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Observations in ecology: you can't make progress on processes without understanding the patterns

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Abstract

Coastal marine ecology is, quite properly, increasingly focussed on experimental tests of hypotheses about processes. These are, however, done to explain observations and patterns. It is therefore appropriate to be able to publish quantitative observations to provide the context and basis for studying mechanisms and processes. Ecologists are concerned about very different types of observations. Some areas of study are still totally dependent on observational, descriptive evidence; some depend on mensurative tests of hypotheses about patterns. Tests of hypotheses about patterns are also needed to validate casual or qualitative observations. Guide-lines for what constitutes appropriate or publishable ecological descriptions are discussed here. These recognize the experimental, hypothesis-testing nature of many descriptive studies and consider the relevance of sound logic and experimental design in the planning, collection and interpretation of observations. © 2000 Elsevier Science B.V. All rights reserved.

Keywords: Logic; Mensurative experiment; Observations; Patterns

1. Introduction

During the last 25–30 years, there has been a revolution in the way ecology and coastal marine ecology in particular have developed. Increasingly, intrusive or manipulative experiments have been used to test formally constructed null hypotheses derived from conceptual or mathematical models of the way processes operate. The rise in experimentation has been phenomenal (see Fig. 1 in Underwood, 1996a). It has, however, been accompanied by expanded understanding about, knowledge of, or

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discussion concerning the accompanying tools of logical structures (e.g. Loehle, 1987; Mentis, 1988; Underwood, 1990, 1991), experimental design (e.g. Hairston, 1989; Underwood, 1997) and analytical procedures (e.g. Scheiner and Gurevitch, 1993; Resetarits and Bernardo, 1998).

At the same time as experimental procedures have developed, some journals have adopted policies or practices, or have simply followed the predilections of referees, to downplay the importance of publishing sound, well-structured observational studies. This has resulted in a potential bias against the development of ecological generality (Foster, 1990), which seriously undermines the establishment of context in which processes and their relative importance can be gauged (e.g. Menge and Sutherland, 1976, 1987), potentially prevents development of better understanding across large spatial scales (Underwood and Petraitis, 1993) and displays lack of understanding of the nature of logical tests of hypotheses about ecological patterns. Descriptive studies are a necessary pre-cursor to sensible experimentally manipulative analyses of ecology. In addition, in many fields that have not developed manipulative experimental methodologies, descriptive studies have retained relatively high status. Properly designed, carefully analysed tests of hypotheses about patterns therefore deserve appropriate recognition as part of the spectrum of experimental ecology.

Here, we briefly summarise the role of observations in the frameworks of experimental ecology. Examples of areas of ecology that routinely depend on observations and so-called mensurative experiments (Hurlbert, 1984) are considered. We re-iterate that critical tests of logically derived hypotheses about ecological patterns are valid as experimental science, including studies that are observational (or mensurative) rather than manipulative. Well-planned, logically constructed observational studies to test coherent a priori hypotheses have the same logical content as any experimental manipulation. As such, observational studies should be published with the same status as other tests of hypotheses.

Although greatly abbreviated, the framework summarized in this paper identifies two clear and different roles for observations in ecological studies. The first is obvious. Observations provide the logical starting position from which to begin to propose explanations or theories. This role is so intrinsic to the whole procedure (Andrew and Mapstone, 1987) that it needs no further elaboration. It is, however, important to explore how well are observations made and patterns described. This is considered in detail later. The second role of observations is that of providing the new information that tests specific hypotheses where these *are* about patterns. In such cases, observational studies, rather than intrusive or manipulative experiments are the tests of specified hypotheses. Hurlbert (1984) distinguished such observational tests from more direct controlled experimentation and called the former 'mensurative experiments'.

In both cases – preliminary observations and descriptions of patterns on the one hand and mensurative tests of hypotheses on the other – observations are made in an entirely quantitative hypothesis-testing framework.

1.1. Descriptive studies in three journals

In the context of a special issue of *Journal of Experimental Marine Biology and*

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Ecology, we examined what proportion of its recent papers were observational. The contents of two years' worth of *Journal of Experimental Marine Biology and Ecology* (*JEMBE*) were examined, omitting any special issues (of which there were 3 during the period). Special issues were omitted because their contents are unlikely to be typical of the usual contributions to the journal. There is no basis to believe that the 1997–1998 volumes (208 part 1 to 231 part 2) were in any way atypical. Of a total of 317 papers in the 39 issues of *JEMBE* examined, 44 (14%) were purely descriptive – on a range of topics, including fisheries, oceanography, biology of marine invertebrates, etc. A further 84 (27%) were accounts of descriptive studies done to test articulated hypotheses. These are what Hurlbert (1984) has called mensurative experiments. 184 papers, or 58%, were manipulative. The remaining 5 (2%) were reviews or some other type of contribution.

Similar analyses were done for *Marine and Freshwater Research* (*MFR*) (a total of 177 papers) and the *Journal of the Marine Biological Association of the United Kingdom* (*JMBA*) (138 papers) – these being specifically marine-orientated journals with a wide range of topics and authors being represented. The percentages of papers in each of the three categories were as follow: descriptive: *MFR* = 51%; *JMBA* = 70%; mensurative: *MFR* = 27%, *JMBA* = 20%; manipulative: *MFR* = 16%, *JMBA* = 9%.

Cursory observations in a number of other ecological and marine journals indicated similar results to the last two. General marine journals publish a majority of descriptive papers and relatively few accounts of manipulative experimental studies. *JEMBE*, because of its special concern for experimental marine biology and ecology, does indeed publish a greater proportion of experimental contributions. Nevertheless, *JEMBE* still publishes a notable proportion of purely descriptive, observational studies.

This provokes the notion that there should be some clear code of understanding about what may or may not constitute an observational study worthy of publication in such a journal. As a result of considering these issues, it is clear that not all descriptive, observational studies are experimental and there has to be some conceptual framework in which authors, referees, editors and readers can operate to determine which contributions warrant space and attention in major journals. From this background, some recommendations are made about criteria for publication of observational studies.

1.2. Ecological logic and logical ecology

There is little need to re-iterate that ecological investigation is only progressive when it can depend on sound logical frameworks. This topic has been discussed in a broad spectrum of contributions (particularly relevant to ecology are Simberloff (1980) and Shrader-Frechette and McCoy (1990)). There are differences in points of view (see discussion in Peckarsky, 1998), but there is no lack of agreement that the primary purpose of ecological investigation is to *understand* and *explain* natural phenomena, ecological processes and, therefore, the resultant patterns of distribution, abundance, diversity and interactions of species. This has a long and respectable history (Worster, 1977; McIntosh, 1985), even though the recognised frameworks for investigation themselves show a temporal succession (Simberloff, 1980). There is, despite any disagreements about the details of possible logical structures, consensus that ecology is about planned investigations, not the cataloguing of accounts of phenomena.

One of the few triumphant consensuses of the last few centuries of philosophical debate about science has been the unarguable demonstration that inductive inferences are never (can never be) deductively valid (see particularly Hume, 1779 and, more recently and accessibly, Medawar, 1969; Goodman, 1973). As a result, the accumulation of observations without any theoretical basis and without a logically constructed framework for interpretation cannot, of itself, provide any advances in conceptual understanding. The publication of purely observational studies – particularly those of single phenomena – cannot, on its own, advance ecology and will lead to increasing its confusion with stamp-collecting (to use the pejorative term attributed to Lord Rutherford). So, it is important and appropriate for editors of journals to have closely-packed filters to prevent phenomenological observations from swamping the literature. Otherwise, nature being variable allows endless possibilities for an infinite documentation of singular observations.

So, the logical basis of more coherent ecological studies needs to be much more than the acquisition of observations. Fortunately, frameworks exist. The one followed here is a falsificationist approach (Underwood, 1990, 1991). In principle, whatever observations have been obtained are to be explained by one or more conceptual models of processes that are able to explain how the observations could be made. Differentiation among these models requires critical experimental tests as attempts to refute them. The experiments must be designed to provide unambiguous tests of hypotheses or predictions derived from each model (or combination of models). The details will not be pursued here, but the principles are well-known and widely used. The logical structure of this sort of research has been described many times and summarised in an ecological context by Underwood (1990).

Ignoring, for now, problems due to statistical uncertainty and issues about why the observations were made in the first place, there are only two possible outcomes of a valid test of a null hypothesis. If the null hypothesis is retained, the model is falsified. If the null hypothesis is falsified, the model is supported. Either way, a disproof is used to advance understanding. Either way, this is not the end of the study because singular disproofs are not adequate as a basis for reaching conclusions (see also Connell and Sousa, 1983; Underwood, 1990, 1991). Nor are single instances of failure to disprove a null hypothesis sufficient to cause irrevocable rejection of a model, even though, in any particular test, its predictions failed. Among other concerns, statistical uncertainties mean that there is almost always a risk of Type I error (rejecting a null hypothesis when it was true, that is supporting a false model). Similarly, there is always a chance that a Type II error occurs (retaining a false null hypothesis and therefore refuting a correct model). In all cases, rejection or retention of null hypotheses leads to a need for more rigorous tests and refinement of models or development of new models.

1.3. Theory-ladenness, observations and falsification

One major criticism of describing any study starting with observations is the so-called 'theory-ladenness of observations' (Hanson, 1959; Koyre, 1968; Feyerabend, 1975). This criticism stems from the notion that no-one freely observes things. What to observe,

how, where, when, and by whom are all cultural biases.

This is a well-known problem. It has been examined in many contexts. For example, fish seem to be more numerous in certain habitats. Several different explanations have been put forward, but all are mistaken. The problem is not to see around the corner, but to see through the glass. A second problem is that the ground for the 'theory-ladenness' of observations is not the roots, as is often claimed, but the branches.

A relevant example is the sampling of roots of trees. Without a theoretical framework, the sampling is from the ground. The results are used. The results are in numbers. The results are for the future. The results are than will be true. The results are possible. Underwood (1990) has discussed this problem in detail.

2. Examples

Given the complexity of ecology, the patterns of interest are complex. Starr (1990) has given an example of current research.

2.1. Variation

Despite the complexity of assemblages in different habitats, this variation in diversity is considered.

how, where and when to make observations are determined by prior biases, theories and cultural backgrounds.

This is not a problem for experimental ecology – provided the alternatives are examined thoroughly. Suppose that someone claims to have observed some pattern (e.g. fish seem to be more numerous around the roots of mangrove trees than elsewhere). Several different models can explain this observation. One is that the observer is mistaken or deluded and the pattern is not really there (for example, fish may be easier to see around the roots than in other places, perhaps because fish are more active there). A second model is that the observer, having been taught (and believing) that fish are supposed to be attracted to mangrove roots or that mangroves form important nursery ground for fish, may have looked much more carefully for fish in such places. This is the ‘theory-laden’ explanation. The third model is that there really are more fish around roots, as observed.

A relevant hypothesis from the third model is that careful, unbiased representative sampling will reveal a greater number of fish per unit area, or volume of water, near the roots of mangroves than will be found elsewhere in similar habitats at similar depths but without mangroves. The new observations required to test this prediction are the data from the new sampling, by whatever defined method and whatever sampling design are used. Predictions from either of the other two models are that there will be no difference in numbers found by careful, unbiased sampling. This forms part of the null hypothesis for the first case (the remainder is that fewer fish will be found around mangrove roots than will be found elsewhere). The first model cannot be true if either of the others is true. The test can falsify the first model or the other two. Thus, if ‘theory-ladenness’ is a possible model, its incorrectness can be investigated. Other details and examples are in Underwood (1990, 1991, 1997).

2. Examples of patterns that ecologists are concerned about

Given that properly quantified observations are always going to have a role in ecology, it is worth a brief evaluation of their intrinsic nature. Ecology is largely about patterns and processes in variable systems. Ecologists are an eclectic group and interested in patterns at numerous scales, often overlapping in space and time (Allen and Starr, 1982). The sorts of issues that are the focus of study are legion, but some examples will illustrate the sorts of observational base that underpin some areas of current ecological practice.

2.1. Variability in time and space is natural

Despite the widespread *belief* in the ‘balance of nature’, it is *well-known* that natural assemblages in marine (Underwood, 1996b), terrestrial (Botkin, 1990) and freshwater habitats (Likens, 1992) are patchy and unpredictably variable through space and time. This variability is measured as changes or differences in abundances of species, or in the diversity of the assemblage. Our perception of such variability has changed from it being considered ‘noise’ or a nuisance, to it being recognised as natural and important in

understanding population dynamics and in developing general ecological understanding (Horne and Schneider, 1995). Variability in abundances and diversity inevitably occurs at an hierarchy of spatial and temporal scales (e.g. Morrissey et al., 1992a,b; Underwood, 1996b). The availability and quality of habitat vary from place to place at scales from centimetres to hundreds of kilometres (Bell et al., 1993; Archambault and Bourget, 1996; Thompson et al., 1996). This can affect abundances and diversity directly by only providing appropriate habitat in some places (e.g. Chapman, 1994), or indirectly by modifying biological interactions among different species (Menge et al., 1985; Fairweather, 1988).

Spatial and temporal variability are generally interactive, i.e. changes through time in one place are not shown in other places, even in those nearby (e.g. Thrush et al., 1994; Underwood, 1996b). Such spatio-temporal interactions affect local abundances of single species (e.g. Underwood, 1996b), interactions among species (e.g. Underwood et al., 1983; Menge et al., 1985) and the diversity of the assemblages (Underwood and Chapman, 1998a). Variability occurs because of individual differences in behaviour, rates of growth, etc., coupled with differences in population dynamics among largely separated populations living in discrete patches of particular habitat throughout a mosaic of different habitats (Hanski, 1991). The structure, sizes and spacing of patches of habitat are each very important in determining abundances of local populations and their rates of change.

Emphasis on variability among populations has often concentrated on large spatial scales, for example, with respect to patterns apparently governed by strong environmental gradients, e.g. tidal level (Lewis, 1964; Underwood, 1996b), wave-exposure (Menge, 1978). At large spatial scales, patterns of abundance may be more stable (e.g. Brown et al., 1995), although, in marine habitats, geographic ranges and local abundances can vary markedly over relatively short times because of changes in temperature or water-currents, leading to changes in patterns of recruitment over large areas (Connell, 1985).

Recent studies have, however, emphasised the large variability in abundances of individual species (Morrissey et al., 1992a; Underwood and Chapman, 1996) and diversity (Vivian-Smith, 1997; Underwood and Chapman, 1998a,b) at very small scales, i.e. among patches of habitat only centimetres or metres apart within a site, rather than among sites on a shore or among shores. In some cases, the physical features of habitat are sufficient to explain the patterns (Fairweather, 1988; Thompson et al., 1996; Archambault and Bourget, 1996). In others, complex behavioural interactions among individuals are important in determining patterns of abundance at different spatial scales (e.g. Chapman, 1998; Underwood and Chapman, 1996). In some areas, variability in species richness and in abundances of single species are best explained by the processes acting at different spatial scales (e.g. Archambault and Bourget, 1996). In others, small scales alone explain most variability in diversity and abundances (Underwood and Chapman, 1996, 1998b).

2.2. Large spatial scales

The patterns of spatial distribution of taxa across the globe and the means by which

these distributions were achieved are two essential concerns of biogeography. Studies of biogeography test hypotheses about patterns in order to assess the validity of models about the existence and causes of past and present spatial distribution. Some spatial patterns are analysed by comparing the numbers of species or groups per region. The most striking of these are descriptions of taxa and their diversity in relation to latitudinal gradients and terrestrial biomes in relation to climate. Description of spatial pattern also relies on observations of the phylogenetic relationships of the components of one region with the phylogenetic components of another. Commonly accepted patterns of major groupings of the world's biota are based on the distribution of taxonomically similar organisms in easily observed groups, such as birds, mammals and flowering plants (Cox and Moore, 1993).

Phyto-sociology, or mapping of patterns of assemblages of plants, has a long history as a largely descriptive discipline. Humboldt (1817) and Grisebach (1872) produced some of the first large-scale maps of vegetation of different regions of the earth. These were purely descriptive accounts of the geographic distributions of assemblages of plants, which were treated as units for classification and mapping. The maps were largely interpreted with respect to variations in climate and other physical variables, by comparison of patterns with little or no quantitative or statistical tests of correlation. This approach was continued and expanded into the present century, with the characteristics used to classify vegetation varying among researchers according to their training and personal predilections (Whittaker, 1978) and the number of environmental variables increasing with ease of obtaining measurements.

In many cases, these descriptions were not simply an end in themselves, but have been used as the basis for describing processes that caused the patterns described. For example, Merriam's concept of 'life-zones' (Merriam, 1894) proposed models for elevational and latitudinal distributions of species and Whittaker (1956) proposed environmental correlates with distributions of plants across large-scale gradients. Many such large-scale processes cannot (and ethically should not) be tested experimentally at the relevant spatial scale of the patterns themselves. They are necessarily inferred from patterns (e.g. Walker, 1982).

An influential example of patterns across a gradient is Whittaker's (1956) famous analysis of patterns of distribution of vegetation up the Smoky Mountains (USA). That purely observational study was a crucial contribution to modern understanding of assemblages of plants, even though there are still disagreements about the nature of distributions of species along a gradient. A further example of similar outcome was Underwood's (1978) analysis of so-called intertidal 'zonation'. Previous quantitative descriptions, starting with Colman (1933) and Evans (1947), indicated that there were discontinuities in the scatter of upper and lower limits of distribution of species across the gradient from low to high tide on rocky shores. This concept persists despite quantitative evidence that there are no such discontinuities in intertidal assemblages (Underwood, 1978; Chaloupka and Hall, 1985).

Analytical methods used to test hypotheses derived from models to explain biogeographical patterns rely on observations of the distribution of extant taxa, the fossil record and geology (Pole, 1994). Much biogeography involves the description of the timing and movements of continents during episodes of fragmentation and coalescence of ancient

continents. Correlations between geological or climatic events and the subsequent divergence of biota provide evidence for vicariance (Platnick and Nelson, 1978; Rosen, 1978). The only data available to test such hypotheses are clearly and solely observational.

The relatively new discipline of landscape ecology has arisen from the need to link the scales and variability of ecological patterns with ecological functions or processes, taking into account environmental heterogeneity and natural and anthropogenic disturbances (Forman and Gordon, 1986). It has made rapid advances in measuring heterogeneity at a range of scales, using a number of new sampling and statistical techniques (reviewed by Gustafson, 1998). Despite the sophistication and detail at which the maps can be made and interpreted, they are still descriptions of spatial or temporal patterns. Processes causing such patterns are still inferred from correlations with spatial or temporal patterns of environmental variables, sometimes but not always in association with small-scale experimental tests of specific hypotheses which arise from such models.

2.3. Large temporal scales

Studies of extinctions focus on analyses of temporal patterns of extinction over geological time. The capacity to distinguish between models of patterns of extinction (continuous vs. episodic) has been critical to discussions of the causes of extinction. Extinctions should have a random spacing in time if they were caused by many independent events that occurred in an unpredictable fashion. Flessa et al. (1986) argued, however, that the major extinctions are more evenly spaced than is typical of random distributions, forcing speculation that extinctions have similar causes. Descriptions of temporal patterns of extinctions are therefore the basis for testing predictions about different models.

Similarly, changes in abundances of species or composition of assemblages in response to large-scale or long-term natural climatic change are generally descriptive because (i) the spatial scales over which they occur are very large, (ii) the time scales over which they occur are beyond the scope of a single research programme – even beyond the lifetime of ecology as a scientific discipline or (iii) many events are unpredictable in time and space and therefore researchers necessarily respond to the event after it has happened. For example, Beatley (1974) described the growth, succession and development of the assemblage of plants in the Mojave Desert in response to unpredictable rainfall over a period of 13 years and 68 sites. Very long-term natural changes in vegetation in response to climatic change, for example the forests of New Jersey over 1000s of years, are descriptions based on a mix of personal reminiscences and data from fossil pollen, old vegetation maps, etc. (e.g. Botkin, 1990). Studies of succession have been largely descriptive, leading to researchers redescribing previously described patterns or processes and re-inventing new terms for perfectly good previous ones (critically reviewed by McIntosh, 1980). Succession should, wherever possible, be examined experimentally because there are always several competing models about different successional processes (Connell and Slatyer, 1977).

For similar reasons, responses of species and assemblages to large-scale natural catastrophic disturbances, such as flood, fire, hurricanes or anthropogenic changes are

necessarily descriptive (Vogl, 1980). Systematic before-after sampling is not usually done because of the unpredictability of the event. Typically, changes of abundance and/or composition of the assemblage are described after the perturbation, e.g. the biotic changes after the eruption of Mount St Helen (del Moral et al., 1995), changes in abundances of the intertidal kelp, *Lessonia nigrescens*, in response to coastal uplift during the Chilean earthquake of 1985 (Castilla, 1988). Sometimes, large-scale events fortuitously affect sites where there are temporal and spatial sets of data prior to the event, thus allowing the magnitude of the effects to be quantified (e.g. Underwood, 1999). Nevertheless, such studies are still descriptive because the environmental perturbation is difficult or impossible to reproduce experimentally, although small-scale experiments can be used to test specific hypotheses associated with pattern of change (Underwood, 1999).

Because with many large-scale disturbances, the disturbance is perceived as being so obvious, comparisons of change with control or reference locations are not or cannot be made. This approach can, however, lead to erroneous conclusions. For example, perceived responses to a very large hurricane in central New England in 1938 (Foster, 1988) were subsequently (experimentally) shown to be probably due to intensive disturbance and removal of the destroyed trees after the event (Foster et al., 1997).

2.4. Trans-specific correlations as evidence of processes

Another class of observational data that is widely described in ecological studies is patterns of correlation between species. These are so widespread that they need little comment here. As an example, it is quite common for competitive interactions between species to be inferred from observations of negative correlations in densities of two (or more) species that are known to share resources (see review by Connell, 1983; Schoener, 1983). Positive correlations between abundances of two species are sometimes taken to be evidence of mutualisms or other positive interactions (e.g. Janzen, 1970; Bell, 1971; Fricke, 1975).

Of course, correlative patterns in abundances or distributions of species do not identify the process(es) causing the pattern. For example (and obviously), a negative correlation of abundances of two species can be due to different requirements for microhabitats, different rates of mortality due to diseases, predators or disturbances in different parts of their joint range or because of interspecific differences in colonization or recruitment of offspring of the two species. To unravel these alternatives and their interactions requires experiments, not just descriptions of negative correlation between species (Simberloff, 1984; Underwood, 1986, 1992). Such studies are appropriate and necessary precursors to most studies of interactive processes. If the hypothesized patterns are not found, the interaction is not present. If the patterns are found, experimental tests are needed to discriminate among predictions about competition and predictions from models for other processes.

Nevertheless, there is no point in embarking on time-consuming and expensive experimental studies unless there is some evidence that a process is actually operating. The starting evidence for the existence of some ecological process is the observed ecological pattern of correlation. So, without preliminary observational studies, there is

no starting situation to investigate! It is possible, for example, that competition occurs between two species without there being any correlations from place to place in their densities. In such cases, there would never have been any observations of negative correlation requiring explanation and the invocation of such processes as competition.

2.5. Management of fisheries

The management of fisheries is based on quantitative choices about how much fishing to permit and what specific limits to place on catches (times of fishing, sizes of fish, total landings, locations of fishing). These choices centre on descriptions of abundance of stock with respect to varying conditions of effort in time and space. Historical data are fitted to quantitative models, new parameters are estimated and quantitative predictions are made about the reactions of populations of fish to alternative choices of management (Hilborn and Walters, 1992).

The inability to distinguish between alternative models has often led to confusion about the actions of management instead of increased understanding. This has led several workers to advocate an experimental approach to management (Larkin, 1972; Walters, 1986; McAllister and Peterman, 1992), so that managerial actions are treated as deliberate experiments to test hypotheses about the dynamics of populations (Walters and Hilborn, 1978). Science and management of fisheries continue to be almost entirely dependent on observational studies.

2.6. Comparative analyses of life-histories

Comparative studies of patterns of life-history are used to test hypotheses about broad patterns in ecology and evolution. This approach has been used to test ecological models about the existence of correlations between particular characteristics of life-history and ecological traits. The most famous of these is the correlation of *r*-selected and *k*-selected species (MacArthur, 1972) to various aspects of their biology and ecology (Pianka, 1970). These models, based on descriptive correlations, have general acceptance in ecological texts (e.g. Krebs, 1985) forming a basis from which to test ideas about the conditions and extent to which populations are regulated by density-dependent processes.

The comparative method is supposedly distinguished from many other observational studies by its comparison of observations across a number of taxa, for example, herbivorous insects (Cappuccino, 1995). Observations are made over a wide range of species, for example species that do or do not erupt, in order to identify traits associated with this tendency. Although the findings vary substantially among studies, the one striking association is that insects which feed in aggregations have a greater tendency to erupt (Hanski, 1987; Hunter, 1991, 1995).

Notwithstanding the insights gained from these kinds of comparison, such correlations cannot unravel the factors that regulate the abundances of populations. Association may be a product of a causal link between aggregation and eruptions or a product of coincidence. The advantages of aggregating are well established in theory and in quantitative models (Hunter, 1991; Codella and Raffa, 1995; Pitcher and Parrish, 1993),

but empirical evidence is inconsistent (Cappuccino et al., 1995). While the weakness of the comparative methods prevents interpretation of the causes of ecological patterns, this approach is central to test of models of evolution.

2.7. Some analyses of indirect interactions

The study of indirect interactions has mainly been comparative, contrasting nearby sites with or without species thought to interact indirectly with other species. The main difficulty with these natural or mensurative experiments (Hurlbert, 1984) is that supporting evidence for models of indirect interactions do not eliminate conflicting models. Consequently, the interpretation of such studies has been treated with suspicion, especially when their findings appear to be exaggerated.

Predation by sea otters having an indirect effect on abundance of kelp is a case in point. In the Northeast Pacific, predation by the sea otter (*Enhydra lutris*) is thought to limit the abundance of a variety of invertebrates, especially sea urchins, which, in turn, allows the development of kelp forests. This interaction has been reported as being 'extremely' or 'most' important in organising assemblages of kelp in this region (Estes and Palmisano, 1974; Dayton, 1975; Duggins, 1980) and has become a major paradigm in subtidal ecology. The evidence for this model is mainly comparative, contrasting nearby sites with sea otters to nearby sites without them (see Estes and Duggins, 1995 and references therein) and the interpretation of these patterns is particularly controversial (Foster and Schiel, 1988). Much of the controversy centres on the extent to which these mainly descriptive patterns have been interpreted as strong evidence for the indirect effects of otters on kelps and whether single observations can be presumed to be general over an entire region (Foster, 1990). Experimental removals of otters have not been done, even though there is overwhelming evidence that properly controlled experiments are necessary to provide any sound basis for unravelling the complexities of indirect interactions (e.g. Wootton, 1993; Menge, 1997).

These cases all illustrate how useful observational studies can be, even where there have been no subsequent experimental manipulations to test processes creating patterns. They demonstrate how analyses of observations collected across gradients can be used to test explicit hypotheses about processes.

3. Ground rules for publication of descriptive studies

The above brief (and selected) set of examples illustrates the incontrovertible fact that ecologists continue to wish to do observational studies. Some of the examples illustrate that making observations is the sole method of attempting to test predictions about the world. This remains fraught with philosophical and logical problems, but is necessary in such areas of ecology. Other examples above demonstrate that some ecologists choose to make observations, even though alternative, more experimental frameworks are available.

There is no possible doubt that observations of patterns or lack of patterns are the fundamental starting-blocks for ecological study. Until patterns have been described,

there is no basis for invoking explanatory models about processes. Nor is there any mechanism for understanding the scale and scope of any process that may be operating. Those outcomes of even a cursory examination of ecological work lead inescapably to the conclusion that observational studies should and must be published.

Observational studies, like experimental studies in ecology, are, however, of varying quality and worth. Some have more value in being published than others. The following, therefore, are offered as recommendations to help achieve some consistency in the publication of observational studies.

3.1. Non-singular nature of the type of observation

There are two classes of observational studies that merit publication. The first is observation of some large-scale, long-term or rare (in space, time or both) phenomenon that would otherwise go unremarked. For such publication, the onus must be on the author to demonstrate why it is not appropriate to have more than one instance of the observation. The appropriate spatial and time-scales over which the observation was made must be completely specified.

The second class of observations are those that can and should be made at several times and sites. In these cases, there is no basis for publication of single instances. This then imposes some serious constraints on the nature of the description. Once more than one time or place of observation is described, it is difficult to avoid the necessity for quantitative and usually statistical description. The size, placement, frequency of units of study (quadrats, traps, walks along the beach) must be carefully designed. The degree to which the times, timing, frequency, placement of observations are representative of possible observations must be spelled out. This carries with it all the paraphernalia of sampling theory, description of the domain, scale, etc., of possible observations and the need to summarize the observations. Hence, relevant and appropriate statistical summaries are needed.

Notwithstanding the fact that the only tests of pattern that have any claim on being unequivocal are those that are replicated in space and/or time, the publication of studies that fail to detect patterns are equally as critical to the science of ecology. The attitude that categorises studies as 'failures' or too trivial for publication because they were unable to detect a hypothesised pattern diminishes our capacity to understand patterns. These studies provide new knowledge needed for a broader understanding of patterns and processes. The 'failure' to detect a pattern is in itself a pattern. Hence, the difficulty in publishing these results belies their importance and impedes progress by committing this information to lie unpublished in filing cabinets.

3.2. Hypotheses, designs and methods

Once it is obvious that quantitative summaries of observations are necessary, it is almost always impossible to make the observations without formal planning, design and attention to requirements of statistical issues. A moment's reflection will reveal why this must be so.

It is not possible to identify the typicality, representativeness or generality of any

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observations without some definition of the 'population' or set of possible observations that they represent. Defining the possible set of observations requires thought in advance about what is to be observed. Once the nature and detail of the things to be observed are clear, thought must be given to the accuracy or lack of bias in the observations that is appropriate to the observer. To get accurate or representative observations requires care in the way the representative set of observations is chosen (or looked for or found). This is the business of design of sampling. In many instances, it is absolutely essential to describe the variation (or imprecision) of the set of observations compared with the set that might have been made. Most measures of imprecision require very close attention to the requirements and assumptions of statistical procedures (e.g. Mead, 1988; Winer et al., 1991).

So, any attempt to publish descriptions should have explicit statements about the nature, scope, etc., of the set of observations represented in the description. This requires a coherently defined choice of methods that is appropriate for the specific observations made. Without those being appropriately defined and properly used, the descriptive paper should not be acceptable to referees or editors.

3.3. Hypotheses and the logic of descriptive studies

The final consideration is whether or not the descriptive study is about observations made to test some prediction. The previous considerations identify why, in general, there are three sorts of hypothesis-testing observational studies or mensurative experiments. The first is where hypotheses are made that predict a certain pattern, relationship or correlative structure in the observations. Examples were given before of studies that require observations rather than manipulations to test hypotheses. So, if competition is invoked as a process operating between two species in some habitat, it must be predicted that some pattern of negative association exists between some relevant variable measurable in the two species. Observations should then be made of the appropriate variable to test whether such a prediction is false or supportable. This type of study can supply *necessary* support for an hypothesis. It cannot, of course, provide *sufficient* evidence because other, alternative processes could lead to the same prediction (Connell, 1972; Hairston, 1989; Underwood, 1990, 1997).

Nevertheless, wherever such studies are described, referees and editors need to evaluate the goodness-of-fit of the methods, data, analyses and interpretation to the necessities of the hypotheses. Where the former are inappropriate or erroneous and where the latter are ill-defined, unclear or illogical, the study must not be published.

A second class of valuable mensurative studies is pieces of work that seek to expand the domain of or to generalize a previous finding. For example, it may be well-known that competitive interactions between grazing fish are important in some habitats. Upon encountering similar fish in a new habitat, it is unwise to invoke competition as an important process until predictions derived from this model (i.e. that competitive interactions are general) have been tested experimentally.

The same logic should underpin *any* attempt to generalize. Although variability of ecological pattern is widely encountered, many arguments for generality of processes have been premature because they have assumed homogeneity of patterns and processes

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Tests of hypotheses about patterns are the primary basis for observational studies. In logic, such tests have exactly the same components as any manipulative experimental study. They do not have the same weight as manipulations because they cannot provide evidence about underlying causes or processes. They are, nonetheless experimental tests. As such, they deserve the same care and consideration and should receive the same critical appraisal as experimental studies about processes.

What is clearly needed is better *ecological* observations to prevent experimental analyses of processes that account for wills-of-the-wisp and chase wild geese. Good accounts of patterns will include clearly defined domains or scales over which they apply, will provide estimates of imprecision for the observations and coherent summaries of repeated cases in space and/or time. They will invoke proper attention to the needs of quantification and the assumptions and requirements of statistical procedures. They will be constructed to survive despite experimental assaults on models for the processes causing them. Above all, they should not be dismissed as ‘only observations’, ‘not experimental’, ‘not about processess’.

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