Hydrobiologia **512:** 1–10, 2004.
P.O. Ang, Jr. (ed.), Asian Pacific Phycology in the 21st Century: Prospects and Challenges.

The culture of marine ecology

Robert E. DeWreede
Department of Botany, The University of British Columbia, Vancouver, B.C., Canada V6T 1Z4
E-mail: dewreede@science.ubc.ca

Key words: ecology, data analysis, statistics, hypothesis, causality

Abstract

Marine algal ecology today faces many of the same problems as ecology in general, e.g. lack of generality of experimental results, the difficulty of making long-term predictions, and an apparent lack of agreement as to what constitutes the proper or 'acceptable' way of doing this particular component of science. These problems, if real, affect marine algal ecology everywhere but, in different geographical areas, specific problems also occur; science in parts of Asia has some problems different from those in other parts of the world. Since its inception, research in marine algal ecology has been motivated by many factors, ranging from traditional needs, to curiosity, to survival, to new technology, and economic needs. Each of these has shaped the questions that have been asked by, and the level of support society has been willing to supply to, ecology. For example the requisites of tradition pushed marine ecology to ask questions about food and ceremonial biota, and our fears today about loss of biota are pushing for answers to questions about the means of preserving biodiversity. The limitations of many marine ecological studies have been pointed out by different individuals. Their comments have been valuable in forcing us to examine what we are doing as marine ecologists, and how we are doing it. Ecology, and marine algal ecology with it, has been accused of carrying out small-scale studies that have no greater generality than the sites at which the studies were done, and of using statistical procedures that are wrong or inappropriate; also, there is disagreement within the ecological community of how to correct for these 'faults'. Some of the problems arise due to the nature of our particular science, e.g. working with organisms with differing genetic makeup and sensitivity of experimental results to small changes in initial conditions. Other problems are more likely due to the individuals doing the science, e.g. an inability to be an 'expert' on all areas of knowledge required for a modern ecologist (taxonomy, experimental design, data analysis, etc.), and perhaps an unwillingness to recognize that in some instances different methods of data analysis are applicable and valid. As ecologists, we must come to grip with these problems, both for the sake of our science, and for our own sake as practicing ecologists.

Introduction

Today, more ecological research is being published than ever before, and amongst these publications are numerous articles on algal ecology. Yet, despite this outpouring of often very good research, some have argued that ecological research lacks coherence, lacks predictive power, and thus lacks the power to suggest appropriate actions for politicians and managers dealing with the marine environment.

Some who make these claims lay the blame on the ecologists, arguing that most experimental ecological studies are of limited scale, and lack generality. Others have argued that this lack of generality is due to the nature of the science itself, pointing to the complex interactions of many biological and environmental factors that determine the outcome of any ecological process, e.g. competition; as field scientists, we are unable to measure and take into account all of these factors.

Others point out that even if we were able to take all these factors into account, small differences in initial conditions can result in vastly different outcomes, e.g. vastly different numbers of organisms in a population. And since we cannot measure these small differences, we are unable to predict the consequences, even if we know the nature of the interacting processes.

If these difficulties were not enough, ecologists often disagree amongst themselves as to, (1) the appropriate experimental design, or what is aceptable as an experimental design, given the inevitable shortage of funds, researchers, and time, and, (2) the appropriate analytical procedures for examining the data. It is my intent in this paper to: 1. Examine the culture of ecology by discussing what I see as the things we are doing well, the things we are doing badly, and some possible solutions to those things we are not doing well. Clearly these are my personal opinions on these topics. 2. Finally, I hope that we may at least be able to agree on some mechanism for improving what we do, and clearly love to do, research on the ecology of algae.

Definitions of science and ecology

Science in its ideal sense is a method of inquiry that proposes and tests hypotheses, is free to roam the intellectual landscape where it will, and is able to disseminate the information it acquires to all.

Popper (1968), for example, has written and argued that what separates science from non-science is the formulation of a falsifiable hypothesis. Popper (1968) concluded that the criterion that demarcates science from non-science (metaphysics) is that of falsifiability.

Others argue that ecology is too complex to reasonably propose and test single falsifiable hypotheses. Are the upper limits of the distribution of algae due solely to only herbivory, only desiccation, or only dispersal? Or do each of these variables contribute in some degree to the limit of an observed distribution (Underwood, 1985, 1991)? If the latter, no single hypothesis will 'explain' this phenomenon, and the proportional importance of several factors must be examined. Ecology has been defined as "the scientific study of the interactions that determine the distribution and abundance of organisms" (Krebs, 1985) and, in a more cynical sense, as "The science which says what everyone knows in a language that no one understands" (Elton, 1927), and as "The science given over entirely to terminology" (McIntosh, 1976) e.g., a spade = geotome; a rocky sea shore formation = actad; a beach plant = agad.

In some of my darkest moments, when experiments have not gone well, and Hurlbert's Demonic Intrusion (Hurlbert 1984) seems to be rampant, I look at Gertrude Stein's aphorism: (cited in: Fulghum, 1999)

> "There ain't no answer.
> There ain't going to be any answer.
> There never has been an answer.
> That's the answer."

and wonder if this is the reality of ecology.

However, a more hopeful view was expressed by Albert Schweitzer: (cited in: Fulghum, 1999)

> "To the question whether I am a pessimist or an optimist, I answer that my knowledge is pessimistic, but my willing and hope are optimistic"

Motivations for studying algal ecology

Tradition and natural history

The earliest roots of marine algal ecology probably lie in attempts to find and maintain seaweeds for traditional purposes (e.g. where to find the best seaweed for a particular ceremonial purpose, or where to find seaweeds with the highest agar content). Here, the ability to recognize the characteristics of a suitable habitat for a useful species would become necessary knowledge, and convey status to the individual possessing such knowledge.

The Asia-Pacific Region has a long history of using marine algae, for medicine, for food, and for ceremonial events:

> "When the soul of the dead person departs, it travels by sea and stops at the first rock, and is moved because he can still hear the voices of his loved ones crying. At the second rock, he can still see the smoke rising from the yam fields where they are burning weeds. And on the third rock he grabs a little piece of algae and heads towards the island of the dead." (G. Scoditti, quoted in: Stille, 1999).

Still today, the Asia-Pacific region leads the world in the quantity and numbers of seaweed species grown and harvested for industrial use and for food.

Curiosity

Started by tradition, marine algal ecology continued its development by adding curiosity to its motivation. In the context of this presentation, I consider curiosity driven questions those dealing with population, community, and ecosystem processes, as these processes occur in more or less natural (relatively undisturbed by human influence) habitats. In this context, hypotheses are posed about such topics as population dynamics, herbivory, competition, zonation.

Today, the dynamic nature of marine systems is the result of both naturally occurring changes and human induced disturbances, and the resulting dynamics can occur in both obvious and more subtle ways. For example, in Kaneohe Bay in Hawaii, there was an obvious impact resulting from sewage input; the increase in nutrients caused the proliferation of *Dictyosphaeria* (a green algae), which overgrew and killed the corals. Subsequent reduction in nutrients by treatment of the sewage has reversed this process, with a reduction in *Dictyosphaeria*, and a regrowth of corals.

A more subtle dynamic within algal populations has occurred due to oil spills. For example, ongoing studies of the impact of the Exxon Valdez oil spill in Alaska have shown a cycle in the population dynamics of *Fucus* different than found in undisturbed populations of this brown seaweed (Holloway, 1996). Apparently similar cycles were found associated with the Torrey Canyon oil spill, but here it was due to interactions between an active herbivore (a limpet) and *Fucus*. This limpet, and similar herbivore activity, are apparently absent from the Alaska sites.

Survival

The increase in human caused disturbances in marine habitats has added another motivation for doing ecological studies, fuelled this time by fear. This fear arises from both a perceived threat to our own survival, and to the survival of other species. As phycologists, we fear that algal populations, communities and ecosystems, will be destroyed by our growing demand for food and living space.

Hinrichsen (1998) provided the following data on population change and its impact on the marine habitat: "Over 50% of the entire population of the planet (3.2 billion people) lives and works within 200 km of the coast, on about 10% of the earth's land area. By 2025, 75% or about 6.3 billion people will probably live along the coast. South-east Asia has the highest percentage (85%) of coastal dwellers in the world, or about 400 million people."

Hinrichsen (1998) emphasized 3 main points regarding our coastal areas: A. "The world's coastal areas are being overwhelmed with people and pollution."; B. "As a result of the concentration of economic activities along coastlines... critical coastal resources – such as wetlands, mangroves, seagrasses, and coral reefs – are being plundered in the name of development and lost through inertia and neglect."; C. "... the inability of governments, with few exceptions, to craft and implement rational coastal management plans is having far-reaching consequences ...". These consequences occur due to onshore development, increased fishing and seaweed harvesting pressure, and increased run-off from agricultural fields (run-off brought on by the combination of an accelerating rate of removal of terrestrial vegetation and an increased use of fertilizers and pesticides, including in sites far away from the coast).

Yet, in the entire 275 pages of Hinrichsen's book, algae are only mentioned twice, and then only in the context of algal blooms. Are algae not threatened, or are we as phycologists not letting our voices be heard?

Studies generated by this fear of our own and other species' survival include topics such as:

Diversity – What is the diversity of different habitats, how do we best measure diversity, and how do we preserve it?

Function – What is the functional role of different algal species? Are there 'redundant' species that we can afford to loose (e.g. functional redundancy) (Menge et al., 1994; Power & Mills, 1995).

Overharvesting – To what extent can we harvest populations of algae and have them remain viable?

Marine Protected Areas – By what criteria should these be selected? On the basis of high diversity? The presence of one or more rare species? Their functional role as a source of gametes or spores? The viablity of the protected species/populations? Or some optimal combination of criteria such as these?

Introduced Algal Species – How do we recognize an introduced species? What properties makes for a successful invading species? Is it possible to predict the impact of any potential introduced species on a given marine algal community? Should we even be concerned about introduced algal species, e.g. *Sargassum muticum* (Yendo) Fensholt, *Eucheuma*, and *Undaria*, to various oceans?

Can marine algae serve as indicators of global changes? e.g. the sensitivity to temperature of sexual reproduction of many kelps is well known. Similarly, some tropical algae likely live near their temperature maxima. Are some of these algae now found in areas further from the equator?

The combination of the natural dynamism of ecosystems and the added impact of human change has greatly complicated our ability to understand marine systems. For example, the collapse of the West Coast Salmon Fishery has been attributed to overfishing, destruction of habitat, and a long-term increase in ocean temperatures in the NE Pacific. The long-term warming in turn has been attributed to a natural cycle and also as due to global warming resulting from increased CO_2 emissions.

Making use of new technology

Many questions in Science are generated by the development of new technology. The 'answerable questions' in the life sciences changed dramatically, for example, with the advent of the electron microscope, and all its modern permutations, and have changed once again with the development of molecular biology.

For ecologists, molecular tools open the possibility of a much better understanding of, for example, what constitutes an algal population (what is the geographical extent of interbreeding individuals?), e.g. Kusumo (1998). The techniques of molecular ecology also provide powerful tools for understanding the life history of species, e.g. Sussmann et al. (1999) on the identity of green algal unicellular endophytes found in a variety of red algal blades. And of course it has opened our eyes to yet one more level of diversity, that found at the molecular level (e.g. Hommersand et al., 1999).

Economics

Increasingly, funds for research in all areas of science are becoming scarce and, as a result, governments, business, and other private funding agencies, are setting priorities for the allocation of these funds. The research priorities set by these organizations are likely to have a powerful influence on the direction of science and also may slow the rapid dissemination of scientific knowledge. Knowledge gained through research funded by special interest groups, e.g. industry, may be delayed in publication to enable the funding agency to gain some economic profit from the new data. It is ironic that such a possible delay in the sharing of information could come about at the very time that innovations such as the internet are speeding up our access to newly discovered facts.

Also, research funded by special interest groups may suffer from the association. For example, after the massive oil spill in Alaska from the Exxon Valdez in 1989, three different groups (Paine et al., 1996) sponsored research on impact and recovery: A. The oil company, Exxon; B. The Trustees (an Alaska government interest group) and C. A U.S. Government Science Agency.

The most sound scientific study of recovery from the oil spill was the study carried out by Exxon; it used a random placement of quadrats, and generally larger quadrats. Nevertheless, its conclusions were widely disallowed, in part for good scientific reasons (Exxon's interpretation of recovery was that a similar percent cover had been achieved, even though the species composition and age structure differed markedly from non-oiled sites), but also because of perceived self-interest. Similarly, the results of the Trustees' study was tainted by the perception of self-interest, as it was seen to be in their interest to prove non-recovery, as this might lead to a larger reparation payment by Exxon. It is of interest that each of these two interest groups reached conclusions that favored themselves.

Yet, despite dismay at the Exxon Valdez related events, are we even focussed on the most important component of the problem? Ships off Canada's (East) coast repeatedly dump close to 100 liters of oily bilge to avoid paying pumping fees when in port. "The equivalent of the Exxon Valdez takes place off Newfoundland every 2–3 years" (Globe & Mail, 1999).

In Canada, we see targeted research funding occurring more and more often; the targets (such as the generation of jobs, or knowledge that will be useful in the health sciences) are set both by government priorities, and by industry (such as better ways to grow a particular algal crop). The setting of such guidelines is frequently justified by the argument that there is a limited amount of money available, hence choices must be made.

In the context of this conference, Mervis & Normile (1998) report that most of the S.E. Asian countries have defined common research areas for targetted funding. One of these four areas is biodiversity; this is a target because of the potential for new chemicals, especially bioactive chemicals, among the diversity of life still to be found in many of the S.E. Asian waters. One proposed solution for protecting biodiversity is the establishment of Marine Protected Areas (MPA's). How is this best done? One approach is the establishment of MPA's wherever and whenever they can be established. Another approach is to develop theory, or draw on existing ecological theory, to determine the placement of MPA's. This topic has recently been addressed by Phillips (1998), especially in the context of such actions in Australia, and their relevance to marine macro-algae.

Phillips (1998) identified the following problems related to current efforts in marine conservation:

1. Belief in the inexhaustibility of the oceans;
2. Lack of an inventory of macro-algal species;
3. MPA's exist, but no studies have examined their effect on macro-algae;
4. Transfer of ecological theory from terrestrial systems to marine systems.

Limitations of marine ecological studies

Just as different motivations have fuelled algal ecological studies, critical voices have been raised about how these studies were conducted, and what the generality and predictability of the results are.

Worldwide

Generality of results

Foster (1990) has argued that marine ecologists have frequently over-generalized results, attempting to proclaim ecological laws from experimental results obtained within a relatively limited geographical area. For example, in the northeastern Pacific, the otter is perceived to be both a 'Keystone Species' (e.g. Laur et al., 1988; Van Blaricom & Estes, 1988) in some sites and simply a predator in others (Foster & Schiel, 1988). The otter is a predator of sea-urchins which, in turn and when present, consume subtidal algae in great quantities. In the presence of otter predation, and thus reduced sea urchin herbivory, kelp forests increase greatly in abundance. This scenario has been repeatedly cited to claim its importance in areas where its role has not been studied.

Similarly, the important herbivore in many northeastern Pacific shores, the chiton *Katharina tunicata*, is said to have a similar range of roles, e.g. it is an important structuring agent of low intertidal communities in coastal Washington, U.S.A., where the kelp *Hedophyllum*, is a major constituent of the community (Duggins & Dethier, 1985), whereas elsewhere *Katharina* has only limited importance, e.g. in southeastern Alaska, where *Alaria*, another kelp species, dominates (Dethier & Duggins, 1988).

Another example of over-generalization has occurred in the case of phlorotannins, found in many brown algae, and their attributed widespread role as a herbivore deterrant (Littler et al., 1986; Duffy & Hay, 1990). However, additional research has shown that phlorotannins often do not deter smaller and less mobile herbivores (Hay & Fenical, 1992; Pavia et al., 1997). Furthermore, the suggestion that these chemicals may in general be induced by herbivory (Van Alstyne, 1988; Yates & Peckol, 1993) is negated by numerous studies suggesting this is not the case (Pfister, 1992; Steinberg, 1994, 1995). Phlorotannins are not so induced in, for example, *Sargassum*, and *Hedophyllum*. Instead, their presence has been shown to be more closely correlated to C:N ratios, so that high concentrations of phenolics are generally found in low nutrient waters. In addition, polyphenolics may also play a role in the protection of some algae from UV-B light (Pavia et al., 1997).

In this latter example, a simple and attractive hypothesis has given way to a much more complex picture, and this complexity is undoubtedly much closer to reality. A desirably simple explanation (e.g. that phloroglucinols are only herbivore deterrants, and that they are induced by herbivory) is unlikely ever to be true in ecology, nor is it likely in marine algal ecology. In each of the above examples, the problem was not that the facts were wrong (otters can be a major determinant of community structure, and for at least one species of *Fucus*, herbivory can induce higher phlorotannin levels) but rather that single experiments were over-generalized, or single hypotheses were tested, as discussed earlier. Foster's (1990) point is that the same organism in a different community context may have a quite different role, and that the critical difference is not necessarily easy to discern.

Vague and untestable hypotheses

Peters (1991) and others have commented on the tendency of ecologists to pose vague and possibly untestable hypotheses. Peters (1991) suggests that ecologists too often pose questions: (1) About 'ambiguous entities' such as community and stability. For example, initial questions about the stability of communities have led to a multitude of interpretations of stability itself, which in turn have led to such terms as 'local' and 'global' stability, and 'elasticity'. The ability to determine whether a community has any of these properties has lagged far behind our ability to propose the terms; (2) Answerable only by personal opinions, such as 'Why' questions; (3) Only answerable by an infinitely large research program.

Disagreement re. statistical analysis

Ecologists are called upon to analyze experimental results using appropriate statistics, yet frequently (increasingly?) the opinions of statisticians diverge as to what constitutes appropriate statistical methodology. One recurring problem relates to the fact that ecological data are often unable to meet the assumptions of common statistical tests.

Some marine ecologists/statisticians have provided quidelines for ecological data analysis [e.g. Underwood (1981) on the Use and Mis-use of ANOVA; and Day & Quinn (1989) on the appropriate choice of a post-hoc test]. However, what is the biologically inclined, but often statistically challenged, ecologist to make of the following? (1) Statements on Assumptions of Non-Parametric Analyses: Underwood (1997): "Note, however, that the Kruskal–Wallis procedure is not free from restrictive assumptions. ... Homogeneity of variances and independence of data are, however, also assumptions for the Kruskal–Wallis test, as is the requirement that the distributions are continuous and of the same general shape." (2) Zar (1996: 198–199): The Kruskal–Wallis test... "may be employed in instances where the latter is not applicable, in which cases it may in fact be a more powerful test. The nonparametric analysis is especially desirable... when the *k* samples do not come from normal populations, and it may also be applied when the *k* population variances are somewhat heterogeneous." (3) Fowler et al. (1998), referring to the Kruskal–Wallis test: "Non-parametric tests... do not require data to be normally distributed or to have homogeneous variance; i.e. they are distribution free."

Statement 1 above is apparently most correct, the others add confusion to the analytical task ecologists face.

Opinions differ regarding the necessity of incorporating the Bonferoni Correction (on setting the appropriate level of alpha) with multiple tests (Peres-Neto, 1999); defining the limits of the occurrence of pseudoreplication (Hurlbert, 1984), the 'correct' analysis of quadrats measured repeatedly but at different times (is a repeated measures analysis appropriate or not?), the increasing complexity of statistical analysis argued to be necessary for multiple choice feeding experiments (Peterson & Renaud, 1989; Roa, 1992; Manley, 1993), or the impact of Chaos Theory on our ability to draw inferences from any ecological data (Maurer, 1999).

The appropriateness of Bayesian Statistics has also been argued. For example, Ellison (1996) writes "Conceptually, Bayesian inference is the most straightforward way of analyzing and interpreting our hypotheses in light of our data.", whereas Dennis (1996) concludes that "Bayesian and frequentist statistics cannot logically coexist. Until I see some new compelling Bayesian understandings of nature, I will not be a believer."

Let me clarify that it is not the disagreements or the several opinions about a statistical procedure that is the problem. Rather, it is the unwillingness of reviewers and ecologists in general to recognize that different opinions currently exist on these matters, and a lack of recognition that in these cases any of several procedures are acceptable at this time.

An apparent lack of agreed-upon standards on how ecology (and marine algal ecology) should be done

"You get a guy with four Ph.D.'s saying no fish were hurt, then you get a guy with four Ph.D.'s saying, yeah, a lot of fish were hurt.... They just kind of delete each other out" [Barker (1994: 74), cited in Paine et al. (1996)].

As a result of disagreements about definitions of ecological terms and concepts, and apparent disagreements among statisticians regarding appropriate techniques of data analysis, as marine ecologists we often find ourselves at odds with each other about these matters. This manifests itself in our frequent inability to choose an appropriate statistical test (at least in the eyes of another ecologist), in our inability to accept that this disagreement exists and that thus more than one type of data analyses may be correct and, more generally, in the absence of an internally agreed-upon set of standards for what constitutes an acceptable paper for publication.

Peters (1991) argued similarly when he wrote that ecologists submitting papers for review work by different standards from those of the reviewers of those papers. He cited the unusually large proportion of submitted papers that are rejected by ecological journals (62%), compared to other science journals (27%), as evidence.

The infinite regress of causality

Peters (1991) also argued that ecologists are not as useful as they could be when it comes to proposing specific courses of action in the face of an ecological threat. He suggested that as ecologists are rarely convinced they fully understand an ecological system or process, the need for 'more studies' is more frequently stated than is advice given on a specific course of action.

Elizabeth Mann Borgese (1995) made the point that, since 1945, the information that science supplies has become a necessary element in the making of any political decision, e.g. in decisions about environmental policy. Yet she also asked "Is science able – or will it ever be able - to deliver the answers to questions which must be answered for sound political decisions to be made?". Her doubt about the utility of ecology to supply necessary advice is substantiated by the conclusions of many papers published in ecological journals which conclude that 'further research is required' or that 'more work must be done'.

To circumvent this problem, Peters (1991) proposed that we (ecologists) must avoid questions that ask 'Why'; instead he suggests we ask 'How much', 'How many', 'When' and 'Where' questions. He went even further and suggested that ecologists avoid questions that search for explanation, cause, mechanism, and understanding!

For example, in the context of the 1998 El Niño event, what is the likelihood that we can answer questions such as: Why was there a decrease in biomass of species X as a result of El Niño? A more answerable question might be "How much did the biomass of species X change during an El Niño year vs. other years?" In a sense this leaves begging the question of whether El Niño was the cause, but the likelihood of finding that answer is probably small. Nevertheless, an answer to the 'How Much' question is useful both to an ecologist and to policy makers attempting to judge the impact of another ENSO event.

Similarly one could ask "What is the mechanism by which *Katharina* (a herbivorous chiton) affects the abundance of *Hedophyllum*?" Instead, one might ask: "By how much must the density of *Katharina* be increased to produce a 50% decrease in juvenile kelp survivorship?"

The unwillingness of most funding agencies to support long term studies, ones that monitor the environment to establish a baseline of what constitutes 'normal'. This topic will be addressed below.

South-east Asia

All of the above problems apply to ecology in general, and thus to marine ecological phycology. Similarly, and again in the context of this conference, if this analysis is correct then these problems apply to S.E. Asia as well. Problems more specific to S.E. Asia are:

(A) The difficulty for local scientists critically to assess scientific research (e.g. peer review). This occurs when the predominant culture in the country is such that younger scientists do not argue with older ones because it is considered impolite to argue and question results produced by older colleagues. This observation is certainly not new with me; it was noted also by Mervis & Normile (1998) where an Asian biologist is quoted as saying "the culture has to be changed... by the researchers themselves. The culture manifests itself in deferring to superiors and avoiding risks in plotting out a course of research."

(B) Young scientists who did successfully attain their post-graduate degrees promptly disappeared into the administration of their home university, and their careers as scientists effectively came to a halt. This occurred despite the shortage of qualified working scientists in many of the S.E. Asian countries, e.g. perhaps 2000 in Malaysia and 4000 in Indonesia (Mervis & Normile, 1998).

(C) Choice of research topic – Basic or Applied? Within many of the countries of S.E. Asia the debate on this issue is an active one (Mervis & Normile, 1998; pers. obs.). Administrators and researchers are quoted (Mervis & Normile, 1998) with opinions from "... Research for its own sake is not something that we can afford." And "... Solving a practical problem is better than producing one publication that nobody reads.", to "... The pool of knowledge is drying up, and the 21st century will belong to those who are doing fundamental research.", and "... We need to do basic biology before we can apply it to biotechnology or genetic engineering."

When these opinions translate into policy, the ability of an individual scientist to make a choice of research direction may be limited by the policy dictates of the local government, and also by the lack of coherence within the larger scientific community as represented by foreign funding agencies. Again, to hark back to my own experience, while CIDA (Canadian International Development Agency) was working hand-in-hand with government agencies to promote basic science, another foreign-aid agency, funded by a different country, was actively promoting applied science (also in Mervis & Normile, 1998). Similarly, several marine oriented programs were operating at the same time in the same place, and frequently this resulted in a duplication of effort or in a competition for scarce resources (e.g. the university staff were being pulled in multiple directions by foreign projects, since each project required local resource personnel).

Given the limited local resources (money and faculty liaison) this was a counterproductive situation.

Moving forward

How do I see our field moving forward? Do we take Gertrude Stein's comment to heart, or do we proceed with Albert Schweitzer's comments in mind?

Generality of results

I think it important that as ecologists we do not accept easily the generality of the results of any given experiment. We need to accept the necessity of repeating experiments both in different habitats, and in the same habitat but at different times. As both Foster (1990) and Hurlbert (1984) suggested, we should not assume homogeneity and generality until we have done the requisite repetitions, and we should take care to not commit mensurative pseudoreplication. Furthermore, as pointed out by Csada et al. (1996), we must also become more aware of good experiments with results that indicate a lack of significance of some factor. Hopefully, if we recognize the validity of such results, we can make a case for their publication in one form or another. With the increased use of meta-analysis of data, the lack of publication of papers with negative results will seriously skew such analyses (Csada et al., 1996).

Vague and untestable hypotheses

This has been addressed by various ecologists and philosophers, e.g. Peters (1991) and Popper (1968), to name two, and in previous parts of this paper.

Disagreements re. statistical analyses

There has been progress here as well. Papers by Underwood (1981, 1997) on Use and Mis-use of ANOVA, by Day & Quinn (1989) on appropriate choice of post-hoc tests, by Hurlbert (1984) on Pseudoreplication, and a group of papers on the appropriate analyses of single and multiple choice feeding experiments (Peterson & Renaud, 1989; Roa, 1992; Manley, 1993) have resulted in a raised common awareness among ecologists of potential problems.

However, there are still disagreements on even the procedures discussed by the authors mentioned above. Until we get the definitive word on such analytical problems, I propose that we attempt to maintain an awareness of the conflicting opinions regarding these issues. Furthermore, that we recommend to editors and reviewers that perhaps of the options available, any given one may be as correct as we can be at this time. In other words, we (ecologists) should be able to achieve some agreement on standards so that we can get on with the ecology and not get bogged down in statistical matters on which even the statisticians disagree.

Lack of agreed upon standards on how Ecology should be done

I do not mean that marine ecologists do not have standards regarding what constitutes an acceptable paper in our discipline, but rather that among ourselves there is much disagreement on these matters. Surely it behooves our science to discuss these issues and come to agree on some common set of standards. Once agreed, these standards should be promulgated. This does not in any way imply that discussion should be stifled, but rather that there should be a recognition of disagreement based on currently inadequate knowledge, and that this should not be the basis for rejecting an otherwise good piece of research.

Infinite chain of causality

This matter has already been referred to when discussing the ideas of Peters (1991).

The unwillingness of most funding agencies to support long term studies, ones that monitor the environment to establish a baseline of what constitutes 'normal'

The necessity for studies of this sort are proclaimed each time there is a an ecological disaster (real or imagined). Events such as the Exxon Valdez oil spill in Alaska (Paine et al., 1996), and the *Acanthaster* events in the tropical Pacific (Sapp, 1999), are just two instances of the absence of baseline knowledge, and a subsequent inability to judge the extent, and even the reality, of the suggested impact.

Paine et al. (1996) wrote that: "In our estimation, the research initiated after EVOS (Exxon Valdez Oil Spill) failed in three ways. First, much of it was carried out to assess injury in terms of changes in population size, using species lacking adequate baseline information.". Sapp (1999) stated: "Although the issues were similar (among *Acanthaster* events and coral reef bleaching), the response of the coral-reef science community differed dramatically from that in the early controversy surrounding the crown-of-thorns. Those differences highlight . . . the coral reef scientists' hard-won awareness of the general need for baseline data from which to distinguish between human-induced changes and long-term natural processes."

In conclusion, a greater common awareness among marine ecologists and phycologists of the issues raised above will, I believe, go a long way to establishing a more coherent set of standards for judging the merits of scientific papers in our field, will accelerate the ability of young marine ecologists to become succesfully published scientists, and may also help to establish a unity amongst ourselves that will strengthen our ability to influence the decisions of governments for funding directions, and increase our ability to provide coherent guidelines regarding environmentally sensitive actions to managers and politicians.

References

Barker, E., 1994. The Exxon trial: a do it yourself jury. Am. Lawyer, Nov.: 68–77.

Borgese, E. Mann, 1995. The challenge to marine biology in a changing world: future perspectives, responsibility, ethics. Helgolander wiss. Meeresunters. 48: 895–902.

Csada, R. D., P. C. James & R. H. M. Espie, 1996. The 'file drawer problem' of non-significant results: does it apply to biological research? Oikos 76: 591–593.

Day, R. W & G. P. Quinn, 1989. Comparisons of treatments after an analysis of variance in ecology. Ecol. Monogr. 59: 433–463.

Dennis, B., 1996. Should ecologists become Bayesians? Ecol. Appl. 6: 1095–1103.

Denny, M., 1995. Predicting physical disturbance: mechanistic approaches to the study of survivorship on wave-swept shores. Ecol. Monogr. 65: 371–418.

Dethier, M. N. & D. O. Duggins, 1988. Variation in strong interactions in the intertidal zone along a geographic gradient: a Washington-Alaska comparison. Am. Nat. 124: 205–219.

Duffy, J. E. & M. E. Hay, 1990. Seaweed adaptations to herbivory. BioScience 40: 368–375.

Duggins, D. O. & M. N. Dethier, 1985. Experimental studies of herbivory and algal competition in a low intertidal habitat. Oecologia 67: 183–191.

Ellison, A. M., 1996. An introduction to Bayesian inference for ecological research and environmental decision making. Ecol. Appl. 6: 1036–1046.

Elton, C. S., 1927. Animal Ecology. Sedgewick and Jackson, London.

Foster, M., 1990. Organization of macro-algal assemblages in the northeast Pacific: the assumption of homogeneity and the illusion of generality. Hydrobiologia 192: 21–33.

Foster, M. & D. R. Schiel, 1988. Kelp communities and sea otters: keystone species or just another brick in the wall? In Van Blaricom, G. R. & J. A. Estes (eds), The Community Ecology of Sea Otters. Springer-Verlag, Berlin, Germany: 92–108.

Fowler, J., L. Cohen & P. Jarvis, 1998. Practical Statistics for Field Biology. John Wiley & Sons, New York: 110 pp.

Fulghum, R., 1999. Words I Wish I Wrote. Harper Collins, New York. Globe & Mail, April 3/99: A1.

Hay, M. E. & W. Fenical, 1992. Chemical mediation of seaweed herbivore interactions. In John, D. M., S. S. Hawkins & J. H. Price (eds), Plant–Animal Interactions in the Marine Benthos. Systematics Association Special Volume, Clarendon Press, Oxford: 319–337.

Hinrichsen, D., 1998. Coastal Waters of the World: Trends, Threats, and Strategies. Island Press, Washington, D.C.

Holloway, M., 1996. Sounding out science. Scientific American, October: 106–112.

Hommersand, M. H., S. Fredericq, D. W. Freshwater & J. Hughes, 1999. Recent developments in the systematics of the Gigartinaceae (Gigartinales, Rhodopyta) based on *rbc*L sequence analysis and morphological evidence. Phycol. Res. 47: 139–151.

Hurlbert, S. H., 1984. Pseudoreplication and the design of ecological field experiments. Ecol. Monogr. 54: 187–211.

Krebs, C. J., 1985. Ecology, the Experimental Analysis of Distribution and Abundance, 3rd edn. Harper & Row, New York.

Kusumo, H. T., 1998. A Parallel Assessment of Morphological and Genetic Diversity of the Kelp *Alaria marginata*. Ph.D. Thesis, Department of Biology, Simon Fraser University, Burnaby, B.C. Canada: 93 pp.

Laur, D. R., A. W. Ebeling & D. A. Coon, 1988. Effects of sea otter foraging on subtidal reef communities off central California. In Van Blaricom, G. R. & J. A. Estes (eds), The Community Ecology of Sea Otters. Springer-Verlag, Berlin, Germany: 151–167.

Littler, M. M., P. R. Taylor & D. S. Littler, 1986. Plant defense associations in the marine environment. Coral Reefs 5: 63–71.

Manley, B. F. J., 1993. Comments on the design and analysis of multiple-choice feeding experiments. Oecologia 93: 149–152.

Maurer, B. A., 1999. Untangling Ecological Complexity: The Macroscopic Perspective. The University of Chicago Press, Chicago & London.

Mcintosh, R. P., 1976. Ecology since 1900. In Taylor, B. J. & T. J. White (eds), Issues and Ideas in America. University of Oklahoma Press, Norman: 353–372.

Menge, B. A., E. L. Berlow, C. A. Blanchette, S. A. Navarrete & S. B. Yamada, 1994. The keystone species concept: variation in interaction strength in a rocky intertidal habitat. Ecol. Monogr. 64: 249–286.

Mervis, J. D. & D. Normille, 1998. Science in Southeast Asia. Science 279: 1465–1482.

Paine, R. T, J. L. Ruesink, A. Sun, E. L. Soulanille, M. J. Wonham, C. D. G. Harley, D. R. Brumbaugh & D. L. Secord, 1996. Trouble on oiled waters: Lessons from the Exxon Valdez oil spill. Ann. Rev. Ecol. Syst. 27: 197–235.

Pavia, H., G. Cervin, A. Lindgren & P. Aberg, 1997. Effects of UV-B radiation and simulated herbivory on phlorotannins in the brown algal *Ascophyllum nodosum*. Mar. Ecol. Prog. Ser. 157: 139–146.

Peters, R. H., 1991. A Critique for Ecology. Cambridge University Press, New York.

Peterson, C. H. & P. E. Renaud, 1989. Analysis of feeding preference experiments. Oecologia 80: 82–86.

Peres-Neto, P. R., 1999. How many statistical tests are too many? The problem of conducting multiple ecological inferences revisited. Mar. Ecol. Prog. Ser. 176: 303–306.

Pfister, C. A., 1992. Cost of reproduction in an intertidal kelp: patterns of allocation and life history consequences. Ecology 73: 1586–1596.

Phillips, J. A., 1998. Marine conservation initiatives in Australia: their relevance to the conservation of macroalgae. Bot. mar. 41: 95–103.

Popper, K., 1968. Conjectures and Refutations: the Growth of Scientific Knowledge. Harper Torchbooks, Harper & Row, New York.

Power, M. E. & L. S. Mills, 1995. The keystone cops meet in Hilo. TREE 10: 182–184.

Roa, R., 1992. Design and analysis of multiple-choice feeding-preference experiments. Oecologia 89: 505–515.

Sapp, J., 1999. What is Natural? Coral Reef Crisis. Oxford University Press, Oxford.

Steinberg, P. D., 1994. Lack of short-term induction of phlorotannins in the Australian brown algae *Sargassum vestitum* and *Ecklonia radiata*. Mar. Ecol. Prog. Ser. 121: 129–133.

Steinberg, P. D., 1995. Interaction between the canopy dwelling echinoid *Holopneustes purpurescens* and its host kelp *Ecklonia radiata*. Mar. Ecol. Prog. Ser. 127: 169–181.

Stille, A., 1999. The man who remembers. The New Yorker: Feb. 15: 50–63.

Sussmann, A., B. K. Mable, R. E. DeWreede & M. Berbee, 1999. Identification of green algal endophytes as the alternate phase of *Acrosiphonia* (Codiolales, Chlorophyta) using ITS1 and ITS2 ribosomal DNA sequence data. J. Phycol. 35: 607–614.

Underwood, A. J., 1981. Techniques of analysis of variance in experimental marine biology and ecology. Oceanogr. mar. biol. Ann. Rev. 19: 513–605.

Underwood, A. J., 1985. Physical factors and biological interactions: the necessity and nature of ecological experiments. In Moore, P. G. & R. Seed (eds), The Ecology of Rocky Coasts. Hodder & Stoughton, London: 372–390.

Underwood, A. J., 1991. The logic of ecological experiments: a case history from studies of the distribution of macro-algae on rocky intertidal shores. J. mar. biol. Ass., U.K. 71: 841–866.

Underwood, A. J., 1997. Experiments in Ecology. Cambridge University Press, New York.

van Alstyne, K. L., 1988. Herbivore grazing increases polyphenolic defenses in the intertidal brown algae *Fucus distichus*. Ecology 96: 655–663.

Van Blaricom, G. R. & J. A. Estes (eds), 1988. The Community Ecology of Sea Otters. Springer-Verlag, Berlin, Germany.

Yates, J. & P. Peckol, 1993. Effects of nutrient availability and herbivory on polyphenolics in the seaweed *Fucus vesiculosus*. Ecology 74: 1757–1766.

Zar, J. H., 1996. Biostatistical Analysis. Prentice Hall, New York.