Evolutionary Ecology Research, 2001, **3**: 1–13

The good, the bad and the reified

L.B. Slobodkin*

Department of Ecology and Evolution, State University of New York at Stony Brook,
Stony Brook, NY 11794-5245, USA

ABSTRACT

In their search for generalizations, ecologists have postulated many concepts and processes. Some of these have become reified. Reification consists of accepting a designation as if it has empirical meaning when, in fact, its existence has either never been tested or it has been found empty. The distinction between a hypothesis and a reification is that hypotheses are created to be tested and replaced, whereas a reification is taken as an untestable axiom. When a research area loses its dynamism, its hypotheses become reifications. Conversely, if reifications are permitted to accumulate, they can destroy the dynamism of a research area. If a science retains an excess number of reifications, it stagnates and ultimately loses its status as a science. Reified concepts include the logistic equation (together with the theoretical constructs based on it), the idea of constant ecological efficiency, the concept of an integrated community, and certain aspects of species diversity, particularly in the context 'good', 'bad' and alien species.

Also, there are reified metaphors, which, if taken seriously, can be obfuscatory. For example, natural communities have been likened to aeroplanes, and each species to parts of an aeroplane. A metaphor is then constructed in which the removal of a species from a community is likened to the removal of an aeroplane part. Just as removal of one or more parts will cause the plane to crash, the metaphor asserts that the removal of one too many species from a community will result in collapse of the community. On closer examination, this is seen as empty. There is no 'aeroplane'.

Also, the designation of certain kinds of species as good or bad – specifically, alien species are bad and 'native' species are good – is empty and misleading. While invasive species, in some cases, actually do damage native species, the generalization that invaders will reduce species diversity is not well founded.

Fields that are required to focus on research defined by social needs, like ecology and medicine, rather than on scientific capabilities, like astronomy and hydrodynamics, generate reifications. Reifications are dangerous to the health of a research area and should be avoided. Only vigorous extirpation of reifications permits a field to preserve scientific integrity.

Keywords: alien species, community aeroplane, ecological efficiency, 'good' and 'bad' species, logistic equation, reification.

INTRODUCTION

The domain of the science of ecology ranges from studies of the behaviour of microarthropods to models of the carbon budget of the earth. The level of abstraction ranges

---

* e-mail: bzll@life.bio.sunysb.edu

from the description of particular organisms to evaluations of conceptual schemes that can be visualized only by sophisticated computer analysis of major data sets.

In the process of seeking regularities, there have been repeated attempts to find new measures, new conceptual schemes, new metaphors, all in the hope of making generalizations that make predictive sense out of observations of nature. There has been a persistent search for patterns and constants in what may seem a chaotic world. Can regularities be found, for example, in the number of organisms per species as a function of latitude? Do there exist correlations in patterns of distribution and change of differently selected sets of organisms – say trees, or herbivores or any other set? Some of the generalizations work, some fail. There is also a class of generalizations that are appealing and suggestive, but upon more careful analysis are seen to lack operationality. Some of these are quickly tested and discarded. Others, however, apear to persist in the literature of ecology, despite their failings. These are the reifications that are the focus of this paper.

I suggest in this paper that the progress of ecology would be enhanced if a continuing vigilance is maintained to discard empty concepts and false hypotheses before they become reified.

NATURAL HISTORY AND ECOLOGY

Historically, ecology grew out of natural history. Most ecologists recapitulate that phylogeny in their intellectual ontogeny. Concern with natural history is common among those reputed to be theoreticians. Evelyn Hutchinson, when discussing Robert MacArthur, would often reiterate, ‘Robert really knew his birds!’

Natural history is different from ecology. It is usually more directly observational and descriptive of particular organisms. Because of the immense number of different kinds of organisms and the immense number of different things that might be studied about each kind of organism, naturalists can spend their lives discovering fact after fact about organism after organism. Proceeding on this path requires a great deal of patience. Those in a hurry may tend to seek broader generalizations, which may or may not be valid.

Some ecologists began their careers in mathematics, engineering, meteorology or some other non-biological field. They then turned to ecology, either because of a shift of interest, or because they saw the rather inchoate state of ecology almost as an invitation to intellectual carpet-bagging.

Research support is usually difficult to acquire for natural history studies. I have been fascinated by hydra for three decades. My students and I have produced research results, which, while significant for hydra natural history, could not be achieved by pure naturalists. Some studies led to mathematical (Gatto *et al.*, 1989), physiological (Slobodkin *et al.*, 1991) or biochemical statements (Bossert and Dunn, 1996) about hydra.

My last proposal to fund a study of hydra natural history was returned by the Nature Conservancy, accompanied by a pencilled, unsigned note on yellow paper reading, in its entirety, ‘Who cares about hydra?’ This is from an organization that claims dedication to knowledge of all species, but actually focuses its energies on charismatic organisms.

Ecologists often attempt to generalize from natural history. The simplest logical form of natural history generalizations is, essentially: ‘I have seen some phenomenon in some group of organisms under some set of circumstances and I suspect that it will be found in other groups of organisms or circumstances’. Some argumentative rationale may also be provided but the prediction may be independent of the rationale.

Some of the most popular generalizations may be broad and simple – for example, the generalizations about control patterns of different trophic levels (Hairston *et al.*, 1960; Slobodkin *et al.*, 1967). In 1999, Hairston noted that these were important enough for some people still to be furious about them (N. Hairston, Sr, personal communication).

REIFICATION

Sometimes a simple-minded prediction can be stretched too far. For example, I found ecological efficiency[1] values of approximately 10% in laboratory populations of daphnia and hydra. Early field studies of ecological efficiency also seemed to average around 10%. I suggested that because the observations seemed to centre on 10%, perhaps 10% is a universal value (Slobodkin, 1962). When the generalization was made, it had no theoretical support whatsoever. But it became popular. No-one seemed particularly concerned with finding a theoretical rationale for it. The value of 10% was taken as a real constant, instead of a hypothesis. It had been reified (Slobodkin, 1972).

To reify consists of assigning to a word, quantity or image an illegitimate ontological status. Reifications must fit comfortably into language, although they need have no empirical meaning. Both reification and hypothesis formation, at their simplest, consist of attributing reality beyond the borders of empirical experience, or even with no empirical basis whatsoever.

Reification involves premature acceptance of the validity of a hypothesis. Ideally, hypotheses assign a temporary or contingent reality, which is then tested empirically so that the hypothesis can be discarded if necessary. It can be argued that all theoretical advance is made by tentatively assigning reality beyond the data and then testing the result. Under the best of circumstances, a hypothesis has a short life span. It is either discarded as false or nonsensical or becomes accepted as established fact.

Hypotheses, if they are neither tested nor discarded for other reasons, become reifications. They become problematic if they are in principal untestable, or if they remain untested but are still invoked in empirical settings. The distinction between a hypothesis and a reification is that the hypothesis is tested and replaced, while a reification becomes an untestable foundation stone. When a research area loses its dynamism, its hypotheses become reifications.

Most actual determinations of ecological efficiency before the 1960s ranged from around 8% to around 20%. Ten per cent was a guess at a reasonable first estimate of a middle value. I was not at all convinced that the efficiency value of 10% was real, in spite of the fact that my experiments had suggested it. I proposed the 10% universal value in the hope of propping up a straw man, a target. I did not reify it. However, it was a round number and seemed to satisfy a desire for a natural constant. It appeared to be beyond normal tests for several years.

---

[1] Ecological efficiency is an operational concept. I can tell you how to measure it, just as I can tell you how to count chromosomes. While I was in graduate school, human chromosomes were counted repeatedly as 48 until someone actually counted with a clear head and came up with 46. It has been reported (Pennock, 1999) that an 'evolutionist' biology teacher in a bible belt school had students count the ribs of male and female human skeletons in the classroom, so they could prove to themselves that Adam's rib loss was not heritable. He reported that, over and over, students would mistakenly count one fewer rib on the male skeleton!

Why was there no empirical refutation? It is strictly impossible to know why an event did not happen, but I can make one suggestion for this case. In the process of evaluating ecological efficiency, as defined by Lindeman (1942) and used by me (Slobodkin, 1962), several steps of data conversion are required. Counts are transformed to biomass or calories by use of conversion constants. There is some choice available in how these conversions are made. Once an accepted value exists, it is tempting to choose conversion constants that cause that value to reappear. This is shown in the history of evaluation of many constants. The first measurements of the speed of light diverged from each other but almost immediately there was a general convergence of measured values. Once a value has been accepted, data that deviate from that value are discarded as 'bad data'.

I finally proved to my own satisfaction that ecological efficiency could not really be a constant (Slobodkin, 1972). The argument runs:

1. Natural selection can hone the properties of individuals but it is much more difficult to evolve constancies in populations (Williams, 1966).
2. Ecological efficiency is a ratio involving at least three separate populations. For example, the ecological efficiency of an herbivorous trophic level involves food organisms, the herbivores themselves and the first-level carnivores.
3. Genes for a tri-specific property cannot be selected by any of the usual mechanisms of natural selection. So, I can imagine no mechanism for evolving constancy in such a triad of populations.
4. Therefore, ecological efficiency is not expected to be constant in any interesting sense.

In fact, ecological efficiency varies widely (Colinvaux and Barnett, 1979; Verheye *et al.*, 1994; Niu *et al.*, 1998). The different values are informative. A conspicuous source of difference in ecological efficiency is that between endotherms and ectotherms, as noted by Turner (1970) and Lawton (1981). Nevertheless, 10% is still taken as a general value for ecological efficiency in elementary courses and popular summaries.

Sometimes non-operational entities are reified despite their being, in one sense, nothing more than the sounds of words. Consider angels, egos and ids. Once the word 'angel' had been defined, people could write about and paint angels. In fact, there is a tremendous amount of writing, painting and sculpture about angels. Writings about gods and angels include some of the most beautiful and, in one sense, the intellectually deepest of human creations, and are therefore extremely important. For these reifications one can build institutes, receive grants, fight wars and even die.

Once enough writing and painting about angels had accumulated, it seemed impossible, at least for some people, to disbelieve in their empirical existence. Angels had been reified. What, then, are angels? They are the sum of what has been written and painted about them. Isn't that a kind of existence? Yes, but not a scientifically useful one.

Similar considerations apply to Freudian nouns like the unconscious, which is what you don't know about what you know, and the ego, id and superego, which are sometimes thought of as manipulating life just as if they were good or bad types of angels (Grunbaum, 1993). The Wilsonian types of genes, which are created to explain behaviour and have no chromosomal locations at all, share the non-empirical nature of the Freudian concepts (Wilson, 1975).

THE COMMUNITY AS AN AEROPLANE

The reification of the value of ecological efficiency is largely a historical matter. What other ecological concepts of more contemporary concern are reifications? Consider the idea of an *ecological community*. There is no problem if the term is used to mean the organisms found in some convenient region of study, but it can be used in other ways.

Consider the appealing image of an ecological community as an aeroplane (Daily *et al.*, 1997; Bazzaz *et al.*, 1998). The story of the ecological aeroplane begins with elementary fact and then blossoms into a full-fledged parable and emerges with a reification. First the facts:

- From exhaustive sampling of a reasonably small area, we can retrieve from ten to ten thousand species.
- We may, if we so desire, designate the set of species collected in this way as a 'community'.
- If a particular species is found in a community, it obviously has some role in the community.
- Should that species be eliminated, whatever the role of that species had been, it is no longer precisely filled, although individuals of various other species can, and do, take over parts of the role.

Now, the parable: Consider that the community can in some sense be likened to some functioning machine. Why not an aeroplane? Imagine a passenger who finds that one screw in the cabin is loose and removes it. Although that screw had a function, its removal will probably make no difference. But now someone else comes by, finds another screw loose, and pockets it. Although a fair number of screws can be removed without evident harm, if a sufficient number of screws are removed, the removal of one more screw will result in the plane crashing!

Interpreting the parable leads to the assertion that removal of one species from a forest may do no visible harm, but if enough species are removed, the loss of one last population will result in collapse of the community. This is a beautiful and vivid image. The parable suggests that loss or addition of critical species will send reverberating waves of change through a community. Sometimes this appears to be valid, but is it general? In what sense is there really an aeroplane? When a species is eliminated from some region, its obligate parasites and monophagous predators will also be eliminated. Its other predators will be hungrier or at least switch diet. Its prey will perhaps become more abundant and, in some cases, there may be second-order effects. Grass may grow taller, sheltering more mice, or some tree may be unable to germinate its seed.

It was believed for many years that the large seeds of *Calvaria major* trees on Mauritius Island could not germinate unless they had been abraded by passing into the gizzard of a dodo. Since the dodo has been extinct for 200 years, it was thought no young *Calvaria major* trees had been produced (Temple, 1977). This would have been a fine example of pulling out one screw and making part of the community disappear. Unfortunately for the story, there are in fact young *Calvaria* trees in the forest of Mauritius (Quammen, 1996).

American chestnut trees comprised almost a third of the large trees of the Southeastern American forests in the early nineteenth century. While a few small specimens still exist, the chestnut trees were essentially wiped out by the chestnut blight. The forest did not disappear. As noted by Hairston *et al.* (1960), the southeastern forest is as dense with trees as it has ever been. It has been suggested that the atmospheric gases in the air over what had been chestnut forest have changed, with unknown consequences (M. Lerdau, personal communication), but that is another story.

We have intuitions about what forests should look like. These intuitions are very personal and largely aesthetic (Botkin, 1990). While forests consist of several species of trees, they do not seem to be an integrated 'community' of trees. Davis (1986), studying the northward expansion of forests after glacial retreat, noted that each individual species seemed to migrate at its own rate – there was no movement of the forest as a community marching together. Whittaker (1970) noted that trees are distributed along various environmental gradients as individual species, not as communities.

It is difficult to specify when one has crossed the boundary between two supposed communities. The boundaries between supposedly different forest communities have the characteristics of fractal rather than clear borders (Ferson, 1988). MacArthur (1972) suggested a theoretical argument for loose connections being the optimal relation between species in a community.

Some communities may behave like aeroplanes, but determining the conditions under which this occurs is a research project and certainly the frequency is not 100%. Theory, experiments and natural history all suggest that communities are not tightly organized. In short, the popular image of an ecological community as an aeroplane in which each part has a vital role for the integrity of the whole (Daily *et al.*, 1997) is dubious. There may be groups of species, each one closely connected to a few others, and only loosely connected to other groups. There is no aeroplane.

But if the aeroplane image is invalid, what is the status of communities themselves? The terms 'community' and 'guild' were once extremely useful, and are still of pedagogic value if carefully taught. However, as reified objects for research, they are now threatening to become 'panchrestons', terms which are as likely to generate confusion as enlightenment.

THE SIGMOID OR LOGISTIC CURVE

To a very coarse approximation, generally S-shaped population growth does occur. If a few plants or animals are introduced into a closed space within which all their physiological and behavioural needs can be met, they will increase in number until the conditions for each organism have sufficiently deteriorated so that increase is no longer possible. If the numbers of organisms are plotted against time, an approximately sigmoid curve will result – the relative rate of increase per organism being very high at the beginning and decreasing with time; the absolute rate of population increase being maximal at some intermediate time.

By incorporating a set of obvious simplifications, the population growth process can be expressed by an equation of the form $\mathrm{d}N/\mathrm{d}t = aN - bN^2$. The constant a is an assumed rate of Malthusian increase in the complete absence of crowding, and b permits crowding to dampen the increase rate. The curve is symmetrical around $K/2$, where K is the level of supposed population saturation. The necessary assumptions of the equation include instantaneous physiological response to crowding and identity of ecological needs and effect among all organisms regardless of age and history.

This equation and its significance as a very rough approximation to the pattern of population growth under some circumstances was described in 1838 by Verhulst. Verhulst described the inadequacies and lack of realism in the equation, indicating that he expected its early replacement. However, more than 100 years later, a leading ecology book asserted, about a collection of population growth curves, that:

> We can assume that the curves are fitted with skill, i.e., that the best possible agreement between the data and the function is represented. *Knowing that the population is indeed growing in a sigmoid fashion*, it follows that any disagreement between observed and theoretical points results from errors of sampling, from technical defects . . . , from environmental factors, or from some biological characteristic peculiar to the species in question. (Allee *et al.*, 1949, pp. 308–309; emphasis added)

More than 100 years after Verhulst, the logistic equation had been reified.

Checking the most recent copies of the journals *Ecology*, *American Naturalist*, *Journal of Animal Ecology* and *Journal of Applied Ecology* in my university library, I find papers that begin their theoretical discussion of population growth with the logistic equation, despite the fact that the equation's assumptions are not met by any actual organisms and the predictions of the equation are not generally correct.

The general weaknesses of the logistic equation have been described (Slobodkin, 1954, 1961; Getz, 1993). These weaknesses do not mean that it will be abandoned. Several recent papers used it (Nelson, 1997; Josefson, 1998; Krivan and Colombo, 1998) and it is defended against its critics (Berryman, 1992; Berryman *et al.*, 1995).

GOOD AND BAD SPECIES

Other terms and ideas have been permitted to grow into reifications, even though many of them, like the idea of community itself, also have rather modest but clear empirical meanings that remain at the centre of their inflated, reified clouds. Ideas like stability, equilibrium and 'pristine' all carry rather poetic images of harmonious stasis. Perhaps the strangest reifications of all are those associated with the ideas of good and bad organisms. In the pre-Darwinian world, the qualities of good and bad could easily be read into natural history. Properties like 'fierceness' are no longer generally assigned the quality of goodness or badness. With the advent of evolutionary theory, the big bad wolf of little Red Riding Hood was replaced by the naturally selected, red in tooth and claw, politically correct wolf of Jack London and, ultimately, the altruistic Wilsonian wolf-cum-teddy-bear (Colinvaux, 1978). In most modern contexts, there are no bad wolves, just wolves that have been misunderstood.

What can it mean, 150 years after Darwin, to say that some species or communities are good and some are bad? A stable, speciose, pristine community is the epitome of a good community and a species-depauperate community is bad. There is also a general condemnation of invasive species.

Good species are native species and, oddly enough, the less one might reasonably call them successful, the better they are. For example, among the best of species are sand hill cranes, condors, black footed ferrets and some species of prairie dog. The worst of species are the ones that are alien and numerically successful.

Ads appeared for sparrow traps in nature magazines from the early twentieth century because house sparrows were seen as a bad species – in fact, were seen as a filthy, ghetto bird. Now we are more tolerant of house sparrows, but starlings, which were good when they were introduced into Central Park, have become bad.

Note that this does not imply that all species are equally pleasant. The relevant question[2] is: 'Should (we) tolerate any species as long as it is successful? Does that include HIV virus? Malaria and its mosquito vectors? Does it include the red fox, the house cat, the cane toad, and the rabbit in Australia? The house mouse, the black rat and the Norway rat in most of the world? A host of noxious insect pests? Have we no ethical responsibility to do what we can to preserve species?' I believe we do but it should be clear that unpleasant, dangerous and destructive organisms, while they may be 'bad' in the sense of being detrimental to human desires, are themselves neither 'good' nor 'evil', which are ethical qualities confined to humanity. By my ethical standards, we should do what we can to preserve species and to control species that are 'bad' from a human standpoint. But we must do so on the basis of factual information and with the understanding that the ethical problems are ours and not those of the organisms.

Plants can also be good or bad. Purple loose strife and *Phragmites* are both bad species. Purple loosestrife has the reputation of being very bad indeed. It is not completely certain that purple loosestrife is a recent immigrant to North America, but there is a general belief that, during a presumed 200 year history, it has cut down local species diversity (Mullin, 1998) and is not doing its share for local insects.

However, recent studies have shown that species diversity, in at least some situations, is in no way diminished by the presence of purple loosestrife (Diehl *et al.*, 1997). Insect diversity is very high on purple loosestrife (Treberg and Husband, 1999), and native bees rather like purple loosestrife (J. Thomson, personal communication). It has been suggested to me that the foreignness of purple loosestrife may not be real, which would explain why it doesn't seem to act like a bad species. Also, there may be a danger in assuming the harmful effects of loosestrife, since that assumption has been used as an argument for the introduction of exotic herbivorous insects, which may themselves prove to be problematic (Hager and McCoy, 1998). Almost identical remarks apply to *Phragmites*, which in many marshes occurs in the place that ecologists believe *Juncus* ought to be.

Ruiz and Fofonoff (1999) estimated that more than 90% of alien species in estuaries have made no discernible impact on the native species diversity or species abundance distribution. It has also been suggested that some of the studies purporting to experimentally test the effects of invading species are subject to criticism on design grounds (Wardle, 1999). Levine and D'Antonio (1999) report a 'consistent positive relation between exotic species abundance and resident species diversity . . . [suggesting] . . . that invaders and resident species are more similar than often believed'. It is as if some invasive species fit into niches that were in some sense empty, so that they are not strongly competitive with native species.

It is difficult to determine, in advance, which species are most likely to be invasive. To what degree is competition a serious factor in nature and how could one tell? Attempts to answer this question sometimes hinge on naturalists' insights (Schoener, 1985; Ferson *et al.*, 1986), sometimes on various statistical analyses (Harvey *et al.*, 1983), sometimes on detailed natural history and experiments conducted in more or less natural circumstances (Hairston, 1991). Each of these has its strengths, advocates and opponents, but at present it cannot be clearly predicted which invasive species will meet competition and which will out-compete native species.

---

[2] This important question was supplied by Mike Rosenzweig.

Ecological society position papers include emphasis on the importance of eliminating bad species and the general notion has made its way to the highest levels of government. In an Executive Order, President William J. Clinton established a high-level committee and charged it with the responsibility to counter the deleterious effects of invasive species (Clinton, 1999).

Given modern trade and transport, species that can spread, will spread. Perhaps we might admit the inevitability except for very specific agricultural and medical pests. How bad would it be if we simply abandoned the notion of bad species?

Am I advocating letting the zebra mussel run about unchecked? Not quite. The zebra mussel, morality aside, is a pest in terms of its capacity to clog pipes and change food chain dynamics and water chemistry. These are real problems and real solutions must be found. If we focus on mitigating the damage due to zebra mussels rather than attempting their complete elimination as an evil *per se*, we may avoid some of the dangers of the elimination process. For example, poisoning waterways is too much of an answer to the zebra mussel problem.

As suggested by Sagoff (1999), if a species is not clearly a medical or agricultural pest, let's learn to love it!

CONCLUSIONS: HOW TO AVOID REIFICATIONS

Ecologists over the past century have struggled to deal with large, complex systems. They have been severely limited by lack of funding and personnel. Perhaps the only unlimited resources were imagination and intuition. Ecologists nevertheless produced a deeper understanding of nature than was available a century ago. In the process, some concepts were introduced that have become reifications and are less than useful. Elimination of such concepts is difficult, even though their persistence impedes further science.

An excess of accumulated reifications can effectively remove an intellectual field from what we consider science. There is probably no simple formula for elimination of old reifications or for immunization against new ones. It would appear that the only hope is in maintaining alertness. Nevertheless, having called attention to the problem, it is necessary to at least consider possible causes of reification and how they may be avoided.

For example, a recent paper replaces the ontological enterprise of looking for static properties with the more dynamic and interesting one of proceeding directly to an attempt at large-scale prediction of change (Salva *et al.*, 2000). The paper focuses on scenarios of changes in atmospheric carbon dioxide, climate, vegetation and land use and the known sensitivity of biodiversity to these changes.

Salva *et al.* attempted to rank the causes and effects of probable change in ecological systems. This involved ranking by probable importance the 'drivers of change' and also ranking biomes (a looser and less theory-laden term than community) in terms of their susceptibility to these drivers. They also specified the main sources of uncertainty as closely as is now possible. The drivers and biomes were connected by large-scale models. All terms enter into the predictive process and thereby retain their status as living hypothetical entities rather than reifications.

Salva *et al.* (2000) conclude, for terrestrial situations, that land use will probably have the largest effect, followed by 'climate change, nitrogen deposition, biotic exchange, and elevated carbon dioxide concentration. For freshwater ecosystems, biotic exchange is much more important'. While these conclusions may or may not be confirmed, at least the evidence will be clear.

They deliberately omit consideration of concepts for which no clear models of change exist. They assert: 'Although biodiversity changes are just as important for the functioning of ecosystems and the well-being of humans, there are currently no scenarios for biodiversity comparable to those of climate and greenhouse gases'. This suggests that one criterion for avoiding reifications might be to ask what difference a change in a quantity might make for some specific purpose. Is it clear that the response of a forest to heavy rainfall will vary with its species diversity or with its complexity?

It may be helpful to consider two other sciences briefly. Classical physics and modern molecular genetics are examples of scientific fields that appear, at least to an outsider, singularly free of reification. The central questions of modern genetics are defined by the applicability of powerful techniques. Questions that cannot be answered by use of these techniques are not of immediate genetic interest, although the development of new techniques is of interest to any science.

The terminology of modern genetics refers only to processes or categories for which there exist operational decision procedures. Classical physics shares with modern genetics the understanding that what is to be included in the field depends on whether or not the accepted operational procedures in the field will supply adequate information about the phenomenon. Unlike medicine, unlike ecology and unlike conservation, genetics and classical physics reserve the right to declare certain questions to be outside their competence and immediate interest (Slobodkin, 1992).

In some fortunate sciences, with relatively small practical consequences, like astronomy and deep sea oceanography, popular concern and scientific concern can focus on essentially the same questions. But when the subject matter of a field is determined by problems assigned by popular demand or granting agencies, then reifications most easily slip in.

This is particularly dangerous if the sense of urgency assigned to the problems is very high, and the testing of the adequacy of a response takes a relatively long time or is poorly defined. In fact, there are fields of intellectual inquiry almost completely driven by a sense of urgent questions with very little capacity to provide explicit answers, for example theology and ethics. Of course, these fields are not sciences in the usual sense.

Medicine and ecology share the problem of being supported in proportion to their supposed solutions to what are seen as important problems, without necessarily focusing on their capacity to find solutions. There is a danger that attempts to garner public support may interfere with being careful about which of the concepts used in the process are reifications. Occasionally, entrepreneurs may create reifications in their effort to enhance public awareness of the importance of problems and of their own importance in finding solutions. Recall the lists of pseudo-ailments that accompany sales programmes for dentifrices and over-the-counter medications.

Reifications can be avoided only by vigilance. We can resolve to prune our terminology by constant re-examination of supposedly basic concepts. We can refuse to use words for their charismatic effect unless it has been most carefully considered whether they are actually useful from a research standpoint – even though funding exists for them. This is not an easy resolution to maintain in the face of how deeply we feel about some concepts, how desperate is the need to gain funding, and how difficult it is to admit to public agencies that there are perceived problems for which we have no solutions.

I have only indicated the importance of the growth of reifications and dealt with a few very easy examples. Wallace (1998) has recently reiterated that not all perceived problems are solvable by science. I suggest that this is true even within the sciences themselves. Sciences must continually examine themselves for dangerously empty verbal growths.

In ecology, such concepts as community integrity, benefits of diversity, natural efficiency, ecological services, ecological entropy and ecological health are all complex, in the accepted mathematical sense of having both real and imaginary parts. The survival of ecology, let alone of ecosystems or the global ecosystem itself, may hinge on careful dissection – separating the real and imaginary parts of these complex terms.

ACKNOWLEDGEMENTS

Rob Colwell and Mike Rosenzweig kindly edited and improved the manuscript and also raised important questions. Dan Dykhuizen commented on how to avoid reifications. Early stages of the manuscript were presented as a lecture at the University of Connecticut (14 September 1999), the University of Tel Aviv (5 October 1999) and the Hebrew University of Jerusalem (12 October 1999). This is contribution #1068 from the Ecology and Evolution Program, SUNY Stony Brook, New York.

REFERENCES

Allee, W., Emerson, A., Park, O., Park, T. and Schmidt, K. 1949. *Principles of Animal Ecology*. Philadelphia, PA: W.B. Saunders.

Bazzaz, F., Ceballos, G., Davis, M., Dirzo, R., Ehrlich, P.R., Eisner, T., Levin, S., Lawton, J.H., Lubchenco, J., Matson, P.A., Mooney, H.A., Raven, P.H., Roughgarden, J.E., Sarukhan, J., Tilman, G.D., Vitousek, P., Wall, D.H., Wilson, E.O. and Woodwell, G.M. 1998. Ecological science and the human predicament. *Science*, **282**: 879.

Berryman, A.A. 1992. Intuition and the logistic equation. *Trends Ecol. Evol.*, **7**: 316.

Berryman, A.A., Michalski, J., Guitierrez, A.P. and Arditi, R. 1995. Logistic theory of food web dynamics. *Ecology*, **76**: 336–343.

Bossert, P. and Dunn, K. 1996. Regulation of intracellular algae by various strains of the symbiotic *Hydra viridissima*. *J. Cell Sci.*, **85**: 187–195.

Botkin, D. 1990. *Discordant Harmonies*. New York: Oxford University Press.

Clinton, W.J. 1999. *Executive Order on Invasive Species*, Washington, DC, 3 February 1999.

Colinvaux, P.A. 1978. *Why Big Fierce Animals are Rare: An Ecologist's Perspective*. Princeton, NJ: Princeton University Press.

Colinvaux, P.A. and Barnett, B.D. 1979. Lindeman and the ecological efficiency of wolves. *Am. Nat.*, **114**: 707–718.

Daily, G.C., Alexander, S., Ehrlich, P., Goulder, L., Lubchenco, J., Matson, P., Mooney, H., Postel, S., Schneider, S., Tilman, D. and Woodwell, G. 1997. *Ecosystem Services: Benefits Supplied to Human Societies by Natural Ecosystems*. Washington, DC: Ecological Society of America.

Davis, M. 1986. Climatic instability, time lags, and community dis-equilibrium. In *Community Ecology* (J. Diamond and T. Case, eds), pp. 269–284. New York: Harper & Row.

Diehl, J.K., Holliday, N.J., Lindgren, C.J. and Roughley, R.E. 1997. Insects associated with purple loosestrife, *Lythrum salicaria* L., in southern Manitoba. *Can. Entomol.*, **129**: 937–948.

Ferson S. 1988. Are vegetation communities stable assemblages? PhD thesis in the program in Ecology and Evolution, SUNY, Stony Brook, NY.

Ferson, S., Stewart, S., Downey, P., Jacques, G., Klerks, P., Ssemakula, J., Weissburg, M., Malenky, R., Kroot, I. and Anderson, K. 1986. Competing reviews, or why do Connell and Schoener disagree? *Am. Nat.*, **127**: 571–576.

Gatto, M., Matessi, C. and Slobodkin, L. 1989. Physiological profiles and demographic rates in relation to food quantity and predictability: An optimization approach. *Evol. Ecol.*, **3**: 1–30.

Getz, W.M. 1993. Metaphysiological and evolutionary dynamics of populations exploiting constant and interactive resources: *r–K* selection revisited. *Evol. Ecol.*, **7**: 287–305.

Grunbaum, A. 1993. *Validation in the Clinical Theory of Psychoanalysis: A Study in the Philosophy of Psychoanalysis; with an Introduction by Philip S. Holzman.* Madison, CT: International Universities Press.

Hager, H.A. and McCoy, K.D. 1998. The implications of accepting untested hypotheses: A review of the effects of purple loosestrife (*Lythrum salicaria*) in North America. *Biodivers. Conserv.*, **7**: 1069–1079.

Hairston, N., Smith, F. and Slobodkin, L. 1960. Community structure, population control and competition. *Am. Nat.*, **94**: 421–425.

Hairston, N., Sr. 1991. *Ecological Experiments: Purpose, Design and Execution*. Cambridge: Cambridge University Press.

Harvey, P., Colwell, R., Silvertown, J. and May, R. 1983. Null models in ecology. *Ann. Rev. Ecol. Syst.*, **14**: 189–211.

Josefson, A. 1998. Resource limitation in marine soft sediments – differential effects of food and pace in the association between the brittle-star *Amphiura filiformis* and the bivalve *Mysells bidentata*. *Hydrobiologia*, **376**: 297–305.

Krivan, V. and Colombo, G. 1998. A non-stochastic approach for modeling uncertainty in population dynamics. *Bull. Math. Biol.*, **60**: 721–751.

Lawton, J.H. 1981. Moose, wolves, daphnia, and hydra: On the ecological efficiency of endotherms and ectotherms. *Am. Nat*., **117**: 782–783.

Levine, J. and D'Antonio, C. 1999. Elton revisited: A review of evidence linking diversity and invasibility. *Oikos*, **87**: 15–26.

Lindeman, R. 1942. The trophic dynamic aspect of ecology. *Ecology*, **23**: 399–418.

MacArthur, R. 1972. *Geographical Ecology*. New York: Harper & Row.

Mullin, B. 1998. The biology and management of purple loosestrife (*Lythrum salicaria*). *Weed Technol.*, **12**: 397–401.

Nelson, T.A. 1997. Epiphyte–grazer interactions on *Zostera marina* (Anthophyta: Monocotyledones): Effects of density on community function. *J. Phycol.*, **33**: 743–752.

Niu, C.J., Nakao, S. and Goshima, S. 1998. Energetics of the limpet *Lottia kogamogai* (Gastropoda: Acmaeidae) in an intertidal rocky shore in southern Hokkaido, Japan. *J. Exp. Mar. Biol. Ecol.*, **224**: 167–181.

Pennock, R.T. 1999. *Tower of Babel*. Cambridge, MA: MIT Press.

Quammen, D. 1996. *The Song of the Dodo: Island Biogeography in an Age of Extinctions*. New York: Scribner.

Ruiz, G.M. and Fofonoff, P. 1999. Non-indigenous species as stressors in estuarine and marine communities: Assessing invasion impacts and interactions. *Limnol. Oceanogr.*, **44**: 950–972.

Sagoff, M. 1999. What's wrong with alien species? *Report of the Institute for Philosophy and Public Policy*, **19**(4): 16–23.

Salva, O., Chapin, F.S., Armesto, J.J., Berlow, E., Bloomfield, J., Dirzo, R., Huber-Sanwald, E., Huenneke, L.F., Jackson, R.B., Kinzig, A., Leemans, R., Lodge, D.M., Mooney, H.A., Oesterheld, M., Poff, N.L., Sykes, M.T., Walker, B.H., Walker, M. and Wall, D.H. 2000. Global biodiversity scenarios for the year 2100. *Science*, **287**: 1770–1774.

Schoener, T.W. 1985. Some comments on Connell's and my reviews of field experiments on interspecific competition. *Am. Nat.*, **125**: 730–740.

Slobodkin, L. 1954. Population dynamics in *Daphnia obtusa* Kurz. *Ecol. Monogr.*, **24**: 69–88.

Slobodkin, L. 1961. *Growth and Regulation of Animal Populations*. New York: Holt, Rinehart & Winston.

Slobodkin, L. 1962. Energy in animal ecology. *Adv. Ecol.*, **1**: 69–101.

Slobodkin, L. 1972. On the inconstancy of ecological efficiency and the form of ecological theories. In *Growth by Intussusception: Ecological Essays in Honor of G. Evelyn Hutchinson* (E. Deevey, ed.), pp. 291–306. Hamden, CT: Archon Books.

Slobodkin, L. 1992. *Simplicity and Complexity in Games of the Intellect*. Cambridge, MA: Harvard University Press.

Slobodkin, L., Smith, F. and Hairston, N., Sr. 1967. Regulation in terrestrial ecosystems and the implied balance of nature. *Am. Nat.*, **101**: 109–124.

Slobodkin, L., Bossert, P., Matessi, C. and Gatto, M. 1991. A review of some physiological and evolutionary aspects of body size and bud size of hydra. *Hydrobiologia*, **216/217**: 377–382.

Temple, S. 1977. Plant–animal mutualism: Coevolution with dodo leads to near extinction of plant. *Science*, **197**: 4–7.

Treberg, M. and Husband, B. 1999. Relationship between the abundance of *Lythrum salicaria* (purple loosestrife) and plant species richness along the Bar River, Canada. *Wetlands*, **19**: 118–125.

Turner, F. 1970. The ecological efficiency of consumer populations. *Ecology*, **51**: 741–742.

Verheye, H.M., Hutchings, L., Huggett, J.A., Carter, R.A., Peterson, W.T. and Painting, S.J. 1994. Community structure, distribution and trophic ecology of zooplankton on the Agulhas-bank with special reference to copepods. *S. Afr. J. Sci.*, **90**: 154–165.

Verhulst, P.F. 1838. Notice sur la loi que la population suit dans son accroisement. *Correspondence Mathematique et Physique*, **10**: 113–121.

Wallace, B. 1998. *The Environment: As I See It*, *Science is Not Enough*. Elkhorn, WV: Elkhorn Press.

Wardle, D. 1999. Is ‘sampling effect’ a problem for experiments investigating biodiversity–ecosystem-function relationships? *Oikos*, **87**: 403–407.

Whittaker, R. 1970. *Communities and Ecosystems*. New York: Macmillan.

Williams, G. 1966. *Adaptation and Natural Selection*: *A Critique of Some Current Evolutionary Thought.* Princeton, NJ: Princeton University Press.

Wilson, E.O. 1975. *Sociobiology*. Cambridge, MA: Belknap Press of Harvard University Press.