am taking this opportunity to respond to several recent messages concerning the Adaptive Bleaching Hypothesis (ABH) that was proposed by Bob Buddemeier and me, and then modeled by John Ware, with input from us. I helped formulate the ABH because I am eager to understand the symbioses. I am writing now because I perceive some of the recent exchanges ostensibly concerning the ABH deal with matters that are not part of the ABH and thus do not advance that understanding.

The ABH was our deduction from experimental results and empirical observations that had been published at the time we developed it; those data and what they contributed to the ABH are detailed in our publications. Thus it is not true, as one lister recently asserted, that there is no evidence for the ABH.

The writers of some recent messages seem to regard the ABH more as a law than a hypothesis. In framing it as "a testable hypothesis," we recognized that additional data could prove to be inconsistent with our inferences about the workings of zooxanthellae symbioses, entirely or in part. Thus, in the manner that science works, falsification would result in more refined hypotheses being advanced and tested, gradually improving our understanding of the symbioses. In a recent message in which he claimed falsification of some of the five critical assumptions of the ABH, Hoegh-Guldberg advocated "discarding" the ABH. What I seek in combination with data that are truly inconsistent with the ABH are second-generation hypotheses that take into account the new data - using the parts of the ABH that work, and substituting for the unworkable parts. More importantly at this juncture, I am not persuaded that those assumptions have been falsified.

The ABH was not meant to apply to every instance of bleaching. By way of analogy, that natural selection is not the only selective force in evolution does not falsify natural selection. To take one clear example, some stresses that result in bleaching are lethal, to some or all the bleached corals, and so, obviously, the ABH is irrelevant in such instances. This is why we confined the models of Ware et al. to non-lethal stresses. (Hoegh-Guldberg correctly inferred this is not an assumption of the ABH but a condition under which the model was run, so I am puzzled why he even raised it; it is irrelevant to the substance of the ABH.)

We did propose "that bleaching is not merely pathological, but is also adaptive, providing an opportunity for recombining hosts and algae to form symbioses better suited to altered circumstances" (Ware et al. 1996). We also recognized that the organisms might be unable to take advantage of such an opportunity. For example, even with sublethal stresses, in places with low zooxanthellae diversity, a new combination would be unlikely. And superior combinations might not form by chance, for the hypothesized recombination is a stochastic - not a deterministic - phenomenon. We also explicitly stated that the ABH applies to the level of bleaching under which the symbiosis evolved -- what has been considered "background" - and that a mechanism that evolved under that level may not be adaptive if what we are now experiencing is as unprecedentedly severe and widespread as some believe (which is consistent with what Hoegh-Guldberg reported has been found in Japan).

The "replacement" zooxanthellae, according to the ABH, can be either exogenous or endogenous. At the time we formulated the ABH, an endogenous source was thought by many experts to be impossible, since it was then considered that any cnidarian polyp or colony would harbor only one "strain" of zooxanthellae. We inferred from the published literature that "strains" could coexist, and so saw a proliferation of one "strain" at the expense of another to be a possible response to altered circumstance. We now know that multiple "strains" can coexist. Thus the comment that "Baker (2001) cannot say that the new bands are due to invasion of external zooxanthellae or a case of up-regulation of a small existing population of the particular type of zooxanthellae concerned" is not germane to the ABH - either alternative supports it. The exogenous source is the surrounding water, and therefore ultimately are zooxanthellae in their free-living stage or those were released under stress. Whether those that leave in the bleaching process are viable, much less infective, was raised in the original publication as a matter to be investigated; it has not, to our knowledge, been resolved. Thus criticisms such as that of Hoegh-Guldberg (1999), "The key observations that corals, when heat stressed, expel one variety of zooxanthellae and take on another more heat-tolerant variety while the heat stress is still present, has never been made," misrepresent the ABH and thus do not test its tenets.

The preceding quote and several recent list messages have focused on thermal bleaching. This is not a requirement of the ABH, which was proposed to operate as a result of any stress or combination of stresses that provoke bleaching.

Hoegh-Guldberg began a recent message with 'a clarification with respect to the biological terms "adaptation" and "acclimation."' I am uncertain how this comment relates to the debate. We have tried to be consistent in application of those terms - see papers in the recent "American Zoologist" volume concerned with how coral reefs adapt, acclimate, and acclimatize (especially that of Gates). Hoegh-Guldberg's definition of adaptation as "genetic changes in a population that lead to genetically based characteristics of that population considered more optimal with respect to the local environment" is the sense in which we created the ABH. For we explicitly regard the zooxanthella-host complex as an ecological entity that is not the sum of its parts (an additive model was used by Ware et al. to be mathematically tractable, but its departure from our concept was made explicit). Thus, in the ABH, under identical circumstances, a species of coral with one "strain" of zooxanthellae might be maladapted but well adapted with another. This seems to be substantiated in patterns of "strains" of zooxanthellae that live in shaded and lighted portions of a single coral colony, and of "strains" of zooxanthellae that live in shallow and deep colonies of a single species of coral. Part of the decision on whether to use the pigeon-hole "adaptation" or "acclimation" that Hoegh-Guldberg raises may depend on one's concept of who is "in charge" in the symbiosis - if the animal is making a selection, it may be nearer the "acclimation" end, whereas if the zooxanthella is choosing a suitable home, it may be nearer the "adaptation" end.

In his message, Hoegh-Guldberg disputed the mutability of host-zooxanthella combinations on the time scale required for the ABH to operate. Our inference that the change could happen was based on experiments such as those of Fitt cited by Hoegh-Guldberg, who stated "To my knowledge, no lab or field infection experiment using dinoflagellates from other hosts (like those of WK Fitt and others) have ever resulted in a new combination of symbiotic algae and host." In fact, we interpreted Fitt's data (and those of Kinzie and Chee) as showing that new combinations could be established in short order - although allochthonous zooxanthellae did not establish in all hosts, some did so temporarily, and others remained longer. Hoegh-Guldberg continued "In cases where foreign types of zooxanthellae were introduced, populations were eventually replaced by the original type of zooxanthellae." As we wrote in the original BioScience paper, because the scientists controlled conditions to minimize stress on their experimental subjects, those experiments were conducted under laboratory conditions that were known to be suitable for the subjects - which are those in which the "native" zooxanthellae-host combination is favored. Thus a reversion to the pre-existing combination is precisely what would be predicted by the ABH. The recently published experiment by Baker put corals into situations that persisted - and his results are also consistent with the ABH.

Hoegh-Guldberg's comment "Also - no one has seen a change in the types of zooxanthellae occupied by a coral following a bleaching event (i.e. new combinations arising from a bleaching event)" is beside the point in the debate over the ABH for several reasons. I stated one above - unless the stress that produced the bleaching persists, the pre-existing combination will be favored, so no change is to be expected. A practical one is being able to know what the situation was before the stress and what it is afterward. For we are searching for changes in an entity that, until very recently, was viewed by most people as unitary (that is, there was one "strain" of zooxanthellae) and we do not yet know the extent of the diversity because we do not yet know what differences might exist. Part of our proposing the hypothesis was to encourage scientists to find ways to distinguish the members of this all-important symbiosis, individually and in combination. Moreover, the ABH does not require that every "strain" of zooxanthellae be capable of living in every host species - we explicitly modeled the ABH on there being generalists and specialists on both sides of the symbiosis (just as there are anemonefish and host sea anemones - in the former case belonging perhaps to two genera, in the latter certainly to three families). I, for one, do not "think of transferring zooxanthellae between hosts as partly akin to transplanting chloroplasts or mitochondria between plant species" - a bit of evidence that clearly shows zooxanthella symbiosis is a less well integrated one is the phenomenon of bleaching itself. The possibilities Hoegh-Guldberg raises with the comment "the types of zooxanthellae that occupy different corals are quite separate genetically and may represent different species or even genera (Trench, McNally et al. 1994 and others) - hence are likely to have a large suite of different requirements and features that have to be integrated (evolved) in order for a symbiosis to function. Adopting life within another cellular environment is not trivial and may involve many coordinated changes in genetic makeup" provide grist for investigation, but do not constitute falsification of the ABH.

We inferred that "stress-sensitive combinations have competitive advantages in the absence of stress, which implies a reversion to stress-prone combinations under non-stressful conditions" to account for the continued existence of combinations that are vulnerable to conditions that recur (such as the annual bleaching Jokiel and others found in Hawaii, and that Fitt has more recently documented in Florida). Otherwise the system would be ratcheted to increasingly stress-resistant combinations with a time course that would seem too rapid for any other known mechanism. Using this assumption, Ware was able to create a model that bears remarkable resemblance to the time course of actual bleaching events.

I look forward to advancing understanding of bleaching and its consequences though well-crafted experiments that are published in the peer-reviewed literature.

Sincerely,
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direct to database of hexacorals, including sea anemones, released 12 July 2001
*** http://www.kgs.ku.edu/Hexacoral/Biodata/ ***

Date: Wed, 26 Sep 2001 17:01:03 -0500

From: "Bob Buddemeier" <buddrw@kgs.ukans.edu>
To: Richard Grigg <rgrigg@soest.hawaii.edu>
CC: Coral-List <coral-list@coral.aoml.noaa.gov>,
Jim Hendee <hendee@aoml.noaa.gov>
Subject: Re: coral reefs doomed -- and the ABH and carbonate saturation

Rick and not-quite captive audience –

I'll answer your questions/comments in reverse order. As far as I know there is no published/refereed statement of the putative effects of Mg-calcite on reef calcification, so it will have to be what I think about what I think it is.

I. As I understand what I will call the Magnesium Salvation Theory (MST for a convenient shorthand), it goes something like this:
1. There is a lot of magnesian calcite in the (low-latitude) carbonate sediments of the world ocean.
2. High-Mg calcite is more soluble than aragonite.
3. As saturation state and pH of the surface ocean drop as a result of anthropogenic CO_2 additions (or for any other reason), high-Mg calcite will dissolve before aragonite does, buffering the surface ocean carbonate saturation state.
4. Therefore concerns about the effects of lowered carbonate saturation state on calcification by corals and coralline algae are not warranted.

Points 1-2 are valid, point 3 is valid in principle but questionable in practice, and the extension to point 4 isn't valid. For the MST to work, two conditions would have to obtain:
a. The saturation state at which the high-Mg calcite buffers the surface water would have be high enough to avoid negative calcification effects, and
b. The equilibration (that is, dissolution kinetics) would have to be rapid on the 50-100 year time scale of anthropogenic CO_2 additions.
Neither of these two conditions will be met.

Since Greek letters do not translate to text files, I use OM in place of Omega, the saturation index (where 1 = solid-solution equilibrium, larger numbers = supersaturation, and smaller numbers = undersaturation). OMh= saturation state of high-Mg calcite, OMa= saturation state of aragonite. OMc= saturation state of calcite.

1. Considering point a above:

Aragonite is more soluble than calcite and the ratio of their saturation states is well-known: to 2 significant figures, OMc/OMa is 1.5. High-Mg calcite is a little less precisely definable because it is not a well defined molecule, but rather a range of solid solutions (0-30 mole % $MgCO_3$ is stable, <8% has little or no effect on calcite solubility, 11% has approximately the same solubility as aragonite), we will be close enough to use the value of OMa/OMh = 1.3-1.5.

Essentially by definition, chemical dissolution does not occur at all above a value of OM = 1. We can see that when high-Mg calcite would first start dissolving, OMa would be 1.3-1.5 or less. If we consider the modeled results of Kleypas, J.A. et al., 1999. Geochemical consequences of increased atmospheric carbon dioxide on coral reefs. Science, 284(2 April 1999): 118-120 (figure 1C), we see that the most extreme and extended prediction is for an average tropical surface ocean OMa of >1.5 in the year 2100. It is this prediction on which the predictions of calcification decline are based, and all of the projected calcification effects occur before there could be any large-scale dissolution of high-Mg calcite – hence, no salvation by magnesium.

2. Relevant to both points a and b:

Equilibrium is defined as the net balance between forward and back reactions (in this case precipitation and dissolution). Not only the fact that the surface oceans are strongly supersaturated with respect to calcite and aragonite, but also a great deal of experimental work testify to the extremely limited occurrence of inorganic (as opposed to biogenic) precipitation. Reaction kinetics are strongly hindered and absolute rates are very slow, almost certainly due to the occlusion of mineral surfaces by organics and/or less soluble mineral phases. Chemical symmetry raises the question of why we would expect the surface ocean saturation state to be controlled by mineral dissolution in the near future when it is not currently controlled by mineral precipitation

This is probably the point to insert the qualifying comment that organisms are constrained by environmental chemistry, but not absolutely controlled at the rates and/or equilibria of inorganic chemistry (that is, they may be able to get around some aspects of thermodynamics, but they are stuck with ultimate conservation of mass and energy). The observations to date indicate that zooxanthellate corals and coralline algae exhibit high rates of calcification at OMa >4, and that most species show significant declines at levels that are still supersaturated but well above 1.

3. Relevant to point b:

Apart from the micro-scale inhibition of dissolution and precipitation at the carbonate surface, there are macro-scale advective issues that reduce potential reaction rates. The large inventory of Mg-calcite in the world sediments is mostly buried. Only the top few cm (in high energy environments) or mm (in low-energy environments) is in any kind of well-exchanged contact with the overlying water. Below that, pore water residence times rise exponentially. Interstitial pore water in reef systems is normally (or at least often) controlled at the saturation state of high-Mg calcite, with the help of biogenically mediated solution or precipitation, but the volumetric exchange of this water with the overlying water is extremely slow compared to both surface layer mixing and the physical and biological processes acting in the open water and at the air-sea interface to maintain the (super)saturation state there. Empirical evidence for this is that the Holocene reef sediments (up to 8000 years in age) are neither flushed of high-Mg calcite by dissolution, nor totally locked up by diagenetic cement formation. And, there is no reason to expect a major change in pore water residence times in the near future.

Another comment or two – the one place in the ocean where you do see reasonably prompt responses of saturation equilibria is in the lysocline-carbonate compensation depth region. This is far below the mixed layer, and is driven by organic/carbonate ratios in the sedimentary rainout – all of which, in the pelagic world, have much higher specific surface areas and therefore reaction rates than the big, organic-rich lumps on a reef. The reason that the surface ocean can maintain its saturation disequilibrium so well is that the mixed layer is rather strongly compartmentalized in terms of its dissolved constitutents (as opposed to particulates, which can fall through the pycnocline). And, since the exchangeable carbon inventories of the mixed layer and the atmosphere are similar in size, and air-sea exchange keeps them nearly in equilibrium, surface ocean response to CO2 input to the atmosphere is prompt and substantial.

Recommended or suggested reading (sorry if this seems egocentric, but obviously it's easiest for me to remember and judge relevance of what I've been involved in, so there are a thoroughly disproportionate number of Buddemeier things):

Morse, J. W. and Mackenzie, F. T., 1990. Geochemistry of Sedimentary Carbonates. Elsevier, Amsterdam, 707 pp.
Gattuso, J.P., Allemand, D. and Frankignoulle, M., 1999. Photosynthesis and calcification at cellular, organismal and community levels in coral reefs: A review on interactions and control by the carbonate chemistry. American Zoologist, 39(1): 160-183.
Kleypas, J.A. et al., 1999a. Geochemical consequences of increased atmospheric carbon dioxide on coral reefs. Science, 284(2 April 1999): 118-120.
Kleypas, J.A., Buddemeier, R.W. and Gattuso, J.-P., 2001. Defining 'coral reef' for the age of global change. International Journal of Earth Sciences, 90: 426-437.
Kleypas, J.A., McManus, J.W. and Menez, L.A.B., 1999b. Environmental limits to coral reef development: Where do we draw the line? American Zoologist, 39(1): 146-159.
Tribble, G.W., Sansone, F.J., Buddemeier, R.W. and Li, Y.-H., 1992. Hydraulic Exchange between a Coral Reef and Surface Seawater. Geological Society of America Bulletin, 104: 1280-1291.
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Buddemeier, R.W. and Oberdorfer, J.A., 1988. Hydrogeology and Hydrodynamics of Coral Reef Pore Waters. In: J.H. Choate et al. (Editor), Proceedings, 6th Int. Coral Reef Symp., Townsville, Australia, pp. 485-490.
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II. ABH –

I think, and sincerely hope, that Daphne's recent response will have clarified the issues. Most of the so-called debate or criticism has consisted of other people redefining or misinterpreting our statements and then claiming that there is something wrong with the concept on the basis of their revision.

Related to your comments – one of reasons for proposing the existence of an adaptively flexible multilateral symbiosis was precisely the points you make – long taxon lifetimes in both corals and algae, in combination with an obligately variable preferred habitat and no particular evidence of high extinction rates. The ecospecies concept preserves the benefits of very rapid adaptation (of the symbiotic combination) in the presence of the other features.

I thought it might be good to get the idea as close to a one-liner as possible – a brief synopsis:

The question is: Can the application of stress (any stress or combination, not just warm water) that results in a diminution of the pre-existing population of endosymbionts (a.k.a. bleaching) lead to a change (from either endogenous or exogenous sources) in the balance or nature of the symbiont types that results in an increase in the fitness of the host-symbiont complex (ecospecies) with respect to environmental stresses?
We hypothesized (on the basis of very real hard, if indirect evidence) that the answer is yes, and proposed some tests. We consider both the indirect and the direct evidence emerging since then to support, but certainly not to 'prove' the hypothesis.

Bob Buddemeier

--
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Note: *Buddemeier had Grigg's whole message in his original message. Grigg's message is already displayed above.*

From: "Mike Risk" <riskmj@mcmail.cis.mcmaster.ca>
To: "buddrw" <buddrw@kgs.ukans.edu>,
"Coral-List" <coral-list@coral.aoml.noaa.gov>,
"Jim Hendee" <hendee@aoml.noaa.gov>
Subject: Re: coral reefs doomed for sure.
Date: Thu, 27 Sep 2001 22:02:02 -0400

Bob, List-Some comments re the general discussion of changes in alkalinity, dooming of reefs, etc.

Some of the following builds on previous postings on this list, and some amounts to a Discussion of the Kleypas et al 1999 Science paper. I was going to write a formal Reply to this, never got around to it...

In general, my reservations about some of your positions are based on my belief that there has been insufficient consideration of two of the big Bio's in reef science: bioturbation and bioerosion. In addition, I have reservations about some of the chemical models/assumptions.

1. Bioerosion. The first quantitative work on the importance of bioerosion was published so long ago only me and Hendee were alive. Since then, there have been several large, exhaustive and exhausting studies of this signal process, and they have all come up with the same answer: on "normal" reefs,

bioerosion and calcification are in approximate balance. On most fringing reefs, subject to increasing terrestrial nutrient input, therefore, the balance has already been shifted towards destructive processes. I will cite no references here. Knowledge of bioerosion should be an integral part of every reef scientist's knowledge base. In short, looking at corals is way less than half the picture: you should all know this.

Unfortunately, this field seems to have fallen off the radar screen in the past few years: in the Amer. Zool. 1999 volume, for example, the word does not appear once. (Stop for a moment, and think of the gaping hole in our understanding that this reflects...) If it weren't for the French, there would be virtually no ongoing research on this process. (Salud, mes amis...et amies.) Any "reef monitoring" program that does not include assessment of bioerosion is a colossal waste of money-and I know of only one that does. Not only does this ignore most of the action-it excludes some prime bioindicators.

Any "reef model" that does not include it...it's hard to be polite, here. These models would better be termed "Less-than-half-of-the-reef models."

2. Bioturbation. Again, an exhaustive literature-lagoon and shelf sediments are vertically mixed on a timescale measured in months. Any number of critters involved here, of which the front-runners (in the Cenozoic) would be the thalassinid shrimp.

3. Oceanic/Climate Models. Notwithstanding their protestations to the contrary, I have found modellers to be resistant to data that upset their models, with that resistance being directly proportional to the amount of federal money invested to date. "One major problem with the current generation of GCM's is that the treatment of ocean circulation is still very crude." (Ruddiman, 2001: Earth's Climate).

The implications of Smith et al, 1997, are that a meltwater pulse can divert or shut down the Gulf Stream in less than 5 years. To all of you out there: when the oceanic part of GCM's can model this, then start believing them-not before. The strong compartmentalisation of the mixed layer to which Bob refers is metastable, and temporary.

4. The Magnesium Salvation Theory-sort of reads like a cure for constipation, doesn't it? Stick to science, Mike.

While I concur with some of what Bob says here, re porosity of reefs and reef sediments, I am not wholly persuaded:
-"...high magnesian calcites are dissolved preferentially in these sediments, although the sediment contains a mixture of (all types of carbonates). In deposits composed primarily of red algae, this early diagenetic reaction has resulted in dissolution of 75% of the carbonate." (Morse and Mackenzie, 1990: Geochem of sedimentary carbonates).
-"The data indicate that all samples are very close to equilibrium with Mg-calcite....alkalinity shifts relative to sea water indicate that initial precipitation may be followed by gradual dissolution in response to CO2 added..." (Buddemeier and Oberdorfer, 1986).
-etc etc. And finally, Bob Halley and his USGS colleagues have done some very nice experimental work, some of which was reported in Bali, showing that, indeed, HMC dissolves.

As far as the large inventory of HMC being buried-I think Callianassa and its cohorts have a great deal to say about that. Ain't going to happen. The sediments that reefs will produce in future, moreover, will likely be lower in relative concentration of HMC. The main contributors of HMC are the calcareous algae-CCA. As we eat the grazing fishes, and the urchins die off, and fleshy algae bloom in eutrophied coastal waters-reef seds will likely be higher in organics and lower in HMC.

Some other points, perhaps more peripheral: high pH's have been recorded inside coral heads-indeed, pH's at which silicates are very unstable (Risk and Muller, Middle Holocene, Limnol. Oceanogr.-give me a break, I have only unpacked the first of 20 boxes of books). This will triggger dissolution of reactive silicates-in fact, the pH inside corals probably shifts 3-4 full units, making possible all sorts of neat chemistry. Don't forget, the sediments being delivered to the world's coastlines now are very different from pre-agricultural times. Now, we see reactive silicates-andesitic ash from 5-year-old falls, delivered to the coastline by rivers, may be seen hydrating and dissolving under 10-odd cm of carbonate sediments, at several locales in Indoensia. This is not a millenial timescale.

So, in short, Kleypas et al:
1. depends on reef models that ignore >50% of the process
2. depends on outmoded oceanic circulation models
3. ignores some fundamental chemical questions.

Other than that-we have to admit that it was an important paper, because it has stimulated a great deal of discussion. From that standpoint, congratulations to the authors. (Most of my papers disappear as neatly and as quickly-and as deeply- as Olympic springboard divers.)

My main concern with that paper is that it may have diverted intellectual and financial resources from more pressing problems. Sure, changes in saturation state will eventually affect....what? What will be left, in say 100 years? pH changes in the ocean, in my opinion, don't make the Top Twenty Reef Threats. The rate of present destruction from land-based sources and overfishing simply dwarfs everything else.

But we have three predictions running, now: I say (something like) "reefs, as some of us knew them, will be gone from most coastlines by 2020." Rupert Ormond says 50 years. Kleypas et al say a century. I hope to God they are right-but I don't think so. In fact, the reason I felt able to make that dire duo-decadal forecast is: it's already come true.

I hesitate to enter the discussion about ABH-not because of ignorance (that has not worked in the past), but because Ove's doing a pretty good job stirring this pot. It seems to me that there might be some help, again, in the fossil record. One would assume that corals would adapt to rising temperatures (perhaps better than falling ones?). I am afraid, however, that my knowledge of the record isn't good enough, nor are the temperature data. Sea-surface temperatures are believed to have gone well above 30 in the Mid-Cretaceous, and mid-Cretaceous "reefs" (piles of rudists, really) are very low in corals...but this is far from conclusive. Perhaps one could look more closely at rudists, which had zooxanthellae, same as does Tridacna...corals, of course, have had zoox since the Paleozoic (Risk et al, Early Holocene, same excuse).

The other problem with the record is the paleotemperatures. Planktonic forams give excellent results, for the open ocean. We really need shelf data-but many reports in the literature of paleotemperatures from benthic shelf critters are just not dependable. The problem is, the six people in the world who really understand KIE don't publish enough, and those that don't, publish too much. So this remains an open, and intriguing, question.

On another note: I have to apologise to the List for exposing some of my personal affairs. That was forgivable only given my state of mind at the time. Nonetheless, several people whom I had never met sent condolences and best wishes! So-thank you, and it will never happen again.

She has gone from
liquid food-IV drip, to
liquid food-juices, to
solid food-mushy stuff, to
liquid food-gin and tonics. So recovery is well under way.

Mike

Date: Fri, 28 Sep 2001 11:55:34 -0500
From: "Bob Buddemeier" <buddrw@kgs.ukans.edu>
To: Mike Risk <riskmj@mcmail.cis.mcmaster.ca>
CC: Coral-List <coral-list@coral.aoml.noaa.gov>,
Jim Hendee <hendee@aoml.noaa.gov>
Subject: Re: coral reefs doomed for sure.

Mike,

Thanks very much -- you raise good points for discussion, and I think this is an area where real (as opposed to definitional) debate can and should be developed. You obviously feel about bioturbation and bioerosion much as I do about pore-water dynamics -- and clearly the two have to meet up somewhere at the budgetary scale. So, let's see if we can get there.

But first, to aid in the determining just what the topic/discussion thread is -- you addressed issues related to my point #3 (heavily) and #2 (somewhat). However, if my point #1 is not in contention, then this is probably a new start and not part of the "are reefs doomed" thread -- that point stated that due to the solubility products/saturation indices of the various carbonate minerals, in combination with the observed effects of reduced saturation state on coral-algal calcification and the projected/modelled saturation state changes, the question of whether or not high-Mg calcite buffered the surface ocean would be moot, because any such buffering would be at a saturation state below that which would produce the projected calcification effects over the next century.

So -- do you buy off on that? Or does anyone else in the audience have doubts/comments on that? That's probably the first point to dispose of; if that's not an issue we can move on to the sediment biogeochemstry questions as a separate topic.

Bob Buddemeier

Note: *Buddemeier had Risk's whole message in his original message. Risk's message is already displayed above.*

Date: Mon, 1 Oct 2001 13:45:42 +0200
To: coral-list@coral.aoml.noaa.gov
From: "christine.schoenberg" <christine.schoenberg@mail.uni-oldenburg.de>
Subject: coral reefs - calcification and bioerosion

Dear list,

just a few comments on Mike Risk's latest letter, from a bioeroding sponge worker's point of view:

>they have all come up with the same answer: on "normal" reefs,
>bioerosion and calcification are in approximate balance. On most fringing
>reefs, subject to increasing terrestrial nutrient input, therefore, the
>balance has already been shifted towards destructive processes.

This matches my own experiences when working on the Central Great Barrier Reef, where the balance may still be better than most other places. We still need to keep an eye on it though.

The common sponge Cliona orientalis reacts to elevated nutrient conditions. _Extreme_ situations may have negative effects, however, so that the sponge's growth is slowed. Bioerosion of this sponge appears to be enhanced by a higher concentration of nutrients. This is a sponge, which is just everywhere on Australian (and other Pacific) inshore reefs, which grows over large surfaces, several m in diameter and which is able to invade live coral.

Another thing I would like to mention: this sponge also contains zooxanthellae, as do some other successful, competitive bioeroding sponges. Cliona orientalis bleaches under extreme conditions (evidence from the aquarium), but during the 97/98 bleaching on the GBR all sponge colonies I knew survived just nicely (in contrast to most corals on my sample site). Revisiting my site at Orpheus Island end of 2000 showed me a reef much reduced in live coral cover and coral diversity, but the bioeroding sponges did very well and seemed much increased in their abundance (no quantification done).

Just some food for thought...

Cheers, Christine

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Date: Mon, 01 Oct 2001 09:36:40 -0500
From: "Bob Buddemeier" <buddrw@kgs.ukans.edu>
To: "christine.schoenberg" <christine.schoenberg@mail.uni-oldenburg.de>
CC: coral-list@coral.aoml.noaa.gov
Subject: Re: coral reefs - calcification and bioerosion

All,

Christine's comment raises some points that relate back to Mike's comments and the whole issue of CO2 and carbonate balance. It is important to distinguish between net and gross bioerosion and among the various functional components of bioerosion --
1. chemical erosion, which returns solid carbonate to dissolved inorganic carbon and is the only kind that is directly involved in CO2 and acid-base considerations; and,
2. mechanical/physical erosion, which reduces the integrity and grain size of solid features (of greatest concern, reef plates and lithified substrate), and which can have two different outcomes:
a. change in the structure, relief, and distribution of grain sizes on the reef itself; or
b. loss of carbonate material from whatever we choose to define as the reef system.

The two forms are related -- a minor amount of chemical erosion can precipitate physical breakup on a much larger scale, and smaller grains resulting from mechanical (bio)erosion have a higher surface-to-mass ratio that facilitaties dissolution, especially in porewater environments.

I assume that discussions of the balance between production and bioerosion are referring to a gross balance that includes all forms of bioerosion -- if not, straighten me out on the conventions in the field, please.

Note that I'm using 'grain' in the geographic sense of granularity, not in the colloquial sense of 'something small.'

All of these, plus the related issue of import of carbonate from elsewhere to a specific reef system, are aqddressed in conceptual models presented by Kleypas, J.A., Buddemeier, R.W. and Gattuso, J.-P., 2001. Defining 'coral reef' for the age of global change. International Journal of Earth Sciences, 90: 426-437.

I hope this clears up the point Mike addressed about carbonate models that do or do not include bioerosion. A carbonate budget model of a reef system has to include bioerosion, but a calcium carbonate production or calcification model addresses the gross input to that system. The CO2-caclification models are production models, not total budget models, which require local/regional inpout and calibration, as suggested in the reference given above.

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Note: *Buddemeier had Schoenberg's whole message in his original message. Schoenberg's message is already displayed above.*

Date: Tue, 02 Oct 2001 10:24:15 +1000
To: <coral-list@coral.aoml.noaa.gov>
From: Katharina Fabricius <k.fabricius@aims.gov.au>
Subject: Are coral reefs doomed? // Land based sources of pollution

Another, recently published study from the Indo-Pacific province, in which we looked at the effects of increasing turbidity on biodiversity:

Fabricius KE & De'ath G (2001) Biodiversity on the Great Barrier Reef: Large-scale patterns and turbidity-related local loss of soft coral taxa. Pp 127 - 144 in: Wolanski E (ed) Oceanographic processes of coral reefs: physical and biological links in the Great Barrier Reef. CRC Press, London.

The article is best to be read in the original book which contains a CD with the colour images and animations of processes. In our chapter, we present a spatial model of increasing turbidtiy (originating from a single-point-discharge), related to decreasing biodiversity. However I'm happy to send out free reprints in paper form (black & white print) or electronically (colour).

Abstract:
Spatial patterns and abiotic controls of soft coral biodiversity were determined from an extensive reef surveys on the Great Barrier Reef (GBR). Taxonomic inventories of soft corals, and estimates of cover of the major benthos forms and of the physical environment, were obtained from 161 reefs, spread relatively evenly along and across the whole GBR. Reefs on the mid-shelf between latitude 13=B0 and 16=B0 represented the "hotspot" of taxonomic richness in soft corals on the GBR. Overlapping distributions of in-shore and off-shore taxa maximised richness on mid-shelf reefs. Taxonomic richness decreased with increasing latitude, and was low and relatively even across the shelf south of 21=B0 lat. Soft coral richness was strongly depressed in areas of high turbidity. It was also weakly positively related to the amount of sediment deposited, and strongly increased with depth. Total cover of hard corals and soft corals was poorly explained by physical and spatial variables, however both varied with depth. The findings presented here have three major management implications: (1) Turbidity and sedimentation affect the generic richness of soft corals. Reefs with highest soft coral richness are < 20 km from the coast, well within the range of terrestrial run-off, and hence a loss of biodiversity could result if turbidity increases due to land use practices which generate soil loss; (2) Taxonomic composition is more strongly related to environmental conditions than total hard and soft coral cover. Taxonomic inventories are thus better indicators of environmental conditions and human impacts than are assessments of total cover. (3) Richness and cover change more within a single site between 0 and 18 m depth, than between

reefs hundreds of kilometers apart along the shelf at the same depth.
Valuable additional information can be gained in a cost-efficient way if
monitoring and survey programs covered several depth zones rather than a
single depth.

Regards,

Katharina Fabricius

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Date: Tue, 02 Oct 2001 09:59:43 -0500
From: "Bob Buddemeier" <buddrw@kgs.ukans.edu>
To: Katharina Fabricius <k.fabricius@aims.gov.au>
CC: coral-list@coral.aoml.noaa.gov
Subject: Re:Land based sources of pollution//source estimates

Katharina, or anyone --

Do you have either estimates or expert-judgement opinions on the relative
extent
to which (or the geographic areas in which) the observed high-turbidity areas
are primarily related to:
a. medium or large river discharge;
b. stream, small river or open coast runoff; or
c. local resuspension of existing sediments?

Getting some idea of the relative importance of these components of the
turbidity forcing is critical to deriving impact predictions from climate, wave,
and land-use models.

Thanks,

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--
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Note: *Buddemeier had Fabricius's whole message in his original message. Fabricius's message is already displayed above.*

Date: Tue, 2 Oct 2001 09:23:09 -0600 (MDT)
From: Joanie Kleypas <kleypas@cgd.ucar.edu>
To: <coral-list@coral.aoml.noaa.gov>
Subject: Re: coral reefs doomed for sure

Thanks to Mike Risk for bringing up some misunderstood issues regarding ocean chemistry changes in response to increased atmospheric CO2 and how coral reefs might respond. Some of his comments are good (e.g. that bioerosion is too often overlooked) but some were broad misrepresentations of science (e.g. his comments about ocean modelers and about the Kleypas et al. paper in Science). So I am compelled to address several of his points:

FIRST
> Any "reef model" that does not include it [bioerosion]...it's hard
> to be polite, here. These models would better be termed
> "Less-than-half-of-the-reef models."

I agree that any modeling effort needs to take bioerosion into account. (and contrary the claim that the word was not mentioned in the Amer. Zool. special issue, Kleypas et al. in the Am. Zool. issue DO mention bioerosion several times as an important control on coral reef development). We have also discussed bioerosion prominently in a follow-up paper in Int. J. Earth Sci. (Kleypas et al. 2001).

Our paper in Science did not model reefs - nor were we trying to model reefs. The thermodynamic calculations and modeling effort concentrated on simply determining carbonate ion concentrations as a function of temperature and pCO2. It is a simple calculation yes, but measured data obtained through the JGOFS, WOCE and other programs illustrate that ocean chemistry is indeed behaving as predicted. So I don't think the challenge to predicted ocean chemistry changes is valid. The chemistry will indeed be complicated in shelf environments by other processes, but the buffering on most reefs, e.g. those which receive significant exchange with open ocean water, will be minimal.

SECOND
> 3. Oceanic/Climate Models. Notwithstanding their protestations to the
> contrary, I have found modellers to be resistant to data that upset their
> models, with that resistance being directly proportional to the amount of
> federal money invested to date. "One major problem with the current
> generation of GCM's is that the treatment of ocean circulation is still very
> crude." (Ruddiman, 2001: Earth's Climate).
>
> The implications of Smith et al, 1997, are that a meltwater pulse can divert

> or shut down the Gulf Stream in less than 5 years. To all of you out there:
> when the oceanic part of GCM's can model this, then start believing them-not
> before. The strong compartmentalisation of the mixed layer to which Bob
> refers is metastable, and temporary.
>

Prof. Risk misrepresents the science presented in the Kleypas et
al. paper. The HAMMOC model results were added to illustrate that
the time-scale to bolster alkalinity (via dissolution of reactive
sediments in response to increased atmospheric CO2, which depends
on deep ocean circulation) was too long to show an appreciable buffering
of the system over the next 200 years or so. At least in terms of
open ocean geochemistry, there is no source of alkalinity which can
adequately buffer the increased atmospheric CO2 for a few centuries, at
least. There have been many papers on this and a good place to start
is with David Archer's.

And in defense of modelers! (I myself am not a modeler, but the
coral-list should hear their side):

The Smith, Risk, Schwarcz and McConnaughey paper above (Nature 1997) is
a nice presentation of isotopic changes in deep-water coral skeletons
during the Younger Dryas event. These data undoubtedly record a change
in the water mass overlying Orphan knoll (50 26'N 46 22'W and 1600 m depth
- note that this location is not really the Gulf Stream, but the North
Atlantic Deep Water). However, these data do not *necessarily* record
a change in the western boundary current. Western boundary currents
can remain unchanged while water masses change (in fact, the Gulf
Stream tends to maintain its track under a wide range of conditions).
So this challenge (with insult) to modelers to duplicate implied
boundary current changes, based on corals from a single location,
does not provide adequate evidence that "a meltwater pulse can divert
or shut down the Gulf Stream in less than 5 years". Now that being said,
in terms of modeling changes in the Gulf Stream (and North Atlantic
circulation in general) in response to surface buoyancy changes
(i.e., changes in temperature and/or freshwater input), there ARE
models that do capture such changes, and they show that the response
CAN be rapid (5-10 years). Two examples of such papers:
Gerdes and Koberle, 1995. J. Phys. Oceanography 25: 2624-2642.
Lohmann and Gerdes. 1998. J. Climate 11: 2789-2803.

THIRD:
> So, in short, Kleypas et al:
> 1. depends on reef models that ignore >50% of the process
> 2. depends on outmoded oceanic circulation models
> 3. ignores some fundamental chemical questions.

Regarding 3 - Bob Buddemeier has already provided enough answers.
Certainly there are complications in carbonate chemistry near
continental margins, which will result in a range of reef response
to changes in carbonate chemistry. But given the volume of the
oceans versus that of river and reef sediments, isn't it likely
that coral reefs will be bathed in waters overwhelmed by the
increasing pCO2? I personally would like for Mike's #3 to be
true, but none of the chemical oceanographers that I have spoken
with (Takahashi, Broecker, Archer, Tans, etc.) have pointed to

any ignored fundamental chemical question in this hypothesis. My
fear is that Mike's statements like those above will convince many
to dismiss the carbonate chemistry issue based on hunches rather
than adequate scientific justification.

FOURTH
> My main concern with that paper is that it may have diverted intellectual
> and financial resources from more pressing problems. Sure, changes in
> saturation state will eventually affect....what? What will be left, in say
> 100 years? pH changes in the ocean, in my opinion, don't make the Top Twenty
> Reef Threats. The rate of present destruction from land-based sources and
> overfishing simply dwarfs everything else.

I agree that reefs sadly face many threats. We anticipated the
that some scientists would feel that their own "reef issue" would
be overshadowed by this problem. Because the calcification question
is global in nature, and because it is a direct and predictable consequence
of CO2 (even predictions of bleaching involve questions about just how much
the oceans will warm), I and others consider this a serious chronic and
increasing threat to reefs (and perhaps to other calcifiers such as
coccolithophorids - see Riebesell et al. 2000). But politically, the
issue is powerful, and any solution which would mitigate increases
in CO2 would certainly mitigate many of the other threats to reefs
as well. And honestly, this issue has gotten so minimal attention
and funding since the paper was published that I can only conclude
that most people don't fully understand its scope. I take some of
the blame for not pushing it hard enough, but there is also a
significant amount of misinformation that is going around.

FINALLY
Thanks again to Mike for bringing up these issues.

cheerio, J Kleypas
~~~~~~
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kleypas@ncar.ucar.edu
~~~~~~

From: "Mike Risk" <riskmj@mcmail.cis.mcmaster.ca>
To: "Joanie Kleypas" <kleypas@cgd.ucar.edu>, <coral-list@coral.aoml.noaa.gov>
Subject: Re: coral reefs doomed for sure
Date: Tue, 2 Oct 2001 14:24:21 -0400

Joanie has provided a spirited defense of her and her co-authors' work. I remain far from convinced that some of these matters are solved beyond the need of further debate. I will respond at length soon, after I finish getting in this year's firewood. But some quick comments-

It seems that most scientific "clarifications" carry with them the seeds of further misunderstandings. Here are some additions:

1. The comment about climate modellers not wishing to accept data that contradicted their models wasn't mine-it came from a well-known NOAA climate modeller, whom I will mercifully not name. My prior attempts to convince modellers to accept the need for extremely rapid ocean overturning were met with benign neglect. I felt it appropriate, therefore, to accept the valuation of someone in the field.

2. The top of Orphan Knoll lies directly in the Gulf Stream Return Flow, so to suggest it is not connected with the Gulf Stream is misleading.

3. Some modellers listen, and solicit data. We are now working very closely with several groups on the East Coast (BIO modellers and their US colleagues), as we begin to obtain long-term proxy records of the NAO, Labrador Current, and the inner Gulf Stream: information that was previously unavailable.

4. I don't consider that land-based sources of pollution are my "reef issue." (But I admit, I feel they are THE reef issue.) As we have seen, there is zero political will in North America for CO2 reductions. (Canadians are worse than the USA, by the way, just to demonstrate that I am an equal-opportunity slagger.) There will be action on this front only after the enormous public health costs sink in, and even then the response will be slow. In the meantime, something could be done about sewage and sediment stress. This is not rocket science, but would require that at least a large proportion of reef scientists speak with one voice. There is already a trend among reef managers to blame "global change" for impacts that have clear local causes.

Back to the maul (not mall).

Date: Wed, 03 Oct 2001 06:57:39 -0500
From: "Bob Buddemeier" <buddrw@kgs.ukans.edu>
To: Katharina Fabricius <k.fabricius@aims.gov.au>
CC: coral-list@coral.aoml.noaa.gov
Subject: Re: Land based sources of pollution//source estimates

List --

Comment first, then some more discussion of (mostly sediment-related) issues.

Special thanks to Katharina and Alina for their observations and comments. Katharina is right on with her comments on single variable arguments -- the problem is, we have to understand the variables one by one to get to the point of effective integration, and that seems to tempt a lot of people into the all-or-nothing false dichotomy. Another problem is the gravitation toward polar positions: "reefs are doomed real soon because people are killing them off" vs "not too worry, they're robust and it's just a natural fluctuation." The first is a very slightly more credible position than the second, I think, but only slightly, and the most useful synthesis combines and is offset from that discussional axis.

Turbidity and sediment are good examples. Without claiming that they are totally generalizable, let's take the recent contributions to the discussion to show that resuspension of sediment (as opposed to new input) is a significant stress factor. I suggest that this is at least partly a 'natural cycle' development. Continental shelves and shallow coastal areas are excellent sediment traps, retaining a lot of what comes off the land. Our present situation is geologically and environmentally anomalous -- a relatively stable 3-6,000 year sea level high stand (the range of times is because it's local, not eustatic, level that counts operationally, and the Caribbean and much of the Indo-Pacific have different local sea level histories). That accounts for a lot of sediment build-up (with or without human intervention), and I suggest that a number of areas may 'simply' have reached a critical threshold in terms of the inventory or load of resuspendable sediment. A glance at the Pleistocene sea level curve will show why corals and reefs are not necessarily adapted to this kind of environment.

I put 'simply' in quotes above to underline Katharina's point that it never is simple -- in this case, one of the complicating human factors is change in the ocean climate. As I understand it, a number of regions of the oceans have shown significant increases in mean wave height over the past few decades. This is the resuspension driver, so it may be that either natural climate cycling or human-induced climate change have pushed the sediment resuspension effects across the threshold very recently.

This underlines a point that I hope was obvious from the earlier discussions -- reef researchers need to understand some oceanography, as well as issues of large-scale dynamics (the latter comment is a shameless plug for an upcoming special issue of Coral Reefs -- sorry).

It also puts some other perspectives on the questions of reef doom and what to do about it. Note that I am going to talk about a particular variable or suite of variables, and do not intend to imply that there aren't others, that people aren't problems, etc.
1. 'Land sources' in the real-time sense may not be as big a sediment issue as often supposed. Most large and medium -sized drainage basins have had their water flow (for sure) and sediment discharge (proabably but not always) reduced and regulated by damming and diversion. Local coastal runoff and small/undeveloped basins have the potential for dramatic increases in sediment load in response to land use and cover changes, but the acute effects of these are often localized near shore (although there is the general contribution to shelf sediment load build-up).

2. There is no realistic prospect of modifying either the coastal zone sediment inventory or the marine energy regime, so -- if this formulation is valid -- chronic sediment stresses in most offshore areas may be something that simply has to be lived (or died) with. This implies a focus on understanding its contribution to multi-stress synergism in hopes of finding a different factor that can be managed to reduce the combined system impact.
3. Conservation/preservation: I have been beating the drum for a triage approach to reef resarch conservation, and management, and I have also from time to time expressed a fondness for atolls (but outer-shelf reefs can be OK too). I suggest that this example reinforces both -- if continental reefs really have "timed out" in terms of Holocene habitat development, the places to look for healthy or at

least preservable systems are in very well-flushed, no-soft-sediment coastal areas or away from terrigenous sediment sources (e.g., ocean islands, especially with small land mass).
4. Research implications: This point goes beyond the sediment resuspension issue to the larger question of combined (and especially land-derived) threats. The idea of chronic stress build-up to a threshold transition that we are now observing implies not only that we are not currently working on normal or 'healthy' systems, but also that what we take as our pre-transition baseline was probably seriously affected at the subclinical level. This means that much of the coral lierature on function and condition has to be interpreted very cautiously if one is interested in determining 'normal' or 'optimal' function. Jeremy Jackson has made this point with respect to anthropogenic ecosystem alterations; I propose extending it to a broader suite of 'natural cycle' considerations including sediment buildup on shelves, the implications (for accomodation space and circulation, among other factors) of reef 'catch-up' with sea level, etc.

All of which may help explain why I am of the opinion that most 'reefs-as-we-know-them' are on their way out of the picture, especially if they are closely associated with a major landmass. I would rather not use 'doomed' as a blanket statement, because I think there may be some (significantly altered) oceanic survivors. The moral of the story: Go to sea.

Bob Buddemeier

Katharina Fabricius wrote:

> Hi Bob and others,
>
> at present the general assumption seems to be (at least here locally) that
> turbidity is driven by physics, ie, resuspension forced by wave height,
> depth, and particle sizes. However, present-day levels of erosion of soils
> and discharge of sediments may increase in some areas the amount and
> proportion of clay and other fine material, which creates greater turbidity
> and remains suspended for longer than equal concentrations of larger
> particles. Together with a group under Terry Done at AIMS, we just started
> looking into modelling it all spatially, to create some sort of "turbidity
> risk map" for the GBR (and we'd appreciate any thoughts/suggestions/
> contributions about this).
>
> I also have data which show that both sediment quality (eg, concentrations
> of transparent exopolymer particles) as well as short-term exposure to
> sedimentation (hours to days) are important factors influencing the scope
> of coral reefs to be recolonised by corals, and these two factors are often
> not part of the lines of argumentation put forward by some sedimentolgists.
>
> With regards to the debate of whether global climate change, increasing
> CO2, or run-off are the "greatest" threat to coral reefs, I am getting
> worried that we may not be getting anywhere with single-cause explanations:
> the coral reef ecosystem is so complex that reefs are dying of a thousand
> cuts rather than of just one single cause, as each individual species and
> life stage has its own little sensitivities to one or the other of the
> human alterations of their environment - and what will suffer first is
> biodiversity. But I'm also convinced that run-off is hampering the capacity
> of reefs to recover from all sorts of disturbances: adult corals can handle
> relatively high loads of nutrients and sediments, but recruits don't. Once

> the adults are wiped out by COTS or bleaching, we'll wake up if the
> juveniles are missing. That's what I'm observing here in some near-shore
> areas of the GBR close to intense land use at present (but again, we need
> to be cautios coming to any single-cause conclusions about our low juvenile
> numbers: we don't have historic data on previous juvenile densities noron
> larvae supplies vs surviviorships from the region).
>
> Regards,
> Katharina
> (for people how may want to send me questions/comments: please apologise
> delays in my replies, I'm off to Palau tomorrow for 3 weeks)
>
> At 09:59 AM 2/10/01 -0500, you wrote:
> >Katharina, or anyone --
> >
> >Do you have either estimates or expert-judgement opinions on the relative
> >extent
> >to which (or the geographic areas in which) the observed high-turbidity areas
> >are primarily related to:
> >a. medium or large river discharge;
> >b. stream, small river or open coast runoff; or
> >c. local resuspension of existing sediments?
> >
> >Getting some idea of the relative importance of these components of the
> >turbidity forcing is critical to deriving impact predictions from climate,
> >wave,
> >and land-use models.
> >
> >Thanks,
> >
> >Bob Buddemeier

Note: *Buddemeier had Fabricius's whole message in his original message. Fabricius's message is already displayed above.*

Date: Wed, 03 Oct 2001 12:06:23 -0500
From: "Bob Buddemeier" <buddrw@kgs.ukans.edu>
To: "Alina M. Szmant" <szmanta@uncwil.edu>
CC: buddrw@KU.EDU, Katharina Fabricius <k.fabricius@aims.gov.au>,
coral-list@coral.aoml.noaa.gov
Subject: Re: Land based sources of pollution//source estimates

Alina et al. --

1. Conrad and Ian covered most of the basic points, but I think that what is
potentially a new twist is considering the role of the build up of specifically
terrigenous sediment (more fines) as a regional, as well as a local lagoon-specific
phenomenon.
2. Your wind comments fit will with my memory of encountering the increased wave
height findings somewhere -- alas, location forgotten. There are a lot of climate
and ocean data available if one pokes around the web...
3. My callous pragmatism says that if all of the factors are operating against a
reef, the manager should flick it in and find something that promises to respond
better to management -- and that's especially true if any of the stresses are
long-term endogenous factors, as existing sediment load could turn out to be. If we

try to save everything we may wind up saving nothing, especially in few of the
apparently inevitable increase in some of the stress factors (committed warming and
CO_2 effects).

It seems obvious from the exchanges that a lot of us have ideas and observations we
never got around to publishing -- maybe the question is how we turn the discussion
thread into a minireview of some sort (?).

Bob
"Alina M. Szmant" wrote:

> Bob and others:
>
> Conrad Neumann and Ian MacIntyre published the phrase years ago about
> coral reefs being "shot in the back by their own lagoons" Proc 5th Internat
> Coral Reef Congr, Tahiti 1985: vol 3 pg 105-110), which is the Holocene
> sea level scenario you described in your email. I agree that for some
> areas (such as Florida Keys) resuspended sediment is a major factor
> limiting coral recruitment (especially sand-blasting by coarse sediments
> during winter storms) and this may have been happening for decades if not
> longer and thus be one reason why patch reefs in Fl Keys often have higher
> coral cover and diversity than more offshore (exposed) reefs inspite of the
> lower water quality (turbidity etc) closer to shore (see Miller et all,
> Coral Reefs vol 19 (2)). I am always amazed at the high numbers of coral
> recruits we see on these inshore patch reefs ins spite of what the text
> books tell us are unfavorable conditions. However, bioerosion is likely
> higher inshore and not many of these patch reefs amount to much.
>
> I have a hypothesis that I have been bandying around for a few years that
> it's been more windy since the mid 1980s and 1990s which could be an effect
> of global warming (more heat, more wind) [this is based on a gut impression
> that in spite of having bigger and better boats than I had access to in the
> 1970's, we have more days that we are weathered out now than a few decades
> back]. More frequent or more severe storms all year long could result in
> lower overall water clarity in areas like the Florida Keys where there is
> lots of sediment to resuspend (I gave a presentation about all this in
> Bali, but mea culpa, mea culpa I haven't written it up yet). If those of
> you that like to work with climate data would have access to good wind
> records, I suggest someone look at the frequency and duration of higher
> wind events over the past 50 years or more, by passing the data thru some
> kind of filter that looks for the higerh energy events (e.g. 15+ knots for
> 24+ hrs): it takes a minimum period of high winds to really get things
> stirred up, but if the rough conditions persist for too long, suspended
> sediments are likely flushed out of the system). Thus, not enough
> resuspension could result in fine sediments building up to eventually
> become a problem (nutrients will also build up); frequent moderate energy
> events may make the system turbid a lot of the time depending on whether
> net flow rids the system of the resuspended fines; occasional major events
> help flush the system of both sediments and nutrients. Thus wind regimes
> (and their change over time as climate changes) could make a big difference
> in the environment conditions reefs have to deal with, and their "health".
>
> Again, things are much more complicated than one-factor causality, and the
> various factors work at different time and spatial scales. Effects of
> elevated temperatures and over-fishing strike pretty much everywhere which
> is why I think they are at the top of my list of what needs to be addressed

> by managers; sediments and nutrients are very important in some areas and
> not others, and should be addressed where appropriate. Some poor reef
> areas have all of the above impacting them and that is real sad. I agree
> with those that write that we shouldn't try to make our favorite cause of
> decline be accepted by everyone as THE ONE to be concerned about, but I
> think we do need a scientifically founded way to attribute relative effects
> because whether we like it or not, that is what the managers need.
>
> Alina Szmant
>

--
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From: "Mike Risk" <riskmj@mcmail.cis.mcmaster.ca>
To: "Joanie Kleypas" <kleypas@cgd.ucar.edu>, <coral-list@coral.aoml.noaa.gov>
Subject: Re: Beyond bioerosion.
Date: Thu, 4 Oct 2001 23:38:38 -0400

I feel there is more that needs to be said on this, and other, issues. This will, however, be my last submission on this particular topic.

Given the involvement of CO_2, I am moved to consider the analogy of scientific papers as automobiles. I view most of my papers as I view my 12-year-old Subaru, that sits mutely rusting outside: inconspicuous, easily ignored, battered and beaten-but dependable transportation nonetheless. Should someone volunteer to put some Bondo on it to fill in some of the holes-well, be my guest. (You have Bondo? We need it up here.)

The responses of Kleypas and co-authors to my comments on Kleypas et al (hereafter KEA, not to be confused with KIE) put me in mind of someone waxing a brand-new BMW: putting further polish on that which is already near perfection. Woe betide those who would point out dents in a fender, or nicks in a windshield...I had hoped for a response something along the lines of: "OK, we know there were some holes in the first version. We invite you all to help us do better next time."-but that isn't going to happen here. The difference between a discussion and an argument is: in an argument, no one has any intention of changing their mind. This is an argument, one that has gone on for over a month.

In that month, I estimate (using totally questionable assumptions!) that SE Asia will have lost 2-3 coral species, and that coral cover on some of the Florida Keys will have dropped another 2%. Reefs are in the midst of a mass extinction event right now, and pH hasn't budged. (Yes, I know about the open-ocean estimates-irrelevant, as you point out.) In the time I have spent crafting these responses, I could have written a formal rebuttal of KEA, and that is what I will now set out to do.

I also sense that the tone of the exchanges is becoming harsher, which is upsetting. I realise I am to a large extent at fault, here, being a direct and rude type. Those who know me may feel I have been well- behaved, whereas those who don't may wonder why Jim Hendee let this raving maniac on in the first place. So. After this one, I will give up. I have concluded that there will be no substantive response to any of my comments.

I remain, as always, available for comments and exchanges, and would be delighted to give of advice or information in any of the areas in which I have some competence, as soon as I figure out what those areas may be.

PREDICTIONS
To begin with: KEA have made their predictions, based on models they have described in print and on the list. I am a field man (Omega, to me, always meant expensive wristwatches), so I tend to look at field evidence. Just about every reef worker (including Gattuso and Buddemeier) reports solution of carbonate at night, when CO2 is elevated-and Halley's work shows that this is solution of HMC. Additionally, KEA predict that corals should show a 6-11% decline in calcification since about 1880. Lough and Barnes (2000) show an INCREASE in calcification of 4%, an increase that closely matched the prediction of increased calcification from elevated SST's. So at least one of their predictions is wrong already.

When I first saw KEA, I predicted that it would be used by managers to divert resources away from local problems. This has already happened. In addition, my doomsday scenario (Twenty and Out) is still running well, and I will finish no worse than .500.

OCEAN MODELS
My rude comments about modellers (which really weren't mine, as I point out-although I ascribe to them) were met by Dr. Kleypas with the following series of responses (paraphrasing):
-KEA really only used the HAMMOC model to illustrate the long time-scale to buffering (although the model doesn't react quickly)
-there are models out there now that CAN react quickly (but we haven't used them)
-and besides, there are all these famous oceanographers out there who agree with us.
What can I possibly do, faced with this response, but retreat licking my wounds? Seriously now, this is not convincing.

Dr. Kleypas attempts to bolster her defense of the ocean models by denigrating/downplaying the importance of Smith et al, Nature 1997 (that's OK, so do the modellers). While she claims "corals from a single location...do not provide adequate evidence" , that same finding was trumpeted, by one of her own quoted oceanographers, as "The New Archive that we've all been waiting for." Would you have asked Newton to wait for MORE apples??? Sure, it's only one location-but it's the most precisely constrained major climatic event ever to be described from the ocean record. The results won't go away. The implications are that the Gulf Stream Return Flow disappeared/deviated/whatever in 5 years. This implies a fundamental mixing of the oceans during major climate changes, mixing which will screw up the rest of the predictions in KEA. (I treat these postings as my lectures-I only repeat myself if I feel the audience wasn't listening.)

Note: for those of you interested in paleoclimatology: Smith et al 1997, and the companion piece, Smith et al, 2000 (PALAIOS), provide an isotopic Rosetta Stone, a solution to the annoying effects of KIE (this is a process which makes many coral isotopic climate records simply undependable). Precise water temperatures, any ocean, any coral, any depth. The "lines" paper, in PALAIOS, took corals from all over the world, used thousands of isotopic measurements to show that the slopes of lines in O-C space, independent of KIE, were a thermometer.

BIOEROSION

After Dr. Kleypas' response, I went back, and I searched through that Am. Zool. volume, and By God I found it! In Kleypas et al, on p. 153, we see (refs removed to save typing) "...nutrient excess probably limits reefs indirectly by enhancing macroalgal competition for space, phtoplankton competition for light, and bioerosion." And that's all. Instead of claiming to have "mentioned bioerosion several times as an important control on reef development," I think she should have 'fessed up, said "OK, we left it out, we'll do better next time. Can you help us?" Ain't going to happen. (By the way, the Gattuso et al paper in that same volume is one of the nicest summaries of coral gas and nutrient metabolism I have read.)

I'd like to go over some of this again. I do apologise in advance for some of the self-citations: there has already been too much of this in these exchanges. I do so only when one of my rusty old beaters was the only one on the lot at the time...

The classic studies on reef budgets were done in the early 70's, based on field work done (in some cases) commenced in the 60's. The results have never been challenged: bioerosion equals calcification, with large errors. (Where calcification spikes up, we get reefs-where it does not...sediment.) There have been a few studies directly relating bioerosion rates to nutrient concentrations. Rose and Risk (1985-Mar Ecol 6: 345-363) found that density of Cliona delitrix increased in lockstep with the abundance in the water column of fecal bacteria. (No phosphates, no nitrates-plain old poop. Turtle poop.)

Since the early 70's, when those papers were done, coastal nutrient concentrations/eutrophication levels have AT LEAST doubled. In other words, bioerosion is now FAR MORE IMPORTANT than the corals! The treatment of this subject in the Amer Zool volume simply exposes the huge lacuna in the skill-set of today's reef biologists.

So reef monitoring programs that omit bioerosion are a joke, as are reef growth models. It is to be hoped that rapid readjustments are under way as we speak.

But let us examine the role of bioerosion in calcification budgets/alkalinity reduction studies.

Microborers have been around since the PreCambrian, and comprise several phyla: blue-green algae (yeah, I know, Cyanobacteria-but geologists still call them blue-greens), greens, reds, fungi...They are in every grain of sediment, every coral, every shell, every coral that has ever been stuck into a metabolic chamber...most of the destruction is done by the green algae, via secretion of short-chain organic acids, such as formic, oxalic (good for taking rust off cars), malic. As usual, the stoichiometry eludes

me, but here is what I see:
-because they manufacture short organic acids thru photosynthesis, the CO_2 balance may be close to a push (one in, couple out).
-their eroding activities, however, crank up alkalinity values, via a process that appears in the gas-exchange models as PS. In other words, the O_2 production of the corals, which is calcification, is mixed with the O_2 production by alkalinity-pushers.

That's just the greens. There is evidence that the blue-greens may be heterotrophic-like graduate students, there's no telling WHAT they do at night...the fungi are saprobic, dikaryomycotan anamorphs-common terrestrial fungi. You have some in your fridge now, on the bottom shelf, at the back there. (Kendrick et al. 1982, Bull Mar Sci 32: 862). They invaded via beachrock or.....African dust!

I had hoped that Bellamy and Risk (1982: Science 215: 1618-1619) would have been more widely absorbed by calcification modellers: we found very large amounts of oxygen, produced by boring algae, stored in the tips of Millepora on the GBR. If you "ping off" a tip, not only will you see clouds of bubbles, you may even hear the hiss of escaping gas. (No, please don't do it!) Shasher and colleagues, in Israel, in a series of elegant experiments on "life in extreme environments", estimated that the amount of respiration, the metabolism, of boring algae lying directly under live coral tissue was small-so perhaps they may safely be ignored? No.

On the contrary: the ones in corals are light-limited. In sediments and hardgrounds, they have a major impact. Tudhope and Risk (1985: J. Sedimentary Petrology 55: 440-447) estimated that boring algae dissolved between 18 and 30% of the TOTAL sediment input into GBR lagoons. These were extremely conservative estimates, and the real value is undoubtedly higher. In that paper, there is a section on the relevance of the results to whole-reef calcification estimates using alkalinity reduction techniques. P. 446: "...loss of carbonate from the reef system due to dissolution of sediments by microborers is a more important factor in whole-reef budgets than previously recognised"-and it remains unrecognised.

I would invite KEA to explain to me, and the list, how the influence of microborers on gas exchange over reefs has been handled in their models.

Finally, I am deeply distressed that my anguish at the demise of the ecosystem in which I have spent most of my life should be dismissed as pique at "my own reef issue being overshadowed" by the predictions in KEA. Firstly, I don't think their predictions are worth much-but far more importantly: I am as far as I know the only reef scientist who has had the courage to speak out in print against the factionalism that paralyses reef science (Risk 1999, Mar. FW Res 50: 831-837). It is unacceptable to me that I be accused of the same turf-war mentality. It is unacceptable, and I am very angry about it.

Message ends-thank you all for your indulgence.

Date: Fri, 05 Oct 2001 19:07:00 +1000
To: "Mike Risk" <riskmj@mcmail.cis.mcmaster.ca>,
"Joanie Kleypas" <kleypas@cgd.ucar.edu>,
<coral-list@coral.aoml.noaa.gov>

From: Clive Wilkinson <c.wilkinson@aims.gov.au>
Subject: Re: coral reefs doomed for sure

Mike and others

I have watched this from afar - but feel that I must comment.

"land-based sources of pollution ... are THE reef issue."

This is attempting to put the magic solution of a single cause to a problem, when in fact there are often multiple causes of reef decline.

Pollution by nutrients and sediments are very pertinent on reefs surrounded by shallow water, with lagoons or in embayments; these are minor issues for remote oceanic reefs with deep water adjacent and strong currents. In SE Asia and nearby, the major destructive forces for such remote clean-water reefs are destructive fishing, especially blast fishing.

However, of the 11% of reefs reported lost in the last Status of Coral Reefs of the World 2000 report, most were either dredged up, smothered in sediment, or had airports and the like built on them. A further 16% were severely damaged in 1998 during the major El Nino / La Nina climate switches. Many of the others are severely threatened by the usual mix of impacts - pollution, sediments, over-exploitation including coral mining, and engineering activities. Many of these threats act together and Global Climate Change will probably add to all of these while also causing bleaching. So reef loss will rarely be attributed to a single cause.

Clive

Note: *Wilkison had Risk's whole message in his original message. Risk's message is already displayed above.*

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lasted updated 10/29/01
by Monika Gurnée
CHAMP Webmaster