4 · Operationalization of terms and concepts

Every part of a theory begins as a concept. There are concepts that could eventually describe ecological entities and classes, concepts that may develop into variables that are characteristic of these classes, concepts that may become relationships among these variables, and concepts that will never lead to anything of scientific interest. Because weak concepts are hard to eradicate and because new concepts are constantly being created, the ecological literature will always contain concepts at every stage of elaboration (Table 4.1).

The creation of a new scientific theory from the range of available concepts is no easy task. The vague primordia of our ideas must be given form in our thoughts, this form must be interpreted in light of our experience, and then refined to remove incongruous material while preserving some essence of the original meaning. At the end of this process, the concept will be associated with a set of external phenomena and the association likely legitimized with its own term. Only concepts that have gone through this process of operational definition can be used in a theory. Scientific criticism encourages this operationalization by identifying the present capacities, limitations and roles of existing concepts.

The chapter begins with a brief discussion of what constitutes operational definition, the importance of theory and practice in achieving this definition, and the role of non-operational concepts as precursors to theory. Next, the attributes of concepts that discourage operationalization – complexity, open-endedness, relativism, scale, non-scientific antecendents, nebulous goals – and the various terms that have been used to describe non-operational constructs – concept cluster, conflation, pseudocognate, omnibus term, panchestron, non-concept – are illustrated by ecological examples. The first set of examples represent conceptual typologies or classifications, including the most fundamental classification of all, the dichotomy between organism and environment. Stability and diversity are examined to illustrate the difficulty posed by undefined variables. Next, two conceptual relations, the stability – diversity relation and competitive exclusion principle, are used to exemplify problems that arise when concepts are confused with theories,

Table 4.1 *The most popular 25 ecological concepts as identified by a poll of the members of the British Ecological Society (Cherrett 1988, 1989).*

Concept	Mean	Variance	Number choosing the concept
The ecosystem	5.18	4.50	447
Succession	2.98	3.38	347
Energy flow	2.56	3.48	274
Conservation of resources	2.29	3.41	257
Competition	2.23	3.10	268
Niche	2.14	3.20	245
Materials cycling	2.13	3.13	243
The community	1.88	3.32	189
Life history strategies	1.88	2.94	238
Ecosystem fragility	1.76	3.13	194
Food webs	1.73	2.98	194
Ecological adaptation	1.71	3.03	199
Environmental heterogeneity	1.57	2.95	170
Species diversity	1.47	2.75	176
Density-dependent regulations	1.43	2.78	172
Limiting factors	1.34	2.67	166
Carrying capacity	1.27	2.52	162
Maximum sustainable yield	1.24	2.55	157
Population cycles	1.17	2.50	141
Predator–prey interactions	1.14	2.40	148
Plant–herbivore interactions	1.10	2.40	148
Island biogeographic theory	0.99	2.27	132
Food chain bioaccumulation	0.84	2.11	113
Coevolution	0.82	2.12	115
Stochastic processes	0.78	2.12	99

Note:

Each of 645 respondents selected 10 concepts from a preliminary list of 50 and scored these from first (score = 10) to tenth in importance (score = 1). The mean is the average of these scores and variance in this score indicates the amount of disagreement in these rankings. The lower 25 concepts were, in descending order, natural disturbance, habitat restoration, the managed nature reserve, indicator organisms, competition and species exclusion, trophic level, pattern, *r*- and *K*-selection, plant–animal coevolution, the diversity–stability hypothesis, socioecology, optimal foraging, parasite–host interactions, species–area relations, the ecotype, climax, territoriality, allocation theory, intrinsic regulation, the pyramid of numbers, keystone species, the biome, species packing, the 3/2 thinning law, and the guild.

The final set of illustrations considers a series of non-operational statements from environmentalism. Although these propositions are not strictly parts of ecology, they are included in this critique because the public has confused them with ecological theory, and because their simplicity and popularity may make the shortcomings of non-operational concepts easier to appreciate: some concepts seem to advance authoritative, factual propositions and so to replace theories, but this substitution is actually a deception because the propositions are predictively empty and atheoretical. The chapter concludes by summarizing some of the scientific difficulties associated with the dominance of concept over theory in ecology.

Operationalization of concepts

Operationalization or operational definition is the practical specification of the range of phenomena that a concept or term represents. As a result, theories apply to the world around us only to the extent that they are based on operational terms, because operational definition describes this application.

The role of theory in operationalization

The importance of operational definition does not lie in fixing the conceptual entity itself, but in the degree to which such a definition allows access to information through theoretical relations that use the concept. For example, a prime goal of ecology is to predict the population density of an organism, but an estimate of animal density alone is uninteresting. The value becomes interesting because it might be used to predict other attributes, like cropping rates, disease control, or extinction rates. Operational definitions are adopted only if they lead to effective theories, and the most informative definitions function in many different relationships.

The importance of functional definition has long been recognized. G.E. Hutchinson (1953) expressed it with characteristic grace over a generation ago: 'knowledge appears to consist of known relationships between entities which are apparently unanalysable. And as they are studied these entities become relations between still more abstract entities'. A similar concern is reflected in Popper's (1985) disdain for definitions that try to capture the essence or truth of the entity being defined. Popper prefers terms that are simply convenient labels for the operations required to identify observable entities with theoretical elements. Such operational definitions obtain their warrant because they function in prediction.

Operationalization is not achieved by decree, by proclaiming that a given concept will be represented by a designated term, defined in such-and-such a way. In ecology, such attempts (Edwards & Fowle 1955; Milne 1961; Hurlbert 1981; Whittaker, Levin & Root 1973) have rarely been successful. Instead, operational definition is achieved by relating the concept to a series of defining operations, and then demonstrating that such an entity so defined can play an important role in theory.

If a definition seems necessary, that definition is likely doing the work of theory. For example, one could define mammals as hairy homeotherms that give milk to their young. Like all definitions, this would be sterile, unless we had a theory of the form 'all mammals (i.e. hairy, milk-producing homeotherms) are X'. The definition would do the work of a theory if part of the definition were used to identify an entity as a mammal and the remainder to predict some further characteristic: any animal suckling its young is hairy and homeothermic.

Maintaining operationalization

Standardized operational definitions are essential if different workers are to make similar measurements of similar entities. Since no definition, whether operational or not, is so precise that all doubt about the meaning of a word is removed (Sattler 1986), initially unnoticed ambiguities in the definition may lead to misapplication of the theory. As a result, operationalization is always incomplete and operational definitions are often amended over time. However, if a concept is to prove scientifically useful, both the original definition and its replacements must be sufficiently operational that informed users associate the concept with similar phenomena (Hull 1968). This degree of operationalization is essential to any assessment of predictive power.

Because complete operationalization is impossible, procedures diverge as different workers interpret and apply standard methods in slightly different ways. This is one reason that interlaboratory tests vary more than replicated analyses from the same laboratory. To control this definitional expansion, scientists have developed several checks on analytical consistency. Routine application of the defining operations to known standards can detect methodological deviation as a drift in the standard values over time. Comparisons of concurrent estimates of the same entity with independent techniques should reveal inconsistency in operational definitions as the difference in the estimates. A third important constraint is achieved by comparing the predictions of the theory in which the operational entity is imbedded with measurements of the response variables. Discrepancies between present and past standard determinations, or among independent measurements, or between observation and prediction warn of the possibility that the methods have deviated.

The place of non-operational concepts

Initially, operationalization seems hopeless. The concept is nebulous and unfamiliar, its terms incomprehensible, its place in theory and practice undetermined, and its predictive power unevaluable. Only those who are drawn intuitively to the idea are likely to pursue it, since such pursuit requires an irrational faith in the chance of success. As the problem becomes better defined and its potential solution seems more attainable, it attracts other researchers who in turn help to define the concept. If the program succeeds, the concept, its terminology, the operationally defined variables, the theories that have been developed, and the applications of those theories become entrenched by frequent use and paradigmatic example (Kuhn 1970).

The process of theory creation and growth therefore begins with a vague sensation of personal insight and consequent belief, followed by greater public awareness, definition, and involvement as new adherents are drawn into the program. The process culminates in wide acceptance of the operationally defined theory.

Ecology, like every science, desperately needs the intellectual pioneers who will explore new concepts. Without them, the scientific enterprise would grind to a stop. It is therefore normal practice for good scientists to entertain irrational, vaguely formed, poorly defined, unpredictive concepts and their associated terms. These may be the stepping stones that eventually lead to strong theories. Many influential works in the literature do not contain testable theory, but are only propaganda for developing concepts. Presumably, the originator's ideas had developed to a point where they could be expressed, but not to the point where the concepts could be cast as hypotheses. As a result, the ecological reader should be acutely aware of the differences between concept and theory; otherwise, enthusiasm of belief and vigour of exposition may allow concepts that substitute for prediction (Fig. 4.1).

Because a concept is vague – Rigler (1975a) defined it as 'a general notion' – the success of operationalization is uncertain before the fact. Until concepts find their places in testable theories, their place is the personal realm that precedes hypothesis (Chapter 2), and they are justifiable only because they might lead to theory. Attempts to operationalize a concept may yield a new theory or a new variable, but they may only produce a succession of non-operational concepts. Although we must use concepts to develop theories, we should be prepared to jettison those that prove unproductive. We must also realize that to teach or promote concepts as theories may be premature; it is to teach the undefined and to promote the unevaluable.

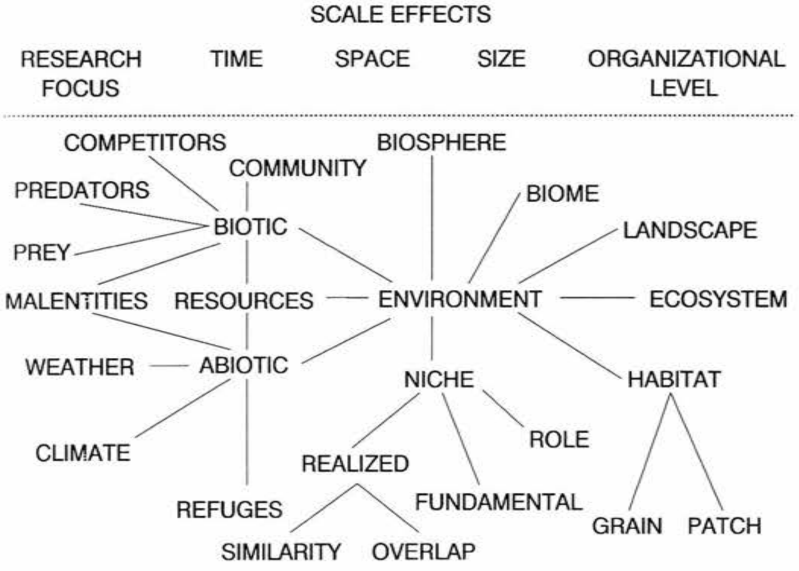

Fig. 4.1. A summary of some interactions among concepts associated with 'the environment'. Such diagrams can only suggest some of the links and associations among concepts, because ecological concepts are so vague and subjective that any rigorous analysis of the concept cluster is sure to fail. Whatever associations are envisaged, they must allow for redefinitions with changes in scale and with changes in the organism upon which the research is focussed.

Because concepts are not open to test until they are re-expressed in theories, the early products of conceptual development are difficult to extirpate when their utility fails. The hopeful spawn of past initiatives haunt the science. Some, like the broken stick model (MacArthur 1957, 1966) and the constancy of ecological efficiencies (Slobodkin 1961, 1972), even survive repudiation by their originators (McIntosh 1982). Some ecological questions – e.g. the natures of competition, regulation, stability, the environment – have been the basis for symposia and discussion for over fifty years (May 1984), yet the questions remain. This lack of resolution suggests that the debates dealt with concepts not theory, since, if theories had been developed, the issues should have been resolved on the basis of scientific evidence.

Typologies and classifications

A recurrent problem in operationalization for ecologists is the isolation of distinct classes that are appropriate for further study. Robert MacArthur (1972b) thought that the ecology of the future would see a fundamental two- or three-way classification of organisms and their environments. Given a system of classification, we would seek theories that predict some class characteristics from measurements of other properties of the class or its environment. Before this could be done, the types and classes must be defined.

The typological approach is essential to questions about the behaviour of populations, species, guilds, communities, and ecosystems, in which these concepts are conceived as distinct entities for study (Fig. 4.2). It is also fundamental to the view that nature involves a set of spatial patterns induced by a mosaic of associations, biocoenoses, communities and biomes, undergoing a sequence of temporal steps between different successional stages (Watt 1947). Such a view can only find application when the different spatio-temporal units can be identified and separated. The realization that such identification was difficult in plant community ecology (Cain 1947; Egler 1947; Mason 1947; Whittaker 1953, 1957), precipitated one of the greatest conceptual changes the science has experienced: the shift from a 'superorganismic' conception of the ecosystem to an individualistic or continuum view (McIntosh 1985; Simberloff 1980a). Analogous typological problems arise whenever discussions centre on class characteristics (McIntosh 1970; Wiens 1983; Schoenly & Reid 1987; Shipley & Keddy 1987; Hawkins & MacMahon 1989).

These problems are not great when the class has already been defined and one response to typological problems has been to suggest that ecologists concentrate on recognized classes (Simberloff 1980a). Taxonomic groupings are particularly obvious for biologists, but presumably any recognized classification would do. Whenever the class can be identified by simple, well recognized properties, a description is straightforward and typological problems are small.

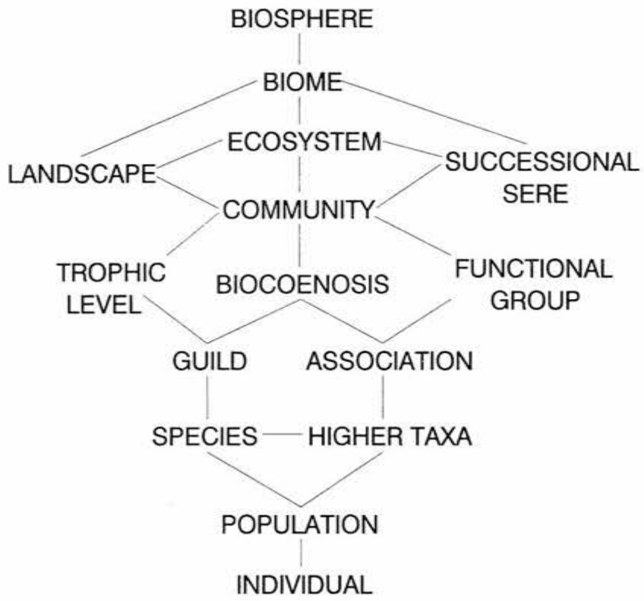

Fig. 4.2. A diagrammatic summary of typological concepts associated with organizational scale in ecology. Although typological analysis depends on the operational definition of the type under study, this is usually only possible with taxonomic entities.

If no prior convention about class membership exists, the properties that define the class must be identified as part of the theory. Selecting these properties can be a difficult problem in operationalization. The researcher must sift through the available data applying a series of alternate class definitions, seeking that which allows the most powerful predictions about the relevant characteristics of that class. This requires a sure grasp of what is relevant to the study, great mental flexibility in entertaining multiple working hypotheses (Chamberlain 1890), and a certain ruthlessness in rejecting unsuccessful pet hypotheses. Nevertheless, the step is essential.

In the absence of a clear operational definition, different users of the term may develop independent, even inconsistent, definitions. In this way, the original conception grows by accretion or 'conflation' of meanings (Lewontin 1979), until any single meaning of the concept appears restrictive and inappropriate. By that stage, the term represents a 'non-concept' (Hurlbert 1971) or an 'omnibus term' (Milne 1961) because it carries so many different meanings that one can never be sure which is intended at any one time. Clarity can then only be achieved if the term is defined at each use, but this proliferates definitions and compounds the problem. For example, Hurlbert (1981) was able to find 27 definitions of 'niche', Hawkins & McMahon (1989) found three basic definitions for 'guild', MacFadyen (1957) lists seven for 'community', Milne (1961) 12 for 'competition', Edwards & Fowle (1955) a similar number for 'carrying capacity' (cf. Dhondt 1988), and Bradie & Gromko (1981) 17 for the principle of natural selection. All definitions for the same term differ and in no case do the reviewers presume to have listed all existing variations. As a result, the class of a study object or phenomenon implies only that it has at least one of the many characteristics identified in different definitions. Peet (1974) has called these groups of similar, but not identical, definitions for a single term, 'concept clusters'.

Some typologies and their operational problems

Lake trophic status

The ambiguity of typologies involving concept clusters is clearly illustrated by the case of lake trophic status. Lakes have long been divided into oligotrophic and eutrophic on the basis of their productivity or trophic status (Naumann 1930; Hutchinson 1969; Rigler 1975a). Since this dichotomy was erected, lake trophic status has become one of the most studied and best known areas in limnology (Carlson 1977; Peters 1986; OECD 1982), but there is still no standard definition of eutrophy and oligotrophy. Oligotrophic and eutrophic lakes may differ in nutrient concentrations, nutrient dynamics, distribution and abundance of plants and animals, hypolimnetic oxygen levels, transparency, primary and secondary production, fish yield, lake morphometry, and the economic development of the drainage basin. In principle, the classification could be based on any of these characters and appropriate relations developed to allow translations among typologies based on different properties (Carlson 1977; Vollenweider 1987). In practice, there are few rules and different lakes cannot be consistently classified. Thus if told that one lake is more eutrophic than another, one knows very little because this phrase could mean so many different things. Indeed, when limnologists from different parts of the world compare notes, they may find that what is an oligotrophic lake for one is eutrophic for another. The classification is too subjective.

The terms oligotrophy and eutrophy now do little more than signal a general area of limnology. Thoughtful limnologists try to avoid these terms in precise discourse, using instead quantitative measurements of algal abundance and activity, or nutrient concentrations (F. H. Rigler, personal communication). These measurements lack the stateliness of an all-inclusive term like oligotrophy or eutrophy, but they are more easily used in quantitative predictions (Peters 1986) and less easily misunderstood. Carlson (1984) has suggested that the terms 'oligotrophy' and 'eutrophy' be abandoned as non-concepts.

Trophic levels

The devision of ecosystems into trophic levels (which are completely unrelated to the limnological term, 'trophic status') as outlined in Lindeman's famous essay on trophic dynamics provides a more complex example of typological problems in ecology. Lindeman (1942) suggested that the aggregation of all the organisms in an ecosystem according to their energetic distance from the sun would reveal important generalities in the assembly and regulation of ecosystems. Lindeman sketched the sorts of generalities that might develop, but he saw his essay as a description of a research program rather than a theory. Because available information was insufficient, he hoped to inspire others to make the necessary measurements from which empirical theories could be constructed. His views, supported by the work of other leading ecologists (e.g. Riley 1946; E. Odum 1953; H. T. Odum 1957; Teal 1957), inspired a generation of ecological investigation, including the International Biological Program of 1964 to 1974, and still represent a fundamental concept for ecology. For example, this view is intergral to various treatments of limitations of different trophic levels made popular by the classic work of Hairston, Smith & Slobodkin (1960), and by many subsequent discussions (e.g. Wiegert & Owen 1971; Van Valen 1973; Fretwell 1977, 1987; Pimm 1980; Carpenter, Kitchell & Hodgson 1985). These studies, and many others, presume that the division of ecosystems into discrete trophic levels will reveal patterns in ecosystem structure or function, but ignore the serious operational problems presented by such a division.

In principle, this division involves only the assignment of the component species of the ecosystem to distinct trophic levels based on their diets, but, with the exception of terrestrial plants (Rigler 1975b), this is an extremely difficult task (Cousins 1987). Many organisms are so flexible in their diet that trophic relations and trophic levels change seasonally (Martin *et al.* 1951), ontogenetically (Hardy 1926; Larkin 1978), and geographically (Livingstone 1988; Rigler 1975a). Omnivores

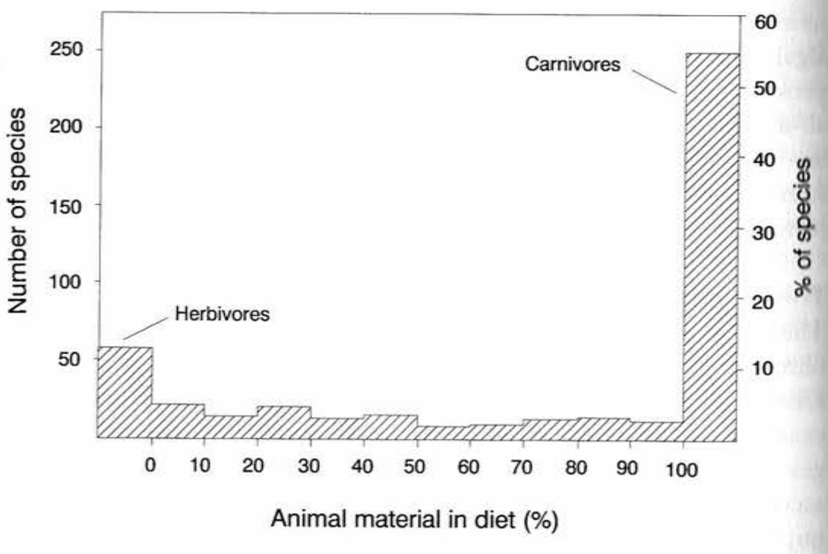

Fig. 4.3. The proportions of plant and animal material identified in gut analyses of species of North American birds and mammals. Only animals studied year round were included in this analysis. From Peters (1977) based on the data of Martin *et al.* (1951).

and detritivores are especially difficult to classify yet the former are common in most ecosystems (Fig. 4.3; Peters 1977) and the latter often dominate energy exchange (Cousins 1985). Finally, the recycling of energy among trophic levels, once considered impossible because of an overinterpretation of the laws of thermodynamics, is impossible to accommodate in Lindeman's scheme (Rigler 1975b), yet it may be quite common in nature (Patten 1985; Cousins 1987).

An example will illustrate some of these difficulties. Herbivores are among the easiest animals to classify to a trophic level (Lindeman 1942). Beavers (*Castor* spp.) eat only plants (Martin *et al.* 1951) and are therefore herbivores, but their position as primary consumers in a trophic dynamic model of their ecosystem is less certain. Not all plant material is equally assimilable (Van Soest 1988), and therefore the beaver's role in establishing the balance between decomposer-based food webs feeding on its feces and herbivore-based webs depending on the beaver will vary seasonally and geographically with the beaver's diet. Even the position of beavers as primary consumers is in doubt; all mammals grow to 10–20% of their adult size while still totally dependent on their mothers' blood supplies or milks for nutrient (Millar 1977), so an age-dependent

portion of the beaver's tissue is certainly that of a secondary consumer. In addition, adult mammalian herbivores depend on intestinal flora to digest their food and, in a sense, rodents are secondary consumers of the wastes of this flora. In practice, these observations are seen as physiological technicalities, irrelevant to trophic dynamics, so beavers would be classed as primary consumers.

The problems with this simple solution loom again when we consider free-living herbivorous micro-organisms in the beaver's ecosystem. To be consistent, we should ignore them and class their predators as primary consumers. Actually, we class these free-living micro-organisms as primary consumers, like the beaver, and their predators as secondary consumers. Such a decision assumes that beavers plus their micro-organisms are energetically similar to free-living microbes, even though this seems improbable because of the basic metabolic differences between homeotherms and unicells (Hemmingsen 1960; Peters 1983). Further uncertainty is introduced when we consider the fate of the beaver's feces and carcass. Should the decomposers of the feces be classed with the intestinal micro-organisms because they feed on the plant tissue that was in the gut, or with the beaver because they feed on the digesta of the gut organisms, or with the beaver's predators because some of the fecal contents are derived from the beaver itself? How should any complex trophic interactions between bacteria, protozoa, fungi and invertebrates in the egesta be treated? Are decomposers of the beaver's corpse in the same trophic level as the beaver's predators, wolves and mosquitoes (Kozlowsky 1968), or should they be classed as decomposers, like the coprophages (Ivlev 1943; Wiegert & Owen 1971)? These questions must be answered if we are to carry through Lindeman's research program (Cousins 1987); instead they have been ignored because they are likely insoluble.

When the trophic dynamic classification of an ecosystem is attempted, the same problems occur with each new species. Moreover, the problems are compounded because the trophic position of any higher element is defined relative to the trophic positions of all of its prey. Any uncertainty in the position of lower-order consumers adds to the uncertainty of the positions of its predators. As a result, the more completely we describe the trophic relations within an ecosystem, the less easily we can divide it into levels (Peters 1988b).

Ulanowicz (1986) and Baird & Ulanowicz (1989) have tried to address these difficulties by defining all decomposers to be primary consumers and by allowing different species to assume intermediate trophic levels reflecting use of several producer levels. Thus trophic levels need no longer be integers. This flexibility allows a more realistic representation of different trophic relationships, but does not address the problems of variance around these non-integral trophic levels, nor the increasing uncertainty of the trophic position of the higher trophic elements, nor the arbitrary solution of the problems of decomposers. Ulanowicz has addressed some of the operational difficulties of the trophic level concept, but I am not yet convinced he has solved them.

Unless we can perform the classification, no statement involving trophic levels can be tested since we have no way of recognizing the entities that such a theory would invoke. Nevertheless, because the term trophic level is only a loose concept, it can easily be associated with selected observations of individual plants, herbivores, or predators, even though these are not trophic levels. These associations sometimes appear to support generalizations about trophic levels (Hairston *et al.* 1960; Slobodkin, Smith & Hairston 1967; Fretwell 1977, 1987). They also illustrate the difference between seeking support for a theory and seeking to test it. To test a theory, we must know what evidence would be inconsistent with it (Murdoch 1966); to support a concept, we need only reinterpret selected evidence in terms of that concept. If the concept is sufficiently poorly defined, we can even claim that supporting evidence confirms the generality but that contrary evidence is inappropriate for the test. Rigler (1975b) believed that Slobodkin *et al.* (1967) used just this defense to argue that they were concerned 'with trophic levels as wholes' so that observations about populations were irrelevant, and that their conclusions were supported by observation, even though the practical difficulties of assigning organisms to trophic levels made falsification impossible.

Foods webs

One response to the operational problems of the trophic level concept has been a renewed interest in food webs (Cohen 1978; Pimm & Lawton 1978;Pimm 1982; Yodzis 1980; Briand 1983; Briand & Cohen 1987; Schoener 1989). This research focuses on trophic interaction, not energy flow, and limits itself to those portions of the ecosystem which have proven tractable in analysis by other authors.

Food web analysis has not sucessfully escaped all the operational problems that dogged trophic dynamics (Paine 1988; Peters 1988b). This approach has yet to develop consistent rules for the construction of food webs in different communities and by different authors. As a result, patterns in food webs may represent the conventions and views of particular schools of ecologists, rather than differences among communities. For example, food webs described from studies of the open water tend to be longer and more narrow than those described by terrestrial or inshore benthic workers (Briand 1983; Briand & Cohen 1987). This may reflect a real difference, or only the tendency of workers in the open water to use larger ranges of organism size in building their food webs but to lump more species together at each level. Food web analysis, like trophic dynamics, has tried to generate theory before operationalizing its concepts. As a result, its components are unidentifiable, the validity of its general constructs cannot be assessed, and its status as scientific knowledge is in dispute (Paine 1988).

Dichotomies

Ecology employs many systems of classification that break continua or complex phenomena into a few alternatives. The resultant problematic typologies include *r*- versus *K*-selection (Pianka 1970; Southwood 1981; Parry 1981), the stages and endpoints of unidirectional successional change (Whittaker 1953; Odum 1969; Drury & Nisbet 1971), the divisions between specialists and generalists (MacArthur 1972a; Fretwell 1975), density-dependent versus density-independent regulation (Andrewartha & Birch 1954), and semelparous versus iteroparous reproduction (Kirkendall & Stenseth 1985). All these concepts map many, imperfectly correlated characteristics onto a simple pair of alternatives.

To the extent that the different characteristics are independent, simple divisions do not adequately represent the phases of the system under study. In most cases, no weighting factors for different phenomena are specified, so that different authors use the scale subjectively. However, even if objective weightings were available, the collapse of a diverse set of measurements into a few classes involves a drastic loss of information. Different workers must reinterpret the typology in the light of their own experience, so the type means different things to different people and in different ecological contexts. Such constructs proliferate meanings, producing first concept clusters and omnibus terms, then non-concepts.

The organism–environment dichotomy

The concept of a separation between the organism and its environment is a simple classification that seems essential to ecology. The dichotomy is implicit or explicit in many definitions of the ecology, including that of Haeckel (1870, as translated by Allee *et al.* 1949): 'the investigation of the total relations of an animal both to its organic and to its inorganic environments'. This dichotomy has been extended until it now includes any division between the identified object of study – an organism, a population, a community, man – and everything else. It represents a characteristic way in which ecologists see the world (Hughes & Lambert 1984).

This section addresses that classification as an important case of operational difficulties in ecology. In a scientific context, it illustrates common operational difficulties posed by concepts that are open-ended, relative, and interrelated. In an ecological context, the particular example is important because the dichotomy is unquestioned in most ecological analyses.

Alternatives

Some examples will illustrate the limitations imposed by the organism–environment dichotomy. In lakes, phosphorus has long been thought to limit phytoplankton biomass and productivity (Hutchinson 1969), but for many years, no convincing relationship was identified because limnologists compared phytoplanktonic responses to 'free' nutrient levels measured in filtrates (Elster 1958). The failure to find clear results was attributed to 'other factors' and 'system complexity', so that the strong relations that exist between total phosphorus levels and most aggregate measures of biological activity in lakes (Peters 1986) were obscured, and phosphorus abatement programs delayed. Some limnologists still consider relations between total nutrient and biological response to be somehow illegitimate because they do not distinguish between the algae and their environment (Horne 1985); oceanographers continue to measure free nutrient levels and to explain the absence of any relation between nutrient and biological activity by the complexity of the systems.

This is not an isolated example, nor is the problem only relevant to aquatic biology. The primary productivity of terrestrial systems is best predicted by evapotranspiration (Lieth & Box 1972), a process which involves both the plants and their environment. The best general models for animal density (Damuth 1981; Peters & Wassenberg 1983; Peters & Raelson 1984; Fig. 2.2) do not use environmental characteristics at all, but depend on animal body weight. This also holds for a host of other allometric models that are the best available predictors of many autecological traits (Peters 1983; Calder 1984).

The separation of organism and environment is therefore not an essential element in ecology. It represents a perspective that may be useful in some situations, but not all. Typological thinking tends to blind us to alternatives and the organism–environment dichotomy is a prime example.

The operational environment

For the purpose of further discussion, I will presume that the organism can be operationally identified. This is usually the case when the focus of the study is a single organism, but is far from certain when the focus of the study is some aggregate, like a clone, population or community. This uncertainty only compounds the problems discussed below.

Definition of environment

The vagueness of the environment concept has long been recognized by ecologists. At least once in every decade, a new and more rigorous definition is advanced. Haskell (1940) recommended a mathematical reformulation, Mason & Langenheim (1958) analyzed the concept using the theory of signs and recommended that environment be restricted to direct interactions between the organism and its world. Maelzer (1965a, b) and Spomer (1973) reached similar conclusions. More recently, Niven (1980, 1982) provided a 'formal definition'.

As yet, these reanalyses have had little effect (McIntosh 1985). Each definition narrows present usage, so new definitions may seem too restrictive. Some discussions use such unfamiliar philosophical contexts that they are opaque to practicing ecologists. Finally, many ecologists are quite content with a vague term, like environment, and see no need for further definition. However, two leading ecologists, Andrewartha & Birch (1984), consider the work of Maelzer and Niven invaluable in their own search for a functional redefinition of the environment. Perhaps, future developments based on 'the theory of the environment' (Andrewartha & Birch 1984) will require re-evaluation of the utility of these rationalizations of the environment concept. What is certain is that without some restriction and redefinition the concept can never find application in a theory.

In practice, the environment is currently identified by stipulating what it is not: The environment is that which is not the object of investigation. Thus the environment of an entity is everything outside that entity. This sweeping definition of environment introduces a number of operational difficulties.

Open-endedness

The definition of environment refers to an infinite series of properties varying in both space and time. This provides an inclusive concept, but since specification of these entities would be an interminable chore, each investigator must choose those aspects of the environment that are appropriate for study. Thus a single term and concept, environment, is represented by many different operational entities.

Because the concept of the environment refers to so much, it often provides an easy explanation. 'Environmental influences' are said to explain the noise in our relationships; apparent falsification of our theories are ignored because 'environmental variations' are said to have abrogated unstated assumptions that all else is equal, and the study of 'environmental factors' is often proposed to explain new phenomena. As an explanation, environment is a 'panchestron', a word that can be invoked to explain everything and anything (Hardin 1957). Not only are such explanations facile and shallow, they are dangerous because they hide our ignorance and discourage more penetrating analyses.

The concept of the environment can only be employed if reduced to something more manageable. We might limit ourselves to environmental properties that significantly affect the focus of the research, but that approach requires us to identify the properties of the environment which are significant to the object of study. In other words, we must have specific hypotheses about the effects of specific factors. Once such hypotheses have been made, a general concept of the environment no longer has a role. It has been replaced by defined variables in a theory worthy of the name. Thus, many theories relate particular environmental characteristics, like temperature or food level, to specific biological responses, but these characteristics are never claimed to represent more than a tiny piece of the environment.

Relativism

An equally serious problem with this definition is that it defines environment relative to the focus of the study. Any change in focus entails a change in whatever constitutes the environment. Minimally the focal individual of one study becomes part of the environment in another, but usually other elements change too. The environment of a litter mite differs from that of a deer or songbird.

The concept environment must accommodate another relativistic shift whenever the scale of the investigation changes. Conspecifics are an important element in the environment of the individual, but are subsumed into the object of study in population ecology. Much of the environment of the population forms the focus for community ecology and the environment of the community can similarly be the focus for ecosystem studies. In another dimension, environmental properties that are relevant on the scale of a day may be less important on the scale of a year.

The relativity of the environment concept results in necessary discrepancies in the way the term is used as the object of study changes. Minimally, this requires care in specifying where the term applies and considerable mental agility in accommodating the conceptual changes which occur when the focus changes. Failure to do so results in inconsistent use of the term and ineffective communication.

Conceptual interrelations

A number of other ecological concepts which specify a multidimensional location of an organism or group of organisms share analogous problems (Fig. 4.1). The community, as the biotic environment, is clearly as non-operational as 'environment', even without considering the associated concepts of biocoenosis and superorganism (MacFadyen 1957). Any argument which requires specification of community or environment is also likely to be non-operational. For example, Elton (1927) described the 'ecological niche' of an organism as its role in the community, Hutchinson (1957, 1959a, b) as the hypervolume that the organism occupies in *n*-dimensional environmental space. Both definitions define the niche relative to a focal organism (or population or species), both refer to infinitely large sets of properties and thus neither concept of the niche can be operationalized. Hurlbert (1981) argues that the niche should be defined on the basis of resources, but because there are likely many resources and because resources are defined relatively (Tilman 1982), this solution is too gentle to be effective. Without radical redefinition of the term, no theory involving the niche can be constructed. Both habitat and ecosystem (O'Neill *et al.* 1986), as multidimensional, unlimited, relativistic entities representing the environment are open to similar criticisms. This has not hindered use of 'the environment', 'the niche' or the other related terms as panchestra for ecological explanations.

The interrelations among environment, resource, community, niche, habitat and ecosystem exemplify a further characteristic of concepts in ecology: interdependence or coherence (Whittaker 1957). Ecological concepts intergrade into and build upon one another. This can blunt the force of criticism by diffusing it over a vast area where the limits of the interdependent concepts are hard to identify. However, persistent criticism can be particularly effective by inducing a 'domino effect' in which the identification of one non-operational concept points the way to others.

Some exceptions

This criticism of the concepts of 'environment', 'habitat', 'ecosystem' and 'niche' should not be construed as a condemnation of all uses of the terms and even less of theories which predict characteristics of organisms using properties external to the organisms as independent variables. Because concepts are at best only vaguely defined, they can be used in different, even inconsistent contexts (Egerton 1973). When use is theoretically sound, the concept has been restricted to represent a few dimensions which are considered sufficient for a particular purpose. Thus Box (1981) defined meteorological hypervolumes for different plant life forms and has shown that this approach permits global prediction of life form. Gates (1980) described the 'thermal niche' within which a songbird can maintain homeothermy. The niche of bluegreen algae can be partly defined on the basis of concentrations of nitrogen, phosphorus and light availability (Smith 1983, 1985), and that of stream-dwelling trout on the basis of temperature (Barton & Taylor 1985). Significantly, none of these authors cast their work in terms of niche theory, and all use the operational definition of their particular variables instead of a general concept like 'the environment'.

Conceptual variables – stability and diversity

Operational problems are not limited to systems of classification, but also effect the properties of these systems which we wish to measure and predict. This is nowhere more apparent than in the continuing controversy over 'stability', its meaning and its ecological relevance, so that debate is used to demonstrate the problems that result when insufficiently operationalized concepts are used as theoretical variables.

The origins of this discussion lie in prescientific concepts of the balance of nature which are certainly much older than the science of ecology (Egerton 1973). In recent years, increasingly strident demands for conservation have forced consideration of natural balance and stability in resource management. Since many ecologists are profoundly conservationist and deplore the wholesale despoilation of the earth, they consider the stability of a natural system to be an inherently valuable property that should be protected. This requires the development of theories to predict stability or its converse, the extent of ecological change.

Despite long-standing interest in stability (*Brookhaven Symposium* 1969; May 1974), the term has never been satisfactorily defined. Part of the difficulty in doing so reflects the dynamism of open biological systems. Because biological systems constantly react to both external and internal changes, they cannot be preserved like a work of art. Instead, stability must be defined in dynamic terms. Such contradictions foster misunderstanding.

Many definitions have been proposed (Holling 1973; Westman 1978; May 1974), but all assume that the concept of stability is too complex to be defined and therefore fragment the general concept into smaller ones. The definitions offered by Orians (1975) are indicative. Orians (1975) suggests that stability may mean many different things: the absence of change ('constancy'), the length of survival ('persistance'), resistance to perturbation ('inertia'), speed of return after perturbation ('elasticity'), the displacement from which return is possible ('amplitude'), the degree of oscillation ('cyclic stability'), and the tendency to move towards a similar end point ('trajectory stability'). These independent concepts are sometimes correlated, and sometimes not (Orians 1975); some, like constancy and resilience, may even be inversely related.

If Orians' scheme were accepted, the sort of stability under examination would have to be specified at each use. This has not yet proved necessary because none of these components of stability are sufficiently well defined to represent a variable in a theory. Instead they represent more concepts, fragments of the original vast concept, which still require operationalization. Attempts to solve operational problems by redefinition produces instead hairsplitting scholasticism and infinite regress (Popper 1985). The debate over stability is a case in point.

The operationalization of stability is rendered still more complex because Orians' categorization does not capture every nuance of this multifaceted concept. In comparing the divisions of stability proposed by different authors, Westman (1978) proposes another set based on a primary division of stability into 'stability' (Orians' constancy) and 'resilience' (the ability to restore structure following perturbation). Resilience was further decomposed into resistance to perturbation ('inertia'), time to recovery ('elasticity'), extent of departure from which recovery is possible ('amplitude' and 'brittleness'), degree to which the pattern of recovery differs from the initial pattern of alteration ('hysteresis') and ease of alteration ('malleability'). By considering constancy and several different aspects of non-constancy (amplitude of regular fluctuations, regularity of fluctuations, persistance and structural maintenance) at different levels of organization ('population', 'community', and 'environment'), Whittaker (1975b) was able to arrive at thirteen different meanings for stability, but he did not intend this to be a complete catalogue. May (1974, 1975) suggested an alternative classification consisting of 'global' and 'neighborhood' stabilities (which are related to, but not identical to, considerations of elasticity), 'neutral' stability (Orians' cyclic stability), and 'structural' stability (which refers to the continuity of change and may be related to Orians' persistence). Since different authors use the same term for different concepts and different terms for the same concept, any ordering of the concept of stability requires a clear head and a thorough housecleaning.

The problem of multiple concepts lurking behind a single term is not the only difficulty that stands in the way of operationalizing stability. Since the parts of a system normally change at different rates, any definition of stability must also determine what the object of this definition is. Whittaker (1975b) intimated that stability would have different definitions when interpreted at different levels of organization. Moreover, within each level of organization, stability will vary among components. For example, populations of small animals will seem less stable than those of large ones (Calder 1984); the stability of species composition may not be highly correlated with the stability of their populations; and some processes or structures may be stable while others fluctuate (Schindler 1987). An operational definition of stability must therefore indicate what aspects of stability of which elements at what level are to be considered. The sort of theory that can be developed will depend on these choices.

Stability, like niche or environment, is one component in a web of concepts shading slowly into one another (Fig. 4.4). 'Equilibrium', 'steady state', and 'balance of nature' are related concepts as are the conceptual mechanisms which maintain stability, like 'negative feedback', 'homeostasis', 'regulation', and 'density dependence'. Stability also invokes an antithesis, 'instability' and its conceptual mechanisms, 'perturbation', 'positive feedback', and 'stress'. Instability, which reflects temporal change, has spatial analogues in 'heterogeneity' and, perhaps surprisingly, 'diversity' (Horn 1974).

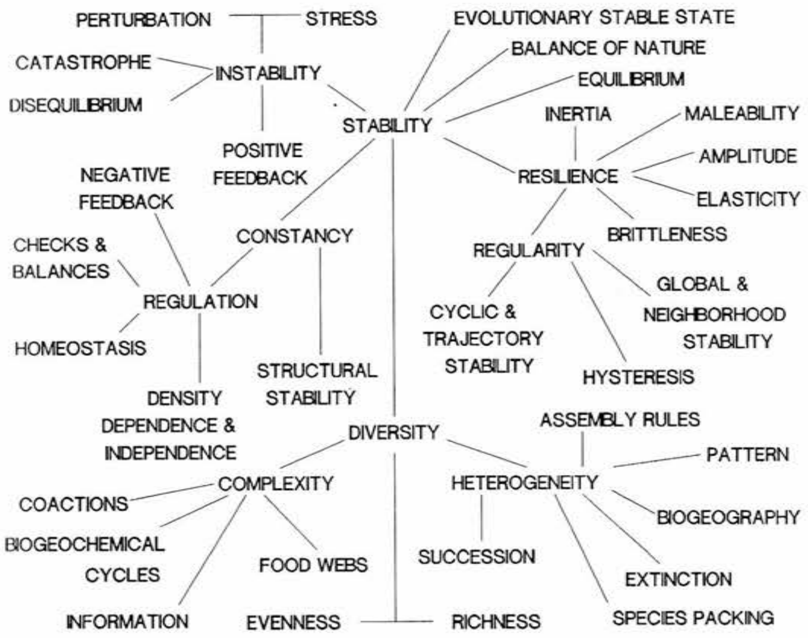

Fig. 4.4. A diagrammatic summary of the relationship among concepts associated with stability. As in Fig. 4.1 and 4.2, these associations are necessarily subjective and incomplete.

Many of these concepts have become the subject of recent criticism. There are fundamental problems in operationalizing 'equilibrium' and 'stability' (Murdoch 1979; Caswell 1978; Connell & Sousa 1983; Wiens 1984; DeAngelis & Waterhouse 1986); Egerton (1973) and Ehrlich & Birch (1967) discuss the difficulties associated with the concept of 'the balance of nature'. 'Diversity' has been termed a non-concept (Hurlbert 1971) and a concept cluster (Peet 1974), and its continued utility has been questioned (Hurlbert 1971; MacArthur 1972a). 'Stress' and 'perturbation' have also been attacked as operationally empty phrases because they can only be defined after the fact, in terms of their effects (Peters 1977).

Stability illustrates several common characteristics of troublesome concepts in ecology (Whittaker 1957). Like all the concepts mentioned in this section, it is a 'concept cluster' (Peet 1974) because it 'conflates' (Lewontin 1979) 'multiple meanings' (Hawkins & MacMahon 1989). In addition, it is a 'pseudo-cognate' (Salt 1979) because a meaning for the term is grasped intuitively, without the onerous necessity of operational definition. Regrettably, different scientists intuit different meanings and failure to define this term has ended in a terminological and conceptual morass. Ecologists have had to use subjective judgments to abstract some partial aspect of the concept. Worse, repetition and familiarity have made us so uncritical of the term that it has become accepted as a concrete property of nature. The vagueness of this and related terms has allowed the elaboration of a grand and complex conceptual system that obscures serious scientific shortcomings.

Non-operational relationships

Because ecologists confuse theory and concept, many conceptual relations in ecology are no longer stages in the development of theory, but alternatives to theory. Thus it may be claimed that a 'theory' can link different concepts, even though these are ambiguously defined. Any statement that involves such terms remains impervious to test and will be abandoned only if the interest of the scientific community flags. A first step in this process is an evaluation of existing relationships to establish their theoretical status.

Diversity–stability

The pursuit of the diversity–stability relationship, described as 'the stability–diversity debacle' (Dayton 1979), provides an example. The debacle refers to the fall from grace, if not the demise, of the view that the stability of ecological entities is positively related to the complexity or diversity of those entities or their environment.

The proposed relation dates at least from Thienemann (Hynes 1970). The concept has a great intuitive appeal because it provides a strong rationale for the conservation of species and species richness (Goodman 1975). Elton (1958) and Hutchinson (1959a) lent their support to the relationship, although the vagueness of both terms made unequivocal support (or falsification) impossible. The tide of scientific opinion quickly reversed after May (1975) showed that complexity and stability were inversely related in model 'random communities' and Goodman (1975) was unable to find good supporting evidence in the published literature. Notwithstanding a call for more empirical studies (McNaughton 1977) and the frank admission that the behaviour of randomly assembled, computor communities may be irrelevant for real ones (May 1974, 1981a), the weight of opinion is now against a positive diversity–stability relation (Auerbach 1984).

Despite its difficulties, the diversity–stability relationship cannot be said to be false. The ambiguity of the literature in its regard and the faddish swings of scientific opinion about it reflect the nebulosity of the terms. Unless these can be defined, the relation will never be falsified.

Competitive exclusion

The competitive exclusion principle is another example of a concept masquerading as a theory (Hardin 1961). This old saw claims that at equilibrium, competing species cannot coexist indefinitely in the same niche. The problem is that all terms are so poorly operationalized that the principle cannot be evaluated. As a result, ecologists have long assumed that it applies and interpreted observations as consistent with the principle (Murray 1986). Coexistence is taken as evidence for disequilibrium (Hutchinson 1959a, 1978), niche differences (Gause 1970; Peterson 1975), or the absence of competition (Pianka 1981); differences between species are said to represent niche differentiation (Abrams 1983; Arthur 1988), and exclusion is said to reflect competition (Connell 1980). Other explanations are almost always possible (Underwood & Denley 1984), but the flexibility of undefined terms has allowed the construction of a tautological argument that can accommodate any observation (Peters 1976), so that no other explanation is required. Indeed, competitive exclusion continues to play an important part in community ecology (James & Boecklen 1984) through extensions like limiting similarity (Abrams 1983), diffuse competition, resource partitioning and species packing (Pianka 1981). The absence of predictive power seems only a minor inconvenience.

Atheoretical concepts

The overwhelming majority of concepts discussed in ecology represent notions that could lead to useful theoretical development, if their various problems were resolved. There are however other concepts which will likely never be phrased as a theory and which should be treated with great circumspection. For example, statements about existence, like 'there is a Loch Ness monster', could be confirmed, but never disproved. The operations which would suffice to show the monster's existence are unknown and the failure of any attempt to prove such an existential statement can easily be explained away. Before existential statements can be used, they must be rephrased as formal theories describing what observations made under what conditions would lead us to consider non-existence more probable than existence.

Table 4.2 *Some environmental truisms that appear to be statements of fact, but are really empty concepts, incapable of test or falsification (from T. C. Emmel 1977).*

Everything is connected to everything else.
You can't change just one thing.
Nature knows best
Nothing goes away.
Dilution is no solution to pollution.
There is no free lunch.

Environmental truisms

A number of non-operational concepts associated with environmentalism are also atheoretical. Although their status as part of ecological science is debatable (Evernden 1984) the familiarity of the phrases in Table 4.2 makes them good illustrative material. These environmental concepts (Table 4.2) look like facts or theories, but they are not. Nevertheless, they are often accepted as such by the public and by policy makers, who therefore expect similarly clear advice from science.

The aphorisms in Table 4.2 should not be confused with scientific knowledge. They represent general notions of very limited scientific utility. This is made apparent by trying to reinterpret these general notions in the form of a theory, relating response and predictor variables given certain conditions (Chapter 2). Such an attempt will fail. The aphorisms do not describe what possibilities are probable under specified conditions. Instead, they invoke seemingly universal statements which are entirely untestable because the meaning of their key terms is never stipulated.

At first glance, a statement like 'everything is connected' is so patently false that the statement would normally be dismissed. However, it is protected from falsification by reference to subtle, indirect, or imperceptible connections of an unspecifiable kind. Thus the failure to demonstrate a connection is not taken as evidence against the statement, but only against the adequacy of the definition of connection used.

Well framed theories make it clear what evidence would be taken as contrary to the theory, so that we realize when the theory has been falsified. In this case, a strong theoretical statement would include definition of the nature of the connection and the operations required to reveal its presence. The inadequacy of non-theories is usually easy to demonstrate by trying to rephrase the construct as a simple Cartesian graph (Fig. 2.3). Such a restatement (Fig. 4.5) would show that there are no preconditions to tell us where the theory should apply, that the axes are essentially undefined because no characteristics have been stipulated, and that the shape of the relation between them is unspecified. The phrases in Table 4.2 are therefore antitheses to science.

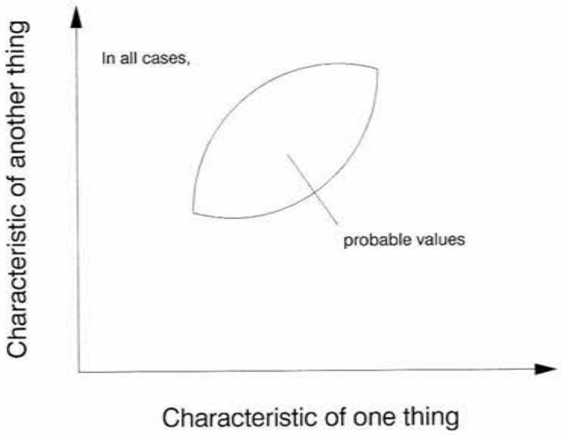

Fig. 4.5. An attempt to rephrase the statement 'Everything is connected to everything else' as a theory in the form of a Cartesian plot (cf. Fig. 2.1, 2.3). Since the characteristics of the 'things' which are addressed are undefined and the region of expected values is not stated, the statement cannot be applied.

The advantage of such formulations is that they are never wrong. This allows environmentalists to claim, with the advantage of hindsight, that their reservations were well founded whenever an environmental project fails. Unfortunately, the problem confronting environmental management is not to criticize acknowledged and obvious failure after the fact, but to predict disastrous consequences beforehand and to provide effective tools so that disasters may be avoided.

A similar analysis can be applied to other statements. This will first show that they appear false: for example, dilution is a solution to some levels and kinds of pollution (like the smoke from a campfire); and some things (like the passenger pigeon or an abandoned apple core) do go away. Second, the analysis will show that there is always an easy escape from falsification because the hypothesis is too poorly phrased to be testable. Finally, it will show that no observation could disprove the statement because the terms are so poorly defined that we really do not know what they are.

Such statements are not an effective basis for scientific testing or for environmental management. They are not intended as pieces of information but as moral strictures for the public and politicians. Proponents of such strictures have made them the basis of ethical and political positions and they may be effective in that role. But they must not become models for scientific theory.

Concepts in ecology: the effects of poor examples

Science is a social activity, so the behaviour of our peers and our leaders has a profound effect on what is acceptable. In ecology, concepts and theory have been confused to the point that the science has been seriously debilitated (Rigler 1975a).

Typological limitations

Birks (1987) identified three distinct problems associated with typological thinking in biogeography, and each has its counterparts in other areas of ecology. According to Birks (1987), typological thinking encourages us to see divisions where none exist, a problem which dogs studies of seral stages (Schoenly & Reid 1987) and of local clinal gradients and mosaics (Shipley & Keddy 1987). Typological thinking also encourages us to identify types which are easily recognized and then to group phenomena which are less easily classified around these types. This appears to be the case for trophic levels, where plants and top carnivores are easier to classify than the bulk of organisms in between. Typological thinking is also easily biased by causal schema, like the organism–environment dichotomy.

Conceptual discussions

Debate and discussion are among the great pleasures of academic life. Regrettably, theories are poor subjects for discussion, because once stated they should be judged on the evidence, not on the quality of exposition. Since concepts cannot be judged on the evidence, they are more appropriate subjects for debate and may be further operationalized in that process.

Prolonged discussions indicate that operationalization is unlikely and that other topics would be more profitable. Participants in conceptual debates should therefore be prepared to conclude that the questions are intractable. Unfortunately, debate makes such a sage decision unlikely because it polarizes opinion and gives all camps some stake in the reality of the question. This mitigates against the realization that the question is insoluble.

Conceptual argument has the advantage that it rarely requires much empirical input. Indeed, discussions can be derailed by authoritative statements of fact. Thus, conceptual arguments are favoured by those who lack the specialist's command of detail: theoreticians, generalists, and novitiates. Writers of textbooks encourage this propensity when they set students to debate questions which do not require any information (Table 1.3). For example, Pianka (1981) suggests that a good 'question for discussion' may be 'Do clusters of functionally similar species exist?' Such a question cannot be answered without operationalizing the terms and setting it in a theory of the form: 'Given that conditions $C_1, C_2, \ldots, C_n$ occur, then functionally similar clusters will be detected as follows . . . '. McNaughton & Wolf (1979) suggest that a suitable 'hypothesis for discussion' is 'What is the importance of competitive efficiency in niche overlap zones in determining the relative abundance of species?' This leads the student to empty discussion in which boundary conditions ('in niche overlap zones'), variables ('competitive efficiency', 'relative abundance'), and the relationships among them are unknown; the nature of hypothesis is simultaneously misrepresented. Futuyma (1979) proposes the following questions, 'for discussion and thought': 'In a resource limited environment, do organisms of different ages compete as much with each other as with others of their own age? Do different age classes specialize on different resources and so avoid competition?'

The only appropriate response to such questions is to demand operational definitions. This lesson should not require so much reiteration, so one suspects that undergraduate discussions frequently proceed in other directions. Such discussions are fun, but they are not science, so this pedagogical device may be a form of misinformation.

Abstraction and scientific interest

The emphasis on concepts in teaching and writing gives an unwarranted lustre to ineffective definition. Ecologists have repeatedly acted as though an intangible abstraction is more appropriate for theoretical analysis than a clearly defined variable or relationship. As a result, contemporary ecology seems a new scholasticism, interminably debating the fine points of unobservables and formalisms. Inevitably, this unconcern has driven a wedge between the specialists and applied ecologists, on one hand, and generalists and theoreticians on the other.

Each group now fears the other holds it in contempt and communication between them is tenuous (Larkin 1978; Kerr 1980; Werner 1980; Rigler 1982b). Thus the specialist loses the context and direction offered by those with a wider view and the generalist loses contact with scientific realities. Neither benefits from such divisiveness.

Reconceptualization of potential variables

The world of everyday deals with a series of very tangible and very important problems which could effect the wellbeing of every person, and perhaps every being on the planet. Besides the global questions of nuclear war, overpopulation, climate change and the ozone layer, there are crucial questions about fish catch, lumber yield, crop harvest, insect damage, pest and disease levels, water clarity, plant height, the abundance of particular species and a host of other very real problems. These concerns may also be seen as concepts, but they differ from those of the preceding section because they are already relatively narrowly defined. Because of this narrowness, such concepts can serve as readily available variables with considerable economic and social importance.

Many of these have already become the basis of successful programs in environmental management and have led to the development of separate schools and faculties producing professional agriculturalists, foresters, and fisheries managers. Many aspects of these problems are still open for study and in general, ecologists welcome such problems because of the greater definition and purpose provided by societal demands.

There can be a more morbid aspect to the ecological analysis of socially relevant problems. Academic ecologists have been trained to approach a problem by abstracting it. Sometimes this is essential to put the problem in the context of the literature and to allow the application of existing theories which address the same problem using other terminological or conceptual variants. However, this abstraction introduces the danger that the original question may be lost, so that the answer generated by the abstraction may be only peripherally relevant to the question. In extreme cases, the abstraction is so abstract that it is no longer operational. This 'reconceptualization' is more likely if the difference between concept and variable is not distinct. Ecologists are capable of substituting a pallid, intangible concept for a perfectly good, concrete variable without realizing that anything has been lost.

For example, if asked about the factors that effect lumber harvest, an ecologist should make recourse to models describing the effects of various factors on lumber harvest (Downing & Weber 1984). However, if this is not appropriate for some reason, then reference might be made to factors affecting net primary production (Lieth & Box 1972), litter production (Vogt, Grier & Vogt 1986), or carbon dioxide fixation (Reynolds & Acock 1985). The relation between these variables and the original question is increasingly tenuous, and the various models hold different implications for management. A matter of considerable importance for the economy and for conservation of our forest resources may rest on the quality and degree of abstraction. A further step leads beyond the pale to discussions about primary productivity, productivity of a given trophic level, or simply productivity, terms which are subject to a variety of conceptual interpretations.

In the case of fish yields, the process of abstraction has proceeded so far that discussions often revolve around non-operational concepts. A concrete question about the likely size of the fish catch has been abstracted to questions about fish productivity, stocks, recruitment, catchability, maximum sustained yield, and optimum yield (Schaefer 1954; Beverton & Holt 1957; Ricker 1958; Gulland 1974; Healy 1984). Few of these terms can actually be ascertained. Because the size of most fish stocks is unknown, it is impossible to estimate catchability (catch/population) or recruitment (population × eggs/individual), or productivity (individual growth rate × population). Because equilibrium has been left undefined, it is meaningless to speak of a sustainable yield (Larkin 1977; Sissenwine 1978) and the supposed catch–effort relationship underlying this concept is apparently illusory (Godbout 1987). Good estimates of future fish catch can be made on the basis of catches in preceding years (Roff 1983) and successful applications of the sustainable yield concept (Ryder 1965, 1982) owe much more to empirical definitions based on observation than to the concepts to which they pay lip service. There is a desperate need to publicize and analyze successful applications of ecological knowledge in applied areas like forestry, fisheries, agriculture and public health, so these may serve as examples of a useful ecology.

Most ecologists use practical rationales and potential applications in their grant proposals. Most would like to be used by their society because they feel that the preservation of the earth demands an ecological perspective (Southern 1970; Andrewartha 1984). These examples are therefore particularly sad because they represent attempts by ecologists to answer questions of social concern and importance. Too often, these have failed because the poor examples provided by ecological practice and training have left ecologists incapable of answering practical questions. Regrettably, the particular ecological perspective we have been able to provide has proven so ineffective that we have undermined our own credibility (di Castri & Hadley 1986; Simberloff 1987).

Summary – The costs of non-operational concepts for ecology

Although non-operational concepts play an important role in the creation of scientific theory, scientific relationships must be built upon operationally defined classes, variables and relationships, because operational definition relates the constructs of science to the phenomena of the real world. Nevertheless, ecology is dominated by complex and inadequately undefined terms which confound the development of predictive theory. As a result, ecological classifications, ecological characteristics and ecological relationships may refer to phenomena that vary with each change in focus, scale, or author, and ecologists are often not sure they are talking about the same thing. The solution to this problem is to embed concepts in a web connecting these entities to methodologically defined relations and to the confirmational tests offered by theory and prediction.

The absence of such controls in ecology allows superficial association of concept with a diverse set of concordant observation, concept, and theory such that an entire conceptual system may be erected and confirmed with observations. In ecology, such conceptual systems obscure the absence of theory and the impossibility of falsification. This same looseness makes any definition of the limits of the concept impractical, so different uses of the single term and different terms blend into another. Thus one term may come to represent a multiplicity of divergent, even opposing, meanings. The complex concept represented by this term can then be fragmented into smaller concepts, which may receive terms of their own. Terminological proliferation thus attends conceptual fragmentation and a welter of terms often signals underlying operational difficulties.

Science may always begin with a concept, and the presence of vague concepts may be essential to both operationalization and the social discourse of science. However, successful sciences go beyond this primal stage to develop variables and theories. Because ecology has been so halting in its development, it is perhaps time that we consider our current concepts more critically, removing those which have proven repeatedly futile from our teaching curricula, from our research proposals, from our future publications, and from our minds.

5 · Explanatory science: reduction, cause and mechanism

The view of science underlying the criticisms of this book (Chapter 2) is a form of 'instrumentalism' (Popper 1983), because the critique views scientific theories as 'instruments' or tools to predict and control the behaviour of our environment. More extravagant claims – for example, that theories are 'true' or 'realistic' in the sense that they capture some essence of the universe, are not made because those claims seem unnecessary, unwarranted, and confining. They are unnecessary because any finite set of observations is consistent with a potentially infinite set of theories, so predictive success cannot establish a theory to be true, even if truth should imply predictive success. The claims are unwarranted because history shows that the theories of science have been revised many times and because philosophy suggests that even the strongest theories be considered hypothetical or conjectural, and possibly ephemeral. They are confining because once a scientific construct is accepted as true, it becomes dogma, stifling theoretical improvement and scientific growth. Scientists are never entitled to conclude that successful theories are true. They can only make the modest claim that the theories which worked in the past are more likely to do so in the future than theories which failed in the past. For this reason, successful theories make more reliable tools.

Loehle (1983) recognized this dichotomy in distinguishing two kinds of ecological models: 'calculating tools' and 'theoretical models'. In a wider ecological context, calculating tools correspond to the predictive scientific theories described in Chapter 2, for they are intended only to inform us about the configuration of the world and are judged solely on predictive success. Theoretical models embody universal statements and mechanisms, and Loehle (1983) suggests they may prove useful despite predictive failure by virtue of their elegance, completeness and explanatory power. In the wider context, this class corresponds to those 'explanatory' concepts and theories that satisfy a widely felt need for plausible, causal descriptions of nature, even if these explanations are ineffective in prediction. Such explanatory constructs may be useful