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EDITORIAL

The Shifting Baseline Syndrome

The shifting baseline is a phenomenon which is becoming increasingly important in many natural sciences. It happens as man changes, even subtly, the systems he is examining. It can be put like this: when we measure how much a system or an area has changed, we generally compare it with what we claim is its starting or baseline condition. That starting condition may be the condition of the same area a few years previously. More often, it may be taken to be the condition of an area around the corner which we judge to be more or less what our area of interest might have been like, or should have been like, were it not to have been impacted. The problem is that each time this is done, the control areas used may have drifted further and further away from a 'true' pre-man condition. The area around the corner may not be pristine any longer. It may be nearly pristine, but close enough to being so that the significance of any small changes is lost. Worse, each generation of scientists, or each new team of consultants brought out to do the EIA for the next development on that bit of coast, might be dealing with a 'baseline' which is drifting ever further away from its original starting point. There comes a point in this trend when the baseline area which is being used for a particular study has itself reached a condition which the original investigators of say, 25 years earlier, might have recognized as being disturbed.

Daniel Pauly (1995) who, I think, coined the phrase 'shifting baseline syndrome' refers to the same problem in fisheries. Here, every generation of fisheries experts have based their figures and referred their facts and models to the catches, landings and known biological conditions of the previous generation of fisheries scientists. He supposes that many of the present day problems in fisheries management may be caused by this and points out the importance of being able somehow to capture anecdotal information, at least qualitatively, from days long gone in the hope that this might indicate some previous condition now lost to science. However, just as Pauly says how difficult it is for modern fisheries managers to accept that centuries old fishing techniques may have had a much greater impact on world fish populations than we would like to think, so might many environmental scientists find it difficult to accept the suggestion that few areas of the world's shallow seas have not been impacted in ways which would place at least some of their work on shaky ground.

If baselines are shifting, then many ecological models which relate in some way to 'natural' conditions will have been programmed with erroneous starting points. But what shallow benthic system cannot have been affected by the removal every year of hundreds of millions of tons of grazers and predators? What apparently clean coastal areas have not been affected by even modest inputs of sewage and increased land run-off, year by year, over the past 200 years? We cannot be sure, but we should at least bear the possibility in mind.

I remember reading a Cousteau book on the Red Sea many years ago, shortly after I had first dived along a large section in the centre of it. Cousteau had lamented the fact that, as he saw it, the reef condition was poorer this time than when he had first seen it shortly after World War II. I, however, had found the Red Sea wonderfully clear, warm, colourful and packed with benthic and pelagic life and found it difficult to relate to (or even believe) his depressing statements of decline. It was not expressed quantitatively which made it even more difficult to be convinced. What Cousteau had which I did not though was a 'baseline' of perhaps 20 years over which to notice change. Having now seen a lot of the Red Sea myself, including many fairly polluted and diver-damaged sites within it, I can now see a similar problem. When scientific groups appear, perhaps on an expedition or on some consultancy project, to 'monitor', 'survey' or 'do a baseline study' (all very well used phrases) several have proceeded without the slightest inkling that the reefs they are measuring have changed radically over the previous few years both trophically and dynamically. Some areas may even have shifted into a different stable state. They still look good and colourful, which disguises their changes.

Of particular concern is when scientists use such cases in areas far from their usual haunts to provide recommendations to an unsuspecting third world government. Without mentioning names, I recently worked in a coral sea where the coral reefs had been substantially killed. Mortality was obvious, coral cover was low, algal cover was high and rising, grazers had mostly all been caught and eaten. I had been preceded by two other biologists used only to algal-dominated temperate systems. Astonishingly, the massive reef disruption was not noticed by them. I learned of their work only fortuitously when giving a guest lecture to the island's Rotary Club and mentioned the killed areas, upon which a government Minister rose, visibly upset, demanding to know why my results were so wrong, compared to the much more welcome information which he had received the year before. 'You scientists can never agree on anything' was one comment, retracted (I think!) when I showed underwater photographs containing the evidence. The point is that the former scientists had only apparently ever visited damaged reef areas—such was their line of consultancy perhaps—and the semi-eutrophic condition now so prevalent was perceived by them to be a more or less normal background condition.

Most areas have no known true baseline condition and we have to accept this, but we must also recognize this in environmental work. There is unlikely to be an effective mechanism to utilize anecdotal information in a useful way. 'A great diving spot' is not easily

translated into a chart of trophic structure or a table of productivity values. Yet measurements subsequently obtained from the 'great diving spot' may become, through default, measurements to which management decisions later refer. This is occurring more and more as firstly, sites which are truly unimpacted are becoming scarcer, and secondly, as the increasing number of consultants who fly all over the place to measure facets of what does exist continues to include so many who are ill informed. Using reference sites from some carefully chosen, more remote areas has been proposed (there are parallels here with the widely spread, carefully chosen sites for some new global monitoring programmes), but this idea also has its drawbacks in that, however unaffected is the condition of the chosen reference site, we might not be comparing like with like.

Is there a solution within the known ecological framework to solve this ecological problem of drift? Perhaps not, and perhaps surrogate measurements will soon be all that is available to us. We can make disappointingly few assumptions of true baseline conditions in an ecological sense for many and increasing areas of the world's shallow seas, but, using a surrogate, we always may safely assume, for example, that the natural level of PCBs in mussels is zero. Surrogate nature may, depressingly, become more and more useful to us than the real thing.

Pauly, D. (1995). Anecdotes and the shifting baseline syndrome. *Trends in Ecology & Environment* **10**, 430.

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