Weather, climate and starfish populations

John M Swan
Chairman, COTSREC

The weather-climate dilemma is familiar. Over days or weeks the weather patterns can vary widely, especially in temperate zones. Melbourne has four seasons - often all on the one day.

But how do we assess climate, and especially climate change, within this envelope of weather variability? When finally do we conclude from a succession of hot days, or hot years, that, yes, the world is warming, the climate is changing? This is the classic problem of time scales, and it is one that concerns ecologists as well as meteorologists. When (and how) does "natural" biological variability over a short timeframe lead to a "cataclysmic" event such as a population explosion or a species extinction over a much longer time frame? What is short and what is long?

Perhaps one of the most valuable decisions made by COTSREC and its predecessors was to support regular annual monitoring of a large number of reefs over a wide extent of the Great Barrier Reef (GBR). It is not always easy to persuade scientists that "routine monitoring" is sometimes more worthy of funding than the exploration of an exciting and novel idea. But amid all the controversy surrounding crown-of-thorns starfish (COTS) outbreaks, it was clear that only a long-maintained record of coral cover, starfish densities, coral destruction by other agencies (cyclones, coral bleaching, *Drupella*), and the rates of coral recovery after damage, could provide a basis for ecological understanding of the COTS phenomenon. Further extension of this record will include water quality parameters and the densities of certain fishes.

Weather and climate have very different time scales. What are the time scales of COTS outbreaks? Are we observing a unique, grossly exceptional event with human causes? Or are the outbreaks, when viewed on a long time frame, episodic events which are a part, possibly an important part, of the natural history of coral reefs ?

The pattern of outbreaks of crown-of-thorns starfish along the GBR since 1966 has now been well documented and analysed (Moran *et al.* 1992). Population models have been developed on the basis of hydrodynamics, larval dispersal and juvenile recruitment (Scandol and James 1992, James *et al.* 1990, Dight *et al.* 1990a, b). Johnson (1992) has well documented the hypotheses proposed to explain causality, and especially the underlying recruitment processes. These are that primary outbreaks arise from (1) mass settlement of larvae emanating from "natural" stochastic events independent of anthropogenic interference, (2) mass settlement as a result of terrestrial runoff enhancing larval survival, (3) increased post-settlement survival as a result of reduced predation rates on juvenile and sub-adult starfish, and (4) aggregation of adult animals of a variety of ages.

But when is a primary outbreak big enough to initiate a wave of subsequent and expanding secondary populations? Here we encounter another problem facing the statistician. Predicting the frequency of "exceptional" events is one thing, predicting their intensity is quite another. There is a strong tendency for individuals and communities always to assume that the hottest recorded day, the largest flood, the most powerful earthquake or the biggest plague of starfish is somehow a true maximum. But the very next such event might dwarf the previous "maximum" by a power of 10, or more. How big is big?

This concern is no excuse for doing nothing. In the long run, even more devastating starfish irruptions might well occur on coral reefs than those recorded around the Indo-Pacific over the last 30 years. But as Lord Keynes was fond of saying "in the long run we are all dead". If we want to understand and preserve coral reefs, we should be conservative in our definitions of "exceptional events", and try to understand them.

This is where the problems start for managers. What level and range of understanding of the COTS phenomenon is required before direct intervention would be justified? How "serious" does a problem have to be before the clamour to intervene is irresistible? Can we indeed "fight these infestations"?

Preliminary findings of a long-term study by Leon Zann of GBRMPA has now shown that COTS outbreaks since the 1950s have occurred on reefs throughout the Pacific and have been a common feature of coral reefs rather than rare catastrophes. The outbreaks have affected reefs separated by thousands of kilometres. Zann suggests that there is a link between outbreaks and ocean-scale influences rather than separate independent local causes. But his study also indicates that outbreaks are often more serious on reefs affected by human activity than on undisturbed reefs. So while outbreaks may be "natural", their intensity, and hence the degree of damage to the coral, could well be exacerbated by anthropogenic factors such as discharge of sewage from towns or nutrient runoff from agricultural land. The management of water quality factors is certainly conceivable and is now under intensive study within the GBRMPA.

In view of the common belief that the remote islands of the Pacific are still pristine and idyllic, it is sad to learn from a recent article (Zann 1992) that there are now many reefs within the island systems in which population pressure, fishing pressure, terrestrial runoff and pollution are extreme. Suva, Fiji's capital, has the highest concentrations of tributyl tin of any port in the world. Pagopago, the capital of American Samoa, has very high levels of heavy metals and fishing has been banned in the harbour area. Apia, Western Samoa, has high siltation, nutrients, eutrophication, faecal bacteria etc, and the adjacent reefs have structurally and functionally collapsed. The list goes on ... Within the GBR the population pressures are generally low in magnitude and, according to Zann, it has so far not been possible to establish any clear evidence of human impacts through experimentation and monitoring. But studies must certainly be continued.

I would like to pose a final question in relation to the management of complex ecosystems. Certainly we must try to eliminate gross human disturbance and particularly the input of noxious chemicals and high levels of nutrients. But if we can be satisfied on this score, then given the diversity and resilience of coral reefs, would we not be better advised to allow nature to restore any "imbalances" (COTS predation, cyclone damage, coral bleaching, etc) and do any healing that may be required? Culling the starfish seems to this observer to be highly problematic.

Some two years ago at a meeting of the GBR Consultative Committee I drew an analogy between COTS damage to coral reefs and the destruction of trees in an African National Park by herds of elephants. Another elephant story has come my way. Trevor (1992) has described how the combined impact of drought, floods and elephants had destroyed most of the vegetation that fringed the Tsavo River in Kenya a decade earlier. The call came to cull the elephants "to save the park". But particular local conditions made this "solution" impossible to carry out. The result has been, not the wholesale destruction of the Tsavo National Park, but a new lease of life. The natural cycle has now reached the dense bushland phase that people remember from the fifties. And the evidence is that it was the elephants, not humans, that created the conditions for regeneration. Indeed, Trevor's article is entitled Elephant as Architect. I recommend it to all my crown-of-thorns colleagues.

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